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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 #1**

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This MS aims at presenting an overview of the trophic efficiency of the microbial food web along the Mediterranean Sea. The study is based mostly on previous data, but some new data are also included (although there is no clear distinction between both data sets). The approach used is the dilution technique of Landry and Hassett (1982), which has been modified to also account for the grazing of nanoplankton on picoplankton (presumably prokaryotes, although this is not clear in the text). Here we find one major problem of the study. The authors claim, and have published previously, that by truncating the food web at the 10  $\mu\text{m}$  level the rates obtained with a dilution grazing experiment will be those of heterotrophic nanoflagellates on smaller prey. First of all, nanoflagellates would not be included in the bottles according the authors definitions. Even ignoring this mistake, the rates obtained by this procedure would only be accurate if the removal of the > 10  $\mu\text{m}$  fraction had no consequences on the grazers and prey.

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This is not the case because grazers and prey within this fraction are usually under a strong grazing control from microzooplankton. By removing this group (microzooplankton) the rates of nanoflagellates on other prey are simply not representative of actual rates.

Another major problem of the study is the lack of clear hypotheses and a concise statement differentiating the conclusions of the work from previous ones obtained from the same/similar dataset. Added to this is the clumpy mixing of data from several seasons and locations, including in the same bag data from surface and from bathypelagic zones. These approximations only make sense when the data set is extensive enough, which is not the case here.

A recurrent problem in the text is also the incorrect use of the term trophic efficiency, here defined as the ingestion rate divided by the biomass of grazers. This is not a measure of efficiency; it is an approximation to the standing stock of one prey consumed per day. Added to this point is the lack of ecological meaning of the results; most times the microzooplankton grazing surpassed by far the standing stocks and potential production of prey. Should we allow something for settling, especially under eutrophic conditions?

Finally, and I hope this is just an omission of the methods, no controls without nutrients seems to have been prepared. Therefore, no actual estimate of in situ phytoplankton growth is possible.

Detailed comments

The introduction fails in properly addressing the need for the study. It jumps between subjects without a logical flow that drives the reader to consistent hypotheses. There are also some inconsistencies; for instance, classic food web cannot predominate in meso- and eutrophic conditions if microzooplankton are targeted as major grazers in upwelling and coastal areas. These two statements are presented one after the other in the text.

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I really doubt the grazing by microzooplankton is scarcely estimated in the literature. Regarding the data sets used, there are two major problems, besides the ones already indicated: 1) the cruises encompassed mostly summer and spring months; however, data on winter and fall is presented. 2) Why not to include other studies from other authors in the area? Are perhaps their results contradicting the major conclusions here?

Were the bottles turned upside down to avoid settling of organisms?

What does natural light conditions mean? The technique is highly influenced by light levels.

When were the chlorophyll samples taken? Moreover, how can it be that 1 up to 5L was filtered if the bottles were 0.5-2L?

One-way ANOVA test is inappropriate to identify clustering. Actually, it is evident in figure 2 that there is no clustering, but rather a gradual distribution of the data.

Please, discriminate between autotrophic and heterotrophic nanoplankton (NP) The use of acronyms is excessive and at times annoying.

How can it be that small flagellates constitute a fraction of microphytoplankton (MPP)? Page. 4375 line 20.

Figures 3 and 4 are too small. Moreover, it does not make sense that the normalized values are much higher than 1. This would mean that the entire biomass of prey is removed several times per day, which could only be under high production rates (several doublings per day) or under a community recession scenario. I do not think none of the above is the case here.

Where there any saturation responses found during the dilution grazing experiments? How were the data processed in those cases?

Figure 5 curve adjustment does not make any sense at all.

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Fig. 9. Add biomasses in the figure. What does bottom mean?

The discussion needs to be more concise to answer the questions highlighted in the introduction and should avoid recapitulation of the results.

There are many nonsense sentences and arguments in the discussion. For instance, in page 4382 we read that "...while in eutrophic conditions PP in most of the cases overcome the ingestion. MZP reached the saturation threshold in the kinetic curves and we might hypothesise an export of biomass from primary producers that can sink or be transferred up to higher trophic levels". Where does this surplus of biomass come from?

Please, revise English grammar and usage of punctuation marks. END OF REVIEW

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**BGD**

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