

Interactive comment on “Whole-system metabolism and CO₂ fluxes in a Mediterranean Bay dominated by seagrass beds (Palma Bay, NW Mediterranean)” by F. Gazeau et al.

F. Gazeau et al.

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Reply to anonymous reviewer 3

We would like to thank the reviewer for his/her comments on and interest in our manuscript.

Reply to General Comment:

In the revised version of the manuscript, we tried to improve the readability of this paper by clearly dividing it in two major parts: (1) planktonic and benthic metabolism at one station in the bay over an annual cycle and (2) comparison between several methods to estimate the whole-system NEP during 2 cruises in 2002. We understand that due to the large amount of data, this was not clear enough. The combination of results obtained over the annual cycle at one station and results obtained in the entire bay during

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2 cruises brought us to the conclusion that the bay might not be autotrophic over an annual cycle but more likely at a metabolic balance or slightly heterotrophic. This latter might be fuelled by external organic matter inputs (either terrestrial or oceanic inputs although terrestrial inputs are more likely; this was added in the abstract). We measured POC and DOC concentrations during the Eubal cruises but unfortunately only at the 4 reference stations which are located near shore. Moreover, low information of organic matter loadings from the city of Palma are available to us although these are expected to be rather low. This must be a subject of research in this area in the future, but for the moment our data do not allow to estimate the amount of organic matter loading from Palma city in this bay.

Reviewer comment: 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 Ca carbonate production/precipitation is low. How could these opposite statements be reconciled? I think the authors should clarify this in the manuscript.

Reply: In page 772 we state “(E) suggesting that CaCO₃ precipitation/dissolution rates were too low” but we also state that “and/or water residence time too short to significantly affect surface water TA values”. The effect on surface water parameters of biological processes is a function of the intensity of the rates of biological processes AND residence time of the water mass in a system AND the overall volume of the water mass (as mentioned in the introduction). Also, we were careful in our interpretation since we originally stated that “The difference between NEPDIC and NEPO₂ could be related to net CaCO₃ production”. The idea of the NEP computations based on DIC comes from the fact that we have high spatial coverage of pCO₂ (underway data) and this allows the computation of NEP integrated at a large spatial scale. In turn this allows a robust check on the upscaling procedure of the O₂ incubations carried out in parallel. The problem is that we do not have a second underway parameter to compute DIC from pCO₂. The only way to do this was to use a TA-salinity relationship. Of course,

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this approach removes any TA signal related to CaCO₃ precipitation/dissolution. If we had TA underway data then the DIC computed from pCO₂ and TA would have included the CaCO₃ precipitation signal and the NEPDIC and NEPO₂ estimates would have been closer. The idea is exactly to show that the difference between estimates can be explained to some extent. We are aware that this approach is not ideal but the consistency between the three approaches (DIC and O₂ budget and up-scaled O₂ incubations) is satisfactory.

Reviewer comment: P775 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 could expect a significant contribution, wouldn't it?

Reply: Considering the C:N:P ratio of 3550:61:1, we computed a PQ of 1.03. Therefore, 10% of below-ground production will decrease the PQ from 1.08 to 1.075. This was clarified in the paper.

Reviewer comment: P776 Why are the vertical gradients of oxygen larger than for DIC? Maybe the authors could add a remark on this.

Reply: The surface water adjustments of DIC and O₂ have been removed from the present version of the paper, and thus the figure with the vertical profiles of DIC and O₂ has also been removed.

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

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