Dear Editor,

Thank you and the two anonymous reviewers for constructive and critical comments on our submitted manuscript. In the revised version, we addressed your comments through a major revision and have altered the manuscript focus--and title--to be on lithologic controls on earthflows, the effect of earthflows on valley width and sediment, and MADstd as a potential method for relative dating earthflow activity. We believe these revisions addressed your and reviewers' 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. Please note that line numbers refer to the track-changes document.

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. We restructured the goals of the papers to focus on controls on earthflow activity. Salmon habitat is mentioned as a motivation for understanding the impacts of earthflows, and as a potential implication of the Teanaway earthlows' effect on sediment loads and valley widths. These changes can be seen most prominently in the introduction, where previous paragraph #2 has been deleted; background where the section on historic salmon habitat disturbance is deleted; and the discussion section 5.2 which now contains all of our implications for salmon habitat and is much shorter than the original manuscript.

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: The distinction between age and activity is a good point, and below we show how we edited the manuscript to make this clear. We also show how we edited the manuscript in recognition that the MADstd method is limited by our absolute age controls.

We added the direct comparison to Struble et al. (2020) in lines 302 and 564 where we acknowledge the potential error in radiocarbon ages [line 302] and compare our sedimentation ages and radiocarbon ages to show ~4000 years over-estimation in the radiocarbon ages. This is similar to values found by Struble et al. (2020) [line 564].

We revised to make it clear what each dating method implies. For radiocarbon and sedimentation ages, we altered the heading to show that these are maximum ages [line 294 "3.1 Earthflow mapping and maximum earthflow ages; line 521 "4.2 Maximum earthflow ages"]. We added clarification of what these ages represent in the methods section [lines 301-305 for radiocarbon; lines 319-320 for sedimentation] and again in the results [lines 555-567].

For MADstd, we revised the manuscript to make it clear that time since last major activity is being evaluated; and that the MADstd metric gives relative earthflow activity, not relative age. This revision can be found in the headings [lines 365 and 815] and in the methods, results, and discussion sections discussing MADstd [lines 366-374 for methods; 577-835 for results; and 905 for discussion].

We also shifted 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 was 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 we make it clear that we are not

tying MADstd directly to absolute dates/ages). We believe these changes addressed Referee #1's concerns about the validity of using MADstd to infer ages and inferences about earthflow timing.

The changes above can be seen throughout the manuscript but especially in:

- Title has been altered to focus on controls on earthflow activity rather than salmon habitat and relative dating.
- Background section focuses on the study site geology and the portions regarding salmon habitat and disturbances have been deleted.
- Results [lines 425 – 491] emphasize the position of earthflows and lithologic relationships.
- Discussion is now organized into three sections on: Drivers of earthflow motion [lines 836-876] and Landscape disturbance [lines 877-903]. Briefly in section 5.2 Landscape Disturbance, we hypothesize some habitat disturbance in the form of valley width changes and sediment loads, but we do not focus on salmon and avoid making absolute conclusions about the timing of salmon habitat change.
- We also altered discussion of the timing of earthflows to be much looser. Recognizing that our absolute ages are mostly in the last few hundred years, we do not extrapolate MADstd beyond those bounds. We briefly hypothesize that anthropogenic activity may have contributed to recent earthflow activity, based on timing and a lack of a strong climate signal [lines 868-876].

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. We added stream erosion to our diffusion model – this is updated in the code stored on Github – and we ran a suite of models with varying diffusion rates and with and without stream power [lines 383-391]. We found that stream erosion did not change MADstd [lines 577-580], and that all diffusion runs result in a linear relationship with roughness and simulation time. The old Figure 5, which is now Figure 6, is updated to show a range of diffusion values.

We added a paragraph to our discussion of MADstd that discusses the potential for this linear model to add precision to surface roughness age methods [lines 970-977].

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 expanded the discussion of MADstd values and estimated ages by adding a section in the results called "4.4 Verification of MADstd relative dating" [lines 576-811]. This section contains much of the content previously in the manuscript discussion.

We re-ran the model with different diffusion values and with advective processes (stream power) (see response to RC1 comments above). However, this showed that for low diffusion values, the low slope between MADstd and age results in a high range of ages for a single MADstd value, if you do not know the diffusion rate precisely (Figure 6). Based on this, we revised the manuscript to avoid making conclusions about earthflow ages in the mid Holocene. Instead, we focus on MADstd values we have absolute age controls for, which are in the last few hundred years [lines 815-835].

In the Verification of MADstd… section, we discuss the range of MADstd values for similar earthflow activity age [lines 711-811]. Through this revision, we hope we address RC2 and RC1's concerns that MADstd is too heavily extrapolated.

We created a new figure to show cross cutting relationships [Figure 7] as well as a summary of our absolute ages and MADstd ages [Figure 5].

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 revised the manuscript to focus on the structural controls and geomorphic implications of the earthflows, rather than focusing on salmon habitat. We removed salmon habitat from the background section and removed the discussion section on it. We retained

habitat as a motivation for understanding earthflow controls, seen in the introduction, and we briefly address how the changes in valley width and sediment loads might affect habitat [lines 896-903]. However, the broader discussion section [lines 877-895] focuses on larger-scale topographic disturbance rather than narrowly on salmon habitat history.

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 shifted the focus to be on the active earthflows in the Teanaway, the structural control, and the valley width impacts. Our discussion was 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 MADstd techniques and areas of future work.

The changes above can be seen throughout the manuscript but especially in:

- Title has been altered to focus on controls on earthflow activity rather than salmon habitat and relative dating.
- Background section focuses on the study site and the underlying geology and the portions regarding salmon habitat and disturbances have been deleted.
- Results [lines 425 – 491] emphasize the position of earthflows and lithologic relationships.
- Discussion is now organized into three sections on: Driver of earthflow motion [lines 836-876] and Landscape disturbance [lines 877-903]. Briefly in section 5.2 Landscape Disturbance, we hypothesize some habitat disturbance in the form of valley width changes and sediment loads, but we do not focus on salmon and avoid making absolute conclusions about the timing of salmon habitat change.
- We also altered discussion of the timing of earthflows to be much looser. Recognizing that our absolute ages are mostly in the last few hundred years, we do not extrapolate MADstd beyond those bounds. We briefly hypothesize that anthropogenic activity may have contributed to recent earthflow activity, based on timing and a lack of a strong climate signal [lines 868-876].

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

AC: We removed the sections about rotational and translational slides to focus the study on earthflows. This is seen in an edited Figure 2 and Figure 3 which now only show earthflows.

RC2: Figure comments

Fig. 3 I think this could be moved to a supplement.

We removed Figure 3, as the results could be summarized in one sentence. [lines 486-489]

Fig. 7 I think this could be moved to a supplement

We removed Figure 7. In our revised version, we did not find it necessary to discuss earthflow area vs MADstd.

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.

We edited the figure, now Figure 4, to show valley width against upstream drainage area, as a proxy for discharge. We also added three hillshade images to show typical narrowed reaches.

Fig. 9 I think this figure should be moved up to the methods or results

We deleted Figure 9 because we added two figures showing the cross-cutting relationships [Figure 7] and summarizing the absolute age controls [Figure 5]. Figure 5 has an inset map showing the MADstd of an earthflow complex and serves the same purpose as Fig 9 in showing the uniform MADstd across a forested hillslope, and the higher MADstd values near scarps.