

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

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Received and published: 11 May 2015

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 The insights obtained from the large dataset, which includes not only published data but also unpublished, will be useful and be fit for interests of BGD readers, because knowledge about efficiency of transfer prokaryotic carbon production to higher trophic level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  $\mu\text{m}$ ), which includes both nano- and micrograzer. To describe in an extreme manner, by changing the size range of MZP, size composition of their food

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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 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. ANSWER: We agree with the observation of the reviewer but it must keep in mind that in the traditional dilution experiments researchers include in the microzooplankton also nanoplankton. We tried to at least disentangle the effect of nanoplankton ( $<10 \mu\text{m}$ ) in respect to the whole micro+ nano community. In the new version we stated: “We are aware that results of MZP dilution experiments include the effect of viral lysis (Parada, 2007; Fonda Umani et al., 2010; Di Pol et al., 2013) and the mortality due to NP predation (e.g. Stoecker et al., 2013). To partially solve this latter problem we performed parallel experiments to estimate the predation of NP alone. We can expect three different models of interaction: i) only NP graze on picoplankton, therefore the ingestion rates calculated in NP experiments are the same obtained in the MZP experiments; ii) MZP grazing on NP reduces the ingestion calculated for NP alone; iii) MZP directly feed on picoplankton, and consequently ingestion rates obtained for MZP experiments are higher than for NP experiments (Fonda Umani and Beran, 2003).”

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

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part. ANSWER: We thanks the reviewer for these questions and we changed the discussion in order to include our possible explanation as follow: “Lastly, testing several functional response models to describe the feeding behaviour of consumers we highlighted as generally grazing activity of MZP (at the surface) and the potential grazing activity of NP (at the surface and in the meso-bathypelagic layers) correlated with the Holling Type III model. Furthermore only MZP on MPP and NP on HP correlated with the Ivlev model. In the sigmoidal curves the inferior range of low ingestion rates, not coupling with slight biomass increases, were detected mainly in oligotrophic conditions and in meso-bathypelagic environments. It might be explained with the dilution of available preys that reduce the prey-consumer encountering rates (Pastor, 2008), that can induce predators to use other food sources (Strom et al., 2000). The high threshold instead occurs only in eutrophied conditions for MPP, and in all trophic conditions for the other preys. The observed satiation threshold can be interpreted as the result of the individual inability to handle higher prey availability as suggested also by a modelling-approach study of Gentleman and Neuheimer (2008). A possible explanation is a delay in the match of consumers’ growth with prey increases. These findings however need to be tested with larger datasets that include more data from ecosystems characterized by high production and ingestion rates.”

Furthermore, authors should present criteria for estimation of thresholds and error of two thresholds, if authors claim the importance of sigmoidal curve. ANSWER: We thanks the reviewer for the suggestion and we add in methods a detailed explanation of the curves’ fitting and on statistic test used to assess the significance and quality of that curves. “The relations between ingestion rates and available biomasses of each kind of prey were investigated for MZP and NP. The functional responses of the ingestion rates over a wide range of prey concentrations were examined against four common models: (For the equations see the attached new version of the manuscript). where  $I$  and  $C_0$  are ingestion rates and biomasses estimated in each dilution experiment,  $\alpha$  and  $\beta$  are constants and represent respectively the maximum rate of ingestion and the rate at which  $I$  changes in relation with  $C_0$ . The values for  $\alpha$  and  $\beta$  that minimize the residual

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sum-of-squares in each equation (4, 5, 6 and 7) were computed with the Nonlinear Least Squares function implemented in the stats package of R. Only fitting models whose parameters were significant ( $p$ -values  $< 0.05$ ) were considered and compared by the analysis of variance (ANOVA) and by the maximum likelihood to the same data (with the Akaike information criterion – AIC, and the Bayesian information criterion – BIC) to evaluate the fitting quality of the models.”

Minor points – 1) P 4376 Line 11: Should grazing efficiency be grazing rate? Is grazing efficiency grazing rate per available biomass? ANSWER: We better clarified the ingestion efficiency in the methods as follow: “The ingestion efficiencies of MZP and NP were calculated for each prey by dividing the ingestion rate by the corresponding preys’ potential production estimated respectively in the MZP and NP dilution experiments. Potential production is considered a good proxy for primary production (Calbet and Landry, 2004).”

– 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. ANSWER: We repeated part of the data analysis including the curves’ fitting. We change the simple regression of a polynomial curve with the fitting of 4 functional response models. Significance and quality of the fitting were addressed by Nonlinear Least Squares function, ANOVA and analysis of maximum likelihood (with the Akaike information criterion – AIC, and the Bayesian information criterion – BIC).

– 3) P4381 Lines 8-11: Any reference? ANSWER: In order to produce a concise and more focused discussion section we delete this lines.

– 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.

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ANSWER: We thanks the reviewer for the suggestion. The two paper described the minimum bacteria concentration that sustain grazing process at the surface, however in our dataset for the surface and meso-bathypelagic layers we do not have enough data of bacterial ingestion at very low biomass (in the range corresponding with the low threshold of the sigmoidal curve) so in the reviewed manuscript we decided to do not state any low threshold for the consumers.

âĀĀ 5) Fig.1: Station name should be added in the figure, although readers can take information from Tables A1 and A2. ANSWER: We thanks the reviewer for the suggestion, in the new maps we added the labels of the stations.

âĀĀ 6) Figs. 3 and 4: Unit for ingestion rate per prey biomass (d-1?) should be presented. ANSWER: We made new Figure 3 and 4.

âĀĀ 7) Fig. 8c: Is label and unit of Y-axis "Ingestion-ug C L-1"? ANSWER: We made also a new Figure 8 (now Figure 7).

Please see the posted new version of the manuscript.

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Interactive comment on Biogeosciences Discuss., 12, 4365, 2015.

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