Dear Gabriel Singer,

Thank you for your thoughtful and thorough review of our manuscript. We have addressed your issues as outlined below.

Issue 1

Prompted by a reviewer comment you have inserted lines 359-367 into your revision where you detail potential effects of misestimated concentration due to sampling during higher flow on your flux estimation. I cannot follow these explanations. Your (potential) flux estimate approach basically assumes that the complete load (!) of CH4 will make it to the atmosphere (minus whatever can be assumed to be left in the water at atmospheric equilibrium). Load is concentration x discharge. Your explanation assumes that higher discharge may dilute concentration, resulting in lower fluxes. Accounting for the simultaneously higher discharge, however, should make that dilution effect nil. Your flux estimation approach assumes flux to be proportional to load, not to concentration. Please explain and/or adapt these lines.

The point we were trying to make in this text is that since we sampled only during the peak flow period of the day, we have only measured concentrations from peak flow times, which may be lower than during low flow times of the day. We are therefore potentially applying that lower concentration to the entire day, even when the low flow periods may have had higher methane concentrations, and therefore it is a potential under-estimation of total flux from each day. Our discharge values are on an hourly basis, and thus our flux is calculated on an hourly basis. During the low-flow hours, when concentrations are potentially higher, we are still applying this lower concentration we measured during high-flow hours. We have updated the text in lines 379-385 to make this point clearer for the reader.

Issue 2

I and reviewers agree with your interpretation of the CH4 found in this study to be of largely geological origin. You essentially tell a story of glacial meltwater acting as a conduit for geological methane to the atmosphere. However, two "side results" seem particularly interesting to me: a) There is no correlation between discharge and CH4 concentration at any of the investigated sites, which is what could be expected if glacial meltwater streams of various discharge get in contact with a geological source of methane which thereby gets mobilized, b) even a dry groundwater "pool" acted as a source despite complete lack of water. I wonder if the CH4 emitted to the atmosphere in the investigated glacier forefield actually needs meltwater to reach the atmosphere. May there be similarly sized emission fluxes during the cold season, without any melt water? You claim at multiple places (particularly in the abstract and discussion) that your results point to "a large climate-sensitive source of greenhouse gas" and "a climate feedback loop driven by glacier melt" or "growing emission point for subglacial methane" (all cited text from abstract). Please explain your reasoning, and consider (i) toning some of those strong claims, as well as (ii) insertion of a paragraph in the discussion that actually puts the meltwater-associated emission fluxes measured during the melting season in this study into perspective with potential non-meltwater associated *fluxes in the cold season that have remained completely unmeasured. Note also that if the association of emission fluxes with meltwater does not hold, then also the comparison to larger glaciers and the claimed potential importance of many small glaciers must be put into question - and this point was already critically remarked upon in the previous review round.*

We have added a paragraph (lines 355-366) to explain why a lack of correlation between concentration and discharge volume is not as concerning as implied in your comment. This is because there are many factors that impact the mobilization and dilution of methane in a glacier drainage system, and the importance or influence of each of these can vary throughout the melt season as the drainage system evolves.

We have also removed the statement about the dry groundwater pool to avoid confusion (from the paragraph starting on line 476). It is a minor part of the study that we feel does not need to be addressed, as it seems to cause confusion. First, the dry vent that was measured was located within 80cm from the groundwater pool - the groundwater pool had shifted and changed shape throughout the summer, and therefore parts became dry that were previously submerged in water. So, it is very likely that the gas being released through the dry vent was still connected to the hydrological system. The ebullition is likely due to the pressure changes of the groundwater as it reaches the surface - lower pressure means that gas is not dissolved as easily and may degas rapidly when pressure decreases. Furthermore, we did a set of chamber measurements of the sediments across a portion of the glacier forefield and found no evidence of methane flux in any of the chambers. The chambers covered areas nearby the groundwater pools and the river and represented a variety of different sediment types. If the ebullition was not connected to the hydrological system, it would be expected that there would be more diffuse emissions of methane through the sediments, which we found no evidence of. We mention these chambers in a paragraph added at the end of the discussion (lines 568-572), as well as add their results to the Supplementary Info.

Finally, we added a paragraph to the discussion (lines 553-571) to address the relationship between the hydrological systems of the glacial catchment and their mobilization of geologic methane. We discuss how increased glacier melt may lead to the expansion of aquifers in the glacial catchment, which can drive further stores of geologic methane to the surface.

Thank you for your time in reviewing and editing our manuscript. We appreciate your constructive feedback.