We thank all three reviewers and the editor for their efforts to improve our manuscript, and find their recommendations very helpful. Here, we provide descriptions for addressing the major comments and suggestions:

First, all three reviewers highlighted a need to more clearly describe geological conditions within the study catchments. As geochemical weathering is a fundamental idea concerning a sink of carbon dioxide within this manuscript, we agreed that expanded descriptions and analyses of catchment geology are appropriate. Following these suggestions, we have explored the dissolved ion data in more detail to explore geologic influences, and made critical improvements as a result:

1) The *Site Description* was expanded to more explicitly describe the geology and soils of the study systems (Lines 129-140; Table 1).
2) We included a summary of major ions in the study streams (Table S2), as well as descriptions of the methods to quantify these analytes (Lines 148-151).
3) We added an analysis of ion concentrations (Lines 377-380; Figure 3), which provide inciteful analysis of weathering among these streams and with glacier fed stream globally (Line 533-552). We believe this analysis has greatly improved the manuscript, both in solidifying the geologic underpinnings of the proposed weathering mechanism, and by providing global context for the weathering products we observe.
4) We clarified the relationship between soil development and vegetation cover (Lines 457-464).

Secondly, we have edited our Methods section to more clearly describe the use of replicates to ensure measurement accuracy (Lines 156-160; 163-168; 184-187), and have removed some descriptions that were not relevant to this study (e.g., grab sample measurements of CO_2). Similarly, we have clarified the inherent uncertainty with regards to assigning allochthony from dissolved organic matter fluorescence (Line 452-456).

Finally, we have addressed concerns about a lack of temporal analysis within our manuscript by highlighting the previous work done on this scale and the course objective of this analysis (Lines 603-613).

Minor comments from the Reviewers are described below:

RC1:

Line 63 – this sentence could use a citation. *Citation added (Bergstrom et al. 2021)*

Line 68 – consider changing to underneath? Below somewhat implies downstream. *Changed to underneath.*

Lines 99-102 – This sentence is hard to parse as written. *Rewritten for clarity.*

Line 127 – Is this a mean elevation? It's not clarified here or in the table, a mean and an elevation range would probably helpful to include. *We have clarified mean elevation or monitoring station elevation.*

Line 279 – catchment is misspelled. *Corrected.*

Line 422 – Events is misspelled. *Corrected.*

Line 422 – How might snowmelt impact the mobilization of DOC? You only discuss rain events, this is related to the major comment above. *Added Lines 449-451 with previous example of how snow melt affects DOC export.*

Line 502 – This paragraph would benefit from a summarizing sentence. *Summarizing sentence added, Lines 530-532.*

Line 538-541- This is a repeat of information recently stated- I would suggest deleting or shortening. *We have kept this line as is to reinforce this important idea.*

RC3:
Line 128: Please add information about the main soil types which characterize the study area as recommeded also for lines 413-414. *While we do not have information on the soil types, we added information about soil organic carbon content and soil types in nearby catchments that should reflect the general conditions within our study systems, Lines 135-140.*

Lines 321-324: Why not using as a measure unit mgL-1? Please change the DOC concentrations into mgL-1. *Corrected.*

Lines 366-368: Maybe it's better to move these sentences to the Discussion paragraph. *These describe the results of the fluorescence analysis, and we believe are most appropriate in the Results section.*

Lines 413-414: May we assume that an increase in vegetation cover corresponds to an increase in soil development? Line 425 describes the concept, which I think is a key concept. Do you have an idea of the soil Corg content in your study area? See also line 496. Or at least it would be useful to report the main soil types in the study area according to the standard classification systems (e.g. WRB and/or Soil Taxonomy). *This has been clarified in Lines 459-464. Additionally, while we do not have information on the soil types, we added information about soil organic carbon content and soil types in nearby catchments that should reflect the general conditions within our study systems, Lines 135-140.*

Line 418: Do you exclude that in some catchments the contribution of glacier melt to DOC concentration in stream water is not negligible? See lines 63-65. *We do not exclude this influence, and highlight instead the increasing influence of allochthonous organic carbon (e.g., Line 439).*

Table 1: Please include also the information about the geology of the area. *Corrected.*

Table 2: As reported for Line 324, I suggest to transform the data of DOC concentation into mgL-1 as you did in Figure 2. *Corrected.*

Editor:
- Line (L) 18: "dissolved" organic carbon, unless you also measured "particulate" organic carbon. *Corrected*

- L 18-20 and associated descriptions in R&D (e.g., 375-377): Past studies have shown that humic-like fluorescence can also be associated with some autochthonous OM. It would avoid an overstatement if you add some caveat (or discussion of this issue) that DOM optical parameters indicating DOM of allochthonous origin, such as HIX or humic-like fluorescence, can derive from various sources including OM of microbial origin. *A caveat is included in Lines 453-456.*

- L 140-146: Given the importance of organic matter characterization in your study, it would be more reader-friendly if you briefly describe analytical details including the information on instruments and QA/QC (at least for DOC and fluorescence measurements). *The methodology has been expanded to include these ideas, including sampling replicates and QA/QC procedures, Lines 456-160, 163-168.*

- L 157-159: As you know, on-site equilibration or laboratory equilibration using poisoned water samples (though the latter can induce cell-derived changes in pCO2) is the typical approach to stream gas concentration measurements. Furthermore, your study deals with inter-site differences in organic matter degradation as a potential source of CO2. Substantial changes in CO2 concentrations can occur even during a short (like 12-h) incubation experiment. Therefore, the delay in equilibration after water sampling could be a point of discussion among researchers. I would suggest that you provide some methodological detail as to how you ensured that <24 h delay in equilibration could not cause significant changes in pCO2, like citing pretests or comparisons with your concomitant sensor measurements. *The grab samples of CO_2 were not used in this manuscript, and this the description of the methods was erroneous. We have removed this description. However, we have included additional detail of the QA/QC procedures for the CO_2 sensors, Line 184-187.*