Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-356-RC1, 2017

Interactive comment on "Influence of Tidal Inundation on CO_2 Exchange between Salt Marshes and the Atmosphere" *by* Hafsah Nahrawi et al.

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Received and published: 25 September 2017

The manuscript demonstrates the instantaneous effects of tidal inundation on daytime CO2 exchange measured using eddy covariance during one year in a salt marsh in Georgia, USA. The authors aim to determine the effect of inundation on CO2 exchange and quantify it. The focus of the study is on the reduction in daytime NEE, even though the authors state that they detect small reductions in nighttime fluxes as well. The authors use a ratio of vegetation height and tide height relative to the surface to classify how much of the canopy is submerged. To quantify the observed reduction in NEE during inundation, they model NEE during non-flooded situation by fitting a light re-

sponse curve to daytime data when the marsh is not flooded. Subsequently, they use these modeled fluxes as a reference and calculate the difference between measured and modeled fluxes as a measure of flux reduction.

Eddy covariance measurements in tidal wetlands are still rare and new data offer valuable information about contemporary carbon cycling in these systems. However, even though there are not many studies, all of the published studies address the impact of tidal inundation. Thus, to me the most interesting aspect of this study is the approach used to quantify the reduction in CO2 fluxes, because this pattern seems to be consistent in salt marshes. However, I find the results mostly descriptive and not always consistent with the method description (see detailed comments below). Overall, I am missing a discussion of the advantages and/or disadvantages of this approach compared to earlier approaches (e.g. Kathilankal et al. (2008) or Forbrich & Giblin (2015)) and of implications for contemporary carbon cycling.

Major comments:
- The authors argue that with future sea level rise (and subsequent prolonged inundation), CO2 net uptake might be lower in the future and the marsh will convert into a mudflat. I disagree with this: Many studies have shown that – while biomass production is important – it is not the only driver for the long-term stability of salt marshes with regard to sea level rise. Mostly it will depend on interactions between factors such as biomass production, sediment availability, tide range, rate of sea level rise as well as the possibility to transgress further inland (e.g. Morris et al. 2002, Kirwan et al. 2010, Kirwan et al. 2016).

- My impression is that the description of the approach and the results are contradictory: From the results (Section 3.3, 3.4, Fig. 14, Tab. 4) I take it, that the August CO2 fluxes were grouped in three classes based on the tide ratio and a light response curve was fitted to them separately as well as to 'non-flooded' conditions. This is not how I understood the methods (Section 2.4): I expected the light response curve to be fitted only to the CO2 fluxes under non-flooded conditions (to get a reference value for non-flooded conditions). Afterwards, the modelled fluxes would be subtracted from the observed fluxes - independently from the tide ratio – to quantify the flux reduction. In a revised version of the manuscript, this should be explained better. It is not clear to me why the light response curve is fitted to CO2 fluxes measured during partially or completely submerged conditions. I thought, the data coverage is so good that you know the magnitude of the 'real' fluxes, but you need to estimate how large they were if there was no tidal flooding (thus the reference value). Subsequently, I am not sure how to interpret the values for *Fmea* and *Fmod* in Tab. 4. Especially since the time series is not continuous (since the night time data are not used), I think the only time period that give us reasonable information is each single daytime flooding event. Thus, I suggest that the difference between *Fmea* and *Fmod* (only determined for non-flooded conditions) be calculated for each single daytime tide event and grouped according to tide ratio afterwards.

- The comparison of neap and spring tide conditions in May and October is only descriptive and not connected to the fitting approach. I suggest to using the fitting approach for each month of the year and use these selected days to demonstrate the approach described above.

Minor comments:

page 1 ll. 22-25: Are all these numbers from the Chmura paper? Otherwise, they need references.
Page 2 l. 2 delete 'of'
page 2 ll 10-10-13: see comments above: There are biogeomorphic feedbacks between vegetation cover, tidal inundation and accretion rates, that are not directly linked to instantaneous CO2 exchange but help marshes to keep their position relative to mean sea level.
Page 2 ll. 29: I would rephrase that, do you 'hypothesize' this rather than 'believe'?
Page 2 ll30: delete 'also'

Page 3 ll 6-9: Can you mention the height differences of the tall, medium and short plants? Which one do you use for the tide ratio? Also, how much variation is there during the entire growing season?
Page 3 ll12-14: You do not need to say here that tides affect CO2 exchange greatly, just mention the tide range.
Page 3 ll23: Are the tide heights reported in NAVD88 or relative to surface?
Page 4 ll. 7: Which quality control steps were applied?
Page 4 ll18-25: See comments above
Page 4 ll26 – page 5 ll 5: Considering the high quality of the data set, I am surprised that you pick only one month and a couple of days to assess the tidal influence. The data coverage especially during the day is high and it would be possible to do this over the entire year and not only restrict yourself to the same climatic conditions (i.e. high irradiation).
Page 5 ll 14 and ll 20-21: Contrary to these statements, Fig 8 shows that the marsh surface IS flooded during spring tide?!
Page 6 ll 2-8: Most of this is descriptive and shown in Fig. 10 anyway. However, the observation that plants suffered from heat stress in July and August is interesting and would merit more analysis and discussion.
Page 6 ll 14 – Page 7 ll5: See comments above
Page 7 ll 7 – 11: See comments above
Page 7 ll12 – 18: Why do you compare two random days (September as opposed to May, October or August as previously used) to give an example for the flux reduction instead of describing the results from the fitting procedure?
Page 7 ll 21-24: I think this should go into 'results'.
Page 9 ll 2-5: See comments above: CO2 exchange might be reduced instantaneously during inundation but that cannot be extrapolated over long periods of time.

Figures:
Fig. 1 and 4 are not really necessary.

Fig. 3 and Fig. 6 could be combined.
Fig. 11: This would work better with days that have been analyzed or discussed before (e.g. May/October).
Fig. 12 and 13 are not really necessary.
Fig. 14 needs more explanation: E.g., the different symbols are not explained, only the fit.

Table 1: All the values are given in the text, so this table is a repetition. Either change the text or remove the table.
Table 4 : See comments above.

References:
Forbrich & Giblin 2015, Journal of Geophysical Research
Kathilankal et al. 2008, Environmental Research Letters
Kirwan et al. 2010, Geophysical Research Letters
Kirwan et al. 2016, Nature Climate Change
Moffett et al. 2010, Water Resources Research
Morris et al. 2002, Ecology

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