Biogeosciences

Discussions

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Interactive comment on "A novel approach reveals high zooplankton standing stock deep in the sea" *by* A. Vereshchaka et al.

A. Vereshchaka et al.

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Dear Editor and Referees, Attached is a new version of the Ms, all new/significantly corrected parts are in blue/red. See detailed comments below.

Yamaguchi (Referee) a-yama@fish.hokudai.ac.jp Received and published: 18 June 2016 This manuscript provides biomass and taxonomic compositions of meso- and macrozooplankton down to greater depths. Correlation analyses between surface chlorophyll a and standing stocks of various large-sized zooplankton are quite interesting and including important issues. Dominance of shrimp in deep-layer is not fully documented previously. This study showed their importance clearly. The message of this study is clear, robust and easy to understand. Following my comment is very minor.

My only concern is at Discussion on net avoidance. For me, mentioning that vertical tow is more robust for net avoidance than that of the horizontal tow is anomalous. While discussion on this subject (net avoidance of shrimp) is not at main focus of this study, their effect may have large impact for implication of this study. Authors' comment 1: We thank Dr. Yamaguchi for his comments and for a high evaluation of our work. Concerning the net avoidance, we just try to explain why vertical hauls give evidence for higher SHRIMP (only shrimp) biomass values (respective to the horizontal hauls). We feel that our explanations based on visual observations may make sense. As a possible compromise, we have removed the last sentence "We propose the use of vertically, not horizontally, towed large nets for more representative assessments of deep-pelagic shrimp abundance". We have also added the following paragraph: "In contrast to decapods, pelagic fishes escape in horizontal direction, as has been observed from submersibles many times by the authors. This reaction is successful when vertical hauls are used and our results are thus not representative for assessment of the pelagic fish biomass. This biomass may occur to be finally correlated with Chl but horizontally towed gears are necessary to prove that".

Interactive comment on "A novel approach reveals high zooplankton standing stock deep in the sea" by A. Vereshchaka et al. Anonymous Referee #2 Received and published: 28 July 2016. This manuscript presents an interesting attempt trying to explain deep sea zooplankton biomass with surface chlorophyll concentrations. The importance of the conclusion is clear as the ability to predict deep-sea zooplankton biomass from satellite-derived proxies would be very useful. However, the results are not fully presented and cast doubt on the overall conclusion.

In particular: - what is the actual data coverage? - how many nets were deployed in total? - how many organisms were counted? - what seasons have been sampled? - what are the uncertainties on your estimates? A detailed list and some metadata analysis (beyond Fig 1 and 2) would be useful here. Maybe a table in the supplementary material (e.g. with sampling date, time and location, temperature, surface Chl,

volume samples, number of individuals counted)? Authors' comment 2: A new Table 1 including additional information about samples is now included.

Also, a figure similar to Fig 2 but with biomass instead would be useful. Authors' comment 3: This figure may be given for a certain transect only, not for the whole dataset discussed. Part of data along a submeridional transect (36th and 37th Cruises of the R/V " Akademik Sergey Vavilov ") has been analyzed in detail and published with transect in a more specialized journal; see also comment 5.

A scatter plot of the raw data (biomass vs Chl) is needed to properly evaluate the correlations summarized in Table 2. Authors' comment 4: Done as a new figure 5. In addition, Table 3 is redone to represent more information about regressions (coefficients of determination ($R2$), equations, and levels of significance).

Over 300 taxa were identified but the data was then combined to three vaguely defined groups. I am surprised that community composition is not presented, and I think it would greatly enhance the manuscript. Authors' comment 5: In fact, the whole database of this work contains two different datasets: (1) data of 2012-2013 (R/V "Akademik Sergey Vavilov", mainly Central and South Atlantic) and (2) data of 1994-2005 (R/V "Akademik Mstislav Keldysh", mainly North Atlantic). Samples have been taken with the same protocol, but identification was much more precise for the first dataset. The community composition, diversity, and other community patterns have been analyzed in detail for the first dataset and presented in a recent paper (A Vereshchaka, G Abyzova, A Lunina, E Musaeva, 2016. The deep-sea zooplankton of the North, Central, and South Atlantic: Biomass, abundance, diversity. Deep Sea Research Part II: Topical Studies in Oceanography. DOI: 10.1016/j.dsr2.2016.06.017). The second dataset contains representative biomass values and significantly contributes to the metadata concerning deep zooplankton; here we combine both datasets for a more comprehensive analysis. Now we clarify the situation in the Methods section.

Throughout the manuscript, Chl concentrations and primary production are used synonymously. They are not the same and should therefore be discussed more carefully (e.g. L53). Authors' comment 6: Corrected

L120ff: Is this contribution normalized for the depth horizons? Otherwise it is not surprising that the integrated biomass in the bathypelagic (1500 to 3000 m depth) is larger than in the mesopelagic (200 to âĹij600 m depth). Indeed Table 1 shows that biomass per m3 was highest in the upper bathy-pelagic. This difference and the implications are worthwhile discussing. Authors' comment 7: No, contribution is not normalized for the depth horizons, as we clearly consider standing stocks. The contribution of different depth layers is not obvious: could the anonymous reviewer foresee that the standing stock in the upper bathypelagic (ca. 700 m thick) is much higher than in the more rich mesopelagic (ca. 400 m thick) and even more productive epipelagic (200 m thick)? By the way, Table 1 shows that biomass per m3 was highest in the epipelagic followed by the upper bathypelagic (NOT mesopelagic) for the total plankton and most groups.

Potential time lags between surface chlorophyll concentrations and biomass in the deep sea have not been discussed. You do not explain why you excluded data from temperate waters (L74). A justification is needed. Authors' comment 8: We have added in the "Method" chapter the following sentence: "We excluded data from temperate waters where the major spring peaks in primary production are being exported from the euphotic zone (0–200 m depth) and reaching abyssal depths (4000 m) with a significant time lag (e.g., 42 days: Smith et al., 2002); this lag differs for different depth zones that may corrupt possible correlations".

The introduction should give a fairer representation of the literature. It is, for example, not true that information about deep-sea zooplankton is available for the North Atlantic only (L37). Authors' comment 9: L37 is an awkward construction, which is now corrected and a greater reference list across all Oceans is provided in the Introduction; for example, the following sentence is included: "Studies on the deep-sea plankton biomass at selected sites include those in the North Pacific (e.g., Vinogradov,

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-145/bg-2016-145-AC1-supplement.pdf

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