

***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.***

**F. Gazeau et al.**

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Reply to Reviewer 2 - Dr. R. Bellerby

We would like to thank Dr. Bellerby for his comments on and interest in our manuscript.

Reviewer comment: 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, O<sub>2</sub>, pH and pCO<sub>2</sub>.

Reply: This paper already presents 6 tables and 10 figures. We think that adding these figures to the manuscript will not be of primary interest. Surface irradiance data are now shown in Table 1.

Reviewer comment: Information on the wind speed (magnitude and variability) would be useful when looking at the gas exchange calculations and the discussion of wind

speed parametrization.

Reply: Wind speed are already presented in Table 5.

Reviewer comment: 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.

Reply: In Navarro et al. (2004), it is speculated that the heterotrophy of the planktonic compartment at one station in the bay (Posidonia station) is fuelled by either excess production from the benthic compartment (Posidonia meadow) or by terrestrial inputs. In the revised manuscript, we state that the whole bay is not likely autotrophic on an annual scale, but more likely at metabolic balance or slightly heterotrophic, with Posidonia meadow production only fuelling the planktonic heterotrophy in shallow areas of the bay. This latter might be fuelled by inputs from land although these latter are expected to be relatively low.

During the Eubal I and II cruises, we measured POC and DOC concentrations only at the four reference stations all located near shore which is clearly not appropriate to see if there is clear input from land.

Reviewer comment: 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, Chla and CO2 system.

Reply: Water column averages (basically integrated values divided by station depth) are now presented in Table 2 and 3. Profiles of GPP and CR for the 4 reference stations

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are already shown in Figure 5. We add graphs for NCP in the revised version of our manuscript.

Reviewer comment: The paper interchanges between discussing and presenting the DIC and AT in  $\text{mmoles kg}^{-1}$  and  $\mu\text{moles kg}^{-1}$ . Would read easier if there was consistency through the paper.

Reply: All DIC and TA values are now presented in  $\mu\text{moles kg}^{-1}$  throughout the paper.

Reviewer comment: 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.

Reply: We do not agree with this observation as maximal salinities in Fig. 4 are about 37.6 (on 01 and 03/03/2002) which is about the maximal value presented in Fig. 7c. Maybe the reviewer compared values from the Eubal-II cruise (black dots in Fig. 7c) which are indeed higher but not presented in Figure 4.

Reviewer comment: 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.

Reply: The reviewer is right and Figure 4 was updated as suggested.

Reviewer comment: Equation 1: The work of Friis et al. (2003) illustrates the limitations of salinity normalisations of the CO<sub>2</sub> 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 normalization in their usage of Friis approach.

Reply: We are aware of the Friis et al. (2003) paper. The “traditional” and “Friis” ap-

proaches to normalize TA or DIC will be critical when looking at ocean basin scales for computations of anthropogenic CO<sub>2</sub> signals or estimates of oceanic carbon transport carried by rivers. This is critical because at ocean basin scale salinity ranges between 32 and 37 (range 5.0). The important computation we carried out in the paper is NEP for the EUBAL-II cruise during which salinity ranged between 37.8 and 38.0 (range 0.2). Considering such a small adjustment of DIC to salinity compared to the “biological” signal, the traditional adjustment method is as good as the Friis et al. (2003) approach.

Reviewer comment: P776 L25 The adjustments made to the surface DIC<sub>37</sub> of 1 and 4  $\mu\text{moles kg}^{-1}$  are outside the accuracy of the calculated DIC (see P764 L20) and therefore these are not significant deviations from the water column values.

Reply: The adjustments are based on the vertical profile comparison and should be independent from the overall accuracy, the critical factor in this case being precision (which is better than the accuracy). Nevertheless, the surface water adjustments of DIC and O<sub>2</sub> have been removed from the present version of the paper.

Reviewer comment: Table 5. The units in the column headings for pCO<sub>2</sub> and DIC units are crossed.

Reply: This might come from an editorial problem as all units were moved to the left.

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

Reply: Typo was corrected.

Reviewer comment: 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.

Reply: The figure 8 aims to present only one example of pCO<sub>2</sub>, normalized pCO<sub>2</sub> and AOU distribution in the bay from the different surveys made during each cruise. In the revised version of the manuscript and for a comparative purpose, we present the

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distributions of these different parameters during the 4 surveys (2 in March, 2 in June) used to derive NEP estimates based on incubations.

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**BGD**

1, S507–S511, 2004

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