Biogeosciences Discuss., referee comment RC1
https://doi.org/10.5194/bg-2021-81-RC1, 2021

Comment on bg-2021-81

Anonymous Referee #1

Referee comment on "Field-scale CH_4 emission at a subarctic mire with heterogeneous permafrost thaw status" by Patryk Łakomiec et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-81-RC1, 2021

The manuscript addresses the very current problem of CH4 emissions from the thawing sub-arctic permafrost regions. The problem is analyzed with novel methods and unique data, and its contribution to the current state of knowledge in biogeosciences is significant. The overall scientific quality of the manuscript is high. It is in general clear and well organized and should be published in the BG with only minor modifications. Below are a few specific comments that should be considered to improve finale version of the manuscript.

Specific comments.

The issue of water table level:
1) in Figure 2 WTL (water depth) is expressed in m a.s.l. It should be presented in relation to the ground level. In line 164 authors report that the EC system collects data from a height of 2.2 m a.g.l., which means that the ground level is somehow determined. The WTL should be related to this level to provide information if the WTL is above ground level or at a certain depth in the ground.
2) Figure 2 shows a jump in WTL between 2014 and the next two years by about 1 m for the western sector and several dozen cm (> 50 cm) for the eastern sector. It is surprising that the formation of such a thick peat aeration layer has not affected the CH4 flux.
3) In the case of such a WTL jump, its distribution should be bimodal (why in Figure S2 it is clearly visible for the eastern sector, and not visible for the western sector?) and the correlation between CH4 flux and WTL should be checked separately for each year.
4) Even for the eastern sector, for which WTL is considered representative (ln. 380), the soil moisture in summer 2014 was lower than in the following two summers, while WTL was much higher in 2014 (Figure 2). How is it possible?

Ln. 397: Please mark the contribution level (e.g. 80%, 50%) on at least a few selected lines in Fig.3.

Ln. 405: *As seen in Figure S4, no diel cycle was observed* – in my opinion Fig. S4 shows weak diel cycle, with 10-20% differences between nighttime and noon fluxes. Moreover, the potential diel cycle should be exanimated separately in the seasons. In summer, changes in solar radiation can cause a significant diel cycle of surface temperature (temperature impulse), which may affect methanogenesis, while in winter there is no such forcing.

Ln. 410-414: Information in Tab. 3 are a bit misleading. For example, for 2016 the coverage by a good data is 99% (sum for eastern and western sector), which seems quite unrealistic for EC method. In fact, the assumption that 10 good data over a full 24 hours is sufficient to calculate daily value (ln. 408) is a kind of gap-filling method and means that up to 58% (14/24) of data might be gap-filled by mean daily value.

Ln. 430-431: *The peak season of the CH4 emission was defined as two weeks forward and backward from the day with the maximum daily emission in a given year*– it is possible that a single high emission does not occur in the peak of the season, so why not use a 14-day moving average and next use the maximum of this function as the peak emission?

Ln. 436: *Wintertime average emissions were 24 mg-CHm-2 d-1 for the eastern sector and 16 mg-CH4 m-2 d-1 for the western sector* – but when we compare these values with Fig. 4, the 24 mg-CH4 m^{-2} d^{-1} level is clearly above the most of green tringles for wintertime (blue areas). Similarly, the 16 mg-CH4 m^{-2} d^{-1} level is above black dots at winter. It means that the quoted average values for the eastern and western sectors are amplified by gap-filled values, i.e., the gap-filled values on average are significantly higher than the measured once. Is that correct? Any reflection on this effect?

Ln. 450 and Table 7: *Controlling factors were examined before and after temperature normalization (Table 7)* – please be more specific about which normalization is concerned. The normalization described in lines 402-405 refers to diel cycle. Of course, it doesn't make sense to correlate such normalized values with other (non-normalized) variables. At this point, the authors are likely to use a different normalization (exponential function of temperature), the same as Rinne et al. (2018). However, this only becomes clear on line 559.

Ln.521-523: …*the fen has the highest percentage of carbon emitted as CH4. The eastern and the western sectors emitted less of the carbon as CH4.* – these sentences suggests that both ecosystems emit carbon also as CO2, while in the annual scale, they absorb CO2 (and total carbon).

Ln. 576: … *small variation, without strong extreme conditions, in the WTL* – can WTL changes in >0.5m (differences between 2014 and next two years) be considered small?

Ln. 619: editorial: “als oin” – also in;

Ln. 699-700:*The seasonal cycles were furthermore characterized by a gentle increase in spring and a more rapid decrease in fall* – in my opinion, Figure 4 does not confirm this, or even suggest something quite the opposite.

Ln.: 706-707: *the temperature at different depths seemed to control the CH4 fluxes for the two analyzed mire sectors* – can the temperature profile measured at one location east of the tower be representative of the entire eastern (patched) sector? Is the temperature at the set depth the same for the entire eastern sector? The same for western sector. So the conclusion seems a bit too firm.