*Following Response to reviewer 1*

**Response to reviewer 2**

The manuscript aims at modeling the seasonal variations of ecosystem respiration in a coastal wetland of China (Liaohe Delta) dominated by Suaeda salsa, in relation with environmental factors. Besides the fact that the present work seems to be redundant with, at least, a previous study conducted in the same area (Ye et al., 2016), I think that the methods used (which, in addition, are not appropriately described) were not appropriate to answer the tackled question. Thus, I do not think that the present manuscript is suitable for publication in Biogeosciences.

General comments - Since the study area is dominated by a herbaceaous species (S. salsa), a global understanding of gaseous CO2 exchanges should also consider photosynthesis. That would imply measurements of CO2 exchanges under light conditions which would be an evaluation of the net community production, i.e. the net balance between respiration fluxes (due to plants, bacteria, etc...) and photosynthetic fluxes (due to plants and other potential primary producers). Here, the measurements were conducted only in darkness (by the way, the authors did not mention it, but it seems that the 8100-103 survey chamber is a dark chamber...), which actually estimated community respiration, but which is not sufficient to understand the global CO2 exchanges.

*Response: We agree with this statement fully. We did the measurements using an opaque survey collar and chamber, which means, our focus was on ecosystem respiration only. These were dark chambers, which is a point we now make multiple times in the revision. Our goal is not to estimate net $CO_2$ uptake versus loss for complete $CO_2$ balance, but rather to focus just on respiration partitioning. We describe that better in the revised manuscript. By identifying $CO_2$ derived from different part of S.salsa wetland, we promote the understanding of ecosystem respiration of S.salsa wetlands, which provides insights that plants contribute to a specific amount of ecosystem respiration. In addition, plant and soil should be separated when evaluating ecosystem respiration. We definitely do not think or contend that our work is sufficient to understand the global $CO_2$ exchanges, just the respiration balance and how that is promoted among different components of the S. salsa ecosystem. This is just one part of the puzzle that we wanted to expand upon from our past efforts.*

Furthermore, measurements were conducted during 90s periods which seems very short.

*Response: It is a short measurement period, but that is by field experiment. Temperature and other variables varying during chamber measurements, known as "chamber effects", create bias to flux estimation that we have had to deal with in the past. For example, leaving chambers on soil patches for 40 to 60 minutes can raise soil temperatures by over two degrees C, and air temperatures by even more. Shorter incubation periods could reduce this uncertainty, which has been the premise driving further development of portable IRGAS (e.g., Li-Cor types) and GCs (e.g., LGC and Picarro types) versus reliance on lab-based analyses. The figure 3R shows $CO_2$ concentration rising consistently during a 30 min incubation (figure 3R: x, $CO_2$ concentration in ppm; y, measuring time in seconds), but the slope drops along with incubation time. This means that longer incubations time leads to underestimation of ecosystem respiration. We are more worried about this artifact in past studies for which we used longer incubations than we are in this study using shorter incubations. Shorter is better, and it limits the issues raised by Reviewer #1 above about exudates as well.*

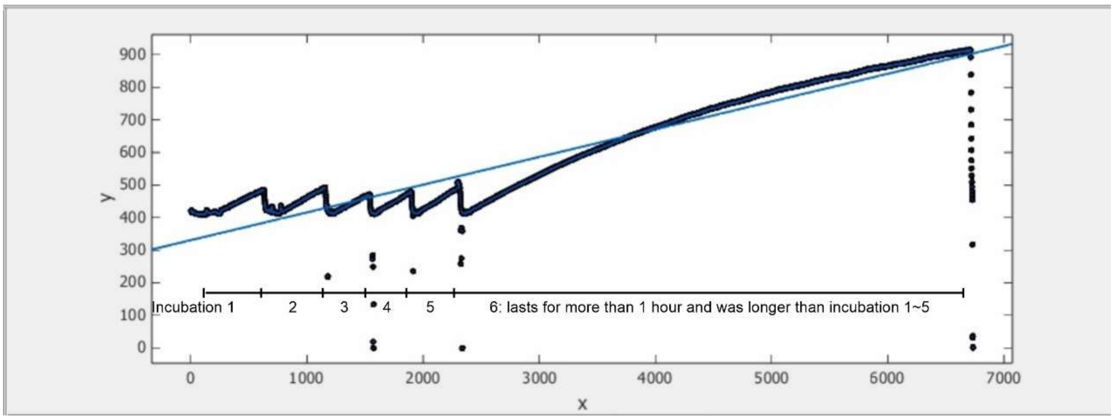


*Figure3R The relationship between CO2 concentration(y, in ppm) and incubation time(x, in seconds)*

We also have no indication on the time of day when measurements were conducted; this is important since respiration in tidal wetlands is known to vary at the day scale.

*Response: Sorry about that. This was an oversight on our part. This is indeed critical, and we have even published on that influence ourselves. We added a table to show the date and time of our experiment. The measurements were performed during 10:00 to 14:00 under full sunlight. So, that is a slight bias as described above in our comments to Reviewer #1.*

"Measurements were replicated twice, and values were averaged" (L. 177) : is it relevant to calculate a mean based on 2 values...?

*Response: This might be a wording issue on our part. For each month, 6 experimental replicates were set up (i.e., chambers). In each replicate, there were 3 experimental treatments. In each treatment, we took 2 repeat sample measurements. We modeled this after the way the original Li-Cor 6400 soil respiration system worked; once the chamber is set, the unit draws $CO_2$ down multiple times and measures respiration multiple times. We found from past measurements that twice is enough. We have indicated this information in our revised manuscript.*

I have a problem with how measurements during the inundation period are discussed...: "the water level of 1 cm could completely block the soil respiration"(L. 388-389). I think this is totally wrong: respiration is not blocked but exhanges with the atmosphere! The authors even wrote it L. 331-333 !!!

*Response: Excellent point and we agree. Thanks for pointing this out. This indeed what we meant but did not say it very well. Now we re-worded to, "Here, a was determined to be 4.6, indicating that water level of 1 cm could completely block the flux of $CO_2$ as soil respiration because of the barrier provided by the water, while -1 cm water level could enable full gaseous exchange between soil and the atmosphere (figure 10)". We also made sure that the discussion text addressed this.*

I generally do not understand how the model was built, in particular equations 2-6...

*Response: We edited this section to make it more clear in the revised version. During unflooded period, $R_{s+r}$ was dominated by air temperature (eq.4). During flooded period, $R_{s+r}$ was simplified as none flux. $k_{WT}$ was a factor drove by water table(eq2), which was visualized in figure 8a. $k_{WT}$ equals to 1 when water surface was below soil and equals to 0 when water surface was above soil(eq3)* (this idea showed in ② of the Fig.3R in this text).

*Plant respiration was dominated by above ground biomass(eq5), where $m_p$ indicated plant $CO_2$ production ability(drove by Julian day) per g dry biomass(eq6)* (please see ① of the flowchart

Fig.3R ). The ecosystem respiration ③ included both ①and ②(Figure4R).

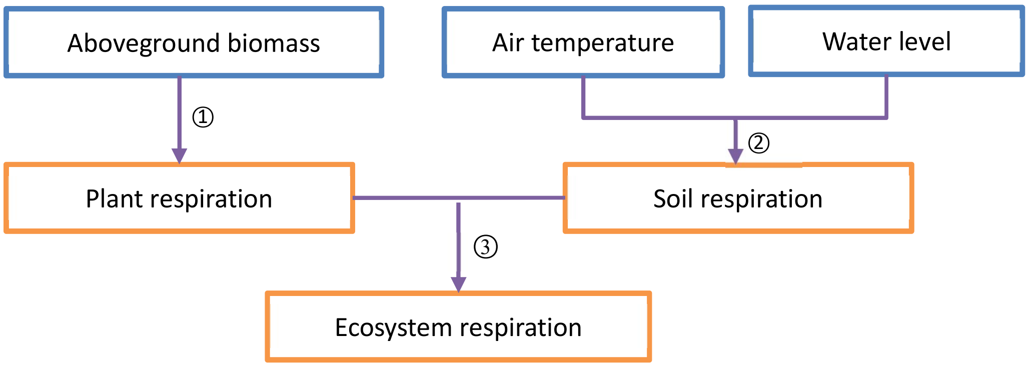


Figure 4R Modeling flowchart

Miscellaneous - The ms should be checked for typos - Use past tense throughout the text (see L. 160-165)

*Response: Sorry. The manuscript was reviewed by native English speaker who checked for grammar and punctuation better this time.*

L. 204-205: measured dark CO2 fluxes

*Response: Yes, we added a "dark" and we clarified that we did measure dark respiration.*

Figure 5: equation (and r2) for the regression? - Figure 6: I think that some linear regressions are actually not significant (June 2013, September 2013, October 2012, November 2013).

*Response: In figure 6, the linear fitting includes the slope and the intercept. We selected the slopes as indicator to demonstrate the influence of above ground biomass on $R_{plant}$. Statistically, the Pearson correlation test is applied when the samples follow the Gaussian distribution. We redrew the figures and merged the points by month. Then the 99.99% confidence was shown, data points outside the range were regarded as outliers. The results exhibited significant linear correlations. And the slopes of each measurement has a systematic seasonal variation trend. We revised the two figures as below (figure 5R &6R).*

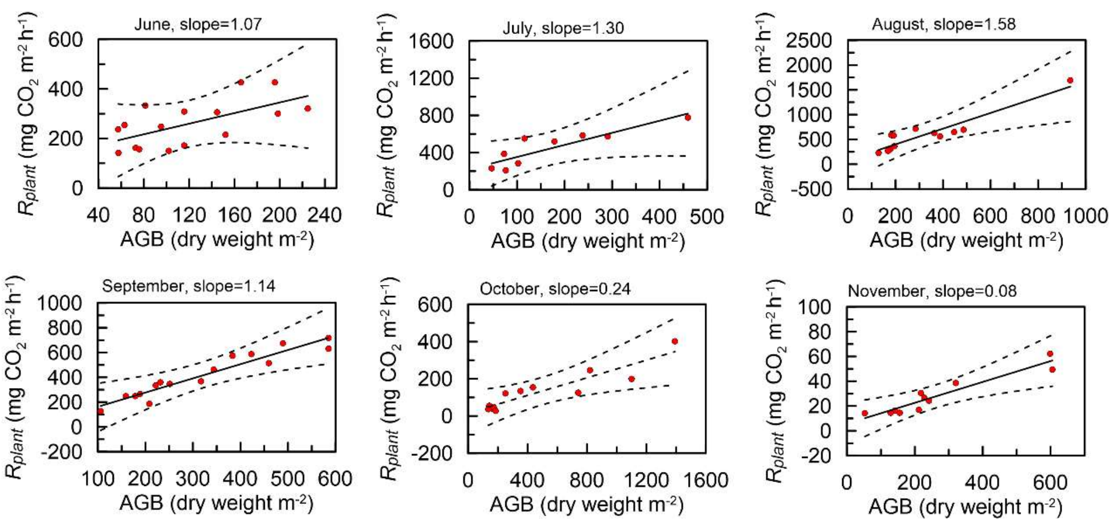


*Figure 5R. The relationship between plant respiration rate ($R_{plant}$) and aboveground biomass (AGB)* *in each month during the three growing seasons. The dash line indicated the upper limit and the lower limit of 99.99% confidence. All points without 99.99% confidence were moved as outliers. Linear correlations were significant at <0.01 level. The slope of each month was extracted to represent plant activity*

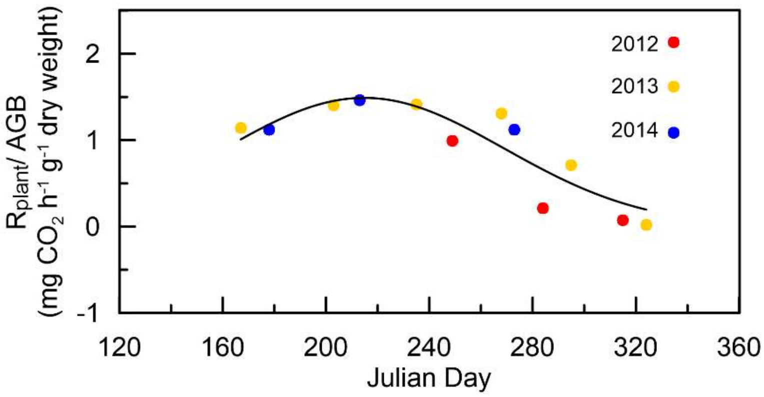


*Figure 6R. The seasonal variation in dry mass specific plant respiration rate ($R_{plant}$/AGB), including all the measurements*

L. 381: does equation 2 come from Fig. 8c?

*Response: Figure 8 is the visualization of equations and the parameters were from our observation result (Figure 5,7 in our manuscript). We have clarified these two figures in our revised manuscript as follows (Figure 7R & 8R). The specific link has now been made in the manuscript.* We have inserted the values of $F_0$ and *b*. *Figure 8R is the key parameters and driving variables; (a) $k_{WT}$ in relation to water level change near soil surface; (b) $k_{act}$ (same as Rplant/AGB, in mg $CO_2$ $h^{-1}$ g dry weight$^{-1}$) in relation to Julian day, the points were monthly individual linear fitting slopes of $R_{plant}$ versus AGB using the same method as in figure 6. The fitting equation is shown via Equation 6, where $D_m$=214.83 (Julian day), $D_s$=76.63(days); and (c) soil and live root respiration ($R_{s+r}$) in relation to air temperature when soil surface was not covered by water.*

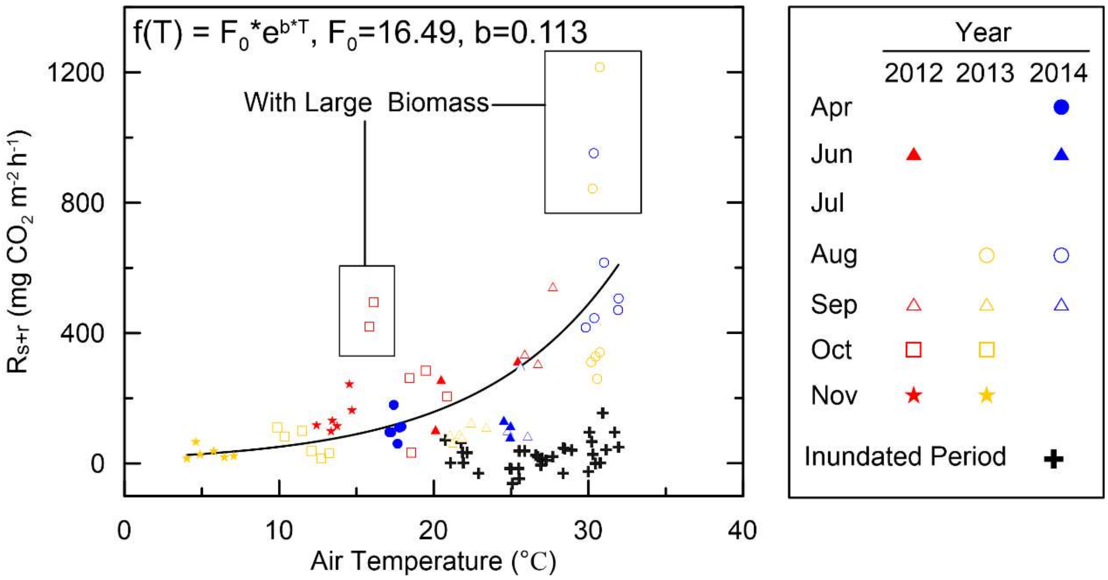


*Figure 7R. The relationship between the observed soil and live root respiration ($R_{s+r}$) rates and* *air temperatures. Fluxes are grouped by month, and for inundated periods are plotted separately using different marks according to the legend provided to the right*

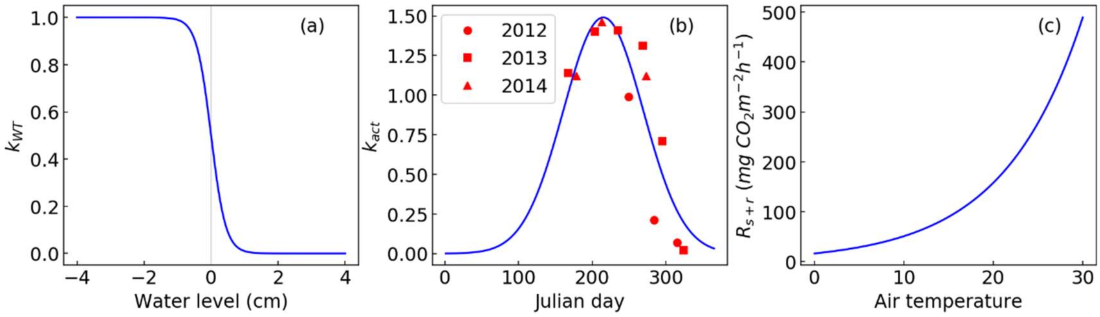


*Figure 8R. Key parameters and driving variables. (a) $k_{WT}$ in relation to water level change near soil surface; (b) $k_{act}$ (same as $R_{plant}$/AGB, in mg $CO_2$ $h^{-1}$ g dry weight$^{-1}$) in relation to Julian day, the points were monthly individual linear fitting slopes of $R_{plant}$ versus AGB using the same method as in figure 5R. The fitting equation is shown via euqation $k_{act} = k_{amax} \times e^{-\left(\frac{D-D_m}{D_s}\right)^2}$, where Dm=214.83 (Julian day), Ds=76.63(days) ; and (c) soil and live root respiration ($R_{s+r}$) in relation to air temperature when soil surface was not covered by water*

L. 453: "from -61 to 2995 mg CO2 m-2 h-1.

*Response: We checked the range. For all measuring result, the -61 mg $CO_2$ $m^{-2}$ $h^{-1}$ came from $R_{s+r}$ (during Sep, 2014), and the 2995 mg $CO_2$ $m^{-2}$ $h^{-1}$ came from $R_{eco}$ (during Aug, 2013). The flux results in the field experiment were variable. The description for this sentence was not totally correct in our original submission. The minimun $R_{eco}$ was 23 mg $CO_2$ $m^{-2}$ $h^{-1}$, observed in Nov, 2013. We modified this issue in our revised manuscript. Thanks for pointing this out.*


Fang, C., Moncrieff, J. B., 2001, The dependence of soil CO2 efflux on temperature, Soil Biology & Biochemistry, 33(2): 155-165.

Lloyd, J, Taylor, JA, 1994, On the temperature dependence of soil respiration, Functional Ecology, 315-323.

Olsson, L, Ye, S., Yu X., Wei, M., Krauss, K. W., Brix, H. (2015). Factors influencing CO2 and CH4 emissions from coastal wetlands in the Liaohe Delta, Northeast China. Biogeosciences, 12, 4965–4977

Wang, Wenjie, Wang, Huimei, Zu, Yuangang, Li, Xueying, Takayoshi, Koike, 2005, Characteristics of root, stem, and soil respiration $Q_{10}$ temperature coefficients in forest ecosystems, Acta Phytoecologica Sinica, 29(4): 680-691.

Ye, S. et al., 2016. Inter-annual variability of area-scaled gaseous carbon emissions from wetland soils in the Liaohe Delta, China, PLoS ONE, 11(8): e0160612.