Comments to Reviewer 2 of "Bulk partitioning the growing season net ecosystem exchange of CO_2 in Siberian tundra reveals the seasonality of its carbon sequestration strength" by B. R. K. Runkle et al."

REFEREE 2: This manuscript by Runkle et al. presents an interesting and detailed analysis of the net ecosystem exchange in Siberian tundra during the growing season of 2006. The document is well written and describes how warming events in different times of the season affect the exchange of CO_2 with the atmosphere, due to changes in the response of respiration and photosynthesis. I would like to see this manuscript published in Biogeosciences, although I do have some remarks which need to be addressed.

My main remark is that this article presents a bulk flux-partition method as a novel approach for partitioning fluxes into GPP and respiration. Although I do agree that it is much better to incorporate the use of light-response curves opposed to partitioning methods based solely on night-time data, this isn't anything new. In a much-cited article from 2010, Lasslop et al. explain in detail how such a method works and it has been applied to many different ecosystems globally, including tundra (Parmentier et al., 2011). To my surprise the article from Lasslop et al. is cited in this manuscript, but without acknowledging that it describes a very similar method.

The method from Lasslop et al. also uses a higher temporal resolution through a moving window of 4 days, which is applied every two days, and also adjusts for increases in VPD, which could affect GPP. The method in this study is applied on fixed one week periods, and doesn't include an effect of increases in VPD. I therefore recommend that the authors include a more elaborate discussion on what has already been done by other studies, how their approach differs from those, and what the added benefit is of the method presented in this document.

These are good points, and we have re-worked the manuscript to better explain how and where our approach is novel, how it benefits from past approaches (Such as Lasslop et al's), and how it may act as a template for studying landscapes that are simultaneously (1) potentially susceptible to both light and temperature stress and (2) have sharp responses to light even under near-dark conditions. Particularly, we find such a strong correlation between fluxes and light at the low-light regions that this first-order effect on NEE should be included in light-sensitive northern ecosystems.

We have added a description in the methods section to help justify our use of a fixed modeling period (rather than a moving window), and feel this approach will enable us to identify possible light or temperature stresses and to look at different temporal periods as independent samples. The length of the chosen fixed period (7 days) was a necessary balance between the desire to cover similar phenological conditions and a variety of light and temperature conditions in order to parameterize a 4-term model. A sufficient temperature range is needed for an appropriate estimation of the respiration temperature sensitivity parameter Q10, i.e. the curvature of the respiration-temperature relation.

Additionally, we explain in the text that we have relatively few data intervals in which to parameterize a VPD-dependent model, and feel such a model would be too weak to stand on its own. The correlation between VPD and temperature enables a temperature-oriented model, looking for temperature stress, to also detect some of the possible effects of low humidity.

Another comment I have is the time period studied. From Figure 1 it is clear that NEE only picks up directly after the first heat spell of the season. This is not a coincidence and common in Arctic ecosystems, where a significantly warm period stimulates bud break. Apart from the higher air temperature, the warm period helps to raise soil temperatures and deepen the active layer. All processes which are important for plants, and first temperatures need to become higher before plants can become active. But this also means that the period preceding this heat spell is not the active part of the growing season, and it is therefore no surprise that higher temperatures increases respiration more than photosynthesis if some of the plants are not active yet. So yes, this early heat spell leads to a release of CO_2 but it also stimulates bud break. So the earlier these heat spells occur, the earlier the growing season starts and significant uptake can take place. The heat spell at the end of July is more typical of a heat spell during the growing season and no release of CO2 is seen then. It would be good if this manuscript focuses more on the physiological differences of the plants during the studied period, and the effect of these heat spells as such. But perhaps the effect of these heat spells can be better studied when using data from more than one season, since there are plenty of years available from this particular site.

References to these helpful ideas regarding the phenology of plants and the role of temperature in bringing out the bud break have been added to the discussion section. We inserted the text: "The hot periods may, though, hasten the onset of sedge greening or bud burst, which at least in several tundra shrub species has been shown to have a strong dependence on cumulative heat-based forcing units (i.e., the time-integrated temperature relative to a threshold value commonly set to 0 °C) (Pop et al., 2000). An earlier or longer growing season,

however, may not contribute to greater net carbon uptake in these environments due to the rise in Reco caused by warmer temperatures (Parmentier et al., 2011).”

Detailed comments:
page 13721, line 16: Why this limit of 500 umol/m2/s? Earlier, you only refer to a study showing light stress for Sphagnum at 800 umol/m2/s. But from your results (Figure 3 and 4) it is clear that there is no light stress at your site, apart from normal saturation with high PAR. Why then not validate your model on all data points? Please give a citation for this number, or show it clearly in the data.

The limit of 500 umol/m2/s is used for several reasons, which are made clearer in the revised manuscript:

First, this threshold allows us to be well under the 800 umol/m2/s possible light-stress threshold presented in the cited paper (Murray et al, 1993). This paper used a light-treatment approach, with light levels 800, 340, and 150 umol/m2/s. The reported stress occurred in the 800 umol/m2/s treatment, but this does not mean the threshold begins only at such a light level – the real threshold would be between 340 and 800 umol/m2/s, and 500 umol is a reasonable intermediate choice.

Second, this threshold is used as a balance between two competing influences:

a) using a higher threshold risks reducing precision on the parameter estimates derived at low light levels (such as α_2, the initial light response parameter) which may, due to model equifinality, negatively influence the respiration terms in the first step of the “two-step bulk partition method”.

b) using a lower threshold risks reducing the number of data necessary to properly characterize the light response (such as through the parameter $P_{max,2}$)

Third, we made preliminary studies shifting this limit between 300 and 700 umol/m2/s that showed little qualitative difference in the interpretation of this data, and only small changes in the final parameter time series.

page 13721, line 21: Why this interval of 7 days? Perhaps it’s good to explain this to avoid that it appears as an arbitrary choice. Weather conditions and plant activity can vary much over a week, and as such the parameters you’re trying to derive, too. Again, it would be good to compare your choices to those in the paper of Lasslop et al. (2010), who used a moving window. That might be more useful here also.

Please see our ideas regarding this question in the discussion above.

page 13723, line 14: The placing of the word ’then’ in this sentence feels a bit awkward and makes it hard to follow your train of thought. Why not leave it out?

This change was performed as suggested.

page 13723, line 19-21: So in this case your condition in equation 6 of NEE==0 has been met when light levels drop, not so much because of an increase in temperature. But your hypothesis is to investigate the influence of heat spells on NEE. A heat spell was obviously not the case on a cloudy, rainy day. So what is worse? Heat spells or storm events? This is something that may be more discussed in the manuscript.

The magnitude (in both directions) of the NEE components GPP and R_{eco} during heat spells is much more than during the storm events, so the hot periods seem of more interest and potential for driving larger-than-expected respiratory fluxes. The counter-case is alluded to in more detail in the text.

page 13725, line 6-7: This is the reason for my first comment. Why the limit of 500 umol/m2/s if there is no light stress?

We did not know *a priori* that we would not find evidence of light stress, and think a threshold in this range is useful for analysis in other landscapes that have the potential for such stress. The light level chosen as a threshold allows a wider range of light and temperature conditions upon which to parameterize the initial step in the model, with less risk of including the conditions which may stress the vegetation community. Additionally, by explicitly including well-lit conditions into the model for R_{eco}, we avoid the extrapolation of dark R_{eco} into light conditions, which is itself problematic (see textual references to Brooks and Farquhar, 1985; Wohlfahrt et al., 2005).

page 13725, line 11-15: I think Figure 5 described here should be expanded with a plot of NEE as displayed in Figure 1. This will also help with my second general comment on the influence of temperature in different times of the summer. The effect of temperature on NEE will then also be much clearer.

This idea was implemented as suggested, so that Figure 5 repeats both the NEE and surface temperature time series from Figure 1. The relationships between temperature and NEE is easier to compare in the new figure.

page 13725, line 17-19: As pointed out in my second general comment, this is simply due to the fact that plants are not fully active yet.

We find it interesting to note that the mosses and vascular plants, to the degree they are active, do not uptake enough CO_2 to exceed respiration during these periods.

page 13725, line 22-25: similar to my earlier comment: the only time the critical temperature was crossed during the active part of the growing season was not during a heat spell. Are these heat spells perhaps not a problem in this ecosystem?

As excerpted above, we have expanded our thoughts about this question in the discussion section.

page 13726, line 6-8: This 'inflection' is probably bud break of the shrubs at your site. Is there no recorded evidence of this? (photos, logbook etc)

We don't have good on-site records during this period, but do now reference the MODIS LAI time series, which indicates the start of vegetation growth soon after snowmelt (around 10 June). The polygonal tundra is less dominated by shrubs than other Arctic sites, and there is relatively rapid development of the sedge communities. We have added the following text: "The onset of greening occurs around 10 June, which is the earliest date when the 8-day MODIS LAI product is non-zero (i.e., 0.1 m2 m 2, relative to season high 0.5 m2 m 2 on 12 July) (ORNL DAAC, 2013)."

page 13726, line 12-13: Of course this period behaves differently, leaf senescence has already occurred. It's common for some shrubs to already start turning red in August in tundra. By September light levels and temperature are so low that the growing season has ended.

This is true; we have added comments about leaf senescence during this time, which at the study site is more affected by sedge leaves browning than shrubs senescing. The sentence now reads "This period behaves differently, ecologically, due to its freezing temperatures, onset of snowfall, and late-August leaf senescense."

page 13728, line 16: Why hasn't the winter respiration been re-estimated with the added knowledge from this new approach?

The winter efflux of CO_2 is controlled by different physical and biological processes, with frozen layers, snow, etc., and the model developed here is focused on the growing season so we are hesitant to extend our model into this period. Additionally we note that in the earlier study of Kutzbach et al (2007), the authors estimated a moderate CO_2 source for the rest of the year by interpolating between beginning-of-winter and end-of-winter measurements (+48 g m-2), implying that in both cases, the site is a CO2 sink even on an annual basis. Such measurements are not existent for the season that is in the focus of this study.

page 13728, line 18-20: When comparing CH_4 and CO_2 fluxes, please do this in the same unit for easy comparison. Grams of C/m^2 is preferred, or even CO_2-eq is better than just presenting the weight of two gases with different molar mass.

We have added the CO_2-eq of the CH_4 flux source estimate.

page 13728, line 29 to page 13729, line 1: In this time of year the growing conditions are not optimal! At the start of the heat spell, the active layer had probably only barely thawed. It is most likely that this early heat spell itself has actually led to more favorable growing conditions, after which the active part of the growing season started. This can happen quite fast, in a few days. Exactly what I can see from your data.

These are good points, and the text has been modified to reflect these ideas.

page 13729, line 13: Again, this is not a new method.

This wording has been changed to reflect what we feel is new about our partitioning algorithm: its approach to a "multi-step bulk method".

page 13729, line 17-19: But is this a problem? An earlier heat spell will only advance the start of the growing season. After that, plants will be more photosynthetically active and later heat spells will not have the same negative impact. Which is also shown from your data.

Reference to these interesting ideas have been added to the discussion section (e.g., in the references to the works of Pop et al and Parmentier et al, excerpted above). In the conclusions section we add the line "An increase in the frequency of hotter weather conditions, following this study and others, should increase ecosystem respiration with an effect of reducing the net uptake of CO_2 by tundra landscapes."

Table 1: Perhaps it's an idea to summarize this real quick in the method section, and move the rest of this table to the supplementary information? Many of the details mentioned, such as coordinate rotation are quite standard and don't need to be specified. Is there perhaps an older paper that can be referenced instead?

The processing list as presented here is based on our current understanding of the state of the science methods of analyzing the data; as these methods often change (and the order in which the various corrections are applied) we have decided to leave this table in place, as it is distinct from previous work at this site. We have found that changes in the order and terms in this list contribute to significantly different flux estimates; should additional corrections later need to be back-applied, such a list can help to orient these changes.

Figure 2: Please label the subfigures a-g and refer to them accordingly. Currently, the text is quite unclear when just left-hand or right-hand panel are referenced. Also, a few commas would be useful in the text.

This change has been performed and the text clarified.

Figure 4: why not mention 'NEE partitioned into photosynthesis and Reco', instead of 'partitioned NEE'?

This change was performed as suggested.