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Interactive comment on “Spatial and temporal CO₂ exchanges measured by Eddy Correlation over a temperate intertidal flat and their relationships to net ecosystem production” by P. Polseñaere et al.

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General

"This is a useful paper reporting novel measurements of CO₂ uptake by a temperate mudflat in SW France, measured by eddy covariance, during different times of year and during emersion and inundated phases. It should be published, although several changes should be considered to improve it. In particular, the authors should seek to reduce the length, reduce some of the extensive description and consider whether the earliest set of limited data be considered in this paper."

We are very grateful to the Referee for this positive comment about our study. The
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manuscript will be reduced (especially section 3.5 as suggested by the Referee#2 and section 4.1.1) but we would like to keep the September/October 2007 data set obtained at Station 2 that demonstrates clear spatial variations of CO₂ fluxes between the two stations in relation with primary producer covers.

"One general point was that there is some ambiguity and lack of precision in defining what the study is about – what area and system it is addressing. Is it measuring the CO₂ fluxes from a mudflat, or from the lagoon – both are used interchangeably in the abstract?"

Scope and main goals of the study will be clarified in the revised manuscript as previously enounced in our responses to other referees. Both lagoon and mudflat are used all along the present manuscript. It could be more accurate to qualify the studied area as an intertidal flat though Arcachon corresponds to a lagoon from a geological/geomorphological point of view.

"Of course the challenge in micromet approaches in a coastal interface system like this is that the footprint can be from a mixed ecosystem, and the mixture changes with time, and the C balance is very hard to define as there is a strong lateral advection of C with the tide."

We indeed discuss these difficulties and challenge in our manuscript, and we will do it with more emphasis in the revised version.

"Hence the value of the work, but it might help understand the system if the paper was less ambiguous. Also, some information on the heterogeneity of the mudflat around the mast on the main site should be given to allow interpretation – the satellite-derived image in Fig 1 suggests it has channels but are there smaller creeks etc that would fill with water, before the water level increases at the mast?"

There are no smaller creeks that would fill with water before the water level increases at the mast.

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"So the source area changes during these tidal transitions. Is it possible (especially for the longer data set periods) to actually filter out the transition periods to arrive at more 'steady state' value of CO₂ flux representing the fully immersed and fully emerged conditions?"

The source area clearly changes during these tidal transitions. To compute the averaged fluxes shown in the manuscript during the four different cases (LT/Day, LT/Night, HT/Day and HT/Night), such filter was already used to differentiate the emersion at a more steady state (only data corresponding to a water height of 0 m were selected) and the immersion. Similarly, PAR threshold of 20 $\mu\text{mol m}^{-2} \text{ s}^{-1}$ was applied to separate day and night cases. Attempts have been also done to see possible relationships between daily CO₂ fluxes and the time of emersion during each day for the four deployments. Nevertheless, no significant correlations could be drawn. According to the immersion, all the data matching with non-zero water height were used. In Polsenaere et al. (submitted) a stronger filter was used to reduce the immersion case to solely more steady state values of CO₂ fluxes. No differences were observed in correlations of CO₂ fluxes at high tide with or without this last stronger filter. For the high tide cases (Table 1), such filter can be used and specified for average CO₂ fluxes computations.

"A second general point is that several conclusions are drawn about the cause of sources and sinks of CO₂ when the mudflat was inundated, but it is difficult to be sure without pCO₂ data for the overlying water, to estimate outgassing/uptake as pointed out in abstract. While this study is an interesting start, the authors try to draw too strong conclusions from it for the lagoon C balance (focus 3 of the paper). To do this with any confidence would require seasonal water pCO₂ exchange information, data on riverine C input and wintertime EC measurements and probably more detail of the spatial variations with different mudflat areas, as this study hints at some considerable variation between sites 1 and 2, and actually has a rather limited data set."

It is true. If processes which control CO₂ fluxes in the tidal flat during the emersion were relatively well assessed (it still needs to be compared with other methodology,

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as suggested by Referee#2), it is clear that quick variations in CO₂ fluxes during the immersion are much more complex as they result from a large panel of processes and associated biological activity in the flat, and outside of the flat, as water moves with the tide. To go further on the carbon balance of the flat (goal 3), we are aware that other measurements are needed, as rightly noticed by the Referee #1 and also by the Professor Cay in his comment. We will shorten the discussion part about the carbon budget of the flat as previously said in the revised manuscript, also in the interest of brevity. Water pCO₂ measurements were carried out in the intertidal flat during 24 hour cycles during each season (2008-2009) along with carbon inputs from the Arcachon watershed by rivers (entire hydrological cycle 2008-2009). Thus, we plan to bring and synthetize all these data in another paper to better estimate the carbon budget of the Arcachon flat.

Methods & Results: 1. "was the factor of 2 for energy to quantum flux checked ? given different instruments etc ? Use of such a factor might "homogenise PAR units" but does not necessarily mean the data are comparable – depending on calibration and site weather differences (which might be significant for cloudiness and its timing at a coastal location – how far apart? Admittedly this 2007 data is very short and not the main focus of the work."

The Cap Ferret Station is solely far from around 14 km to the Station 2. Consequently these global radiation (R_g) measurements are representative of our Station 2. The factor of 2 for energy to quantum flux is based on simultaneous global radiation and PAR measurements made over several years and sites in France by the INRA institute (Gironde, Grignon, Nancy, Toulouse...). The relation between R_g and PAR varies of less than 10% according to the site over time. Thus, this uncertainty of about 10% does not affect the conclusions of the submitted manuscript. Furthermore, these data were not used to search Fc/PAR linear correlations (Figure 8) but solely homogeneously presented in Figure 3 and describe in the result section 3.1 for weather conditions at Station 2.

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2. "There is such little data for the Sept 07 period, and the site was different, as were the instruments and data frequency capture that I question its use here, and its comparison with the much more extensive data collected at another site. Is it really valid to make such a comparison and draw conclusions from it?"

Despite all that is rightly mentioned above on the CO₂ fluxes measured at Station 2, we would like to keep this data set because it permits to clearly show strong spatial variations in terms of CO₂ fluxes linked to seagrass cover differences between both stations. However, this comment by the three referees has to be taken into account in the revised manuscript and our conclusions might be lessened.

3. "Skye instruments – not 'Skype'."

It is correct.

4. "'fetch around the mast always ranged between at least 1000m and 700m' might be clearer to say that the 'available fetch over homogeneous mudflat' or something."

This sentence will be modified in the revised paper.

5. "The very large occasional 'excursions' on the Fc graphs in Figs 3-5 means that the authors have changed the scale – which reduces the majority of fluxes to something invisible. This is a shame. If the aim is to show C budgets the fluxes that are more 'normal' should be given more visibility, although occasional large changes cannot be ignored. More attention should be given to exploring the reason for these large 'excursions' in the records – are they combination of tide state and wind direction and hence fetch?"

Efforts have been done on the scale shape of each y axis from Figures 3 to 6 to homogenize data series between deployments and make as clear as possible temporal variations. It will be taken to focus in the revised manuscript on the more normal variations that occurred especially in CO₂ fluxes (as in Figure 3.E). These large excursions in the records explained as destocking phenomena seem to happen during the emer-

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sion of the flat jointly with sudden wind direction changes and the turbulence set up in the morning. So, the tide is apparently not directly involved but this is rather mostly due to different wind directions and hence footprints (different areas contributing to CO₂ fluxes).

6. "the Fc-PAR relationships in Fig 8 – would be better to give values of fitted relationships in a table, with standard errors to enable reader to establish validity. Given the scatter of data, some of the fitted relationships would not be significant. Why is only one wind direction shown for Sept-Oct 08 when there is apparently lots of data for daytime in this period (Fig 7)?"

As also noticed by the Referee#2, it would be better to give values of fitted relationships in a table indeed. However, only significant linear relationships have been shown in Figure 8. It explains why in September/October 2008 (Station 1), despite the numerous data available for this period, only one significant regression was computed. In the revised manuscript, this figure will be replaced by a table.

7. "Fig 9 – given very limited data (just 4 days), and its evident scatter I question the validity of the non-linear curve fitted, and the 'beyond 50 cm' comment in the caption."

It is fair, we also hesitated to put this last Figure in the manuscript and it will be removed in the revised manuscript.

8. "Much is made of the analysis of fluxes with wind from different directions, but no details are given of the fetch differences associated with the different (arbitrary) wind sectors chosen, except that for *Zostera* cover in Table 2. However, that shows little sensitivity to direction, and no significant correlation with mean flux. So this suggests that other factors linked to direction are causing variation which could be env conditions like T, RH, PAR or it could be vegetation state not detectable from satellite cover assessments or factors like micro topography or tide state. For example, the reader does not know whether the tide approaches a measurement site from E, S, W or N – (in a complex estuarine situation like this there might be many possibilities?)."

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This relevant comment will be taken into account in the revised manuscript especially in the new discussion part about the results obtained by the other methods and their comparison with the Eddy Covariance technique.

"It might be better to make the case that e.g. the fetch conditions are different in a particular measured sector compared to another (using the satellite derived indices) and then analyse accordingly, rather than using multiple arbitrary sectors, particularly as there is limited data. So – plot Fc/PAR relationships for a large sector with low Zostera compared to a large sector assessed with high. This would be more akin to a footprint analysis. But I accept it remains difficult to separate cause and effect if weather conditions are also different with wind direction, and data runs short, relative to the factors involved – vegetation cover season, day/night and tide state."

It is indeed difficult. The results in Table 2 show little sensitivity of the relationships between mean CO₂ fluxes and seagrass covers to the direction, but a general pattern can be drawn as the most negative CO₂ fluxes are generally linked to the highest seagrass covers and inversely. However, other environmental factors are probably involved as suggested by the Referee. To investigate this point, other calculations of the seagrass cover using different wind sector sizes (45°/4 at minimum, due to the index computation methodology) will be attempted for diameters around the mast of 1000 m. The results (Table 2) will be modified consequently in the revised manuscript according to the significance of these new computations. The tide approaches the measurement sites by the west-southwest direction. But, for this analysis, only CO₂ fluxes matching with a zero water height were used so the fetch is solely influenced by the wind direction.

More minor editorial points 1. "The paper uses eddy correlation throughout. But actually the technique is eddy covariance – as expressed in their equation 1. There is a difference."

The technique is Eddy Covariance as the covariance between w' and c' is computed to

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get the CO₂ fluxes. In some references, Eddy Correlation is used as in the submitted manuscript, but referred to covariance basically. Eddy Covariance will be used all over the revised manuscript.

2. "Abstract: confusion likely over the use of negative and positive fluxes and uptakes effluxes and minimum and maximums. This is exemplified in the abstract line 12. "CO₂ fluxes showed generally low negative (influx) and positive (efflux) values and ranged from -13 to 19 at maximum". Difficult: -13 is certainly a lower value than +19, but there is potential for confusion over what is being said. Is this sentence useful anyway? are the authors trying to say that the absolute flux rates were small (not low ?). But -13 is not that low – not compared to the mean rates quoted later of -1.7 for example."

It is true this sentence can be quite confusing for the reader and has to be rewritten. It is better to say "Absolute CO₂ flux rates showed generally small negative (influx) and positive (efflux) values even if larger values (up to -13 and to 19 for influxes and effluxes, respectively) have been measured".

3. "Abstract too long, going through all the results – needs to be made briefer."

The abstract will be lightened in the revised manuscript.

4. "In several places main text could be shortened, as this would improve its impact. For example, the intro covers basic definitions of NEP and GPP and role of CR etc and heterotrophic ecosystems etc which most readers would be familiar with ; a shorter focused statement about differences between static terrestrial systems, and aquatic systems and the challenges in assessing C budgets in later due to 'advection' might be better. Also repetition in the text between e.g. end of introd, (2.1) and (2.2.2) over site details etc and 4 measurement periods etc (2.1, 2.2.2). Also too much detail about exact times of data, what direction sectors (standard ?) used etc."

Throughout the whole manuscript, we will strive to lighten the text, notably by avoiding repetitions and unnecessary details.

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5. "some quite 'loose' sentences: e.g. Abstract L 11. 'lagoon rapidly shifting from sink to source' explain which cause ? spatially, seasonally, or as tide covers it ? Abstract I. 13 'Low tide and daytime conditions were always associated with uptake" – I think this means "when there was low tide during the day", or "Combined low tide and daytime". Abstract I. 22 Why would resuspension of microphytobenthos cause them to become CO₂ sources during day ? (perhaps because of turbid water and lack of light – but not clearly stated; difficult to conclude without pCO₂ info for water; see discussion). Methods: 2.2.2 – 'entire system . . .replaced every 4 days ' – probably just the batteries replaced?"

Abstract I.11: It will be rephrased as "lagoon rapidly shifting from sink to source with the tide".

Abstract I.13: It will be rephrased as "low tide during the day was always associated with uptake".

Abstract I.22: This sentence will be reformulated in the revised manuscript.

Methods 2.2.2: The referee's remark is correct. We will rephrase the sentence.

6. "although widely used in environmental science 'Julian Day' is a specific calendar for astronomers – what the authors are using is 'day of year' actually."

"Julian Days" will be replaced by "days of the year".

7. "the grey shading is only explained on Fig 4 not when first used in Fig 3. In general captions show too much technical detail about processing of PAR values etc which is already in the text, or obvious. Fig 7 and Table 2 explains what wind direction abrevs are – unnecessary given that the graph & tables already give numerical directions. Fig 9 caption has a discussion point – delete?"

In Figure 3, the explanation for grey shading was omitted and it will be added. Technical detail about processing PAR will be removed from the captions along with the wind direction abbreviations. Figure 9 will be removed as explained in our answer to

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