Response to reviewer1

The manuscript of Yu et al aims studying the ecosystem respiration in a coastal tidal flat in order to assess the effects of T◦C, plant biomass and inundation regime with the purpose to model respiration in these ecosystems known to contribute significantly to the carbon cycle. For that purpose, the authors have conducted a field survey in the Liaohe Delta (China) during three consecutive years. For every field survey six plots were established and CO2 fluxes were measured as well as plant biomass in order to cover different amount of vegetation. For each plot, CO2 flux was measured in three distinct conditions, i) vegetation and soils, ii) plant material alone, and iii) soils without vegetation.

Additional measurements (not described in the text) were also taken when soils were inundated.

Response: Note that many of the additional measurements are shown in the supplementary materials.

To test the difference between removing S.Salsa above water surface (AWS) and below water surface (BWS) during inundated periods, an additional test was added to our regular procedure during August and September of 2012. The result shows there is no significant difference between the two treatments of AWS and BWS (Figure 1R)

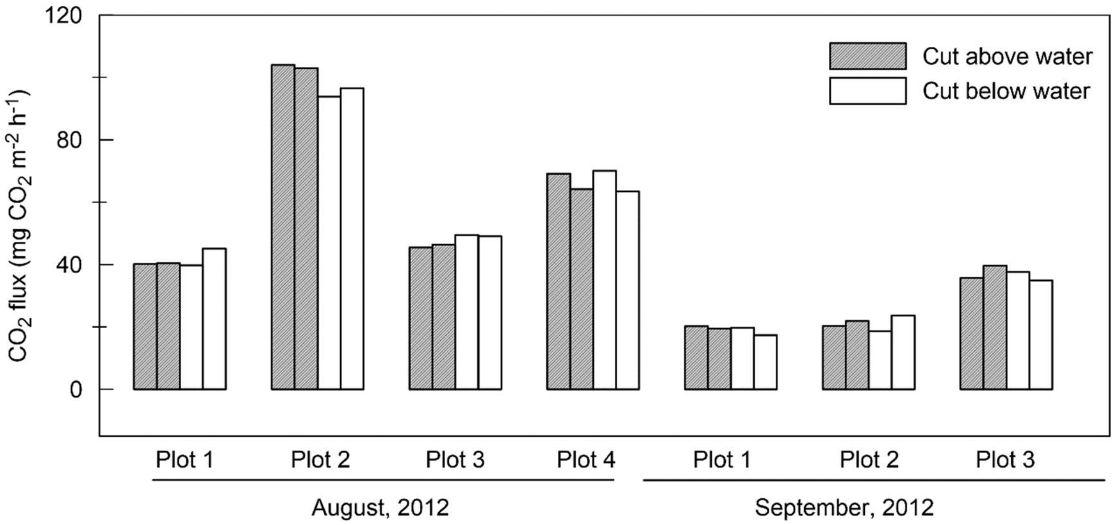


Figure 1R Measured CO_2 flux when the Suaeda Salsa was cut above water surface (AWS) and below water surface (BWS). The additional tests were carried out in 2012 when the observation site was inundated. There were four plots and three plots tested in August and in September, respectively, each test was run twice to ensure the repeatability

The manuscript is very descriptive and suffers from several critics that preclude publication in Biogeosciences.

Response: Thanks for reviewing our manuscript. During our revision, and here, we detail why this is a distinctively different manuscript from either Olsson et al. (2015) or Ye et al. (2016).The sites were different, although that was a little cryptic though not intended. Primarily, though, we measured CO_2 fluxes with four different treatments in order to partition the fluxes by component, which is rarely done in the literature and has not been previously done from Saueda salsa (i.e., we called partitioning observation, please see below response). We provide better details for this in the revised manuscript.

1 Lack of originality. Functioning of tidal flats have received a lot of attention, and the effects of air

temperature as well as the inundation regime or the plant biomass have been well studied in coastal ecosystems. In this context, it is hard to figure out what are the real advances provided by the present manuscript in the understanding of these dynamics ecosystems. Numerous studies have observed the positive influence of air temperature in regulating the functioning of these ecosystems during the period of emersion. Similarly the influence of vegetation cover on the metabolism of tidal flats has received attention these last years. Furthermore, recent works have already studied the seasonal variations of gas exchanges in these ecosystems, demonstrating the importance of air temperature in the flux dynamic.

Response: We agree in part, but in fact, it is important to note that the rationale provided by the reviewer (i.e., that LOTS of studies have been done on CO_2 fluxes from wetlands with attention to correlation to environmental variables) was THE primary driver for our study. We have now highlighted this better. We have been conducting a number of greenhouse gas studies in the Liaohe Delta with similar scope as the reviewer mentions, and we are often asked how the resultant gas fluxes equate to the different known sources for the greenhouse gases. We have not been able to answer this for this Delta for this wetland community type until now. We have previously measured ecosystem respiration (R_{eco}: Olsson et al. 2015; Ye et al. 2016); therefore, partitioning sources among plant respiration, soil fluxes, and other components was the primary focus of this new work. We need these data partitioned to develop modeling capabilities that partition as well (see formulation in section 3.4). As for originality, we request that the reviewer recognizes that few studies separate CO_2 derived from different component parts of the wetland ecosystem such as soil and plant. Most scientists use simple chambers, and we wanted to think and model outside of that constraint here. Thus, the purpose of greenhouse gas exchange observations here is to figure out the key factors by component and then scale them up. Our partitioning directly explains the origin of CO_2 in this wetland, which we think provides more insight on wetland respiration. In addition, since this wetland ecosystem (Saueda salsa) comprises such a huge area in Northeast China and into places like Korea, we hope to have a model robust enough to start constraining fluxes from this system globally through a model framework linked to remote sensing (eventually). This approach also has potential to be applied to other ecosystems.

2 The approach proposed is not appropriate to tackle the proposed objectives. The authors have measured CO2 fluxes during 90 seconds using a field portable gaz analyzer. From these measurements they estimate three different respiration according to three conditions, Reco for all vegetation and soil, Rp for plant without soil, and Rs+r for soil without standing plants. This direct conversion of CO2 measurements to respiration strongly indicates a misunderstanding of the processes involved in CO2 fluxes in tidal flats. CO2 fluxes represent the net balance between autotrophic processes and heterotrophic processes, therefore the measurement of CO2 not only measure respiration but also primary production(photosynthesis). In darkness, CO2 fluxes represent respiration but in the light CO2 fluxes represent the balance between respiration and photosynthesis, often described as net metabolism. In the present study, the authors indicate that measurements of CO2 were performed on field, which supposes, even so it is not mentioned, that the measurements have been performed under natural sunlight. Consequently, the CO2 fluxes do not represent respiration but the net balance between respiration and photosynthesis.

Response: We have a full, and indeed in-depth, understanding of the processes involved in CO_2 exchange among wetland soil, water, and atmosphere, including the fundamental process of photosynthesis. But, the reviewer makes a great point that we need to be more deliberate about *stating that. We add several new lines that mention this to make it clear that we understand photosynthesis, what it does, and why we are not including it here-in. Our focus here in on the processes of respiration only, which includes plant, soil and root, ecosystem, and "other"; all measured in a dark chamber that excludes light and prevents or reduces to nearly zero any CO_2 uptake. We re-iterate that several new times in the revised manuscript. Please note also that this study is not about net radiative balance, in which case, R_{eco} would have to be balanced with net uptake of CO_2 from photosynthesis for that analysis to work. We discuss this in more detail in the Ye at et. (2016) paper for Phragmites, but do not expand on it here-in. Our apologies for this oversight, and we ask that the reviewer re-consider the manuscript with its new verbiage.*

In addition, the authors do not give any information about the time of day (and consequently the light conditions) observed for each measurements. This information is not trivial because the respiration in tidal flats is strongly influenced by the light conditions, respiration can be strongly stimulated by photosynthesis activity due to the release of carbon exudates that can be rapidly used by benthic bacteria enhancing so their heterotrophic metabolism.

Response: We agree. We added a table to show the date and time of our experiment in the revised manuscript. The measurements were performed during 10:00 to 14:00 under full sunlight, which would bias the collection somewhat because night fluxes are never included. This is fairly common, however, which does not make it right necessarily, but only eddy covariance designs or really specific auto chamber designs would be able to address this properly. We concede, however, that for net balance studies, this can be a consideration. It is true that root exudates can stimulate soil heterotrophic respiration, and if we exclude the photosynthetic process, this short-term stimulation would also be excluded. However, the degree to which this stimulation is important is unknown, but likely very minor given that we do not have the simultaneous restriction on temperature stimulation. To the degree that exudates are stimulated acutely by photosynthesis versus temperature and sent to the soil immediately at time scales <90 seconds, the reviewer has a point. We contend here that not only would it be a minor component of flux, but also this is not something well known enough to science to provide the basis of manuscript rejection.

To conclude, the authors have used a methodological approach that does not allow correctly addressing the question asked.

Response: We agree that different techniques need to be developed, but we do not agree that exudate production from net photosynthesis would produce vastly different rates of R_{eco} or R_{s+r} to warrant our experiment not applicable.

3 Data presentation and interpretation. The dataset of the present study is relatively limited as it comprises few respiration measurements (4-6 measurement per year for three consecutive years) together with the vegetation biomass. Concomitantly, the authors have measured air temperature as well as other parameters which are not described.

Response: In order to evaluate regional scale respiration of S.salsa wetlands with easily obtained variables, the plant biomass, water regime and air temperature were used. The amount of data was sufficient enough to demonstrate seasonal life cycles and represent the partitioning of greenhouse gas fluxes adequately (as per Ye et al. 2016). Note also the soils are frozen solid for ~5 of the remaining months per year. It might also be important to re-iterate here that partitioning was reported on a per hour basis, not on an annual basis. We contend that 4-6 replicate measurements per year for 3 years is more than adequate to represent partitioning of fluxes on *these sites at those temporal scales.*

Using this small dataset, they try modeling the respiration dynamics as a function of the season considering that T◦C and the water level above the sediment surface are the only factors that explain the CO2 fluxes.

Response: And above ground biomass… Also, we do not make the assumption that temperature and water level are the only factors, but these are the only environmental variables that we measure here based on the literature. Collectively, biomass, temperature, and water level were adequate in explaining respiration. Other factors such as wind speed, sunlight, input DOC, nutrition elements, etc can certainly influence ecosystem respiration. We do not preclude those here, it is just that we did not measure them here. We also contend that the model (for our purposes) would not be improved much by adding additional variables.

The description of the model is very succinct. There is no explanation about the key parameters used for the model and their dynamics (figure 8) as a function of water level (Kwt), of Julian day (Kact) and, of air T◦C (Soil respiration). The description of the model is totally unclear and it is really difficult to understand how the models were designed.

Response: Yes, we agree. This needed to be re-visited. We tried to make the description of the model succinct, and missed some important details, like many units. In the empirical model, water table works as a factor that restricts soil respiration, and the effect was simplified as a logistical model derived by water level. In the revision, we describe and visualize every formula we use in the model with parameters from observations. It is a simple model compared to other process-based models, so some details are missing because there is no real complexity to it, which we also hope means that the model is more applied.

Furthermore, it is interesting to see that the model proposed do not correctly fit the dynamics observed in 2012; which clearly suggest that other variables have to be taken into consideration to fit as much as possible the dynamics observed.

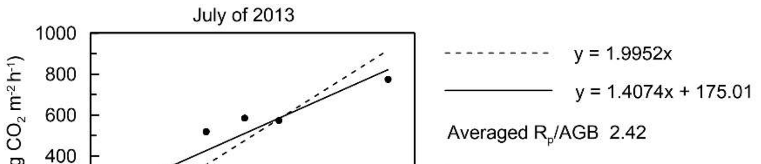
Response: This is a good point, which we address in the revision. In June and July of 2012, plants exhibited higher activity (as indicated by k_{act} value), this may be relate to unbalanced site conditions. Our model did exclude these anomalous (June and July) data. Again, there is no doubt that factors other than air temperature, plant biomass and inundation regime influence ecosystem respiration. However, we contend that these are essential here and, as importantly, are easily obtained to predict regional ecosystem respiration of S.salsa wetlands.

Other remarks. The data of figure 5 presenting the relationships between Rs+r and Air T◦C should be used to calculate the Q10 using an Arrhenius plot. Q10 is often used to clearly determine the impact of T◦C on a metabolic activity.

Response: The exponential function ($R=a*e^{b*T}$) is widely used to demonstrate the relationship between temperature and respiration (Fang and Moncrieff, 2001;Lloyd and Taylor, 1994). Also, and more importantly, Q_{10} is equal to e^{10*b}, and is used as an indicator of temperature sensitivity. Q_{10} is 3.10 according to our result, which is within the range of related studies (Wang et al., 2005). So, in effect, we did do what the reviewer suggests.

The data presented in Figure 6 are very confused, I would recommend to the authors to plot over time the specific activity (plant fluxes/plant biomass) which is a good indicator of the plant physiology instead of using this very confusing figure with a lot of correlation slopes which are not always significant. The data of the figure 7 should be shown with the standard deviation in order to figure out whether the specific activity is really seasonal dependant.

*Response: Under laboratory conditions, when there is no plant tissue inside the measuring chamber, the R_{plant} is equal to 0. It is better to plot plant fluxes/plant biomass along with time. According to our result, the intercept of linear fit curve(y=a*x+b) is not equal to 0. Taking the data of July, 2013 as an example (Figure 2R in this text below). When the fitting curve is in a "y=a*x" mode, the slope is 1.9952, which is higher than that of "y=a*x+b" mode (1.4074). The averaged plant fluxes/plant biomass is calculated to be 2.42. Of the three indicators interpreting the influence of above ground biomass (AGB) on R_{plant}, the slope of the "y=a*x+b" curve is better as it includes the uncertainty derived from field experimental measurements.*



Rp	AGB	Rp/AGB
283. 05	102.48	2.76
209. 67	76.03	2.76
518. 88	178.51	2.91
774. 41	459.50	1.69
572. 49	290.91	1.97
585. 16	238.02	2.46

Figure2R The relationship between R_{plant} and aboveground biomass