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Biogeosciences Discussions

Interactive comment on "Multi-year effect of wetting on CH_4 flux at taiga-tundra boundary in northeastern Siberia deduced from stable isotope ratios of CH_4" *by* Ryo Shingubara et al.

Anonymous Referee #1

Received and published: 14 November 2018

This is an interesting article, which shows the long lasting effects - at least two years - of extreme precipitation events on CH4 emission from Arctic wetlands. It also unravels the mechanisms behind this persistence and contains an analysis of the time evolution of the CH4 production pathways over these years, showing a shift from hydrogenotopic to dominant acetoclastic methanogenesis over time.

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 CH4 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 CH4 emission after extreme precipitation is necessary.

Detailed comments:

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

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

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

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

P. 4, Line 13: Can you give an explanation on the detection limit of your chamber measurements, for low magnitude fluxes, e.g. negative CH4 fluxes?

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

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?

P. 4, Line 26: How do you define ground surface in a sphagnum cover?

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.

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.

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.

P. 11, Line 4. The 16S rRNA gene sequencing was not introduced in the Methodology section.

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