Response to Referee Comment 2 (RC2)

GENERAL COMMENTS:

Comment 1: My first general concern is, although the authors obtained an interesting and original full-year EC dataset, the tidal effect on CO_2 fluxes is solely addressed at the daily and monthly scales through small chosen data parts. Annual air-marsh CO_2 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 CO_2 exchanges through continuous and non-invasive measurements during particular periods (i.e. flooded and non-flooded). Although well quantified, the tidal effect on CO_2 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 CO_2 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).

Response 1: We will provide more data instead of just using one month of data (August) to better explain the total monthly flux reduction based on our approach. For carbon budget, we have separate paper that discuss on that matter. This submitted manuscript only focused on tidal effect on CO_2 fluxes and how this reduction is translated into quantitative estimation of total monthly reduction for the study month.

Comment 2: 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 CO_2 Pump" concept initially proposed by Wang and Cai (2004) at the same location and studied by others later (to conceptualize tidal marshes as atmospheric CO_2 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 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.

Response 2: We will look into the suggested references. However, one of the main purpose of this study is to quantify the daytime monthly reduction of CO_2 fluxes. Most of the study only reported the instantaneous flux reduction rather than looking at a longer interval such as monthly basis. By having some knowledge on how much monthly (or even seasonally and annually) reduction in CO_2 fluxes, we will have a bigger and better picture on how much overall reduction for some interval of time which are more meaningful than just looking at a single point of time which most reported by previous study.

Comment 3: My last general concern is authors clearly observed a CO_2 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 pCO_2 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.

Response 3: Thank you for pointing this out. We will look in details on this subject and incorporate it in the revised version of the manuscript. However, as mentioned earlier, since most of previous studies only reported the instantaneous reduction of CO_2 flux, we, in this paper, use monthly basis to quantify CO_2 reduction in salt marsh ecosystem due to tidal inundation. The reduction could be small instantaneously but very significant when we quantify it for a long-term period (i.e monthly, seasonally or annually) which gives more meaning than just at one single point of time.

<u>SPECIFIC COMMENTS:</u>

Comment – Abstract: - l.12, 14-15, 17-18: as the authors got a full-year EC CO_2 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).

Response – Abstract: Yes, we have full-year EC CO_2 flux dataset. However, in this paper, we only focus on effect on tidal on CO_2 reduction and to emphasis how we quantify the monthly basis of CO_2 reduction. Besides, there is still lack of studies on neap and spring tide comparison in terms of CO_2 reduction.

1 Introduction:

Comment 1.1: In the introduction section, there is a shortage of references on the different studies dealing with carbon dynamics over salt marshes (air-marsh CO_2 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 CO_2 fluxes over salt marshes are poorly explained (l.30-31).

Response 1.1: We will include more information on the stated subject based on the existing references in the revised version of the manuscript. Thank you for pointing out several references related to the subject. We will also add more information and references related to EC technique in the revised version of the manuscript.

Comment 1.2: 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 CO_2 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.

Response 1.2: We will add more explanations for the CO_2 flux reduction during immersion as suggested. However, as mentioned earlier, this paper only focuses on the effect of tide on CO_2 exchange and we are in the final stage of preparing our paper on carbon budget in similar ecosystem.

2 Material and methods:

Comment 2.1: Please remind studies that have already been carried out at the same place.

Response 2.1: Thank you for the reminder and we will look into it.

Comment 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?

Response 2.2: The complexity and heterogeneity of the ecosystems drove us to use two EC systems at the study area. Installing two EC systems at one flux tower minimise the gaps in the data due to maintenance and calibration, instruments malfunction, and accommodates seasonal changes in changing in wind direction. The south system facing south covers the angle from 90° to 270° and the rest of the area is covered by the north system facing north. The instrumentation is 5 m above ground, assuming that footprint sampled by the tower has a radius ranging from 0.5 to 1 km. However, it still depending on the wind direction and fetch, surface roughness, measurement height and atmospheric stability. Based on the footprint calculation, we make sure that only areas that are covered with *Spartina alterniflora* is being sampled. As mentioned in section 2.4 page 4 l. 26 – 26 and page 5 l. 1 – 5, only days with clear sky condition during spring tide and neap tide days were used. There were very limited days with such condition. Therefore, we only able to use days in May and October for neap and spring tide comparisons in our study. The two different months represent the comparison between neap and spring tide days. Meanwhile, August data was used randomly to quantitatively estimate monthly CO_2 reduction. These are the two main objectives in our study. We only select the data based on specific cases between July 2013 and January 2014.

Comment 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).

Response 2.3: Based on our footprint calculation, the footprint is approximately from 300m and can be up to 8000m. We will include some ideas on the component mentioned above in our revised manuscript as well as wind direction. We did not see potential influence of water during neap tide. The sensors height was not modified throughout the study period.

Comment 2.4: Please specify the non-linear model equation for Fmod. 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 during clear sky only" needs to be better explained and justified. Same calculations done for each tide (Ftide), each month (Ftot), each season and finally over the whole year would be very instructive.

Response 2.4: We used August 2014 daytime data to estimate monthly CO_2 reduction. For neap and spring tide comparison, only days with clear sky were selected. We will specify in detail the equation and calculations for Fmod, Ftide and Ftot in the revised version of the manuscript.

<u>3 Results:</u>

Comment 3.1: In all sections of this result part, no statistics are given to indorse CO_2 flux or associated variable comparisons and correlations.

Response 3.1: We will include a statistical analysis in the revised version of the manuscript.

Comment 3.2: 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.

Response 3.2: In this paper, we are only interested in the effect of tide on CO_2 exchange. We might not be able to include the subject in this paper because we are in the final stage preparing a paper related to the subject.

Comment 3.3: Please see technical comments for comments on associated figures and tables. - l.26-27, p.5: I don't understand why a 0.4 tide ratio corresponds to 40% of submerged plant parts in August 2014? –

Response 3.3: The tide ratio was calculated as: Tide ratio = Zt/h

Where Zt is the tide height and h is the mean plant height. When the tide ratio is equals to 1 (or 100%, if it is converted into percentage), the plants are completely submerged and the plant were completely exposed to the atmosphere when the tide ratio is equals to 0. Therefore, we assumed that based on the tide ratio, we converted the value into percentage which represents how much the plant is submerged.

Comment 3.4: The introductive 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 (l.2 "late morning to noon time"; l.7 "respiration rates . . . increase. . ."; l.12 ". . .10 times. . ." ?) - 3.2.2 l.26-28 " reduction", please quantify them!

Response 3.4: We will quantify them as suggested.

Comment 3.5: 3.3 (and associated figure 14): interesting but are R2 significant? Here again, adding data from other months in 2014 (than August) will probably bring more consistency and significance to the analysis.

Response 3.5: We will include statistical analysis in the revised version of the manuscript. We will also add more months in the study as suggested.

Comment 3.6: 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.

Response 3.6: We will explain in detail how we obtained the data in this section in the revised version of the manuscript. We will also add more months (minimum 4 months that represents different seasons) for comparison.

4 Discussion:

Comment 4.1: The discussion part needs to be reorganized and reviewed with regards to previous general comments. In the submitted manuscript, it rather corresponds to result (subsection 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.

Response 4.1: We will reorganize and review our paper as suggested. We will also cite more reference to strengthen our findings on the subject matter in the revised version of the manuscript.

Comment 4.2: l.1, p.8: " a net uptake of CO_2 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?

Response 4.2: We admit that we did In this study, we did not discuss much night time fluxes as how we described the day time fluxes. It is because, we lost a lot of night time data due to a very low u* that resulted in a very large footprint. Besides, we have lack of information related to nighttime condition of the study area, thus we focused more on daytime.

Comment 4.3: 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.

Response 4.3: We will review it and provide details explanation on how we obtained the data in the revised version of the manuscript. Some previous study had introduced a water table threshold (Kathilankal et. al, 2008; Forbrich and Giblin, 2015). The mentioned value was the overall daytime CO_2 reduction in August. Detail explanation will be included in the revised manuscript.

5. Conclusion:

Comment: The first two paragraphs are too general and the third one should be specified with estimations of CO_2 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.

Response: We will revise our conclusion based on the suggestion. Thank you for pointing many important things which give us more ideas how to improve our manuscript.

TECHNICAL COMMENTS:

Comment 1: 14 figures are really too much.

Response 1: We will try to revise this figure so that it won't look too crowded and easy to digest.

Comment 2: Figure 1 is maybe not necessary.

Response 2: Figure 1 will be deleted

Comment 3: 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?

Response 3: We also believe it is a good idea to include all the mentioned information as suggested above. We will try to provide all the information in the revised manuscript.