Author response to Anonymous Referee #2:
Our replies to referee comments (black italics) are provided below in blue.

Anonymous Referee #2 comments:

Overview

This is an interesting research manuscript with well-defined objectives of using EMMA to understand relative variation in stream water sources in forested and alpine watersheds of the Canadian Rocky Mountains. Overall the manuscript is sound and has relatively minor grammatical errors. However, it is suggested that the introduction and discussion sections more clearly identify the secondary objectives and how the study . It is unclear if the results of this study have a primary goal of improving understanding of the variability between different groundwater sources of (e.g. bedrock groundwater vs. glacial till groundwater) in alpine and sub-alpine watersheds, or to understand/predict how future forest disturbance will impact watershed hydrology and runoff generation.

Reply: Firstly, we thank the referee for their positive comments.

Referee #1 has suggested that any discussion of hydrologic resilience be removed. We agree that revising the draft Introduction will strengthen the context of this research and clarify that the impetus to conduct this study was to improve our understanding of runoff generation in watersheds with permeable fractured bedrock and deep glacial till (compared to impermeable bedrock and shallow soils, like in some research areas). The primary goal was to improve understanding of variability between different groundwater sources, rather than predicting how future forest disturbance will impact watershed hydrology and runoff generation.

It is suggested that the study site description be expanded to more clearly quantify the size and extent of surface and sub-surface biological and geological features (talus slopes, alpine areas, glacial till, forested areas, riparian areas, etc.). It was hard to determine where and why the division of upper and lower sub-watersheds was chosen for this study and how that division was critical to addressing the question of the impacts of forest disturbance on hydrologic disturbance. Both upper and lower sub-watersheds had forested areas so the separation of upper and lower sub-watersheds did not appear to be a proxy for forested vs unforested/disturbed areas.

Reply: The separation between upper and lower watersheds was made in 2008 in anticipation of the Mountain Pine Beetle expanding into our research watersheds. The upper stations were positioned at the transition between 1) primarily lodgepole pine dominated forests and 2) the narrower band of subalpine fir dominated stands and the treeless alpine valley above this. The higher elevation sub-watersheds would have theoretically been spared from the pine beetle attack allowing for a comparison after beetle attack. Pine beetle did not reach our research watersheds, so they were maintained as reference watersheds. The upper sites were maintained because they represent this difference between subalpine/alpine zone and the upper montane zone. The upper montane zone is fully forested, and slopes are slightly gentler. The subalpine/alpine zone is partly forested but also has regions of talus slopes, near vertical bedrock cliffs, and alpine grasses. The streams in the alpine flow over bedrock in some places with little colluvial or alluvial material. Although both are "forested", the upper site is only partly forested. The comparison here comes from the conceptual diagram of runoff generation in Star Creek from Spencer et al. (2019) where the authors suggested that these two regions process water differently, have different amounts of subsurface storage, and affect the stream hydrograph at the watershed outlet in different ways. Thus, rather than being a comparison of forested and unforested sub-watersheds, the research presented here is meant to advance the conceptualization from Spencer et al. (2019), comparing subalpine/alpine to upper montane responses.

The draft study description will be expanded to clarify the extent of subsurface features and differences between the upper and lower sub-watersheds. The objectives will also be clarified as indicated above.

Secondly, the discussion and conclusion sections should be expanded to more clearly indicate how the results in this study advance or improve the scientific understanding of "how forest disturbance may impact streamflow quantity". It is unclear if the study was able to confirm or reject the hypothesis stated in the abstract that "slow release of groundwater from glacial till" (line 24) generates "hydrologic resilience" in the Rocky Mountains?

Reply: Referee #1 suggested that the Discussion and Conclusion sections relating to hydrologic resilience in the Rocky Mountains be removed. As such, this section will no longer discuss how forest disturbance may impact streamflow quantity. The hypothesis that there is a slow release of groundwater from glacial till will be revised to reflect this change. Clarification on the role of groundwater from glacial till will also be added to the revised manuscript.

Line 18: Suggest defining "old water" as related to time the water has spent in the watershed rather than the true age of water.

Reply: Text will be revised as suggested.

Line 20: In Star east in September and October the stream water was unlike the sources. What is the additional source or is it a mixed signal?

Same statement is in the conclusion but the proposed explanation of the "missing" sources is either absent or not clear in the conclusion. Clarification would be helpful.

Reply: In part, the confusion comes from the fact that it is not entirely clear to us what the additional source is. We can infer from water temperature in seeps that it is likely a bedrock source, but some questions remain. Text will be added to the draft Discussion and Conclusion to clarify our postulations about the missing source.

Line 29: What is the specific reference for beetle infestation? A few Studies from the Rocky Mountains to consider reviewing, Pugh & Small, 2011. https://doi.org/10.1002/eco.239

Bearup et al., 2014. https://doi.org/10.1038/nclimate2198

Reply: Boon (2012) is the specific reference for beetle infestation. The authors will incorporate Pugh & Small (2011) and Bearup et al. (2014) as suggested.

Line 53: Consider reference of Cowie et al. (2017) here as that study does use EMMA to examine potential source waters from bedrock groundwater, glacial till groundwater, talus slope water, and soil water on streamflow contributions in forested and alpine watersheds in the Rocky Mountains.

Reply: Cowie et al. (2017) was not initially included in this section of the draft Introduction because bedrock in the Colorado Rocky Mountains is mainly granodiorite, rather than permeable sedimentary bedrock and we were trying to make a distinction between bedrock types. However, Cowie et al. (2017) is relevant to the current knowledge and overall discussion in this paper. Although bedrock is not as permeable as the sedimentary bedrock present in Alberta's Rocky Mountains, it is still shown to be a source of streamflow, thus, it will be incorporated into the revised Introduction as suggested.

Line 77: Define area weighted precipitation. Was precipitation measured at multiple elevations? With > 1000m elevation change how much does the total precipitation change over that gradient? One

suggestion is use of a hypsometric curve to distribute precipitation over elevation (see Cowie et al., 2017)
Reply: Precipitation was measured at nine precipitation gauges in Star Creek and a neighbouring watershed (North York Creek). Spencer et al. (2019) used the thiessen area-weighted method to estimate average watershed precipitation rather than using one particular rain gauge to represent the entire watershed. The area-weighted method is a good approximation of average precipitation if rain gauges are distributed at a range of elevations. Our gauges are distributed between 1482 m and 1873 m. Vertical headwalls and talus slopes in the alpine basins limit the presence of precipitation gauges above 1900 m. While hypsometric curves can be useful to display the change in precipitation over a gradient, it would not add to the current story in this manuscript.

Clarification will be added to the draft manuscript to define area weighted precipitation and a citation to Spencer et al. (2019) will also be added.

Line 78: Please cite the precipitation and % snow. Is this from the same study (Spencer et al., 2019) which is cited in the discussion in reference to the sub surface storage capacity of the watersheds?
Reply: Yes, this is the same data from Spencer et al. (2019), although 2015-2018 were added to the years of record. Spencer et al. (2019) will be cited as suggested.

Line 83: "Talus slopes" Please expand this description to include more information on the relative size of this geographic feature in the upper watersheds. Previous studies of source waters to alpine watersheds in the Rocky Mountains (suggested references listed below) indicate that talus slopes and underlying features can be significant source water areas.
Is there any information or indication of permafrost, ice lenses, or rock glaciers in the alpine talus areas that could provide a unique source water?
Caine, N, 2010. Recent hydrologic change in a Colorado alpine basin: an indicator of permafrost thaw? https://doi.org/10.3189/172756411795932074
Clow, D. W., Schrott, L., Webb, R., Campbell, D. H., Torizzo, A., and Dorblaser, M.: Ground water occurence and contributions to streamflow in an alpine catchment, Colorado Front range, Ground Water, 41, 937–950, 2003.
Hood, J. L., Roy, J. W., and Hayashi, M.: Importance of groundwater in the water balance of an alpine headwater lake, Geophys. Res. Lett., 33, L13405, doi:10.1029/2006GL026611, 2006.
Roy and Hayashi, 2009. Multiple, distinct groundwater flow systems of a single moraine-talus feature in an alpine watershed. https://doi.org/10.1016/j.jhydrol.2009.04.018
Williams et al., 2006. Geochemistry and source waters of rock glacier outflow, Colorado Front Range. https://doi.org/10.1002/ppp.535
Reply: Talus slopes terminate in the alpine and forested regions of the watershed. Streams or tributary features flowing from the talus slopes have not be observed. Snowmelt and rain may be temporarily stored in talus slopes as documented in other Rocky Mountain watersheds (Cowie et al., 2017; Clow et al. 2003; Hood and Hayashi et al., 2015; McClymont et al., 2010), but it is likely that this water would infiltrate into the subsurface prior to arriving in the stream, thereby changing the chemical concentrations in this water. Permafrost, ice lenses, or rock glaciers are not present in the alpine talus areas based on the data we have from the Alberta Geologic Society, so it is unlikely that they could serve as a potential unidentified source.

The description of geographic features will be expanded so differences between Star Creek and other Rocky Mountain watersheds (indicated above) can be better understood by the reader.

Line 86: Can the amount of glacial till deposits be estimated or quantified for the sub-watersheds? There is no indication of spatial extent beyond description on line 80. It would help the reader to understand the potential storage capacity of the till especially since till water was excluded as a potential source water *due to sampling well contamination (line 181). One suggestion is moving the citation on line 444 (AGS, 2004) to section 2 study site description and elaborating on the description of the "spatially heterogenous surficial deposits…" to help describe the watershed(s) in more detail.*

Reply: The description of geologic features will be expanded in the revised site description. Data from the Alberta Geologic Society can be used to estimate the extent of glacial till, talus slopes, and other surficial deposits.

Line 95: Figure 1. It would be helpful to define tree line (separation of alpine from forested area within the sub-watershed. Important because the paper is framed as a study related to "forest disturbance" so the alpine portion of the study areas should be clearly separated from the forested areas.
Also please add the locations of the seeps that were sampled and used as potential end members in EMMA.

Reply: Although the way the paper is being framed is changing in response to comments from Referee #1, adding the extent of the forested area will help readers visualize the watershed. The extent of the forested area will be added to Figure 1.
Seep locations will be added to Figure 1.

Line 125: Snowmelt collection methods. Perhaps expand explanation of the snowmelt sample timing in order to reduce known uncertainty of changes in snowmelt chemistry related to timing of the melt. There is a known ionic pulse at the initiation of snowmelt (see Williams et al., 2009), which can be followed by dilute meltwater.

Reply: Snowmelt collection methods will be expanded but in general, snowmelt samples were collected at random and opportunistically when our field crew happened to be on site rather than at specific intervals. A time series of source water will also be added to the manuscript (suggested by Referee #1), which should identify the presence of a pulse in ion concentration at the onset of snowmelt if it was captured by the sampling campaign. There were some samples collected in early May that had elevated concentrations of ions compared to samples collected in early June, but ion concentrations were still far lower than concentrations observed in soils. The known ionic pulse in snowmelt to which Referee #2 is referring is very interesting and we will investigate this further.

Are there any occurrences of dust on other impurities in the snowpack in this region which could impact the snowmelt chemistry or the timing and magnitude of snowmelt? Dry deposition was mentioned for rain water collection (line 121) but not for snowmelt.

Reply: Dry deposition of dust/dirt can be a problem in the summer when the landscape is directly exposed to wind but is not an issue in the winter when the ground is frozen and snow covered. Dry deposition from major cities and industrial areas is not known to be a problem because neither are in proximity to the study site. However, organic material shed from forest vegetation and excreted from wild animals would be deposited onto the snowpack.

Line 171: Is the data from the Hobo sensors used in this paper? If not then this method does not support the paper and should be removed.

Reply: Data from Hobo sensors were used in Figure 10 and to determine the temperature range of bedrock and till groundwater in wells.

Line 274: Bedrock groundwater, "excluded as a source at the upper sites". Please explain how the groundwater seep used in SEU (line 313, figure 8b) was classified as having consistently cool GW temperatures, but was not considered to be a "bedrock groundwater source"?

Reply: We were hesitant to classify the groundwater seeps as "bedrock groundwater" with moderate certainty because although the water was consistently cool, the water chemistry and temperature range was not the same as the water sampled from the bedrock well. Groundwater seeps were 2 °C cooler than temperatures in the bedrock well and the ion concentrations in the seep groundwater were more variable than those in the groundwater well. A better explanation of groundwater seeps will be added to the draft text.

Line 276: Suggest replacement of "a couple samples" with a more quantitative description.
Reply: Will revise as suggested.

Figures 5-8: Suggest a more detailed explanation of the hysteresis present in the stream water samples. One option is to place the day of year (DOY) on each sample so readers can decipher movement within months which are plotted as one color. For example in figure 7A are the September samples temporally migrating in the mixing space or are sample points randomly distributed?
Reply: We will explore this suggestion and include this in revisions if it clarifies this issue without creating too much clutter. Adding a Julian day is an option but there are two years of data in this figure so it might add more confusion. Time series (as suggested by Referee #1) of stream and source water ion concentration would also help decipher movement at a finer scale. At the very least, more details will be added to the explanation in the Results to improve understanding of the patterns presented.

Figures 7 and 8: SEU and SEL both appear to have an unidentified source water in October as the October samples plot further away from the identified potential end-members. A more detailed interpretation of this observation is recommended for the discussion?
Reply: More details will be added to the Discussion as suggested.

Line 325: Section 5.3: It is understood that you were not able to sample in the winter, however you state that sampling stopped "before fall rains" (line) in previous section and in this section the "end" of seasonal sampling is stated as "start of the next year's snow accumulation period" (line 326). Just want to be clear on the terms used to describe the end of seasonal study periods.
Reply: The only sampling that was conducted 'before fall rains' was for groundwater seeps. Groundwater seeps were sampled during three sampling campaigns to target peak flows, recession flow, and baseflow (a range in watershed "wetness"). The final sampling campaign at the end of the summer (before fall rains) was an attempt to capture the clearest signal of "true" groundwater from the seeps if they were influenced at all by snowmelt or rainfall through the spring and summer. All other samples for other sources and the stream were collected from April to October (the start of the next water year). We will ensure that the language used in the revised manuscript is consistent and clear so there is no confusion for the reader.

If precipitation is lumped by rain and snow how do you know which form of precipitation is influencing stream flow in which season? For example line 342, the stream is "more similar to precipitation in June and July" Is this recent precipitation from rain or assumed to be the lagged input of snowmelt from the previous winter?
What would be helpful is a hyetograph over the study period so reader has some better sense of when the annual precipitation occurs. Also is there a way to present the timing and magnitude of snowmelt? Figure 10 suggests that there are multiple snowmelt pulses in winter and spring, can this be elaborated in the description of site climate and hydrologic inputs?
Reply: Ion concentrations of rainfall and snowmelt are essentially identical so there is no way to decipher between these two sources based on the chemistry we have (we do not have isotopes). Thus, the lagged input of snowmelt and recent rainfall cannot be separated. Further, the 2-week sampling schedule does not allow for the resolution needed to really identify a rainfall pulse moving through the watershed. A hyetograph could be combined with a hydrograph and

continuous snow depth data to show these dynamics in part and help the reader visualize the climatic conditions. Spencer et al. (2019) showed that snowmelt was the main period of hydrologic connectivity and that there was a larger streamflow response to rainfall events closer to the snowmelt period than later in the summer. Many rainfall events in the summer did not show a response in shallow groundwater wells suggesting that most percolated to deeper layers. These points and others suggested above will be added to description of climate and hydrology and to the revised manuscript where discussed to clarify these issues for the reader.

Line 399: "increases the concentration in water" should be "increases tracer concentrations in the soil water.." if you are speaking about the inverse of water chemistry "dilution" from snowmelt.
Reply: Will be revised as suggested.

Line 429: Please clarify "increases in stream water chemistry" to specify that you are speaking about tracer concentrations or "concentration of stream water ions" (line 450). Consistent terminology will help the flow of the manuscript.
Reply: Will be revised to 'an increase in stream water ion concentrations…'.

Line 457: Please provide citation for this statement "Excess water associated with forest disturbance would infiltrate into the subsurface". These assumed hydrologic dynamics should be discussed in more detail because there is potential for a varying hydrologic response from forest disturbance.
For example, in a forested snowmelt dominated watershed the timing and magnitude of snowpack accumulation and ablation in relation to canopy cover/density dynamics may be variable depending on forest dynamics. Sublimation rates on canopy snow interception (see Classen and Downy, 1995), and impacts of forest shading on radiative forcing on snowpack ablation could influence infiltration rates. I would also suggest mention of rainfall intensity relative to infiltration capacity in forested vs alpine or disturbed areas. Recommended references to review:
Molotch et al., 2009. Ecohydrological controls on snowmelt partitioning in mixed-conifer sub-alpine forests. https://doi.org/10.1002/eco.48
Harpold et al., 2014. Soil Moisture response to snowmelt timing in mixed-conifer subalpine forests. https://doi.org/10.1002/hyp.10400
Musselman et al., 2012 Influence of canopy structure and direct beam solar irradiance on snowmelt rates in a mixed conifer forest. https://doi.org/10.1016/j.agrformet.2012.03.011
Reply: The discussion on hydrologic resilience will be removed as suggested by Referee #1.

Line 475: Figure 10 caption revision. Second sentence is an interpretation of the graph rather than a description and should be included in the text. Recommend clarifying text description of "more responsive" and "slower recession slopes" in reference to depth to groundwater below the surface.
Reply: Figure caption will be revised and clarification of these terms will be added.

Figure 10: In the soil/till GW, what causes the sharp response (increase in water table elevation) in November? Is this related to early season snowfall that melts or other factor such as vegetative senescence? Does the chemistry change in that water source in late fall?
Can you explain the two separate groundwater level increases in the till well that occur in February and then again in March/April? Is this related to intermittent snowpack throughout the winter (as briefly mentioned in the snowmelt sampling methods line 125)?
Reply: Figure 10 was added simply to characterize the water table recession and infer the conductivity of the bedrock well compared to the glacial till well. The specifics of the responses in November, February, and March/April were not investigated in part because these responses were for 2017 and we were focusing on 2014/2015 seasons. However, we will look into the

glacial till water table/snowmelt dynamics to address this comment and determine if more information on runoff generation can be inferred from these data.

Line 480: Replace "old water" with a more accurate description representative of transit time or sub-surface residence time rather than speaking to the age of the water, or define old water to mean "reacted" waters that have had extended contact time with the sub-surface (see Liu et al. 2004) The same suggestion was made previously for defining the use of "old water" in the abstract.

Reply: Any reference to "old water" will be defined and revised to mean "reacted" water or water that was stored in the watershed over winter rather than a specific age of the water.

Line 485: Indicates that till groundwater could be slowly released to the stream (longer recession in Figure 10). It is not clear if the intention was to suggest that this could be the unidentified source water end member in late fall in Star East, but was not was not captured or used in EMMA due to experimental design issues leading to well contamination?

Reply: There is a possibility that till groundwater is the unidentified source in late fall in Star East; however, there is no concrete evidence presented here that allows us to make this conclusion. This would simply be speculation based on other lines of evidence and observations made in other regions of North America. This section will be clarified and any direct inferences we can make will be added.

Line 486: Please expand the conclusion/suggestion that till groundwater (although not used as an end member for EMMA) has the potential to mute the effects of disturbance on peak flow. I assume you are referring to forest disturbance, but it is not clear of the locational relationship between till groundwater sources and forested areas within the watersheds. Is the till groundwater believed to be sourced from direct overhead recharge (in the same location as currently existing forests)? or is there another hypothesized mechanism of recharge such as mountain block recharge from higher alpine regions already void of forest cover?

Reply: Reference to resilience will be removed from the conclusions (and the rest of the manuscript) as suggested by Referee #1. However, clarification of till groundwater responses/sources will be added to the manuscript.