Response to Referee Comment 3 (RC3)

Major Comments:

Comment A: The overall premise of the study – that "documentation on the exchange of CO_2 between salt marsh ecosystem and atmosphere measured by modern eddy-covariance systems are still very limited (Kathilankal et al., 2008)." (pg 2 line 17-18) – is substantially flawed in that the manuscript does not thoroughly review and cite literature review on this very specific topic. Specifically, 5 key progenitors to this manuscript are: Kathilankal et al., 2008; Moffett et al. 2010; Schafer et al. 2014; Artigas et al. 2015; Forbrich and Giblin, 2015. These may not be all the relevant papers, but each of them has measured, analyzed, discussed, and published on the topic (tidal flooding effects on NEE) of this manuscript. Thoroughly reviewing these and other potentially related papers should have been the first responsibility executed by the study. In particular, there is no important physical difference between the "spring vs neap" factor that is the focus of this manuscript and the presence vs absence of tidal flooding studied by both Kathilankal et al., 2008 and Moffett et al. 2010. It was not initially clear in this manuscript that the study would compare flooded to non-flooded conditions. This was suggested, but not clear, on page 3 line 13 "During high spring tide, most of the vegetation is submerged and exposed during low spring tide and neap tide period." Only upon getting to Figure 8 was it clear to this reader that the "neap tide" conditions actually represent "no flooding" from the perspective of the vegetation, so the comparison is flood vs no flood (not higher spring vs lower neap flood depth as this reader mistakenly assumed at first). If multiple prior studies have compared salt marsh NEE during flooded and non-flooded conditions, and even taken into account the effects of different flood depths (starting with Moffett et al. 2010), the what is the unique contribution intended by this manuscript?

Response A: Thank you for your comment. We are aware that there are similar studies about the effect of tide on CO_2 exchange. However, these studies mostly reported the instantaneous CO_2 reduction due to tide and mostly descriptively compare flood with no flood. In our study, we try to quantitatively estimate the reduction of CO_2 and expand our estimation to monthly basis. We believe that, by having a quantitatively data on the reduction, we would have a clearer picture on how much the reduction we are talking about.

Comment B: The specific model used to calculate the CO_2 exchange during non-flooded periods is not specified in the methods. All that is said is "Fmod is calculated CO_2 flux from a light response curve for CO_2 exchange model during non-flooded conditions," (page 4 line 24) with no model or methodological citation.

Response B: We will specify in detail the model that we used to calculate CO_2 exchange during non-flooded periods in the revised version of the manuscript.

Comment C: The methods paragraph beginning on page 4 with "Data of August 2014 was used to study. . ." is very unclear. After reading it 4 times and also referring to the table and figures I still cannot understand what analysis was done on the August data, what on the May, what on the

October, and why the same analysis seems not to have been done on either all or just one of those time periods.

Response C: August data was used to quantitatively estimate daytime monthly CO_2 reduction. This month was selected randomly to demonstrate the daytime monthly reduction of CO_2. We will add more months (different season) to put more meaning into this estimation. As mentioned in section 2.4 page 4 line 26 – 28 and page 5 line 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.

Comment D: I am further concerned with the aspect of the study based on a tide-to-vegetation ratio. On page 4 the ratio was defined as (tide height) / (mean plant height). However, on page 5 and in Table 1, I see that the ranges of plant heights were quite large. It was reported on page 5:

- "The mean plant height in May 2014 was 0.64 ± 0.38 m."

- "In October. . . the mean plant height was 0.56 ± 0.41 m."

- "in August. . . monthly mean plant height was recorded at 0.61 ± 0.45 m."

It is not stated what the second number in these cases was (0.38, 0.41, and 0.45), but I assume it may be a standard deviation; if so, these results seem to say that the distribution of plant height was very broad, with many plants of nearly zero height and also many of around a meter or more. If instead these second numbers are standard errors (as perhaps they should be?) then it suggests that the means are not at all well constrained. In either case, how then is the ratio (tide height) / (mean plant height) a metric that captures flood-vegetation interactions in a comprehensive way? Lastly, the methods section did not include information on how plant height was surveyed, over what area, whether by plot sampling and extrapolation or some kind of exhaustive sampling, whether by LIDAR (which is impossible to use to obtain plant height and difficult to use even for sediment height over low-relief, low/soft vegetation marshes), etc., so it is impossible to interpret what these standard deviations or errors may be representing in terms of sampled variability.

Response D: Spartina has a very wide range of plant height and classify as three different types; short, medium and tall. We used mean value to calculate our tide ratio. A quadrat survey was conducted for plant sampling and the plant height was measured monthly. The second number represents standard deviation (not standard error as in manuscript, apologize for the error). We are aware that we simplified the method by using the average of the plant height. However, we believe that this potential method could be improved in the future and be used as a tool to estimate the amount of biomass exposed to the atmosphere.

Comment E: Although Figure 14 appears interesting, I find it unpublishable as is since there was no disclosure in the Methods section of how these light response curves were obtained. I am doubly concerned because I myself attempted some years ago (unpublished) using a LICOR 6400 to gather light response curves from Spartina foliosa contained in a bucket in a laboratory and flooded to different depths. Over short terms – if using the rapid measurement technique of collecting data over only seconds to minutes at each flooding or light level – I did see what appeared to be response curves. However, I also conducted the study using the slow equilibration technique, collecting data for tens of minutes to hours for each flood or light level; those curves appeared bizarre and even inverse from what one would expect. Only after plotting all the data chronologically I realized I had actually measured the diurnal circadian cycle of the Spartina (due to the long day/evening in the lab of continuous experiments) and therefore negligible, if any, actual response to the flooding itself. Although it is nearly certain that the authors conducted a more nuanced and thorough experiment than my one failed attempt at such a thing, lacking any information about how the light response curve portions of the study were done, I cannot say! Likewise, I cannot have confidence in the conclusions of Section 3.3 without further methodological information.

Response E: In this study, we used direct relationship between CO_2 fluxes and PAR to obtain the light response curve. We will explain in detail the method that lead to Figure 14 in the revised version of the manuscript.

Minor Comments:

Comment 1: pg 1 line 8 – It is not appropriate to quote in the abstract a quantitative value, the precise magnitude of which is the subject of a whole field of ongoing research (this manuscript included), and for which other values have been offered (e.g., in Forbrich & Giblin 2015), especially when it is a value that was not derived by the study itself and is deprived of a proper citation (to Chmura et al 2003). *Remove* this value of 210 g C /m2 / yr from the abstract. Use a qualitative magnitude instead, if need be to make the point.

Response 1: The value has been removed.

Comment 2: pg 1 line 13 – Amend to "The conditions with a high tide-to-vegetation height ratio. . ." Without reference to HEIGHT it is unclear what values are being divided. Look for this omission and correct throughout manuscript.

Response 2: Corrected

Comment 3: pg 1 line 14 – Amend to ". . .conditions with a low ratio." It is no more a "tide ratio" than it is a "vegetation ratio" – the numerator nor denominator can stand on its own, so just call it a ratio. Look for this confusion and correct throughout manuscript.

Response 3: Corrected

Comment 4: Figure 1 is not needed.

Response 4: Removed from the manuscript

Comment 5: What are the sources of the ecoregion and land classification data in Figure 2? Should be cited.

Response 5: Will add the citation.

Comment 6: Figure 3 not needed.

Response 6: Noted

Comment 7: Figure 4 not needed.

Response 7: Noted

Comment 8: Figure 5 seems to show that hardly any nighttime data were retained after QA/QC.

Analysis and discussion should be provided of whether sufficient data remained to make calculations and inferences at night. The figure should be moved to an appendix/supplement, however.

Response 8: Due to night time data losses (mainly because of very small u* and large footprint) this paper focuses more on daytime events.

Comment 9: page 4 line 8 – I do not understand "Data from north and south systems were combined and selected based on the climatological footprint". Please explain further.

Response 9: In this study, we installed two EC systems at one flux tower minimize the gaps in the data due to maintenance and calibration instruments malfunction and accommodates seasonal changes in changing in wind direction. One tower facing north direction and another one facing south direction. The systems can cover all different angles of the study area which means more high-quality data were captured based on the prevailing wind that was coming from all different directions throughout the year. 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 data from these two systems were combined and filtered based on the footprint analysis where only areas with Spartina alterniflora is studied.

Comment 10: page 4 line 9 – I do not understand "Only measurements that contributed to more than 70% of the CO_2 flux within the study area were used". Please explain further.

Response 10:

We used Kormann and Meixner (2001) in our footprint calculation. This method provides the cumulative source contribution (CSC) of the study field and the surrounding areas expressed in terms of percentage. 70% CSC implies that the distance from the measurement point contributes 70% of the observed flux. In our study, only data that fulfill the 70% CSC was retained.

Comment 11: Figure 6 not needed.

Response 11: Noted

Comment 12: Figure 7 not needed.

Response 12: Noted

Comment 13: Figure 9 is not needed; also see Major Comments C and D, above, regarding related confusion as to what the study actually did.

Response 13: Noted

Comment 14: Figure 10 is impressive and demonstrates the incredible volume of interesting data collected by the study team. However, see Minor Comment number 8 – I wonder a bit at the small standard deviations reported for nighttime NEE values given that the sample size after QA/QC was quite small for night times. The plot is very similar to that by Kathilankal et al., 2008 that spanned May through October, although this manuscript helpfully expands the figure through all 12 months.

Response 14: We believe this figure helps to understand the CO_2 exchange pattern at the study site. The study site growing season is almost a full year. We will look into the standard deviations as mentioned above although we are confident with how we filtered and processed our data to come out with good quality ones.

Comment 15: Figure 11 appears nearly identical to the kind of data presented in Kathilankal et al., 2008 and in Moffett et al. 2010. What is the new scientific insight added by this study that warrants re-publishing a known phenomenon?

Response 15: We found that figure 11 which similar to both Kathilankat et al., 2008 and Moffett et al. 2010 could give a very good view on how CO_2 exchange change diurnally and throughout the year.

Comment 16: Sections 3.2.1 and 3.2.2 – The manuscript to this point has not made it clear to me why we should be interested to compare May and October data, and so I do not see the point of these sections or Figure 12 or 13. Recommend omitting.

Response 16: The comparison was made for neap and spring tide events. We will reconsider to omit the results (as suggested). Otherwise we will add more details to it.

Comment 17: Page 8 line 18-19. This manuscript writes "Site studies of these authors are dominated by marsh grass species which grow upright, either Spartina alterniflora (Kathilankal et al., 2008) or Spartina foliosa and Distichlis spicata (Forbrich and Giblin, 2015; Moffett et al., 2010)." This is a direct quote – actually a mis-quote – of Forbrich and Giblin 2015, who wrote (page 1835) "Sites studied by these authors are both dominated by marsh grass species which grow upright, either Spartina alterniflora [Kathilankal et al., 2008] or Spartina foliosa and Distichlis spicata [Moffett et al., 2010]." but also clarified that "At our site, Spartina patens often lies prostrate forming a dense, green carpet. . ." (hence the mis-quote). [And actually the site by Moffett et al. was as much Salicornia virginica as Spartina and Distichlis; west-coast US marshes are odd compared to east.]

Response 17: We apologize that we overlooked this sentence which at first it was put there for author’s reference and intended to be restructured. We will amend this sentence in the revised version of the manuscript.

Comment 18: If use of a digital online supplement is enabled by the journal, the figures to be removed could be provided in a supplement.

Response 18: Noted.