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Interactive comment on “Bulk partitioning the growing season net ecosystem exchange of CO₂ in Siberian tundra reveals the seasonality of its carbon sequestration strength” by B. R. K. Runkle et al.

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General comments:

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 CO₂ with the atmosphere, due to changes in the response of respiration and photosynthesis. I would like to see this manuscript

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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.

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 CO₂ but it also stimulates bud

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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.

Detailed comments:

page 13721, line 16: Why this limit of 500 $\mu\text{mol}/\text{m}^2/\text{s}$? Earlier, you only refer to a study showing light stress for Sphagnum at 800 $\mu\text{mol}/\text{m}^2/\text{s}$. 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.

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.

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?

page 13723, line 19-21: So in this case your condition in equation 6 of $\text{NEE}==0$ 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

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or storm events? This is something that may be more discussed in the manuscript.

page 13725, line 6-7: This is the reason for my first comment. Why the limit of 500 $\mu\text{mol}/\text{m}^2/\text{s}$ if there is no light stress?

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.

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.

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?

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)

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.

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

page 13728, line 18-20: When comparing CH_4 and CO_2 fluxes, please do this

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in the same unit for easy comparison. Grams of C/m² is preferred, or even CO₂-eq is better than just presenting the weight of two gases with different molar mass.

page 13728, line 29 to page 13728, 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.

page 13728, line 13: Again, this is not a new method.

page 13728, 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.

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?

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.

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

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Lasslop, G., Reichstein, M., Papale, D., Richardson, A. D., Arneth, A., Barr, A. G., Stoy, P. and Wohlfahrt, G.: Separation of net ecosystem exchange into assimilation and respiration using a light response curve approach: critical issues and global evaluation, *Global Change Biol.*, 16(1), 187–208, doi:10.1111/j.1365-2486.2009.02041.x, 2010.

Parmentier, F. J. W., van der Molen, M. K., van Huissteden, J., Karsanaev, S. A., Kononov, A. V., Suzdalov, D. A., Maximov, T. C. and Dolman, A. J.: Longer growing seasons do not increase net carbon uptake in the northeastern Siberian tundra, *J. Geophys. Res.*, 116, G04013, doi:10.1029/2011JG001653, 2011.

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