

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)?

C1

Also, a figure similar to Fig 2 but with biomass instead would be useful.

A scatter plot of the raw data (biomass vs Chl) is needed to properly evaluate the correlations summarized in Table 2.

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.

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).

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 ~600 m depth). Indeed Table 1 shows that biomass per m³ was highest in the mesopelagic. This difference and the implications are worthwhile discussing.

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.

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).

Minor comments: Throughout the manuscript, it would be useful if the type of biomass that is referred to would be made clear (e.g. Table referring to wet weight?).

You used a 500- μ m net (L82) but only present data on zooplankton >1 mm (L60). Why is this?

C2

Overall, this study is very interesting and the results could be important. However, the data presentation and discussion need work to give the overall conclusion credibility.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-145, 2016.