Response to interactive comments from Anonymous Referee #1 (bg-2018-456)

We would like to express our sincere gratitude to Anonymous Referee #1 for helpful comments and corrections. Our responses to specific comments (reprinted in bold) are given below.

The methodology is sound, but in particular the field methods need further clarification (detailed below). The conclusions drawn from this paper are justified, but I would have liked to see some support from field measurements of redox potentials; the field methodology description suggest that these measurements have been taken. The same holds for active layer thickness measurements. The authors conclude that long term water saturation is the cause of the enhanced CH_4 emission, which is made plausible. However, a more extensive discussion of alternative explanations, such as increase of active layer thickness, or change of vegetation is necessary. If, for instance, the active layer measurements also suggest an increase in active layer thickness over the years following the extreme precipitation, the conclusions of the authors about the effects of water saturation should be adapted.

I have no doubt that the authors should be able to accommodate the remarks above. I recommend publication with moderate revision. In particular the Methods section needs improvement and a more in-depth discussion of the alternative causes of the long term changes in CH_4 emission after extreme precipitation is necessary.

Reply: We truly appreciate your positive suggestions.

1) Redox potential

We do not have redox potential data that can be compared among 2011 (wetting event), 2012, and 2013. We found a large artifact in redox potential value after installing the ORP electrode into soil, which lets the atmospheric O_2 intrude to the soil and can increase the redox potential value. It took from several to 10 days for the redox potential to decrease and stabilize in anoxic soil.

In 2012 and 2013, we monitored temporal changes in redox potential for days or weeks after electrode installations, and obtained some redox potential data after stabilization (added as Table S6). As we have added to Sect. 4.2 in our manuscript, 'in this period, we observed redox potential values lower than −100 mV in wet areas (Table S6), which are well below the upper limit for CH_4 production in soil (Conrad, 2007; Street et al., 2016). Methane production at a potential higher than −100 mV can also occur, because soil is heterogeneous and can have more reducing microsites than the rest of the bulk soil, where redox potential can be measured (Teh et al., 2005; Teh and Silver, 2006).'

2) Alternative explanations for the long term changes in CH_4 emission after the extreme precipitation

We have added data of interannual variation in thaw depth to the supplement as Table S1. In wet areas, we found increase in thaw depth from 2011 (22 ± 4 cm) to 2012 (25 ± 8 cm) and 2013 (35 ± 7 cm) in observations made during mid-July. We have added this information to Sect. 3.1 in the main text.

Indeed, it appears that the extreme precipitation in 2011 (Fig. 2) led to the thaw depth increase. In addition, we did not evaluate vegetation cover quantitatively. Although we did not find drastic change in vegetation cover in the observed wet areas, abundance of sedges might have increased after the wetting event.

We have added these alternative explanations to the end of Sect. 4.2 and mentioned them in the concluding remarks in the main text.

Detailed comments:

P. 3, Line 27-29: poorly readable sentence, reformulate.

Reply: We have rewritten the sentence in a simpler way. For further clarity, we have also added short explanations on each name of the observation points shown in Table 1 (such as sedge_K).

P. 3, Line 30: How is 'predominantly' determined? Did you do any vegetation cover analysis?

Reply: No, this work did not analyze vegetation cover. We named a micro-relief covered by sphagnum mosses (*Sphagnum squarrosum*) as sphagnum_K, and micro-reliefs covered by sedges, especially by some cotton-sedges (*Eriophorum angustifolium*) as sedge_V, sedge_K, sedge_B. We have taken out 'predominantly' from the manuscript and corrected Table 1 accordingly.

On the other hand, Morozumi et al. (in review) observed specific altitude, soil moisture, and plant species along a 50 m transect in site K. They defined four vegetation classes (tree, shrub, sphagnum, and cotton-sedge), and found cotton-sedges at the lowest and wettest areas. From clustering analysis of plant species composition in site K and a local scale (10 km × 10 km) including site K, they identified these four vegetation classes as different clusters. The vegetation types in this work, i.e. tree mound, sphagnum (wet area), and sedge (wet area) correspond to their vegetation classes (tree, sphagnum, and cotton-sedge, respectively).

P. 3, Line 31: What is meant by 'snapshot' measurements? What was the measurement frequency?

Reply: We observed volumetric water content in surface soil for three to six times at each observation point in July 2011 (Table 1). We meant by 'snapshot' that these measurements had not been conducted continuously from early July to late July but only on 1 to 3 days in July for each observation point (please refer to Table S2 for detailed observation dates). We have deleted the word 'snapshot' from the main text and added this information to the footnote of Table 1.

P. 4, Line 8: What is meant with 'principally closed'?

Reply: We mean that 'the chamber was closed for 30 min and headspace gas was sampled at 0 min, 15 min, and 30 min after chamber closure' in most cases. We have reformulated the sentence as follows; 'The chamber was closed for 15–30 min and headspace gas was sampled for two to three times after chamber closure. In most cases, chamber was closed for 30 min and headspace gas was collected at 0 min, 15 min, and 30 min after the closure.'

P. 4, Line 13: Can you give an explanation on the detection limit of your chamber measurements, for low magnitude fluxes, e.g. negative CH_4 fluxes?

Reply: The detection limit of CH_4 flux was 0.8–2.4 mg CH_4 m^{-2} day^{-1}, depending on the height of chamber headspace and conditions of the gas chromatograph in CH_4 concentration analyses (mentioned in Sect. 2.4). This limit also applies to negative CH_4 fluxes.

P. 4, Line 18: Which atmosphere? Was this ambient air or some prepared gas mixture, and what was its composition? Please clarify.

Reply: This atmosphere was collected in Chokurdakh village or our observation sites, and filtered by Molecular Sieves 5A (1/16 pellets, FUJIFILM Wako Pure Chemical Corporation, Japan) to remove contaminants such as ammonia and n-alkanes from ethane to n-butane (carbon dioxide and water vapor are also removed significantly).

The filtered atmosphere was preserved in a 10 L aluminum bag. Its methane concentration was also analyzed by gas chromatography and found to be 2.0–4.3 ppm. We have corrected our manuscript accordingly.

P. 4, Line 23: An ORP electrode appears to have been used for temperature measurements, but I miss the redox potential data in this article. Why have these data not been used?
Reply: As we described above, we found large positive biases in redox potential after installing ORP electrodes. We obtained some redox potential data after stabilization in 2012 and 2013, and we have added these data to our manuscript as Table S6.

P. 4, Line 26: How do you define ground surface in a sphagnum cover?
Reply: In the wet area of sphagnum moss (sphagnum_K), we defined the moss surface as the origin of height, and measured water level relative to this moss surface. Thaw depth values and the depths shown with soil temperature measurements, dissolved CH_4 analyses, soil incubation experiments also mean the depth relative to the moss surface if the ground surface was covered by moss. We have reformulated the sentence in our manuscript as follows; 'The water level was expressed in height relative to the ground surface or the moss surface.'

P. 5, Line 24: Calculation of the chamber fluxes: two to three values are used for calculation of the fluxes, while the field methods section says that three samples have been taken from the chambers. If in some cases only two values have been used, some of the analysis results apparently have been rejected. Clarify the reasons for rejection of samples.
Reply: We are sorry for our misleading explanation. From one chamber observation, two to three samples were collected, and no result of sample analyses was rejected. The field methods in our manuscript have been corrected accordingly.

P. 6, Line 15. The Chokurdagh weather station appears to be at some distance from the sites, in particular site V. Please indicate the distance.
Reply: The distance between Chokurdakh weather station and site V is approximately 45 km (Fig. 1). We have added this information to Sect. 2.5 in the main text.

P. 10, Line 21. In the methodology section, it is suggested that redox potential measurments were taken, however, they are not mentioned in the article. At this point, it would be very interesting to know the redox conditions over the years.
Reply: As we described above, we do not have enough reliable data for comparing redox conditions among 2011 (wetting event), 2012, and 2013. But we partly obtained some redox potential data without biases in 2012 and 2013, and we have added them to the manuscript as Table S6. We found potential values lower than −100 mV in wet areas in 2012 and 2013, which is well below the upper limit for CH_4 production (Conrad, 2007; Street et al., 2016).

P. 11, Line 4. The 16S rRNA gene sequencing was not introduced in the Methodology section.
Reply: We have added a description of 16S rRNA gene sequencing to the methodology section (Sect. 2.3).