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Biogeosciences Discussions

Interactive comment on "Landscape analysis of soil methane flux across complex terrain" *by* Kendra E. Kaiser et al.

Anonymous Referee #1

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The manuscript 'Landscape analysis of soil methane flux across complex terrain' reports soil-atmosphere methane exchange from a Rocky Mountain watershed. The manuscript focuses on understanding the environmental drivers of variations in methane exchange and the relation between these patterns and the topographic position of the sampling sites. Kaiser et al. found that upland sampling locations acted as net sinks for atmospheric methane and those close to the river could act as either weak sinks or sources. Whilst the controls on methane emission were unclear, an increase in methane uptake rates over the course of the growing season was attributed to the influence of decreasing soil water content on diffusion. Kaiser et al. proceed to show that this behaviour by the upland soils could be characterised by landscape scale descriptions of water availability. They conclude that such an approach is a desirable

avenue by which to up-scale from point measurements of methane uptake to upland soil budgets at the watershed scale.

The subject area addressed is of general interest and the manuscript is both well-written and presented. I have outlined a number of questions below that should be considered and addressed, followed by a few technical suggestions.

Methane fluxes were estimated using a gradient approach by measuring methane concentrations in gas wells and the effective diffusivity of methane (P8 L4 - L8). Effective diffusivity was characterised across the study site based on the relationship between flux measurements made with a static chamber and volumetric water content (Fig. 3). In doing so it is assumed that the total porosity and tortuosity of the soil is constant (P8 :17). How sensitive are the reported fluxes to this assumption given the actual variability in porosity reported in Table 1? Is the temperature sensitivity of the diffusion coefficient also accounted for here (Fig. A3)?

As soil physical properties were determined for each sampling location (P7 L1), did you test whether water-filled pore space (reflecting both soil porosity and water content) was a better proxy for diffusional constraints than volumetric water content (P20 L27 - 31)?

For how long or for how many cases did high water tables at the start of the growing season prevent measurement at the riparian sites (Fig. 6; P22 L13)? If flux measurements are lacking for periods when the water-table is within $\sim$ 5 cm of the surface, the general conclusion that methane emissions from these sites were rare (P22 L17) does not at first glance seem to be very robust given this apparent measurement bias.

Did this issue also influenced the ability to measure O2 concentration? The discussion of controls on methanogenesis (P21 L17 – L 26) suggests that water-table depth and O2 concentration are insufficient to explain the source behaviour of the riparian sites. However, I'm not sure that it is made sufficiently clear whether the authors are trying to generalise about the studied the system or are limiting themselves to instances when there is a significant layer of soil exists above the water-table. This is important because we might expect the superficial soil layer, especially in mineral soils as this is where the majority of available carbon is found, to be the critical zone linking the balance between methanogenesis and methanotrophy to soil-atmosphere exchange (see Chamberlain et al., 2016 ,doi:10.1002/2015JG003283 for example).

I rather like the idea of using TWI as a spatially scalable proxy for the average hydrological conditions at a given point. It's clear from the text that the relationship between methane uptake and soil water content is well approximated by TWI (Fig. A1 & A2). As TWI is a function of the slope and up-slope drainage area at a given sampling location, the mechanistic link here is obvious (Section 4.2). Can you clarify what role you think elevation (above sea level?) plays in explaining methane uptake in best fit topographic model (Fig. 9)?

More generally, how applicable is the presented approach for other systems, for example, the humid tropical forests discussed in Section 4.1 which can vary between sink and source activity? The fact that topographic scaling isn't extended to the riparian zones seems to highlight a key limitation that should maybe be addressed more directly in the discussion and conclusion.

P9 Eq. 4: Should 'CH4' be 'DCH4' ?

P41 Table 1: Please check the units for C and N content.

P15 Fig. 6: Initially I found + and – for source and sink a bit distracting. Six panels separating flux and groundwater depth might look better?

P19 Fig. 9: Please check the panel labels a) through e) are correct and consistent in the legend. Also remove the extra bracket following units in on the x-axis.

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