

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

K. Moffett (Referee)

kevan.moffett@wsu.edu

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Summary

This manuscript presents monthly aggregates of a year of eddy covariance data from an intertidal salt marsh. From within that data, subsets are extracted for assessment of the reduction in net marsh CO₂ uptake (Net Ecosystem Exchange, NEE) during daytime flooding tides (occurring during spring tide at this location) compared to days without mid-day flooding tides (occurring during neap tide at this location). A non-dimensionalization of the effect of flooding tides on NEE is attempted via the ratio of tidal depth to vegetation height on the marsh platform. Some *Spartina alterniflora*

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photosynthesis light response curves are also presented with some possible, but unclear, relation to tidal flooding (as methods are not disclosed). These three foci, NEE data, tide-to-vegetation ratio, and light response curves could be better integrated into a coherent through-line for the manuscript. Overall, although the manuscript presents interesting-looking data, I unfortunately cannot recommend it for publication. My criteria for recommending rejection are the lack of disclosure of multiple important aspects of the methodology, confusing presentation of the results, strikingly lacking review or thorough discussion of highly pertinent literature, and lack of substantively new scientific contribution beyond nearly replicating figures of prior papers with new data. In my opinion, revising the manuscript so as to fix these issues, especially the last one, would thereby generate a wholly new manuscript that should be considered afresh, not as a revision.

Major Comments

A. The overall premise of the study – that “documentation on the exchange of CO₂ 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.

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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), then what is the unique contribution intended by this manuscript?

B. The specific model used to calculate the CO₂ exchange during non-flooded periods is not specified in the methods. All that is said is “F_{mod} is calculated CO₂ flux from a light response curve for CO₂ exchange model during non-flooded conditions,” (page 4 line 24) with no model or methodological citation.

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.

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.”

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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 heights 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.

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

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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.

Minor Comments

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.
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.
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.
4. Figure 1 is not needed.
5. What are the sources of the ecoregion and land classification data in Figure 2? Should be cited.
6. Figure 3 not needed.
7. Figure 4 not needed.
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

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make calculations and inferences at night. The figure should be moved to an appendix/supplement, however.

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.

10. page 4 line 9 – I do not understand “Only measurements that contributed to more than 70% of the CO₂ flux within the study area were used”. Please explain further.

11. Figure 6 not needed.

12. Figure 7 not needed.

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

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.

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?

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.

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;

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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.]

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

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