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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.

Anonymous Referee #1

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Review of the manuscript "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. Barrón, N. Navarro, S. Ruíz, Y. T. Prairie, M. Calleja, B. Delille, M. Frankignoulle, A. V. Borges, submitted to BIOGEOSCIENCES

The manuscript "Whole-system metabolism and CO2 fluxes in a Mediterranean Bay dominated by seagrass beds (Palma Bay, NW Mediterranean)" by F. Gazeau et al. reports on the investigation of the carbon metabolism of a seagrass dominate ecosystem in Palma Bay, NW Mediterranean Sea. This issue is addressed by a variety of experimental approaches in order achieve a comprehensive and complementary view of the system.

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I am impressed by this approach, which had required enormous efforts to be carried out. Ultimately, I would be happy to see this work published. However, in my view, the authors only partly exploit the unique opportunity to establish a comprehensive view and also to balance the strong and weak points of the individual methods against each other. Instead, a rather descriptive manuscript is presented, showing the enormous variety of data without really relating them to each other. Moreover, I think the authors tend to ignore or weaken substantiated signals, notably from oxygen and pCO2 data, in order to justify seemingly opposing observations using other methods.

As an overall judgement, I think the data set and the manuscript definitively bear the potential for a high-quality paper. Still, I cannot recommend the paper to be published before some major revisions. These are required most notably in two respects: Firstly, the manuscript needs to be much better focussed, and a clearly visible "red line" needs to be elaborated, even if this would require to drop one or the other aspect. The comparison of the various results is difficult. Secondly, the main outcome seems to be (primarily) based on and biased towards metabolism measurements and all other information is exploited to support these data, even if the latter one tends to imply a different or contrasting outcome. The line of argument needs to be much more balanced and considered from a somewhat more remote point of view in order to complementarily exploit the entire data set. This might require an adjustment of the main findings of this paper. Please find my more detailed comments below.

Detailed comments:

Abstract P756, l7-10 From the annual study...: 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?

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).

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).

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 Alkalinity vs. subsurface: pH and Alkalinity), 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.

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.

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

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.

Results, section 3.4: My major concerns with the ms. are related to this section and its relation to the earlier, "metabolic" estimates of NEP, i.e., of the trophic state of the system. From Fig. 7 it is more than obvious that the system is more productive in June than in March. Both AOU (a temperature normalized parameter) and the normalized pCO2 indicate a clearly autotrophic systemin June, which moreover is clearly more autotrophic than in March. Thus, NEP can be assumed to be more positive in June. Also in March quite a large number of positive AOU (i.e, negative NEP) values have been observed. To me it appears to be rather a weak and questionable line of argument to ignore the message of the AOU and pCO2 by referring to different residence times of the water in the bay, although the authors state the strong uncertainty of these estimates. In both cases the residence time appears to be on the order of some days and as the authors stated, the estimate from only a few observations is strongly uncertain. One option might to seek help from hydrodynamic models. In any case, I do not think, the clear message of the AOU and pCO2 data can be ignored using this rather vague argument, which is accompanied by some further arguments, whereas in contrast the rate measurements obviously have been considered as fact. One should also recall here that the determination of oxygen is amongst the most accurate chemical measurements, which is for the analytical point of view certainly more reliable than any other measurement carried out in the present study. My recommendation here would be to assign the AOU and pCO2 data somewhat more weight and use these to critically assess the other NEP estimates. One might also argue that we are approaching the limits of the "metabolic" estimates.

In the overall discussion, one fact is entirely missing here: eutrophication or nutrient inputs from land. A reasonably large and busy city is located at the bay and one can speculate that organic carbon and organic and inorganic nutrients are released into the bay. Fig. 8 shows rather a clear signal along the coastline. I cannot see, whether these effects are large or not, however they need to be considered in the discussion.

A further option, which I would like to recommend to the authors, is a simple calculation, whether their obtained rates would be appropriate to generate the observed AOU and pCO2 concentrations. I.e., would the oxygen release and CO2 uptake, predicted by

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the methods described under 2.3, be sufficient to create the observed AOU and pCO2 conditions, and which residence times would be required. One also could perform the "counter" calculation and assess at given residence times, on what order of magnitude the rates needed to be to supply/remove the required oxygen/CO2. This would help to exploit the unique opportunity here having different estimates available.

Further assistance for resolving this problem might be obtained from nutrient data, which have not been discussed at all.

This is also related to the comment in line 16 of page 772, where a highly variable NEP is proposed. It could also be that NEP it is simply too low to create defined spatial patterns. This idea would be in agreement with the AOU and pCO2 data.

P771, line 20: please replace DIC by carbonate system P772, lines 6-7: Please delete the last sentence of this paragraph "This suggests..." One small semi-enclosed bay - probably receiving vast amounts of terrestrial inputs - cannot and must not be used as representative for the entire continental shelf of the Northwest Mediterranean Sea.

P773, l20: Please delete "actually", it gives the impression, as if the normalized values were the observations.

P773, l23-25: Please delete this sentence. 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. See for example: Gattuso (2004) BIOGEOSCIENCES DISCUSSION 1, S125-S126 and references therein. Moreover, this statement is in contradiction to the AOU and pCO2 observations (see above).

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

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.

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 subsurface layer data have been calculated with different data without cross check and without information on the accuray of the related measurements. Please delete the entire section and the corresponding figure.

Figures: Many of the figures (3, 4, 5, 7, 8) are too small, but this might be an editorial problem. Nevertheless, in the printed version it was difficult to read them and to see/read all scales, legends ets. Please improve or enlarge for a final version.

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