



## ***Interactive comment on “Whole-system metabolism and CO<sub>2</sub> fluxes in a Mediterranean Bay dominated by seagrass beds (Palma Bay, NW Mediterranean)” by F. Gazeau et al.***

**Anonymous Referee #3**

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Review of: Whole-system metabolism and CO<sub>2</sub> fluxes in a Mediterranean Bay dominated by seagrass beds (Palma Bay, NW Mediterranean), by: F. Gazeau, C. M. Duarte, J.-P. Gattuso, C. Barr on, N. Navarro, S. Ruiz, Y. T. Prairie, M. Calleja, B. Delille, M. Frankignoulle, and A. V. Borges

This is a fine paper which tests and discusses the metabolism of a small Mediterranean region using two different approaches. The authors have collected a lot of good data, which have optimally been used for constructing local and annual budgets. It is laudable that the authors have undertaken the effort of comparing two approaches of NEP estimates.

Because of the many data, there are some problems with the readability. The amount

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of information which must be conveyed to the reader is that large that it is sometimes difficult for the reader to keep track of the main points of the paper. Perhaps it is unavoidable that this occurs in such a paper with that many data, but maybe the authors could try to improve somewhat.

There is one point that I must mention, but this is more related to the planning of the study. In attempting to quantify the fluxes of organic matter through the various reservoirs in the bay, where indeed it is an additional aim to compare this with fluxes derived from inorganic carbon and oxygen data, one must be sure to have caught all relevant sources of organic carbon. As the authors mention in the description of the study site, the Bay of Palma is situated near a moderately large city. It should not come as a surprise that close to such a region, the impact of allochthonous organic matter originating from the land may be considerable. Therefore, it is a pity that the authors did not consider measuring organic matter during their campaign. In the conclusions section they deduce indeed that allochthonous organic matter must play a major role in bringing the system to heterotrophy. This is an important conclusion and I think a remark should also occur in the Abstract.

p 762, line 9 determine (instead of: determinate)

p771, line 18-21 My suggestion: Comparison of pCO<sub>2</sub> between cruises requires normalization to a constant temperature, which in the present case should be 19°C ( ). This is because temperature strongly affects ... solubility coefficient of CO<sub>2</sub>, where a temperature increase of 1°C causes a pCO<sub>2</sub> rise of about 4%.

p771, line 22 delete: to remove temperature effects and

p774 Here it is stated that Ca carbonate production is an important process. However, on page 772 it was reported that TA was conservative during the cruise, which made the authors state that the Ca carbonate production/precipitation is low. How could these opposite statements be reconciled? I think the authors should clarify this in the manuscript.

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p775, line 13 lower than but not significantly different from 1.

p 775 It is stated that the below-ground production does not significantly contribute to the PQ. However, considering that the below-ground production is 10% of the above-ground production, and the C:N:P ratios are much higher below-ground, one would expect a significant contribution, wouldn't it?

p776 Why are the vertical gradients of oxygen larger than for DIC? Maybe the authors could add a remark on this.

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Interactive comment on Biogeosciences Discussions, 1, 755, 2004.

**BGD**

1, S375–S377, 2004

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