

REVIEW  
for qualifying work of the student of SPbSU  
Fisak Elena Maksimovna on the topic:  
"Seasonal dynamics of the *Zostera marina* community at the White Sea"

One of the features of the White Sea that hit me at a close acquaintance with it was a pronounced seasonal change of its appearance. In spring, summer, autumn and, especially, in winter, the sea coast is always different. At the same time, the White Sea is prone to tidal changes in the water level, sometimes forming extensive drainage zones. Obviously, the intertidal is fully affected by seasonality. However, until now the studies of the seasonal dynamics of the population of the White Sea littoral are being exhausted by the work of 2-3 scientific teams. Partly, this is due to the methodological difficulties of benthic works during the autumn-winter period, in part - to the specifics of work at the biological stations at the White Sea. Therefore the seasonal observations of an important component of the silt-sand littoral - communities with the dominance of the seaside *Zostera marina*, undertaken by the author of the present work become extremely valuable.

Three small areas were chosen as the model range (however, the map does not indicate a scale or a grid, so you can judge the size only by trusting the indications of the natives) or a sandy littoral in the area of the "Belomorskaya" Biological Station of SPbSU. One of them was deprived of *Zostera* thickets, the other two were characterized by thickets of varying severity (although this is not quantitatively analyzed). The applied methods of collection and processing of samples do not allow us to doubt the reliability of the material obtained. It is unclear just why it was necessary to select five samples by a diver, in which the "parameters of the *Zostera* itself" were evaluated, because the author completely ignores the results of processing these samples. In addition, as far as I understand, the plots are very small in area. Was the author evaluating how the successive sampling of a large area affected the sea grass settlements? Was the sampling within the sites localized? The situation with the statistical processing of the material is somewhat worse. Based on the material provided, it is unclear whether the applicability of the variance analysis, which assumes the independence of observations, was tested and observed (especially when "Dates" is included as a predictor)? Are the residuals of the regression model normal? And when using ratios, as in case of analysis of individual trophic guilds? What measures of similarity were used in the application of multidimensional methods of analysis, by what matrices they were calculated? Why were such indices of biodiversity applied (Shannon and evenness indices)? The results of the analysis are not fully presented, in particular, the degree of freedom is not indicated in all the tables. In my opinion, it would be extremely interesting to analyze the interaction of the factors of spatial and temporal variability, and not just their consideration singly. This is a good tool for assessing synchronicity in the dynamics of indicators at different sites, and with existing data it is quite possible.

Let us pass to the results. The author rather skillfully operates with them, arranging a generally logical picture of the influence of *Zostera* on the communities of macrozoobenthos. However, unfortunately, we do not see any estimates of the influence of the dynamics of the abundance of the *Zostera* declared in the working hypothesis, and indeed we cannot see it, because this abundance is not numerically characterized either in dynamics or in statics. The obtained data does not always fit into the existing ideas about the influence of the vulture on the macrozoobenthos communities, and here there are not enough examples of close, White Sea ones. In particular, five kilometers from the St. Petersburg State University biological station, there is an exploratory polygon of the Zoological Institute of the Russian Academy of Sciences, which has been monitoring the littoral communities for more than 30 years, the dominant plant species being *Zostera marina*. With a more thorough study of the results of these studies, there would most likely be general patterns that could shed light on the important question of the effect of this sea grass on the littoral population. Unfortunately, the limitations of the presented results are not given in the submitted work. In fact, despite the frequency of the surveys (by the way, was it determined only by methodical convenience, or did it have any biological basis?), It is still 1 year of observations, and, as shown by A.D. Naumov ( Naumov , 2013), the interannual variability of seasonal dynamics can be very significant.

Let's note less significant drawbacks of the work. What is deferred along the axes in Figure 4 and onwards? Average and errors? Confidence intervals?

It is completely incomprehensible that it is encrypted in algebraic expressions of type  $3.83E-15$  in the column of values  $p$  of table 1.

Why is the inverse quasi-logarithmic scale on the abscissa in Figure 5 applied?

Since the "Date" factor influences reliably, why on the histogram 6 all dates are combined?

Despite the references cited, I do not agree that *Peringia ulvae* is a phytophagous. Rather, judging by the arrangement of the radula (Fretter & Graham, 1996), it is a gathering detritus. In the case when it is one of the mass species, I would recommend more careful attitude to the appropriation of trophic guilds.

In the text several times the term "environmental engineers" occurs, in my opinion, the term "edificators" that has settled in Russian literature is more appropriate. And even more so, the "effect of ecosystem engineering" looks defiant Anglicism.

Links in the text do not always correspond to the sources given in the "Literature" chapter. They are not uniformly decorated (for example, "Lee et al.", and "Moore et al."). Some sources are missing, although there is a link to them in the text. The list itself is written carelessly and not completely in alphabetical order, which complicates the work of the reviewer.

Despite the revealed shortcomings caused, in my opinion, by the inexperience of the author and the complexity of the topic chosen by her, the work is quite consistent with the necessary requirements for the work, the results after some refinement can be recommended for publication.

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