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Interactive comment on "Zero to moderate methane emissions in a densely rooted, pristine Patagonian bog - biogeochemical controls as revealed from isotopic evidence" *by* Wiebke Münchberger et al.

Anonymous Referee #2

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Comments to the ms bg-2018-301

"Zero to moderate methane emissions in a densely rooted, pristine Patagonian bog - biogeochemical controls as revealed from isotopic evidence"

General comments The MS of Münchberger and co-authors contributes to the knowledge on processes of methane turnover (production+oxidation) and transport in rarely studied southern bogs. Authors combined field sampling with the advanced analytics (porewater chemistry and stable isotope analyses) to report relationships and peculiar

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mechanisms between microrelief forms, dominating vegetation communities and the net processes affecting the CH4 efflux from the Patagonian peatland during two consecutive summer seasons. Field-based studies are critically important for understanding processes related to functioning of ecosystems and therefore interesting for the broad scientific community. Accepting the field experiments typically operate with much larger spatial and temporal variability in measured parameters (and as the result, relatively lower statistical power as compared to controlled conditions), still there are several issues which I would like to point out for the discussion and improvement. Below authors find general comments while specific recommendations and technical corrections are incorporated directly in the draft file attached. 1. First of all, the MS is rather long and too repetitive and descriptive. Thus, the Introduction is definitely too extended, especially regarding the common knowledge about methane in the very beginning and peatlands in general. Authors could immediately start the story of the importance of southern peatlands and have the necessary information on peatlands' biochemistry and vegetation specialty in there. Then the information on the isotope issue would be sufficient to formulate hypotheses without any loss of logic. 2. In the proposed hypotheses, it has to be clearer why pools are so much different from lawns in terms of methanogenesis pathways. This was not strait forward from the introduction; I suggest to omit statements as "remains less affected" because they are more confusing then explanatory; please, rephrase. 3. In the Methods section, I was confused with relatively short time (3 min) of chamber exposition even under the conditions of rather low atmospheric temperatures and low fluxes expected. Why also transparent and not opaque chambers were used for CH4 fluxes measurement? 4. Discussion section contains repetitive and partly speculative information and therefore is currently too long. For instance, in the discussion of results on 13C-CH4 depth profile (page 14, lines 5-8) authors seemingly "oversell" their results: "scattered between" may also indicate no significant difference (this is not clear from the data). Indeed, Fig. 4d demonstrates rather narrow d13C-CH4 range along the whole depth profile. So, in fact, d13C-CH4 signal alone was not informative

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enough to approve the strong oxidative properties of rhizosphere of A. pumila. I agree that both methanogenesis and oxidation may co-exist in close vicinity, but still it may not explain lack of d13C-CH4 variation between upper and lower horizons unless CH4 produced in the rhizosphere region is even more depleted in 13C than in deeper layers. The explanation of this phenomenon because of "more reduced...microsites" is not fully clear. More than below the rhizosphere? Why? 5. Contribution of acetoclastic pathway to methanogenesis in the rhizosphere of A. pumila was not convincingly verified (e.g. page 14, lines 23-27) and looks therefore speculative: having acetoclastic methanogenesis and co-existence of oxidation should generate much more enriched d13C-CH4 values in comparison to deep peat. Fig. 4d cannot support this. Seemingly, change of fractionation factor with depth was not significant either. The available data are not enough to approve existence of acetoclastic methanogenesis, and this has to be acknowledged. 6. Another critical point is again a speculative discussion of the results on pools and lateral flows on the site (page 15, lines 23-35). Explanations on gas diffusion along gradient were clear for me (from pools to lawns) but water movement is not the same. Pools are local depressions, so water should flow from lawns into pools. If this flow is so low, then the gas diffusion in opposite direction can be stronger, but this means almost standing water. In case there is a lateral flow of water (what is very natural), then the gas flow can't be counter to it. Therefore, I could understand the inflow of oxygen from lawns into pools, but not CH4 from pools to lawns. The overall picture may change if there is a slope, but then lawns and pools have to be arranged accordingly. Pools will get matter of those lawns which are exposed higher and transfer it downwards to other lawns. If there is a slope on the site, then the conceptual figure should somehow reflect it. Such important information was not provided in Mat&Meth or any other parts of MS. 7. The section 4.4. is rather long and at several places contains repetitive text (e.g. page 17, lines 15-17, 21-23, 27-28; the effect of A. pumila roots was very clear, no need to repeat many times). I recommend condensing text strongly. 8. Depending on the available information from authors, the conceptual Fig. 6 can be changed (see more detailed comments in the

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text).

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-301/bg-2018-301-RC2-supplement.pdf

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