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Interactive comment on “Major constrains of the pelagic food web efficiency in the Mediterranean Sea” by L. Zoccarato and S. Fonda Umani

Anonymous Referee #3

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General The present paper addresses food selectivity of microzooplankton (MZP) and major constraints modulating carbon transfer efficiency to upper trophic level in the Mediterranean Sea by analyzing large dataset consist of results of more than 80 dilution experiments conducted along productivity gradient from oligotrophic to eutrophic environments. The insights obtained from the large dataset, which includes not only published data but also unpublished, will be useful and be fit for interests of BGD readers, because knowledge about efficiency of transfer prokaryotic carbon production to higher trophic level than MZP is limited. However, I feel that authors' definition of MZP is too broad (10-200 μm), which includes both nano- and micrograzer. To describe in an extreme manner, by changing the size range of MZP, size composition of their food particle will change. Change of food selectivity of MZP in this study may be shift of size of nanograzer included in MZP. This issue may be not important because readers can

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use the results considering the size range of MZP. But I feel that broad size range of MZP somehow lower the value of the results because change of MZP size may result in change of a number of trophic level between MZP and large predator (for example fish). Anyway, the insights in this study will be useful and be fit for interests of BGD readers. I recommend this manuscript, if some concerns described below are clarified.

Major Point Authors approximated the relationship between ingestion rate and biomass of food particle by sigmoidal function (Fig. 5). If ingestion rate is expressed as per individual, I can easily imagine the reason why high food concentration saturate ingestion rate of individual. For individual food handling time can limit ingestion rate under high food concentration. Do you have any idea to explain a mechanism to saturate total ingestion rate of whole of MZP assemblage under high food concentration? And I hardly understand a reason why “critical threshold” should be considered. Authors discuss food biomass at minimum ingestion rate as if it is equal to threshold for growth. Why should it be? If authors have reasons, clarify these in discussion part. Furthermore, authors should present criteria for estimation of thresholds and error of two thresholds, if authors claim the importance of sigmoidal curve.

Minor points 1) P 4376 Line 11: Should grazing efficiency be grazing rate? Is grazing efficiency grazing rate per available biomass? 2) P4377 Lines 1-10: R-square should be presented rather than “r” in order to show how well data fit a statistical model. And probability should be presented. 3) P4381 Lines 8-11: Any reference? 4) P4382 Line 24: Authors should present object compared with 0.5 ug C/L. I agree that the value, which corresponds to 25000 cells.ml, is lower than threshold for HNF growth in Andersen and Fenchel (1985; Limnology and Oceanography 30(1), 198–202) and Wikner and Hagström (1991; Limnology and Oceanography 36, 1313–1324). But authors should explain how authors estimated “0.5 ug C/L” as describe in Major point. 5) Fig.1: Station name should be added in the figure, although readers can take information from Tables A1 and A2. 6) Figs. 3 and 4: Unit for ingestion rate per prey biomass (d-1?) should be presented. 7) Fig. 8c: Is label and unit of Y-axis “Ingestion-ug C L-1”?

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