

## ***Interactive comment on “Distribution of known macrozooplankton abundance and biomass in the global ocean” by R. Moriarty et al.***

**Anonymous Referee #2**

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The way I understand the purpose of the paper is to collate abundance and biomass observations on marine macrozooplankton (> 2 mm as adults) for contributing data on plankton Functional Types (FTP) for use in global biogeochemical models (Dynamic Green Ocean Models, DGOMs). I regret observing that macroplankton is not a natural group but a man-chosen size fraction. It comprises herbivores (salps), as well as carnivores (two FTPs). Also, the models to my knowledge do not use abundance, but biomass (carbon) values. Granted these conceptual weaknesses, I am surprised about the omission of biomass/carbon observations for copepods, among which many dominant species of the upper layers in cool-temperate and subpolar seas are 2-5 (8) mm long. On the other hand, the biomass data for the HOTS and BATS stations represent mesozoo-plankton (Landry et al., Madin et al., and Steinberg et al., as cited), but not macrozooplankton as in the ms. and the figures.

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Quite dismaying are the large blank regions in Fig. 1b. Excepting the thorough study of the Southern Ocean, the authors have not found the existing macrozooplankton data for most of the open sea in all three oceans, which are emphasized by the most of the current DGOMs. These biomass observations may not be in the data bases used by the authors but they should have looked for them. I do not think that the conclusion in the third-fourth lines of section 4, p.13, (“an insight in the distribution of macrozooplankton from the poles to the tropics has been gained”) is justified. Also, I do not see the relation of specimen numbers to studies of biodiversity as alluded to.

Other weaknesses of the ms. are citing authors in Table 1d without listing them in the References, mislabeling Fig. 2c, and including the in my view useless juxtaposition of the Northern and Southern hemispheres in Figs. 2e and f, which moreover clearly show non-significance. What are we to do with the global annual median of epipelagic macrozooplankton of 0.02 Pg C, which comprises herbivores and carnivores? What lessons are modelers or other readers to draw from the paper?

I recommend against publication.

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