

Interactive comment on “Temporal and spatial distribution of the meiobenthic community in Daya Bay, South China Sea” by L. Tang et al.

Anonymous Referee #3

Received and published: 19 June 2012

This manuscript aims at describing the temporal and spatial distribution of meiobenthic communities from a shallow bay located in the south China Sea. Altogether, nine stations were sampled during four seasonal sampling campaigns, each during autumn, winter, spring and summer. The meiofauna samples were analyzed at the level of major taxa and apart from abundance, several other biological attributes of the community were considered such as diversity and biomass. In addition to the faunal parameters, a number of environmental characteristics were also measured, both from the water column and the seabed.

Overall, this is a very comprehensive work presenting a large and important data set. The sample and data analysis have been performed by using standard methodology. However, while the work is important and from an area where only limited information's

C606

are available, the manuscript fails to deliver anything new into meiofauna research, mainly because of its purely descriptive nature. The spatial and temporal variation of meiobenthic communities at the major taxa level has been documented repeatedly over the last two decades and this paper goes through it again at a very basic level to obtain results similar to other areas of the world. There is nothing wrong with this approach but it doesn't add much, if anything, to our knowledge. Modern meiobenthology should be done at a lower taxonomic level (e.g. genus, species). Nevertheless, the authors may be lacking the taxonomic expertise and since they seem to have put a lot of effort into this work and their results may be useful at a local/regional scale I could recommend it for publication. It also fits nicely within the scope of the journal. There are however several points, as outlined below, which need to be considered before the paper can be accepted.

1. One of the main problems is the lack of a clear concept or hypothesis linked to the work. This is particularly obvious in the Introduction which is rather pure and has the form of a general introduction of a technical report. While the data presented here may be new for the south China Sea, there are several other papers from tropical areas and these should be properly reviewed in the introduction. It would be also nice to link some hypothesis to the study, e.g. would we expect the China Sea to be different compared to other similar areas or are seasonal differences in sub-tropical areas milder compared to other climatic zones etc. In addition, even if no other meiofauna data from the area exist, I am sure there should be other biological data available. For example the authors refer to the work of Wang et al., 2008 but they don't provide an overview of what has been found from others and what is expected from their own work. In general, the authors should make an effort to place their work within a more general concept taking into account the specificity of the area as well as research that has been done by others.
2. It is not clear how the stations were selected. Was it on the basis of geographic coverage, depth or just random?
3. With respect to the analysis, particularly the anova test, the authors should make an effort to make clear what they did. For example, several times they mention that they found significant interactions

C607

(e.g. section 3.3. line 12) but it is not clear if the differences they report are from a separate one-way anova within each level of the interaction term, which is what one should do if there are interactions. In general, it would be useful if these results, including the interactions, are reported in the form of a Table. 4. The graph style chosen by the authors could be improved, particularly the ones created by Surfer. While these graphs are nice to present data such as salinity, temperature etc. they are not suitable to present biological data as they are difficult to read. 5. From the graphs and the text, the reader does not get a clear picture of what exactly seasonal changes have been found, although the authors claim that they have investigated temporal dynamics. In the text, the reader gets confused as some parameters show peak values during one season while other parameters show peak values in other seasons. No clear trend is apparent. This is made worse as in none of the figures one can see which sample corresponds to which season. This problem can be also clearly seen in the conclusions where seasonal conclusions are virtually absent (with a small exception of conclusion No. 5 but see next point). 6. In the final conclusion (No. 5) the authors state that the observed seasonal changes are caused by anthropogenic activities however this statement appears somehow out of the blue as nowhere else in the document there is any reference of such activities. 7. Fig. 5 and Fig. 6 show the same results and their only difference is that they are made with different analysis techniques. Why do the authors show both? Usually cluster is suitable for biological data and PCA for environmental data, so it is not clear what additive value they have. Another problem is that in almost all the temporal graphs, the seasonal effects are not visible because the authors didn't use any specific coding for the different seasons. The only exception is the second MDS graph in Fig. 9. I would suggest to use a coding with different symbols for the four seasons sampled. 8. Finally, the authors used some diversity indexes to characterize the diversity of the meiofauna community, disappointingly however they used them only to perform some correlation analysis against the environmental data. No other diversity aspects are discussed or presented.

C608

Interactive comment on Ocean Sci. Discuss., 9, 1853, 2012.

C609