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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.

F. Gazeau et al.

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Reply to anonymous Reviewer 1.

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

Reply to General Comment:

A revised version of our manuscript is available. First of all, as suggested by all reviewers, we tried to improve the readability of our paper. In the revised version, we divided the different sections in 2 major parts (1) estimation of the metabolism (benthic + planktonic) at one station in the bay over an annual cycle and (2) estimation of the whole-system metabolism using different approaches during 2 cruises. Other comments below.

One of the major concerns of Reviewer 1 in our paper is the opposite signals provided by 1) incubation and DIC/O2 derived NEP values during the 2 Eubal cruises and 2) the values of pCO2(19°C) and AOU. Indeed, this is a major conclusion of our paper (although this is clearly not newĚ) which was not enough developed in the former version of the manuscript and more weight will be given to this matter in the revised manuscript. pCO2(19°C) and AOU are significantly lower in June than in March. One can conclude, as the reviewer stated, that the system is more productive in June than in March. But as mentioned in the introduction, the ability of a system to act as a source or a sink of CO2 (or O2) to the atmosphere depends on several parameters and not only primary production and respiration (NEP). One has to take into account the initial values of pCO2 and O2 of the water mass entering the system and the residence time of this water mass in the system. That is exactly what we did to estimate the NEP over the Posidonia meadow by estimating its impact on DIC and O2, and knowing the DIC (O2) concentration over and out to the Posidonia meadow as well as the residence time of the water mass, we estimated NEP rates fairly consistent with values derived from incubations. To conclude, pCO2 and O2 surface concentrations can be indications of the metabolic status of a system only if initial (when entering the system) conditions and residence time of the water mass are known.

Another point raised by the reviewer is the neglect of inputs from land. Inputs from land to the Bay of Palma are very low due to (1) low annual precipitation in the region (about 400 mm year-1); (2) the lack of rivers and streams to deliver the run-off to the water, as the island is in a kartstic area, where surface water percolate to ground waters; and (3) the zero-loss policy of urban waters, which are treated and then pumped back inland, although there are sporadic inputs during storm surges (Jansá et al. 1994). The conclusions of our study were slightly modified in the way that our results during 2 periods do not allow to clearly evaluate the trophic state of the bay of Palma on an annual scale. Nevertheless, during these two cruises we saw that the bay is heterotrophic when the planktonic compartment is productive and at metabolic balance (or slightly heterotrophic) when the benthic compartment is productive. During the annual study, we saw that the planktonic compartment indicated an heterotrophic status at a shallow station. These observations indicate that on an annual scale, the bay of Palma might not be autotrophic but more likely at a metabolic balance or slightly heterotrophic. This latter might be fuelled by inputs from land which are expected to be relatively low.

As the reviewer wrote, Figure 8 clearly shows lower pCO2 values along the coastline in June which can be attributed to the Posidonia meadow production, but this does not bring any information on the organic matter and/or nutrient inputs from land.

The reviewer suggested to use NEP values derived from incubations to estimate pCO2 and O2 values (we supposed over the Posidonia meadow). As long as NEP rates derived from DIC/O2 and incubation approaches predicted very similar rates, we do not see any interest to perform such calculation.

Reviewer comment: Abstract P756, l7-10: From the annual studyĚ: Comparing the statement here to the (related?) section 3.1, there is a discrepancy. In 3.1 it is stated that during 2001 the planktonic NCP points to heterotrophy and in 2002 to autotrophy. There seems to be no clear message in section 3.1, even considering the uncertainties of the figures. What about terrestrial inputs, which could support heterotrophic activity or even mask a possible autotrophic state of the planktonic system?

Reply: Indeed, there was an error in the abstract which has been corrected. Overall, the planktonic compartment present a metabolism not statistically different from zero. As mentioned above, we have low information on terrestrial inputs although these are expected to be rather low. Anyway, if there is an organic matter input from land then it should stimulate community respiration. Thus, the actual system would be even more autotrophic in absence of those terrestrial inputs.

Reviewer comment: Methods, section 2.4: Page 764, l10: How was the accuracy of the alkalinity measurements determined? It is of highest relevance for the later calculation of the DIC in the water column (see below comments regarding section 3.6)

Reply: We did not perform specific tests of total alkalinity (TA) accuracy during these cruises. However, in the past we have carried out on several occasions TA accuracy determinations with Certified Reference Materials from Scripps Institution of Oceanography (CRM) and the accuracy was in the range of the precision of the measurements. Also, the fact that our TA versus salinity distribution is consistent with the independent relationship of Copin-Montégut (1993, Global Biogeochemical Cycles 7:915-925) would suggest that our measurements are fairly accurate.

Reviewer comment: Page 764, l15: How was the accuracy of the pH measurements determined? It is of highest relevance for the later calculation of the DIC in the water column (see below comments regarding section 3.6)

Reply: The accuracy of pH measurements was not specifically determined during these cruises. Actually, it is very tricky to determine the accuracy of pH measurements. One of the methods used is to compute pH from the dissolved inorganic carbon (DIC) and and TA values of the CRM and check it against the pH value. We believe that this method is flawed since pH computed from DIC and TA is subject to a large computation error (Millero 1995, Geochimica et Cosmochimica Acta 59:661-677). In the past, during cruises with large data-sets of pH, TA and pCO2 (equilibratior-IR measurements), such as the OMEX II cruises we checked the accuracy of pH measurements from the comparison of pCO2 (computed from pH&TA) versus pCO2 (equilibratior-IR measurements). This comparison shows that the accuracy of pH is in the range of the precision of the measurements (Borges & Frankignoulle, unpublished).

Reviewer comment: Given the uncertainty of the calculated DIC values, the (much lower) uncertainty of DIC in Table 5 appears to be questionable. Moreover, section 3.6 must not be relied on the DIC data, of which uncertainty seems to be far too high for this interpretation. Additionally, the DIC data for the upper and lower water column originate from two different calculations (surface: pCO2 and TA vs. subsurface: pH and TA), none of which was checked for accuracy and they were not checked against each other. Given the small signal shown in Fig. 11, any conclusions might have been derived from direct DIC measurements (which are not available), but not from calculated ones, even if one would use only one of the two possibilities for the calculation of DIC applied here. Thus in my opinion section 3.6 and Fig. 11 are entirely unsubstantiated.

Reply: The reviewer is right and this section which is not of primary interest in our paper was removed.

Reviewer comment: Results, section 3.1: Page 767, lines 1-2: From Fig 2a any trophic state is hard to identify. If the data allows this, one might apply a different scale to the sediment observations. Maybe it was helpful to the reader to mention the averages here.

Reply: In order to compare easily NCP for the Posidonia oceanica and bare sediments, we think that it is better to use the same scale for both. Moreover, NCP for the bare sediment is not statistically different from 0. As suggested by the reviewer, we added the mean and standard errors in the legend.

Reviewer comment: Page 767, line 16-19: Would there be an explanation for the different seasonal behaviour ? The conclusion is hard to understand. What would be the role to terrestrial organic carbon inputs into the bay ?

Reply: We added a sentence in this section linking this paper to the one of Navarro et al. (2004) Aquatic Microbial Ecology 37:47-54. In this paper, authors reported that higher autotrophic conditions in 2002 were related to frequent and strong storms which were likely to bring high loads of nutrient by runoff. We tried in the revised version of the manuscript to improve the readability of this section.

Reviewer comment: Results, section 3.3: It is difficult to get an overview about the different trophic states of the different locations. Possibly a short summary at the end of this section was helpful.

Reply: Instead of a summary, we will provide in the revised version, interpolated maps of NEP in the bay during the 4 surveys which improve the understanding of the NEP variation inside the bay and, more important, will help to compare NEP and AOU/pCO2 values during these 2 periods (no clear impact of the NEP in March, low residence time while an impact of the Posidonia meadow is evident in June).

Reviewer comment: P772, l6-7: Please delete the last sentence of this paragraph “This suggestsĚ” One small semi-enclosed bay - probably receiving vaste amount of terrestrial inputs - cannot and must not be used as representative for the entire continental shelf of the Northwest Mediterranean Sea.

Reply: We agree with that and removed the corresponding sentence. However, we note that inputs from land are not vast, and are likely modest.

Reviewer comment: P773, l23-25: From the definition of the trophic state it might be obvious that the direction of the CO2 fluxes are no necessarily related to the trophic state.

Reply: We agree with that and the sentence was removed. This is clearly related to the matter discussed above.

Reviewer comment: Results, section 3.5: Why is there no compilation for the first cruise ? What would be the outcome ?

Reply: No NEP estimates were performed during the first cruise as pCO2 and O2 did not exhibit recurrent spatial features (short residence time in March and change of the flow pattern during the cruise). This was clarified in the text at the beginning of the DIC/O2 budgets results section.

Reviewer comment: Again, we find a detailed search for errors in the oxygen and CO2 based assessment of NEP, however hardly any comment on the uncertainty of the alternative method. One could also graphically compare both NEP estimates.

Reply: The reviewer is right and errors associated with NEP estimates based on incubations are reported. As long as we estimated only NEP values from the DIC/O2 budgets and incubations during the second cruise, we do not see much interest to add a figure just to compare 3 values.

Reviewer comment: Results, section 3.6: As indicated in the methods section above, I do not think that this section can be supported by data. The surface layer and sub-surface layer data have been calculated with different data without cross check and without information on the accuracy of the related measurements. Please delete the entire section and the corresponding figure.

Reply: As suggested by the reviewer, this section was deleted.

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