

Dear Mr. Vandromme,

you present an extensive dataset of 7 years (2005-2012, 2008 missing) of zooplankton abundance and size spectra (from LOPC (816 stations) and WP2 (89 stations, mainly 2009-2011)) from the Bay of Biscay. In the abstract you write that this study will provide a new look at regional and inter-annual variability, and it clearly has the potential for that. However, I agree with Dr. Bachiller that the message you try to bring across is hidden in a manuscript focussing (too) extensively on methods.

Methodological considerations:

The proposed correction of LOPC counts from the WP2 has some major weaknesses.

1. No information is given on the possibility of net clogging, but 100% filtration efficiency is assumed at all stations, even at coastal ones with high chlorophyll concentration as far as I can see from the data. Later (p2228, line 25ff) you mention that chlorophyll concentration helped to improve the explanatory power of the analyses and interpret this to confirm that the major difference between WP2 and LOPC can be explained by particulate organic matter. If net clogging were a problem, the WP2 might have under-sampled zooplankton to a greater extent at stations with high chlorophyll concentrations, thus increasing the discrepancy between LOPC and WP2.
2. You cite the study by Petrik et al. (2013), which allows to separate different fractions of the plankton community, but make no effort to compare your corrections to those obtained by the methodology of her study. This could increase the credibility of your corrections and could also give an indication on net clogging.
3. The comparison is based on sub-sampled stations from 4 years, but is extrapolated to be valid for all stations and years. It would be helpful to see which factors influence the correlations. Little information on the taxonomic composition of net samples is given, but of those listed in Table 4, most will be under-sampled dramatically by the 200µm-WP2 (*Acartia*, *Cladocera*, small calanoids).

Statistical analyses are sound, but the description is too extensive, as Dr. Bachiller already noted.

Structure & Language

The manuscript structure is not very clear. There is a frequent mention of methodological issues in the results and the discussion, leading to re-iteration of many points and the focus being drawn towards Methods. Unfortunately the ecological significance of the factors influencing the size spectra and thus the energy flow within the pelagic ecosystem are not presented in detail, neither in the results nor in the Discussion. The structure of the discussion is weak, many parts should be moved to the Methods, and remaining methodological issues can be shortly mentioned in the beginning.

Language: Frequent mix of present and past tense and inappropriate use of these. In general, Methods and Results sections should be written in past tense, while facts that are proven (published) should be referenced in present tense. Use of non-English words (e.g. lisibility, losanges) and incorrect grammar in many places. This needs to be corrected.

Discussion:

It would strengthen the ms considerably if the observed size spectra and their variability would be discussed in the light of temporal variability (pre-bloom, bloom, post-bloom), in relation to the hydrography, and in connection to other studies (references!). Broadening the discussion in that way would also allow to compare results to areas elsewhere, thus making the study more relevant to the research community outside the Bay of Biscay. The spatial resolution across the Bay with strong ecological gradients is relatively coarse (~20 nm), and does not allow to resolve spatial patterns across the shelf. Though this is mentioned, the description of this (p2226, l23ff) is very unclear.

P2232, l11: There exists an in situ instrument measuring the size range of ca. 1-300 µm ESD, the

LISST (Laser In Situ Scattering and Transmissiograph, Sequoia Scientific), which has been used with success e.g. Mikkelsen et al. 2005 (doi:10.1016/j.csr.2005.07.001).

Figures: Quality of figures is good, but there is an overweight of methodological figures. Fig. 2 can be removed. Also of Figs. 8, 9 and 10 only one could be selected.

Based on the above and the general ecological interest of the extensive study I recommend to publish this manuscript after major modifications in the comparison of WP2 and LOPC, and also in manuscript structure and language.

With kind regards,
Sünnje Basedow