Dear Referees,

Thank you for your constructive and critical comments on our submitted manuscript. We plan to address your comments through a major revision and will alter the manuscript focus to be on lithologic controls on earthflows, the effect of earthflows on valley width and sediment, and we will introduce/discuss MADstd as a potential method for relative dating earthflow activity. We believe these revisions will address your concerns, particularly regarding the relationship between MADstd and absolute age and regarding the historic analysis of salmon habitat. We are confident these revisions will improve the quality of the manuscript.

Our detailed comments are below.

Sincerely,
Sarah Schanz, on behalf of the authors

Reply to Referee #1

RC1: First, to test the hypothesis that high earthflow frequency corresponds to times of large salmon populations, the salmon population would need to be independently known over the Holocene. I'm not a salmon expert, but that information doesn't seem readily attainable, and the manuscript doesn't provide any independent references that have determined it. Instead, there is a suggestion that salmon populations may have stabilized 4-5 ka based on very broad scale inferences about climatic and tectonic changes.

AC: We agree that this hypothesis cannot be readily tested by the data, and plan to restructure the goals of the paper to focus on investigating earthflow activity in the Teanaway. The impact of earthflows on salmon habitat would then be relegated to an interesting point of future research in the discussion section, rather than the driving hypothesis.

RC1: Second, the study did not present a significant, data-derived relationship between earthflow age and surface roughness, which would be necessary in order to use surface roughness to infer age of undated earthflows.

Age vs. activity. Since age (~time since a landslide happened) and activity (~how active the landslide is currently) both affect a landslide's surface roughness, it can be quite challenging to disentangle these two effects for earthflows in particular. This may be one reason why there is not a relationship between landslide age and roughness for the study site. Fig. 5 is a bit misleading on this point, since it shows a modeling result, which by design will smooth the landslide deposit over time. If you add the age and roughness values from Table 1 to the figure, it is clear that the radiocarbon-based ages and roughnesses are not correlated and do not follow the model-predicted trend. Those data are so scattered that that general inferences about mid or late Holocene timing aren't supported either (e.g. the smoothest landslide is the youngest (Dickey Cr.) and the oldest is the roughest (Indian Cr.)). Perhaps the fact that age and *roughness don't correlate is a useful observation for thinking about what other factors control surface roughness of these earthflows.*

Landslide dates. As stated in the manuscript, charcoal provides only a maximum age, and often this age is not a close maximum. E.g. see Struble et al. (2020), GSA Bull. where charcoal from a landslide's deposit is up to thousands of years older than the landslide. Also in the present manuscript, the discrepancy between the minimum and maximum ages of the Rye Creek earthflow (346 and 4,353 yrs, respectively) suggests that either the earthflow's age isn't accurately enough known to determine an age roughness model, or that a single age for the earthflow may not be representative of its long term behavior, such as persistent movement and/or reactivations.

AC: This is a great distinction, and thank you for the careful explanation. We assume that radiocarbon ages give some measure of the activity, but don't constrain when the landslide occurred (age). This is one reason we avoided directly comparing the radiocarbon ages to the MADstd values, noting that there is a wide variability in what the radiocarbon age represents relative to landslide age (and we plan to add directly the example in Struble et al. (2020)).

We plan to revise the manuscript to make it clear what is meant by age vs activity. For example, MADstd examines time since last activity, assuming the earthflow has stopped moving. This should not be compared directly to ages estimated from radiocarbon (maximum) but should be more closely related to minimum ages from sedimentation (minimum). We will make it clear that earthflows are not a single event, thus a single age, or even a single dating technique, will not provide complete information on earthflow age and activity.

We also plan to shift the focus of the manuscript away from salmon habitat and timing of earthflow activity. We recognize we did not have enough information to attempt this before. Instead, following Referee #2's suggestions, the manuscript will be edited to focus on lithologic controls on earthflows, impacts of earthflows on valley width and sediment, and introduction and discussion of a potential method to rate earthflow activity (but make it clear that we are not tying MADstd directly to absolute dates/ages). We believe this should remove many of Referee #1's concerns about the validity of using MADstd to infer ages and inferences about earthflow timing.

RC1: An exciting, if somewhat technical, finding of this work is that the chosen roughness metric decreases linearly with model simulation time. Previous studies have found an exponential age v. roughness relationship, which means that absolute uncertainties of predicted ages are quite large for the older landslides on the steeper part of that curve. A relatively simple fix of using this or a similar roughness metric may dramatically reduce the uncertainty on predicted ages of old landslides.

AC: This is an excellent point that we hadn't thought about. Since the model of MADstd values only applied diffusion to a landscape, we would be hesitant to predict a linear relationship over long time scales. As addressed in our Referee #2 response, we plan to do more diffusion models of MADstd to see how the roughness values vary with diffusion rate, and what a reasonable range of MADstd values with age is. We could include some runs with stream erosion and see if the MADstd-age relationship continues to be linear. In any case, following Referee #2's suggestions, we plan to expand the discussion section to include limitations of the MADstd technique. We plan to incorporate Referee #1's note that incorporating this technique or a similar metric to landslide studies could increase the accuracy of predicted ages for older landslides.

Response to Referee #2:

RC2: One thing I found confusing is the discrepancy between the model MADstd values and the MADstd values for earthflows in the Teanaway basin. I don't understand how mid-Holocene ages were estimated for the older earthflows in the Teanaway when MADstd values are much lower than the lowest in the model. Perhaps this can be expanded. Unlike reviewer #1, I don't think it matters that there is poor correlation between the absolute ages presented here and the MADstd – I think it's clear how much uncertainty there is in the earthflow ages, particularly the older ones. That said, I think it might be nice to show in a figure that where you do have field evidence of relative ages (e.g., cross-cutting relationships), it does work. Also, I think this part of the discussion should be moved to the results.

AC: We plan to expand the discussion of the MADstd values and estimated ages. We assumed that MADstd values would vary based on diffusion rates and advective processes acting alongside the earthflow; we assumed this would mean the model MADstd and actual MADstd values may not match, thus the apparent logic leap when we estimated a mid-Holocene age. However, we plan to test this assumption; for one, we can run the MADstd model again with different diffusion values to estimate a likely range in MADstd values with earthflow age. This would allow us to better support an age estimate for the Teanaway earthflows, whether that age ends up being mid-Holocene or not. We also plan to shift the focus of the manuscript away from concrete dates/timing of earthflows into a focus on earthflow controls and morphologic effects.

We plan to add a figure that shows the cross-cutting relationships, and move this discussion section to the results.

RC2: There is too much focus on salmon habitat. Although I think it's a good motivation for investigating the timing and controls on earthflows and how they impact valley width, the focus on it (e.g., section 2.2) implies there will be a solid conclusion relating to it. In the end, the claim is that given that earthflows were active when salmon populations stabilized, they don't seem to have negatively impacted salmon habitat. It's not a big conclusion, but reviewer 1 may be right that the data still doesn't support it. We can never know what fish populations would be if there were no earthflows. I think all you can say is that earthflows contribute to topographic heterogeneity and that non-catastrophic disturbances and topographic heterogeneity are generally good for biodiversity.

AC: Thank you for this point. The study was originally conceived as a senior thesis related to salmon habitat, and we got stuck on this point without stepping back and assessing whether it was accurate. We plan to revise the manuscript to focus on the structural controls and geomorphic implications of the earthflows, rather than focusing on salmon habitat. We do think habitat disturbance is an interesting implication and perhaps motivation for further work, but we plan to confine this to a smaller section in the discussion that does not offer definitive conclusions about habitat and earthflow timing, but rather speaks generally to the topographic alterations being good for habitat.

RC2: The hypotheses about climate control on earthflow activity seem like a bit of a stretch given the uncertainty on earthflow ages. But, if you're going to discuss this, Bennett 2016 probably ought to be referenced (see below). Perhaps though, instead, more focus should be on the main results: earthflows are active in the Teanaway basin, are structurally controlled, and act to modify valley width and hence floodplain habitat, as well as sediment flux and most likely grain size. Much of the discussion could instead be used to discuss limitations on the techniques employed and areas of future work.

AC: We plan to shift the focus to be on the active earthflows in the Teanaway, the structural control, and the valley width impacts. Our discussion will be revised to focus on impacts of the earthflows on sediment flux, grain size and habitat, as well as a discussion of the limitations of the techniques and areas of future work. This will shift focus away from the climate control, which we agree is a tenuous connection.

RC2: I suggest deleting everything about rotational and translational slides to better focus on earthflows.

AC: We plan to remove the sections about rotational and translational slides to focus the study on earthflows.

RC2: Figure comments
Fig. 3 I think this could be moved to a supplement
Fig. 7 I think this could be moved to a supplement
Fig. 8 Why isn't valley width plotted against discharge as in May, 2013? It would also be really nice to see some lidar hillshade images of these constrained and upstream widened reaches.
Fig. 9 I think this figure should be moved up to the methods or results

AC: We plan to add a figure showing the cross-cutting relationships that support the MADstd values. We will move figures 3 and 7 to a supplement, or remove them altogether. We plan to move Figure 9 as well as the discussion of MADstd ages to the results section. For Fig 8 and the valley width results, we will add images showing the constrained and widened reaches. We will plot valley width against drainage area (as a proxy for discharge) to better match previous work.