

Interactive comment on “Influence of Tidal Inundation on CO₂ Exchange between Salt Marshes and the Atmosphere” by Hafsah Nahrawi et al.

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GENERAL COMMENTS:

The submitted manuscript of Nahrawi et al. under review for journal Biogeosciences presents the tidal rhythm effect on atmospheric Eddy Covariance (EC) CO₂ fluxes between a salt marsh situated close to Sapelo Island (GA) and the atmosphere. Through three chosen data sets obtained in 2014, EC CO₂ fluxes are described and compared at the daily and monthly scales (neap/spring tides) during different *Spartina alterniflora* air exposures according to vegetation amounts/biomass and tidal water levels. Salt marshes represent key coastal systems among carbon budgets where the highest car-

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bon assimilation rates in the biosphere are measured. These heterogeneous and dynamic coastal systems remain particularly hard to study due to the high spatio-temporal heterogeneity in terms of CO₂ fluxes at terrestrial-aquatic exchange interfaces. Then, this study is of particular interest.

- My first general concern is, although the authors obtained an interesting and original full-year EC dataset, the tidal effect on CO₂ fluxes is solely addressed at the daily and monthly scales through small chosen data parts. Annual air-marsh CO₂ fluxes could be presented and discussed as well, to clearly quantify the tidal influence on the carbon budget of the studied marsh at the seasonal and annual scales. It is too bad as the EC technique allows computing such annual CO₂ exchanges through continuous and non-invasive measurements during particular periods (i.e. flooded and non-flooded). Although well quantified, the tidal effect on CO₂ fluxes is only shown through three chosen periods in 2014 on purpose. To go further and gain a real interest for the scientific community working on carbon budget over coastal systems, the manuscript should present or at least discuss the significance of the tidal effect on air-marsh CO₂ exchanges and associated partitioned metabolic fluxes (i.e. NPP, GPP and CR) at the annual scale in my opinion (please see for instance Rocha and Goulden, J. Geophys. Res., 113, 1-12, 2008 and cited references below).

- It leads to my second general concern on the submitted manuscript; I recognize that studies on carbon processes and fluxes over intertidal salt marshes are still scarce and their influence on adjacent water systems is maybe not the main point of the study here. However as the tidal rhythm influence is precisely addressed here, why the important “Marsh CO₂ Pump” concept initially proposed by Wang and Cai (2004) at the same location and studied by others later (to conceptualize tidal marshes as atmospheric CO₂ sink and inorganic carbon source to the coastal ocean) is not discussed here? The submitted manuscript as it stands now only deals with CO₂ flux comparison during spring and neap tide periods without encompassing the annual scale for carbon budget computations. Studies dealing with carbon budget over similar coastal ecosystems

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exist; the present study would significantly gain interest taking into account these latters and going toward the seasonal and annual scales as well. Please see studies of Guo et al. (Agr. Forest Meteorol., 149, 1820-1828, 2009), Yan et al. (Glob. Change Biol., 14, 1690-1702, 2008), Wang and Cai (Limnol. Oceanogr., 49, 341-354, 2004) and Wang et al. (Limnol. Oceanogr., 61, 1916-1931, 2016) for instance.

- My last general concern is authors clearly observed a CO₂ flux reduction at high tide during the day in comparison with low tide periods as already observed over same coastal systems, i.e. salt marshes (Houghton and Woodwell, Ecology, 61, 1434-1445, 1980; Kathilankal et al., Env. Res. Lett., 3, 1-6, 2008) and elsewhere over intertidal flats (Zemmelink et al., Geophys. Res. Lett., 36, 2009; Polsenaere et al., Biogeosciences, 9, 249-268, 2012) or Amazon floodplain (Morison et al., Oecologia, 125, 400-411, 2000) for instance. However, no explanation is given or even discussed to try to understand mechanisms involved in this reduction, especially those taking place at the air-water or air-marsh interfaces or underwater through the different involved inorganic carbon forms (i.e. gas transfer velocity and water-air gas exchange, water pCO₂ and DIC, GPP and CR as NEE drivers, . . ., please see cited references and others). Please see the next comments among with cited references above to help in the revision of the different sections of the manuscript. I would recommend further revisions in this way to allow the publication of the present paper of Nahrawi et al. for the journal Biogeosciences.

SPECIFIC COMMENTS:

Abstract: - l.12, 14-15, 17-18: as the authors got a full-year EC CO₂ flux dataset, analyzing the tidal effect on CO₂ fluxes for each month of 2014 according to vegetation biomasses, tide ratio per month, etc. . . at the daily, seasonal and annual scales would give to the submitted manuscript much more consistency and interest (as explained above).

1 Introduction:

- In the introduction section, there is a shortage of references on the different studies

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dealing with carbon dynamics over salt marshes (air-marsh CO₂ fluxes, lateral inorganic carbon fluxes/exports with adjacent systems. . .) but also over similar intertidal coastal systems (freshwater marsh, tidal flat, floodplain, . . .) where tidal effects have also been studied not solely with EC technique (see Clavier et al., Aquatic Botany, 95, 24-30, 2011; Ouisse et al., Mar. Ecol. Prog. Ser., 437, 79-87, 2011 and others). No information/reference is given about the atmospheric EC technique too. Mechanisms involved in the control of CO₂ fluxes over salt marshes are poorly explained (l.30-31). - There is also a lack of quantitative data from bibliography to indorse different statements (for instance l.3, 21-25). - With regards to objectives and as already explained, I would recommend to add explanations for the CO₂ flux reduction during immersion in the two first objectives and add a main third objective integrating the seasonal and annual scales to go further toward carbon budgets of the studied salt marsh.

2 Material and methods:

2.1 Please remind studies that have already been carried out at the same place. 2.2 Lack of information: why were two EC systems deployed (nothing is explained in the whole manuscript)? Why was a 5m-height used for the EC sensors (see footprint calculations)? EC systems were deployed in July 2013 and only data from May, October and August 2014 are presented, why? The reader understands it is for Spring/Neap tides comparisons at different vegetation growths but nothing is explained about it; also between July 2013 and January 2014, what has happened? 2.3 Figure 5 justified the interest to use the whole data set of 2014. According to footprint calculations, could the authors give to the reader an estimation of the footprint size (5 meters high ok but what about surface roughness, wind speeds, turbulence etc. . .) and directions (two EC systems were used with two opposite directions)? What about the potential influence of water during measurements especially at low tide (neap tide)? According to tide periods, the footprint size is modified (varying sensor heights). 2.4 Please specify the non-linear model equation for F_{mod}. It is not clear for the reader as it stands in the submitted manuscript. The last paragraph l. 26-5 on “August 2014 data selection dur-

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ing clear sky only” needs to be better explained and justified. Same calculations done for each tide (F_{tide}), each month (F_{tot}), each season and finally over the whole year would be very instructive.

3 Results:

In all sections of this result part, no statistics are given to endorse CO_2 flux or associated variable comparisons and correlations. Measured CO_2 fluxes could be specified through NPP, GPP and CR values. The effect of immersion on these metabolic fluxes (instead of CO_2 fluxes during the day and night only) could be studied to go further as mentioned before. Please see technical comments for comments on associated figures and tables. - 1.26-27, p.5: I don't understand why a 0.4 tide ratio corresponds to 40% of submerged plant parts in August 2014? - The introductory paragraph in 3.2 sub-section is too general and imprecise and maybe useless as flux values are given next in 3.2.1 and 3.2.2 (1.2 “late morning to noon time”; 1.7 “respiration rates . . . increase . . .”; 1.12 “. . .10 times . . .” ?) - 3.2.2 1.26-28 “reduction”, please quantify them! - 3.3 (and associated figure 14): interesting but are R^2 significant? Here again, adding data from other months in 2014 (than August) will probably bring more consistency and significance to the analysis. - 3.4 I don't fully understand this sub-section at the end of the result part although the monthly analysis in August is interesting and should be done for other months (or seasons) over the year.

4 Discussion:

The discussion part needs to be reorganized and reviewed with regards to previous general comments. In the submitted manuscript, it rather corresponds to result (sub-section 4.1 for instance) descriptions than a real discussion on carbon processes and fluxes over salt marshes with associated environmental controls. Very few references are cited. Again, I really believe orientating the paper toward carbon dynamics at both diurnal, seasonal and annual scales would deeply increase the impact of the paper to the scientific community working on such coastal systems. - 1.1, p.8: “a net uptake

of CO₂ during nighttime immersion”: it is necessarily associated to inorganic carbon dynamics in water bodies close to the tidal marsh system (advection, hydrodynamic, air-water gas transfer velocity, ...). But it is not discuss in the submitted manuscript? - l.16, p.8: “a certain water table threshold”? l.29-30: “140.79 micromol m⁻² s⁻¹ corresponding to as much as 15% of the total monthly reduction”? I don’t understand the flux value; please review it.

5. Conclusion:

The first two paragraphs are too general and the third one should be specified with estimations of CO₂ flux reduction by immersion at the annual scale from a carbon budget point of view. Also a point could be done here on the interest to use simultaneously the atmospheric EC and aquatic EC techniques (see Berg et al., Mar. Ecol. Prog. Ser., 261, 75-83, 2003 and other publications on *Zostera marina* seagrass meadows of the eastern shore of Virginia for more information on the technique) associated to water DIC measurements (cited references) to better measure and integrate salt marsh metabolism processes/fluxes during both emersion and immersion periods to specify the role of salt marshes among regional and global carbon budgets.

TECHNICAL COMMENTS:

- 14 figures are really too much.
- Figure 1 is maybe not necessary.
- Figure 3 (caption) needs to be specified to help the reader to understand exactly when the marsh is totally emerged, partially emerged/immersed and totally immersed during neap tides and spring tides. Spring and neap tides occur twice during each month so an associated table with number of hours during which the marsh is fully emerged/immersed and partially exposed to water during each month could be useful for instance. Fully exposed to air: low tides during neap tides? Low tides during spring tides? High tides during neap tides? Fully immersed: high tide during Spring tide?

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High tide during neap tide? During transition periods (rising/ebbing) of the tide, how is the marsh? This part really needs to be clearer.

- Figure 4: as it is, this latter doesn't bring a lot of information (weak captions. . .)
- Figure 5: interesting figure that could be associated to a footprint analysis/diagram according to wind speeds/directions, surface roughness and turbulence. I am wondering why EC systems worked so poorly during nighttime periods (00:00-06:00 and 18:00-24:00). Could it only be explained by the associated low turbulence regimes at night!
- Figure 6: not very useful as this interesting parameter is comprehensible without figure.
- Figure 7: How many phenocam images were taken during the study? Four? The phenology of the salt marsh is particularly important in the control of carbon flux dynamic. Do satellite images exist for the studied area to estimate the relative contribution of the marsh in the EC footprint to measured CO₂ fluxes?
- Figure 8: not necessary (cf. see previous comments).
- Figure 9: very informative figure except it concerns August 2014 only!
- Figure 10: very informative figure. Partitioned fluxes, i.e. NPP, GPP and CR could be specified on it.
- Figures 11,12 and 13: It is very hard for the reader to follow and understand these figures and associated text in the result sections (daily fluxes, mean fluxes?). All of them are not necessary. Wouldn't be possible to do one figure with averaged CO₂ fluxes with corresponding immersed and emerged periods during day and night for each month? Keep Time of the day (24 hours) in x-axis and for the y-axis, add monthly mean CO₂ fluxes (with associated SD), PAR and water level curves with clear/shaded areas for day/night periods respectively and vertical dotted lines for immersed/emerged periods for instance.

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- Figure 14: see previous comments.
- Tables: see previous comments too.

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