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Biogeosciences Discussions

Interactive comment on "A one-month study of the zooplankton community at a fixed station in the Ligurian Sea: the potential impact of the species composition on the mineralization of organic matter" *by* L. Mousseau et al.

Anonymous Referee #2

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This study presents data on zooplankton abundance and on measurements of oxygen uptake, CO2 release and ammonia excretion on dominant large zooplankton species, with the aim of comparing the role of copepod vs. non-copepod zooplankton on the fluxes of organic matter, during a summer-autumn transition. Although the physiological measurements presented in the paper are potentially useful, I have several problems with this manuscript:

- In my opinion the paper does not discuss properly (or at all) the questions presented in the abstract, such as zooplankton as top-down vs. bottom-up controllers (p. 996,

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row1-5), the importance of zpl in organic matter fluxes (p. 996, row 25 onwards), the effect of wind events on zpl on a species level (p. 997, rows 1-5) or the importance of copepods vs. non-copepods in fluxes of organic matter. - The abundance data only covers animals $>$ 200 μm. This excludes many important groups, such as small copepods (Microsetella, Oncaea) or copepod nauplii. The physiological data is only presented for larger species $>$500 μm (Why??). In my opinion, the excluded groups are too important for generalisations to be drawn without them. - The volume of 50 ml for 10 copepods for 12-15 h appears very small. Also, starvation has a large effect both on respiration and ammonia excretion rate (e.g., Fig. 6 and 7 in Kiørboe et al. 1985; MEPS 26: 85-97), which makes one wonder how representative are the measured rates. The potential effect of crowding and starvation in the results should at least be discussed. - The physiological results could be presented much more in detail: for instance: please, show the rates of oxygen consumption, CO2 release and ammonia excretion. It would also be interesting to see these rates or the quotients plotted against the body size of the individuals. - Please, be more specific in listing the methods: for instance, which statistical tests were used (p. 1003, r. 15; p. 1001, r. 9), what assumptions were used to calculate minimum ingestion (p. 1004, r. 1), what portion / min. number of individuals were counted (p. 998, r. 24). - In general this manuscript needs revision of the language as it is in places difficult to understand.

For these reasons I unfortunately can not recommend publication of this manuscript. I hope my comments above, and some minor comments below, will, however, be useful for the authors.

Minor points: - The abstract reads like an introduction: unless this is the habit of the journal, please rewrite. - P. 997: r. 2-4: The contrasting situations are not really visible in e.g., chl-a data (see also p. 1005, r. 7 onwards) - Were the two biomass nets pooled? If they were analysed separately, please, add the SD to the Fig. 3. - Conclusions: Please, write what did the data show. - Acknowledgements: Since V. Andersen is one of the co-authors it is not necessary to specifically thank her. - Table

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1, Fig. 6+7: Since the purpose was to compare copepods vs. non-copepods it would be helpful if these would be separated. Also the average RQ and MQ and the total ingestion of both groups could be given. - Fig. 1. Please explain the crosses in the figure legend.

Interactive comment on Biogeosciences Discuss., 6, 995, 2009.