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Interactive Comment

Interactive comment on "Whole-system metabolism and CO_2 fluxes in a Mediterranean Bay dominated by seagrass beds (Palma Bay, NW Mediterranean)" *by* F. Gazeau et al.

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Review of: Whole-system metabolism and CO2 fluxes in a Mediterranean Bay dominated by seagrass beds (Palma Bay, NW Mediterranean), by: F. Gazeau, C. M. Duarte, J.-P. Gattuso, C. Barron, N. Navarro, S. Ruiz, Y. T. Prairie, M. Calleja, B. Delille, M.Frankignoulle, and A. V. Borges

This paper describes estimates of the whole system metabolism in a Mediterranean Bay using planktonic and benthic incubations and CO2 system measurements. The authors have done a commendable job and have illustrated the great utility of performing multidisciplinary research in order to view metabolism studies using diverse approaches. The paper highlights the importance and complexity of coastal and shelf regions in the cycling of carbon and of understanding the interplay between pelagic and benthic systems. The paper is very relevant to the scope of the journal.

On the whole, the paper does well at describing the general results of the studies involved but lacks detail necessary to fully evaluate the excellent results and thus fully understand the processes that control community metabolism in this area. Following the application of the amendments below I hope that the extra information will enable a more thorough discussion on what controls the metabolic balance in the water column.

General comments: I would like to see a more comprehensive description of the data preferably in the form of figures showing profiles of T, S, Chla, irradiance, At, DIC, O2, pH and pCO2.

Information on the windspeed (magnitude and variability) would be useful when looking at the gas exchange calculations and the discussion of wind speed parameterisation.

In concordance with Reviewer 3, I would like to see an incorporation of the discussion of organic matter. Some of the authors are present on a recent paper describing plankton metabolism and DOC which could greatly aid the discussion in this paper (Navarro et al., 2004, Plankton metabolism and dissolved organic carbon in the Bay of Palma, NW Mediterranean Sea. Aquatic Microbial Ecology, 37, 47-54.

As the authors elude to, the use of integrated water column values is biased towards the deeper regions of the Bay and yet these are the only values shown in the tables. To get an instant visualisation from the tables of which stations contained the most productive waters, for example, I think it would be useful to also list the column averages in m-3. I would also like to see Figures showing profiles of the plankton NCP, CR etc so the column variability can be compared to the hydrography, Chl a, and CO2 system....

The paper interchanges between discussing and presenting the DIC and AT in millimoles.kg-1 and micromoles.kg-1. Would read easier if there was consistency through the paper.

I suggest that the authors take account of the following detailed comments:

P762 L2 The authors state that the water column was fully mixed as evidenced from the CTD profiles. However, the salinities at 2m shown in Figure 4 do not match with the salinities shown in Figure 7c. suggesting that the waters were not mixed (the salinities go higher in Fig 7c). The incorporation of salinity profiles as mentioned above would be useful in clarifying which is the correct interpretation.

The contour plots in Figure 4 are misleading and the extrapolation leads to erroneous salinities outside the study area. The data would be better presented as in Figure 8.

P764 L1 The reference should also include the corrected version of the Copin-Montégut 1988 paper: Copin-Montégut, C., 1989. A new formula for the effect of temperature on the partial pressure of CO2 in seawater. Corrigendum. Mar. Chem., 27, 143-144.

Equation 1: The work of Friis et al GEOPHYSICAL RESEARCH LETTERS, VOL. 30, NO. 2, 1085, doi:10.1029/2002GL015898, 2003, illustrates the limitations of salinity normalisations of the CO2 system. The slopes for specific alkalinity (Fig 4) show that the intercept at S=0 is far from zero for total alkalinity and the negative sign suggesting a source of carbonate in the high salinity waters. The authors should discuss this and also justify their use of their " traditional" salinity normalisation in their usage of DIC37, or use the Friis approach.

P776 L25 The adjustments made to the surface DIC37 of 1 and 4 micromol.kg-1 are outside the accuracy of the calculated DIC (see P764 L20) and therefore these are not significant deviations from the water column values

Table 5. The units in the column headings for pCO2 and DIC units are crossed

Figure 3d. The date should read 25/06/2002

Figure 8. The legend and figure titles suggest that the measurements were made on one day (3 March; 19 June). I presume these should be changed to the dates that

correspond to the entire studies.

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