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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 #1

Received and published: 25 February 2009

The manuscript by L. Mousseau et al. entitled -A one-month study on the zooplankton community at a fixed station in the Ligurian Sea: the potential impact of the species composition on the mineralization of the organic matter- reports the trend in zooplankton biomass over a short period of time but sampling at short intervals. They also obtain the species composition using different nets and estimate the oxygen and carbon dioxide respiration of mesozooplankton as well as ammonium excretion rates to obtain respiratory and metabolic quotients. Finally, they estimate the potential control on primary production by this community. In general, the authors obtained a valuable data

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and performed a good and hard work at sea. However, a problem with the sampling design precluded the study of an oceanic and stable oligotrophic site. The intrusion of low salinity waters was a shortcoming to the general objective of understanding the role of mesozooplankton in the carbon flux. The authors should pay an effort to justify this intrusion as there are contradictory results (see below). Last, the way they wrote the manuscript is far from being published in the present form. This reviewer feels a rather short experience by the author in writing scientific manuscripts, and I miss some help from the more experienced co-authors. Major revision is required before this work could be published. Below are some considerations.

1. No abstract is given. Write introduction in the heading instead of abstract.
2. In the introduction the authors should give the state-of-the-art of the problem to be studied (what is known, what is not known, which one is the problem to be studied). Avoid very general statements. Avoid specific references to specific cruises. The authors should go to the point. The introduction is rather short and should be re-written in order to give a general picture of the subject and objectives.
3. Material and Methods is full of subheadings. Perhaps they are not necessary for a short manuscript. Use proper names to sampling gears. For instance, the WP-2 net is written all along the manuscript in a wrong way (WP-II). Use proper units. In line 18 of page 997, micrometer is written as μM instead of μm , minutes in written as mns. In line 18 of page 998 insert -and considered- after -of the samples-. Change Lines 19 and 20 of the same page because the two vertical hauls are not performed -to have an idea-. Perhaps to estimate, to assess, nycthemeral migrations. The method to measure carbon dioxide, I suppose, should be coulometric and not colorometric titration. Is the author familiar with this method?
4. Results. Once again the authors should do an effort to reduce subheadings. The Figure number and references are normally not shown in the subheading. The word -de-stratification- is not normally used. I suggest to change by the most common -

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mixing-. Perhaps, -the erosion of the thermocline- could be the right sense. The authors also mix results with discussion (e.g., page 1001, lines 4-5). This reviewer also suggests discerning between the day and night sampling. For instance, in page 1002, lines 5-7 the authors observed the copepod *Pleuromamma* coinciding with the low salinity water intrusion. I guess that the authors refer to night sampling. As it is written, no conclusion could be drawn about the presence of this species. Please, give details. Physiological measurements. The respiratory quotient obtained seems rather high. The authors compare here their results with those of Mayzaud et al (2005). Perhaps, this comparison should be moved to the discussion section, also to avoid repetitions. In the carbon ingestion section the authors use the respiratory quotient of Ikeda et al. (2000) instead of their own obtained quotient. Why not the obtained values? Figure 8. Please revise the units for carbon ingestion. Are they expressed in a square meter basis?

5. Discussion. In general, the discussion section seems rather short and poor. The authors should make an effort to give the pros and cons of the Raybaud et al. (2008) suggestion about the -advection of distinct coastal water mass with its own zooplankton community-. This is important because the second episode of low salinity intrusion had no effect on the zooplankton community (page 1006, line 1-2). The two mesozooplankton maxima are related to this event and to mixing at the end of the sampling period. The latter seems to be related to the start of mixing although there is only a sample by night (Figure 3) and no conclusion can be drawn. The former should be discussed. For instance, although quite exotic, there is evidence of a lunar cycle in zooplankton biomass and abundance (see Hernández-León et al., 2004 and references therein). The day and night mesozooplankton biomass seemed to peak around the full moon during September. Is there evidence in the Mediterranean of such a cycle?

6. Conclusions. This paragraph is not a true conclusion section. Please re-write.

Finally, the manuscript needs a deep English language revision

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