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Biogeosciences Discussions

Interactive comment on "Spatial and temporal CO_2 exchanges measured by Eddy Correlation over a temperate intertidal flat and their relationships to net ecosystem production" *by* P. Polsenaere et al.

Anonymous Referee #3

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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.

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

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CO2 fluxes from a mudflat, or from the lagoon – both are used interchangeably in the abstract? 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. 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 ? 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 emersed conditions ?

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 sties 1 and 2, and actually has a rather limited data set.

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. 2.

BGD

8, C2317–C2321, 2011

There is such little data for the Sept 08 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 ? 3. Skye instruments – not 'Skype' . 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. 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 ? 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) ? 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. 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 ?). 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)

BGD

8, C2317–C2321, 2011

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.

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

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.

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

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 focussed 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.

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 l.

BGD

8, C2317–C2321, 2011

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 l. 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?

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.

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 abbrevs are – unnecessary given that the graph & tables already give numerical directions. Fig 9 caption has a discussion point – delete ?

Interactive comment on Biogeosciences Discuss., 8, 5451, 2011.