

Interactive comment on “The impact of
**anticyclonic mesoscale structures on microbial
food webs in the Mediterranean Sea” by
U. Christaki et al.**

Anonymous Referee #2

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Review of “The impact of anticyclonic mesoscale structures on microbial food webs in the Mediterranean Sea” by Christaki et al.

General comments

Christaki et al. present an interesting data set of the abundance and activity of the major members of the heterotrophic microbial community along a longitudinal transect carried out in the Mediterranean Sea in summer 2008. The main goals of this study were 1) to provide a general description of the heterotrophic compartment along a W-E transect, and 2) to characterize the relative contribution of the microbial food web to the cycling of organic matter in three distant anticyclonic mesoscale eddies. My main

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concern about the manuscript is that so far it fails to accomplish the second goal, being the main reasons for that:

1) Identification of mesoscale features is not supported by hydrographic information

Mesoscale features are very dynamic and exhibit important temporal and spatial variability. Physical characterization is not trivial and requires a combination of different approaches including remote sensing techniques and different types of hydrographic data. When only CTD data is available, at least two perpendicular transects are needed to indentify mesoscale eddies. High spatial resolution data is also required in order to characterize stations that are under the influence of mesoscale dynamic and those stations that can be considered as background reference stations. The first step in order to study the effect of mesoscale features in the microbial community is to carefully identify stations under the influence of mesoscale dynamics and background reference stations. In this study only single salinity profiles of same stations that are considered to be inside and outside of the mesoscale features are presented. The salinity profile at station A shows that the center of this mesoscale feature is characterized by lower salinity compared to the reference station (st 21), whereas mesoscale features centered at stations B and C were characterized by higher salinity compared to the reference stations. The paper does not provide to the reader with any evidence that stations A, B and C were located inside of mesoscale features. A detailed explanation of how the mesoscale features were identified is the first step needed to accomplish the second goal.

2) The proposed hypothesis is very vague.

The authors present as a vague hypothesis that the impact of eddies will be recognizable within the W-E gradient of oligotrophy. However they do not specify which type of impact it should be expected. This point is related to the previous one as a complete description of the type of mesoscale features, their impact in the surface layer and the expected effect in the microbial communities is needed. Moreover, the au-

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thors conclude that mesoscale features were characterized by a reduction in different heterotrophic compartments compared to the background. The paper is very descriptive and no explanation is given for this observation that disagrees with several previous publications reporting an increase of heterotrophic activity inside of anticyclonic mesoscale features (Ewart, 2008, DSR11; Baltar et al., 2010, ISME; Mouriño et al., 2009, DSRI).

3) The presented data set does not allow to discuss the role of mesoscale features on microbial activity fluxes.

Main fluxes reporting microbial activity are only presented inside the mesoscale features (Figure 7). As not information is provided about the magnitude of these variables in stations not influenced by mesoscale dynamics is not possible to distinguish the effect of these features over the background.

Detailed comments

Title: The title is too ambitious. So far the paper does not correctly address the impact of mesoscale eddies. Also it should clarify that the paper only deals with the heterotrophic component of the microbial community.

Abstract: The abstract should be modified to address the comments raised above. PpP is not defined.

Page 188, line 1: Krom et al., 1993 (inconsistent with the reference list, Krom et al., 1991)

Page 189, lines 1-4 “One of the central ideas of the BOUM cruise (Biogeochemistry from Oligotrophic to the Ultra-oligotrophic Mediterranean) in summer 2008, was that, besides the general aspect of oligotrophy in the Mediterranean, the mesoscale discontinuities likely influence biological processes”. Specify the expected impact.

Page 189, lines 11-13 “Our hypothesis was that the impact of the eddies will be recognizable within the broader W-E gradient of oligotrophy.” Again specify the expected

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impact.

Page 189, line 23: temperature and oxygen data are not presented in this paper.

Page 190, line 23: Porter and Feig (1980) not in the references list.

Page 192, lines 21-22 “They sampled sea water for such measurements at selected depths from a CTD the CTD cast used for the 24 h-long. . .” modified to “They sampled sea water for such measurements at selected depths from the CTD cast used for the 24 h-long. . .”

Page 194, line 7 “Mean Chl-a values in the upper 150m layer were very low. . .” For this and all the other variables presented in table 1 I recommend to use depth integrated values in the photic zone instead of mean values in the upper 150 m.

Page 194, lines 17-19 “VLP was relatable to Chl-a concentration ($n=116$, $r^2 = 0.293$, $p<0.0001$) but a tighter relationship existed between VLP and heterotrophic bacterial abundances (HBA) ($n=116$, $r^2=0.505$, $p<0.0001$).” For this and all the variables specify if the correlation was built with volumetric or mean values. The authors may consider to include a new table reporting all the correlations coefficients between all the variables and why some of them were converted to log distributions.

Page 194, line 25: Fig 2c does not correspond to bacterial production.

Page 195, lines 5-6 “Highest ciliate abundances were recorded at the DCM level or just above it.” Is this true in the western side of the transect? I recommend to overlap chlorophyll distributions in the different panels if figure 2.

Page 195, line 12: does Fig. 2e refer to Fig. 2f?

Page 195, lines 12-13: the contribution of mixotrophs to ciliate biomass is not shown. I recommend to include this information (based on depth integrated values in table 1)

Page 195, line 14: does Fig. 2d refer to Fig. 2e?

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Page 195, lines 21-22 “At site A the bottom depth was 2800 m, the anticyclonic eddy was detectable down to 800m and the core depth of the eddy was 100–250m (Fig. 3a).”

For this and the other eddies provide information to support the identification and vertical extent of the eddy core.

Page 195, line 23: here and through the manuscript, what does smooth mean?

Page 195, lines 25 “All the heterotrophic parameters recorded showed lower values. . .” Is this true for HNF? Here and for the other variable I recommend to make the comparison based on depth-integrated values.

Page 196, line 1: Explain what “irregularities” means.

Page 196, line 12 “flat nutrient profiles”, does flat mean lower?

Page 196, lines 14-15 “. . .and generally lower values and smoother profiles inside the eddy 1 (Fig. 5a–f).” Figure 5 does not show this pattern for virus and HNF.

Page 196, lines 25-26 “Finally, ciliates showed in both cases maximal abundance just above the DCM and were about $\times 2$ more abundant at St. 13 compared to inside the eddy.”

This pattern is not very clear inside the eddy. I recommend to compare depth-integrated values.

Page 197: Biological fluxes in the 3 anticyclonic eddies

Note that as biological fluxes are only shown inside the mesoscale features it is not possible to discuss the effect of eddies in these variables.

Page 198, lines 9-10 “Our study is the first, to our knowledge, to encompass all the major components of the microbial food web...”

Indicate heterotrophic microbial food web.

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Page 199, lines 19-21 "The mixotrophic/autotrophic ciliate *Myrionecta rubra* was pooled with taxa of mixotrophic ciliates (*Tontonia* spp., *Laboea strobila*, *Strombidium acutum*, *Strombidium capitatum* and *Strombidium conicum*)."

Indicate that these data are not shown.

Page 200, lines 2-3 "Overall, mixotrophs showed a high variability in their contribution in total ciliate biomass (Table 2)..."

The authors may consider to include a new column in table 2 showing mixotrophs contribution to total ciliate biomass.

Page 200, lines 13-19 "Our hypothesis was that the impact of eddies would be recognizable within the broader W-E gradient of oligotrophy. The 3 eddies were indeed associated with the lowest values for different metrics of the heterotrophic compartments of the microbial food web. This was particularly pronounced for ciliates (both heterotrophs and mixotrophs) which are the link between microbial food web and the higher trophic levels."

This hypothesis needs a justification. Which is the reason to expect a decrease in the heterotrophic activity inside these anticyclonic eddies?

Page 200, lines 13-19 "A question that arises is: Is the oligotrophy gradient detectable among eddies? The W-E gradient, although attenuated, was clearly recognizable among the 3 eddies and except for virus in site C, all stocks are higher at the stations outside the eddies (Fig. 8)."

I have strong concerns that with the presented dataset the authors can distinguish between the background oligotrophy W-E gradient and the eddy effect, as mesoscale features are not well characterized. Based on figure 8 bacteria stocks are higher in station C than station 11.

Page 200, lines 27-28 "Overall, a W-E gradient among the eddies was generally recognizable in terms of heterotrophic biomass values but not in terms of production."

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The background pattern in terms of production is not shown

Page 201, lines 28-29 "Integrated BP was very similar at the 3 eddies ($33.5 \pm 6 \text{ mgCm}^{-2} \text{ d}^{-1}$)."

Where does this value come from?

Page 202, line 10 "Crombet et al., this issue"

Update this reference

Page 202, lines 11-13 "A plausible explanation might be the presence of relatively elevated PO₄ values in the entire surface layer (0–150 m), generally 4 times higher than those at stations A and B, despite a deeper phosphocline at station C."

Were higher PO₄ values observed in more than one nutrient profiles carried out at station C?

Page 202, lines 14-17 "However, we might argue that the elevated phytoplankton biomass and production observed at station C were related to those higher PO₄ concentrations, at an optimum depth (80–100 m) where there were enough NO₃ and light to sustain productivity and growth."

Why were not the higher PP values observed in both production experiments?

Page 202, lines 17-19 "Most probably, the bottom topography and vertical structure of the eddy at station C (warmer and shallower) may have played a critical role in structuring the above mentioned characteristics."

Explain how bottom topography and the vertical structure of the eddy are expected to play a role in the vertical distribution of primary production.

Page 202, lines 28-29 "BR ranged from 0 to 36.4 mmolO₂ L⁻¹ d⁻¹ (Lemée et al., 2002; Navarro et al., 2004)."

When? Give more details about this study

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Page 203, lines 16-19 "Our integrated GCP ($28-75 \text{ mmolO}_2 \text{ m}^{-2} \text{ d}^{-1}$) and DCR ($39-58 \text{ mmolO}_2 \text{ m}^{-2} \text{ d}^{-1}$) values fell within the range of previously recorded values except the ones reported by Regaudie-de Gioux et al. (2009), which were higher (mean GCP $118-196 \text{ mmolO}_2 \text{ m}^{-2} \text{ d}^{-1}$)."

When and where? Give more details about the study by Regaudie-de Gioux et al. (2009)

Page 203, lines 23-24 "NCP varies with geographical, temporal, seasonal scales and is also strongly influenced by mesoscale variability (del Giorgio and Duarte 2002; Maixandeu et al., 2005)."

Seasonal are temporal scales. Mesoscale involve temporal and spatial variability scales. Modify this sentence.

Page 203, lines 27-28 "Assuming then that heterotrophic bacterial respiration was 50% of DCR (see below),..." modify to "Assuming then that heterotrophic bacterial respiration was 50% of DCR (see above),..."

Page 204, lines 1-3 "The net heterotrophy, although not statistically significant at site A, is in accordance with the BCD/PPtotal ratio slightly higher than 1."

Is not the net heterotrophy at station A inconsistent with the W-E increase gradient in oligotrophic conditions?

Page 204, lines 10-12 "Limited data on ciliate community composition suggested that eddy microbial communities differed from those in adjacent stations outside the eddies."

This pattern is not clear at station C

Revise the conclusions section based on the comment above.

References: Schlitzer, R.: Ocean Data View 4, <http://odv.awi.de>, 2009. (Schlitzer, 2010 in the text)

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Table 1: Consider to show depth-integrated values instead of mean values. Which is the justification for the 150 m limit? Why don't using the base of the photic zone?

Table 2: I do not see the need for this table if table 1 shows depth-integrated values

Table 3: Include error from replicates

Figure 2: Consider to include an overlapping plot showing chlorophyll distribution. Increase label size. Indicate station number.

Figure 3: Temperature profiles are not included.

Figure 7: Modify labels A2, B2 and C2 to A3, B3 and C3.

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