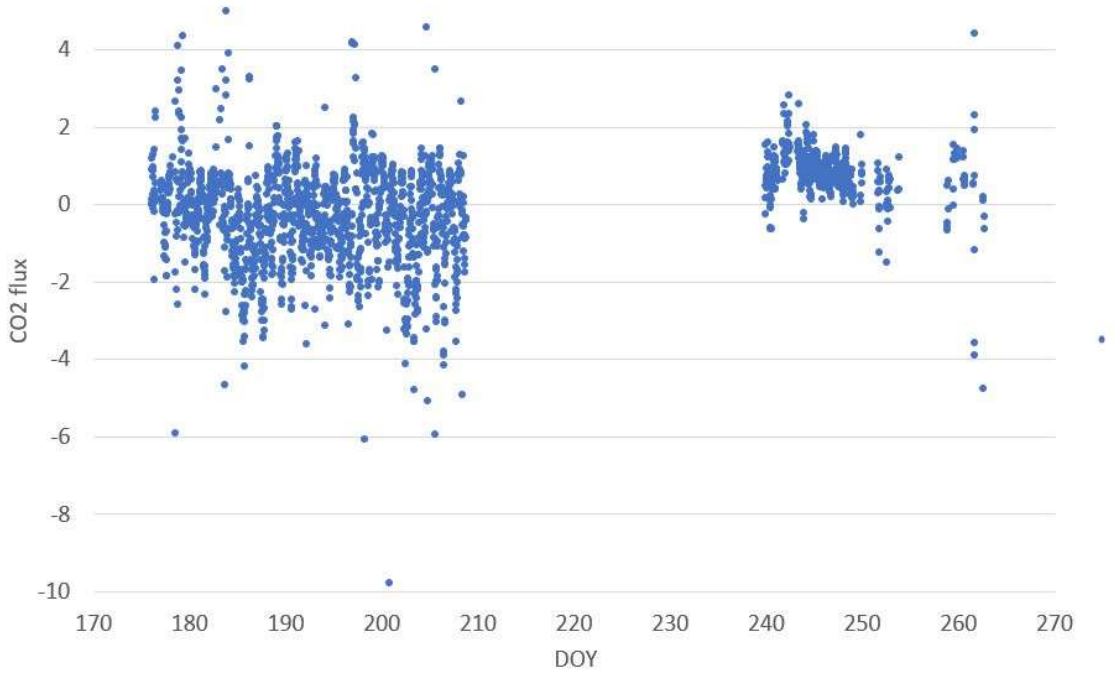
Lindroth et al. Moist moss tundra on Kapp Linne, Svalbard is a net source of 1 CO2 and CH4 to the atmosphere.

Overall:

The ms addresses GHG measurements from a very remote part of the World and in the high arctic. As pointed out in the introduction few trustworthy measurements are available from the high Artic area as a whole and especially full year estimates and measurements are rare. The ms is written in an unpretentious way and aim to fill a piece of the GHG puzzle of the Arctic ecosystems, where also seasonal observations can be appreciated. As such measurements from the summer (late June – mid September) 2015 are reported on and also used for an interpretation of the annual CO2 /CH4 GHG budget of this tundra landscape.

So far so good. When I despite this feel that the authors still need to do some work with this ms it is because I find that especially the interpretation is take a bit further than the data can justify. Even if several assumptions are made (which is fine) it is not clear to the reader why some of the figures look like they do or why e.g. there are uncertainty of some definitions (e.g. growing/summer season) which I find confusing and suboptimal for the further interpretation of the data. I was first puzzled by the way data was presented in fig. 8 where supposing half hourly observations are compiled into a bi-weekly plot, leaving a peculiar impression of the dynamics of summer season in this ecosystem. It also leaves you with the impression that the peak of the summer is missing. However, it was not until I downloaded the data (which the general reader will not do) that I discovered that a whole month of data from later July to late August is completely missing in the flux observations. This should be made very clear in text as well as a general improvement of the description of the data coverage, which is not apparent from the figures and is critical for the further interpretation of the data. Based on this it is difficult for the reader to have confidence in the annual flux estimates (despite assumptions made) when also the peak of summer is missing in observations.

It would therefore be my recommendation that the revised manuscript relied more on the direct comparison of the measured data and the observed balance in GHG effect during the summer season, and made less attempts to extrapolate these into annual values, which it is difficult to have any confidence in. The observed data are rare and look fine (those which are there) and the authors could avoid a lot of the uncertainty by having a focus on that and a more direct comparison with other seasonal measurements from the Arctic.



Specific:

L22: the numbers seems to contradict the conclusion L535

L74: other studies could be mentioned e.g. Jammet 2017- Year-round CH4 and CO2 flux dynamics in two contrasting freshwater ecosystems of the subarctic, Biogeosciences, 14, 5189–5216, 2017 https://doi.org/10.5194/bg-14-5189-2017

L117: the hypothesis seems unargued and not very helpful, e.g. what is average CH4 flux ? please strengthen

L187: Some of these observations are quite far away, please reflect on the impact of that.

L291: I find the definitions of the seasons difficult to relate precisely to e.g. what is "*daily air temperature started to stay above zero more steadily*" or "*when most of the snow had disappeared*" – quantified how?

L330: a further sub-division of the vaguely defined season does not help me. Please consider a simplification.

Table 1: May all be significant but none of the three explain much of the variance – please explain

L367: No permafrost here???

Figure 8: as indicated earlier I find the time steps and general impression of the seasonal variability off and not a good representation of the actual measurements.

Figure 9: I have difficulties with this one as well both because of the negative GPP values – negative photosynthesis? Which appears because GPP is normalized for light response. I don't think the you can do that with two parameters which as dependent as temperature and light.