Deep-Seated Landslide Mapping & Classification Study Design						
Comment topic	***Reviewer***	***Location: Page/Line in original doc***	***Reviewer comment***	***Author response***	***Project Team Comments***	***Reviewer response to author action***
General Overview	**1**	N/A	This study design outlines a plan to characterize spatial patterns of deep-seated landslides with respect to a variety of geospatial data sets, and then to analyze temporal patterns for a subset of those landslides that are detectably active. This would be carried out in three sub-county regions in western WA. The proposed work should result in a classification system for deep-seated landslides that would help applied geologists understand conditioning and triggering factors and inform decisions related to land use. Overall, this is an ambitious study design that will likely lead to some interesting and useful results. The plans for characterizing landslide types, determining if they are active or not, and grouping them into classes and clusters based on geospatial data is likely to be successful and useful. The main challenge will be characterizing velocity patterns. None of the remote sensing methods are ideal for capturing a large representative sample of active landslides from which to draw conclusions based on statistical analyses. This is not the fault of the authors, but just the state of the data and analysis techniques at present (and the authors describe/acknowledge the main limitations well). More details below, but in general both InSAR and lidar change detection have major biases as to what velocity ranges and geometric arrangements of landslides they can reliably detect. And compared to all the landslides present, only a small subset of them are likely active to begin with. So, I think the goal of documenting different velocity patterns and capturing changes in velocity that can be attributed to a particular triggering mechanism for a large number of landslides is a bit beyond reach. I do think it's worth giving a try though, and hopefully it will yield some useful information for at least some landslides. Below, I provide more detailed comments as answers the 8 ISPR questions, and have some more specific line comments after that. Although there are a few issues that I think should be addressed with revision, the work is generally well thought out and describes a plan worth moving ahead on.	Thank you for your guidance in your overview. We agree, this will be a challenging undertaking! We have addressed many of these broad comments and detailed comments in the document and will use this table to direct the reader to the specific sections for which we've provided an expanded explanation or more detailed methodology.		AE: I appreciate the thorough revisions in light of the reviewer comments. I think this document and plan is more practical and more cognizant of the challenges associated with this endeavor. Response accepted.
Q1. Are rigorous, transparent, and sound research and statistical methods proposed?	**1**	N/A	Generally, yes. I especially appreciated the forethought given to organizing the large volumes of data to be collected in a way that will make then accessible to "big data" analysis techniques down the road. Lidar change detection and InSAR methods are now fairly standard as described in the document, and limitations are also described well.	Thank you for your comment. No response from the authors.		N/A
Q1a. Is there enough detail in the Study Design to understand and implement the statistical methods and workflow needed to conduct this study?	**1**	N/A	Assuming the people who will be doing this work have previous experience and expertise in LCD and InSAR, the study design provides enough guidance to conduct the study. There can be a steep learning curve and lot of nuance in interpreting results, so having a team with that expertise will be important for accomplishing the work.	Thank you for this comment. We agree and have tried to lay out the Technical Limitations (Section 7) and provided suggested guidance for selecting a contractor to carry out the study (see Intent of this Document, lower bullets). It will be imperative that the study is overseen by a team with a rich background in these technologies.		AE: I appreciate the acknowledgments of uncertainty and limitations in section 7. I also note significnatly more detail added to section 5 relating to the nuances of these approaches and the interpretation of their results. Response accepted.
Q2. Is there sufficient detail in the document to conduct the study as written?	**1**	N/A	As above, assuming prior experience, yes.	Thank you for your comment. No response from the authors.		N/A
Q3. Is there sufficient detail to support consistent, reasonable, and reproducible data interpretations?	**1**	N/A	For the first phases of the plan, yes. The underlying data (existing landslide inventories) are quite objective, and the LCD and InSAR techniques should be reproducible. For the later phases of the plan – characterizing velocity time series – there will be more ambiguity regarding interpretations. Part of this is due to the discrepancy between a landslide's actual 3D velocity and the single component of velocity that LCD or InSAR captures (more on this in 3a, below). The other part is that how to objectively interpret the velocity time series and identify jumps and associated triggers isn't clearly articulated. The study design suggests taking a categorical approach, and noting when a landslide transitions from one velocity range to another. But that transition is rather arbitrary, and simplifying things down to a couple classes seems to be missing the rich and varied temporal behavior of landslides. Some are seasonally active, others are mostly stuck, but episodically reactivate, etc., so it would be great to see a plan for more systematically and objectively analyzing temporal patterns.	Thank you for your comment. We will respond to the 3d vs 1d movement vector comment in the next bullet. We also agree that the velocity transitions cannot purely fit into the velocity class segmentation proposed. Each change in velocity will need to be properly attributed to a timeframe such that the magnitude of the change can be compared against external drivers (natural or human induced). As we are applying satellite LOS data, in many cases the absolute value of what is measured will not be representive of the actual displacement and therefore care will need to be taken to best integrate this data into a meaningful frame to compare across various landslides. This consideration is described at the beginning of Section 5.2.3.3. - We've attempted to clarify that the LCD method we are recommending is indeed a 3D change vector and not 1D velocity as suggested by the reviewer. Section 5.2.3.2 - InSAR decomposition (from Line of Sight to East-West and Vertical components) is now included in 5.2.4.2. - Velocity time series interpretation is now covered in 5.4.4.3. - Another addition related to landslide velocity that may be of interest to the reviewer is in 5.2.1.2		AE: These sections have notably more detail, which is appreciated. There is still some ambiguity about the specific approach taken towards connecting triggering (e.g. soil moisture, rainfall) and movement response (statistics and categorization of movements), but the numerous examples of datasets and frameworks for interpreting data provide more confidence as to proposed approach. Response accepted.
Q3a. Are the proposed uses of different data for classification and statistical analyses, including InSAR and lidar change detection, reasonable given the data limitations?	**1**	N/A	Generally yes, but one important limitation that should be addressed throughout the study design is the meaning/interpretation of landslide velocity. Velocity is fundamentally a 3D vector field for the entire landslide, but both LCD and InSAR are only capturing one component of that motion. So 'velocity' as described isn't capturing the actual velocity of the slide, and the same 'velocity' can result from a wide range true 3D velocities. For example, a relatively large but shallow slide on a gentle slope can have a fast true velocity (mainly horizontal), but a small vertical rate of change distributed over a large area in the head, which is what LCD/InSAR captures. In contrast, a very deep slide could have a slower true 3D velocity for the same measured ~vertical rate of change. Furthermore, all else being equal, the same slide on a steeper slope would have a larger vertical rate of change. All this is to say that 'velocity' as described in the document is not a reliable way to characterize landslide movement rates and divide them up into velocity classes. This issue is in theory resolvable, as there are well-established techniques to measure horizontal components of deformation from remote sensing images, which would complement the 1- component measurements described. These are generally referred to as "image correlation" or "pixel tracking" techniques, and I would say they are now widely applied in landslide studies, in addition to glaciers, fault slip, etc. A variety of remote sensing products can be used including aerial or satellite imagery (although probably not good in WA b/c of trees, but maybe on clear cuts), lidar DEMs, or SAR images.	The LCD methodology described in the document results in 3d displacement vectors with a limitation that it does not detect surface-parallel movement. From 5.2.2.2: "This method produces enhanced results over DEM differencing or surface comparisons as the results represent a 3D change based on the full resolution of the point-cloud data." The reviewer brings up a fantastic point, however, that not all LCD methods result in 3D change. We mention other methods in 5.2.2.2, that, while simpler to execute, do only report the vertical or 1d change. We do not recommend using these 1D methods in the study design for the reasons the reviewer brings up. Furthermore, the surface-parallel limitation is stated in 5.2.2.3. For clarity, we have removed "true" from 3D change descriptions in the manuscript. In regards to InSAR the reviewer is correct that by utilizing a single InSAR look direction we are only measuring a portion of the actual displacement. There will be judgement required based on the orientation of the satellite LOS and what is understood about the landslide kinematics and movement style in order to properly attribute the InSAR displacement values to the landslide for further analyses. This consideration is described at the beginning of Section 5.2.3.3. The understanding of the landslide is essential prior to exploring any of the techniques outlined. Since the development of the Study Design there is a very applicable paper that has been published by the Italian National Research Council that describes this very application and we have added this reference to the study design: Cignetti, et al 2023, https://link.springer.com/article/10.1007/s10346-023-02114-7. - Two of the discussed LCD methodologies (surface based methods and point based methods) result in a 3D change vector and not a 1D component as suggested by the reviewer. This is discussed in 5.2.3.2. - LOS displacement from InSAR can be decomposed into vertical and east-west components when using ascending and descending look geometries. This is discussed in Section 5.2.4. - We have added a section on Pixel Tracking (5.2.5). Additionally, some new text on landslide velocity in Section 5.2 may also apply to this comment.		AE: Response accepted.
Q3b. Is it clear how data will be evaluated for inclusion in the proposed analyses?	**1**	N/A	Yes, the document did a thorough job describing available datasets and their spatial and temporal coverages.	Thank you for your comment. No response from the authors.		N/A
Q4. Is the methodology/workflow clear, reproducible, and capable of achieving the project goals?	**1**	N/A	Generally, yes -- See also comments on #2 and #3 above. The workflow starts out very clear, then tends to get vague through the downstream parts of the proposed work. Some of this is to be expected as results from initial phases will inform choices for latter phases. I would recommend a more objective workflow for the time series analysis than is given.	Thank you for your comment. You are correct, the early results will greatly influence the downstream decisions, specifically around statistical modeling methods. We just won't know how large of a dataset we have until we start interrogating the data (we mention this in Section 7 - Technical Limitations). As noted above, in the time period since issuing this draft report there has been a very applicable paper and methodology presented by the Italian National Research Council (Cignetti et al, 2023) that could be followed for this study execution and we will revisit the report to update. We have added a section specifically on velocity time series analysis (Section 5.4.4.3) to address this comment.		AE: I think the authors make the "known unknowns" of this aspect more clear in the revisions. Response accepted.
Q5. Do the literature citations include the latest applicable information and represent the current state of scientific understanding on this topic?	**1**	N/A	Two main areas that could be improved in terms of citations and current scientific understanding are (1) quantifying 3D landslide velocity, and (2) velocity time series analysis.	Thank you for this comment. These items are addressed elsewhere in the response to comments and no response is provided here.		AE: Response accepted.
Q6. Are uncertainties and limitations of the proposed work stated and described adequately?	**1**	N/A	Aside from the velocity definition issue, uncertainties and limitations are acknowledged and well described throughout the document.	Thank you for your comment. No response from the authors.		N/A
Q7. Are assumptions stated and described adequately?	**1**	N/A	Same comment as for #6.	Thank you for your comment. No response from the authors.		N/A
Q8. Is the information presented in an accurate, clear, complete, and unbiased manner and in a proper context?	**1**	N/A	Yes, the document is well organized, easy to read, and clearly spells out the proposed work.	Thank you for your comment. No response from the authors.		N/A
Change Detection	**1**	p. 27, last para	Wouldn't the lower point densities in forested environments bias identification of active landslides toward recently harvested areas? Since one goal of this work is to inform timber harvest decisions, I think getting a handle on these types of biases would be important.	The measurement of displacement in vegetated slopes is why the specific data types have been chosen. Our comment here about lower point densities is specifically in regards to C-band data. For this exact purpose (lower point densities), we recommend L-band data (e.g., ALOS-2) be used in the study as a primary InSAR data source. By utilizing the archived L-Band InSAR data sets we would expect to have sufficient ground returns that are able to reflect consistent coverage of ground deformations in vegetated vs. non-vegetated slopes. The study design team works extensively with InSAR in heavilly vegetated terrain in Western North America and has found that the deep stacks of L-Band ScanSAR data are able to provide reliable delineation of zones of deformation, considering velocity ranges and landslide orientation. The same cannot be said with the application of C-Band Sentinel-1 data where we see signficant bias towards only collecting data reliably in non-vegetated areas. A recent paper by Yang et al (2023) demonstrates the increase in data returns using L-Band data vs. C-Band data for landslides on heavily vegegated hillslope in China. Please refer to Section 5.2.4.1 (Vegetative Cover bullet) and the newly added Figure 5-7.		AE: Response accepted.

Landslide Morphology, Velocity and Classification	**1**	p. 31, penultimate para	Although it's common practice for the state landslide inventories, this depth estimation is problematic, as it only estimates depth at one point on the landslide, right at its head scarp. If the landslide has a planar failure surface of the same depth, great, but otherwise it may be wildly different than the landslide's true maximum or average depth. E.g. think of a deep-seated rotational slide that's experienced limited displacement – it can have a very small head scarp, but a very large depth.	We are in full agreement with your comments. The attribution of a measured surface displacement point to movement at depth is not trivial. The combination of the high resolution surface morphology derived from lidar and an understanding of geology and landslide kinematics is required to properly assess whether a measured point is representitive of a near surface process or an indicator of what is happening at depth. We agree with this entirely and have addressed in Section 5.4.2.3 Depth to Rupture Surface.		AE: I'd add that more simplistic approaches such as Area-Volume relationships might be a simple metric of initially "guessing" landslide thickness, although it has its own limitations versus mass conservaton-based approaches. Response accepted.
Landslide Morphology, Velocity and Classification	**1**	p. 41, 1st para	Since these class boundaries are arbitrary (i.e. velocities are continuous over the range, and there aren't clear changes in physical processes or mechanisms associated with those boundaries), you lose a lot of valuable data contained in the actual velocity time series by simplifying into categorical data. Would it be possible to propose some more advanced time series analyses that would better capture the wide variety of landslide behaviors that may be captured?	The velocity class boundaries are not quite "arbitrary" - they are largely based on Cruden and Varnes 1996 with the slight modification of class 2 landslides to include a 2a and 2b class. This is consistent with the authors' experience working with slow moving landslides in western Canada. Additionally, no data will be "lost" with this approach. The objective here is to assign a velocity class (e.g. Class 2a) to each time step of analysis *in addition* to the measured or inferred velocity (e.g., 100 mm/yr). We are currently working on advancing time series analysis methods for landslide velocity evaluations that could omit the use of categorical breaks, but at present, do not have a solution for this. More advanced methods could be proposed in a pilot study proposal. Please refer to new sections 5.2.1.2 and 5.4.4.3.		AE: This is fair - there needs to be a balance between simplicity (i.e. categorical means of classifying landslide velocity behavior) and fidelity to the true landslide motion. This response is accepted and I appreciate the revisions that at least suggest pathways for more advanced classification schema. I'd encourage the authors to give this specific challenge some thought if this plan moves forward, however.
Change Detection	**1**	p. 45, bullets	These are clear, established methods for change detection, but only capture one component of the true 3D ground deformation. Recommend adding image correlation/pixel tracking to this list.	Thank you for this comment. In Section 5.2.2.2, we list 3 of the more common conceptual LCD approaches. In the 3rd bullet, for point based methods, we describe a method that results in 3-dimensional vector changes between point clouds and not only "one component of the true 3D ground deformation", as suggested by the reviewer. We have added a section on pixel tracking (Section 5.2.5) and believe this will greatly strengthen the scientific merit of the study!		AE: Response accepted.
Change Detection	**1**	p. 45, after the bullets	Change "noise" to "bias," as aligning the lidar point clouds reduces the latter, but doesn't affect the former. I.e. it just shifts one point cloud relative to the other.	Thank you for this comment. We have incorporated this change.		N/A
Change Detection	**1**	p. 45, last para	Use of standardized terminology and some more nuance in interpreting one- component ground deformation is needed here. For one example, a positive difference could also represent a roughness element, like a large hummock or bump, that translated downslope, without any accumulation or bulging of material. Maybe that is implied in the term "bulging," but I'm not sure. Similarly, I would consider "slumping" to describe the pattern of negative change at the head and positive change at the toe of a typical rotational landslide, not just a negative model difference. Compaction or dilation of landslide material can also produce vertical changes – unclear if that's meant to be included in "accumulation," "bulging," or "slumping."	Thank you for this comment. We have made minor revisions to increase clarity. We have moved material accumulation and bulging into examples of positive model difference causes, of which,a s you point out, there are more. We agree with your assessment of slumping and have removed it from the examples given for negative model difference. You are correct there are a variety of surface changes that could be attributed to positive or negative model difference. It would be beyond the scope here to include a comprehensive LCD interpretation guide. Instead, we recommend analysts trained in the evaluation of three-dimensional change vectors perform the LCD analysis.		AE: Response accepted.
Landslide Sensitivity	**1**	p. 59, bullet #2	Could you use supplementary information to narrow down the time of movement, rather than assuming an annualized rate? I would expect some landslides captured in this way to have failed rapidly in one or more brief episodes, others to have moved more continuously (or anything in between), so using an annualized rate could be misleading. I think it would be important to distinguish episodic reactivations from continuous movements in terms of hazard and planning for future land use.	The reviewer raises important points here. Care will need to be taken to not underestimate the rate of movement of landslides, particularly when utilizing repeat lidar that may have several years separating the reference and secondary datasets. At the scale of many thousands of landslides, this can be quite difficult. With single or small populations of landslides, one can use hydroclimatic records to attempt to identify triggering storms. However, for this study, given it's objective of study deep-seated landslides, triggering storms and single bouts of movement will be even more difficult to identify. While we appreciate this comment and struggle with the same question, we are not aware of a solution at present. We just must keep this in the front of our mind during velocity assessments.		AE: Could InSAR better constrain these rates (or at least the bounds of time when movement has occurred) through tracked veocities or temporary loss of coherence? Satellite imagery might also be available for ad hoc analyses; however, this might be scope creep. Just some thoughts - response accepted.
Landslide Sensitivity	**1**	p. 63, last para	Note that Iverson et al. (2015), EPSL, did a similar analysis for precipitation trends that would be relevant here.	Thank you for this comment. Dick's work here is indeed applicable. We've added a reference in 5.4.2. An important point to note is that the international landslide community is moving away from the use of precipitation-only thresholds for DSLs. This follows on the acknowledgement that antecedent soil moisture and seasonal water balances are key factors that contribute to activity changes in DSLs. Some applicable examples in the literature include work by Distefan et al (2023), van Natijne et al (2023) and Wang et al (2023).		AE: Response accepted.
Additional Considerations	**1**	p. 69, 2nd full para	Recommend removing "from experience," as this is subjective, and no work is cited here to support the statement. These are great as hypotheses though, and should be described as such.	Thank you for this comment. We have removed "from experience" and changed the verbiage to represent these hypotheses as such.		N/A
Additional Considerations	**1**	p. 69, last para	As described in the questionnaire above, it would be great if instead of "experience, judgement, and trial and error," systematic and objective time series analysis techniques could be employed here.	Thank you for this comment. The Markov-Chain velocity transition work is still young, with only conference papers published on the topic (referenced in the manuscript). Experience and judgement are currently critical in the workflow to evaluate the liklihood of transitions, though this work is evolving. We'd strongly prefer to leave this language as such.		AE: Response accepted.
General Overview	**2**	N/A	This is a highly ambitious and compelling project that seeks to document landslide behavior across vast areas in Western Washington in order to decipher the impact of forest practices on landslide activity. Rather than rely on landslide maps and inventories, the project describes an extensive program to systematically quantify landslide velocity to assess the multitude of factors that affect their movement. In my understanding, something of this scale and scope has not been attempted, much less accomplished, and it was quite inspiring to see the breadth of tools that would be marshalled in order to tackle this effort. From an academic perspective, this project would require a multi-year and multi-PI team and a very large budget with numerous graduate students and postdocs to accomplish the tasks described. While this is an excellent research plan, at the most fundamental level I'm not aware of much evidence that landslides tend to exhibit detectable velocities in the run-up to changes in movement. Is precursor movement typical in most deep-seated landslides? From my experience with deep-seated landslides in the PNW, many appear to be largely dormant prior to failure. That said, until recently the scientific community has lacked the ability to measure the velocity of deep slides across broad areas. Nonetheless, the implicit assumption of this study is that velocity is a key observable and will be useful for assessing the sensitivity to disturbances. Can the study proponents cite previous studies or evidence that landslide velocity serves as a useful metric for susceptibility to changes in behavior? In the Northern California coastal ranges, for example, hundreds of slow-moving DSLs tick along for decades at 0.5 to 1 m/yr and exhibit strong seasonal fluctuations that do not vary with land use activities like grazing and timber harvest (Handwerger et al., 2013, EPSL; Mackey et al., 2011, GSA Bull; Xu et al., 2022). In order to test this precursor velocity concept, can the study proponents use known events to make their case? What does InSAR show for the 2014 Oso landslide, for example? That event followed episodes of activity associated with river migration along the toe (D. Miller), but does InSAR reveal precursor activity in the source/upslope area that eventually failed catastrophically? My sense is that there have been a sufficient number of active deep landslides in recent years to perform a proof-of-concept analysis showing what may be possible. This would help test the notion that landslides tend to be active if they have a chance of posing problems to land use activities. A related conceptual assumption of this project is that the study will be able to isolate the role of land use by normalizing for other controls on landslide dynamics. In essence, can the proponents associate or perhaps partition deformation time series into causal mechanisms? The factors that control landslide behavior are highly varied and nonlinear, which makes this endeavor a challenging one...in other words, sorting out controls on slide behavior is tough even in the absence of land use impacts. This is not to discourage the attempt, I am strongly in favor of this work being done, but it should be done with clear eyes in terms of	There are a number of these discussed in this section. The following points serve to address these: 1. Similar Studies: Please see Cignetti et al., 2023 (DOI 10.1007/s10346-023-02114-7), which is perhaps the most directly relevant recent study. Additional information on similar works are provided in Line 28-29. 2. Pre-Collapse DSL Behavior: Some references that describe precursory deformation prior to collapse include: 1) Lato et al. 2019 (DOI: 10.1061/(ASCE)GT.1943-5606.0002073 who used lidar change detection to identify precursory deformation in the vicinity of the Oso headscarp prior to the 2014 collapse. 2) Morris et al, 2023 (https://doi.org/10.5194/esurf-11-1251-2023) use InSAR and optical image pixel tracking to identify movement starting in 2015 at the release zone of the 2022 Chaos Canyon Collapse in Colorado. 3) Van Wyk de Vries et all 2021 (https://doi.org/10.5194/nhess-22-3309-2022) used InSAR and optical image correlation to identify pre-collapse movement in the 5 years prior to the ultimate slope collapse in 2021 in Uttarakhand, India. 4) Lacroix et al 2020 identify precursory movement via InSAR over 3 years prior to a collapse of the Maoxian Landslide, China. There are many more references to document this pre-collapse behavior, though we do recognize the space is still relatively data sparse. 3. Land Use: The reviewer is correct here! This is a hard problem. The ability to partition the data to understand how discrete land changes impact on landslides will be dependent on the ability of the proposed approach and techniques to be able to gather temporal data of sufficient resolution to reflect pre- and post-event trends. More information will need to be gathered during data analysis to attempt to link land use changes with measured velocity profiles. - The Cignetti et al. (2023) framework has been added through Secton 5.2.4. - The references regarding precursory deformation identification have been added in Section 5.2.1. - A new section on land use provides some additional considerations when leveraging land use data (Section 5.4.2.8)		AE: I think the authors have done a commendable job of addressing the notable uncertainties associated with change detection, precursory landslide behavior, and climatic forcing vs. land use change. All research efforts have uncertainties and risks - that does not mean that measurable success is not possible, however. Even if decadal-scale changes may not serve as a strong statistical indicator using the proposed tools, it is possible that other signals may provide a modest precursory signal for acceleration that would be discovered using the proposed study plan or modifications thereof. With that said, there are some risks and unknowns in this proposed plan; nonetheless, the authors more clearly state these in the revised document and provide more citeable exmaples and details that provide a bit more confidence for success in one form or another. Response accepted.
Q1. Are rigorous, transparent, and sound research and statistical methods proposed?	**2**	N/A	The methods are indeed rigorous in that the project seeks to undertake lidar change detection and satellite interferometry combined with a wide array of climate, topography, surface change, and other datasets to explain landslide dynamics. The project plan is focused on the approach for mapping velocity changes and the available lidar and SAR images are well documented and described. While several lidar change detection tools are described, the study authors appear to conclude that lidar change detection is limited in that translational movement without vertical change will be difficult to detect. As it turns out, I think there are techniques with lidar that can uniquely define horizontal deformation. Specifically, recent work by Booth et al. (2020, Landslides) shows that sequential lidar datasets can be used to do "pixel-tracking" style quantification of landslide behavior and their work reveals different kinematic zones that move up to 2 m/yr. In that sense, this project plan could better account for some tools that could be well-suited for extracting landslide velocity from differential lidar datasets. Furthermore, the extensive treatment of InSAR techniques in the project description could be more convincingly supported with studies that show detectable slide activity in heavily forested areas. The L-band capabilities that are currently best-suited for forests in the PNW are limited to ALOS and few studies have in fact made a compelling case that landslides can be accurately mapped in this region.	Thank you for this comment. Pixel tracking for lidar change detection, as shown by Booth et al. (2020) is indeed a useful technology. However, it is very difficult to do at scale and we are unaware of any successful scaled (several thousands km2) pixel tracking efforts, which may make it prohibitively difficult to carry out for the present work. We recognize that with our LCD approach, there could be some examples where our methods do not identify ground surface parallel movement. However, as in the Booth et al. work, where surface parallel movement is occuring, it is expected that change vectors at the head, toe, and lateral scarps of a landslide will be greater in magnitude. We don't expect this to have a substantially negative influence on the number of velocity timeseries we can extract from the lidar comparisons, though it is incredible important to remember this limitation during analysis. Please see line #29 of this review matrix for our response to L-band capabilities in the PNW.		AE: Response accepted.
Q2. Is there sufficient detail in the document to conduct the study as written?	**2**	N/A	My sense is that this project description is a conceptual basis for a range of specific analyses. It does not specify statistical tools or methods to clarify some of the key questions or uniquely determine controls on landslide activity. That's not to suggest that it wouldn't be possible to sort out the appropriate pathway, but this document does not constitute a manual for the completion of the tasks as my sense is that each step could be accomplished with a wide range of tools, some of which would be well-suited to the work and others not so much. As a result, much more detail, as would be provided by a pilot project, would need to be specified to clarify analysis pathways that could work. In each of the sections much more specificity would be required to perform the work. For example, in assessing the role of topographic position (section 3.1.4), study notes that landscapes are fractal and the relevant scale for contextualizing landslides will need to be determined. From my experience, the tools and parameter choices are non-trivial and would necessitate substantial investigation. Similarly, other sections put forth reasonable ideas for analysis of soil moisture, geology, and climate data, but much more description of the actual work is needed in order to assess the likelihood of success.	Thank you for your comment. You are spot on with this assessment. We discussed this with the project team early on and determined it would be entirely too burdensome (for the authors, or the readers!) to provide a truly step by step manual. The current study design is indeed intended to be a guidance document to steer technically experienced and capable individuals to accomplish the study objectives. We've tried to emphasize that this study should begin with a pilot study (see Section 5.1). Much of this will indeed require substantial investigation. Considerations and case studies for integration of various physical landslide characteristics (e.g., topographic position index) are now included in Section 5.4.2.		AE: I appreciate the emphasis on a pilot study and it is acknowledged that exploratory work must be done to refine these methods and provide nuance as to what tools work well as well as where and why they work well (or don't). Response accepted.

Q3. Is there sufficient detail to support consistent, reasonable, and reproducible data interpretations?	**2**	N/A	As written above, the project description provides ideas for analyzing data but doesn't specify the necessary steps. For example, the study puts much weight in the notion that landslide velocity can be used to develop sensitivity classes. As change detection studies show, however, most active landslides exhibit highly nonuniform velocity fields. As such, a significant task is to determine how different kinematic zones of the slides respond to different forcings. While the toe may be affected by channel migration, the mid or upper section may be moving to a different rhythm, perhaps due to soil moisture changes or unidentified processes. As such, how will the study account for spatial variability in landslide velocity fields? Again, this is potentially tractable, but requires substantial work. One notable omission in the study design that could be clarified is how the land cover information will be used to determine whether and how landslides respond to forest practices. Presumably there are forest stand age coverages (in addition to the datasets cited in the project report) that could inform this aspect of the work, but how well do they account for the actual impacts, such as road construction that affects surface hydrology in the source regions? Or changes in the water balance induced by tree removal and decrease in evapotranspiration? More generally, if the primary goal of this project is to determine how forest practices may influence landslide activity, the project design would benefit from a more substantial treatment of the actual impacts of those practices.	As to the velocity field interpretation, we agree this will be challenging. One potential solution is to employ the methods of Cignetti, et al 2023 (DOI 10.1007/s10346-023-02114-7), which was published in the interim time between the initial submittal of this project and the present review period and has been added as a reference to this report. Interpreting how landslides respond to different external forcings at a regional scale is very challenging. The approach where landslides are segmented and classified based on similar geological and topographic controls will be a key first step in this process. The results obtained from lidar change detection are expected to support identification of the most active landslides (over the analysis time period). However, there will be a significant proportion of the overall landslide population that has undergone displacements that are below the lidar differencing detection limits. This is where the L-Band InSAR data, with a much lower limit of detection (~20 mm/yr compared to ~15 cm/yr for InSAR and high quality LCD, respectively), may be able to identify slow moving landslides. Given the current state of the science, we expect this LCD and InSAR combination to best support the grouping of landslides that have 1) been exposed to similar external factors and 2) appear to have responded in a similar manner. The ability to assess sensitivity of these different landslide groupings will determine how far the next stage of the study can progress in terms of linking displacement trends with external inputs/forcings. Significant judgement will be required within the landslide groupings to understand the kinematics and how to properly attribute the displacement data. With respect to utilizing land use information, we must be careful to not attempt to provide a deterministic evaluation of why a landslide accelerated. It is unreasonable to think we can do this from predominantly remotely sensed data and at the scale of many thousands of landslides. We bring this point up because the reviewer's comment appears to suggest we estimate road construction impacts, hydrological impacts, changes in ET, etc. Instead, we propose for a first step, we attempt to simply link land use or the occurrence of timber harvesting activities to landslide velocity (acceleration, no change, or deceleration). Perhaps in future work, the level of detail suggested by the reviewer could be investigated, but the current stage is likely premature for this.	To ISP Reviewers: There are two literature reviews, Miller 2017 and 2018 previously completed by UPSAG, which extensively discuss the hypothetical impacts of forest practices to DSLs; these have guided the larger Strategy of our research path, and have been referenced in this study design. We acknowledge that this project will not answer all of our questions about the potential for forest practices impacts - other projects in the Strategy, guided by the results of this project, will focus more closely on processes and impacts.	AE: I appreciate the authors' response and the context from the project team helps clarify the scope of these objectives. However, I do caution that there may potentially be challenges and significant effort in trying to isolate the relative importance of land use change versus the variability in climatic forcing, which is something the team appears to consider and recognize as difficult. Response accepted.
Q3a. Are the proposed uses of different data for classification and statistical analyses, including InSAR and lidar change detection, reasonable given the data limitations?	**2**	N/A	While the emergence of NiSAR will be a major boon for landslide work in the PNW and elsewhere, the current coverage has limitations and the study plan could more clearly demonstrate what's currently possible. As discussed above, the use of InSAR is essentially untested as a tool for comprehensive classification schemes in heavily forested terrain. If some landslides can be detected with this approach will they be representative? I could imagine that slides that exhibit chronic activity might respond to forest practices in a very different way than slides that do not exhibit chronic activity. In that sense, I am curious if the emphasis on velocity could result in a systematic bias in the resulting classification-sensitivity outcomes. In terms of InSAR applicability, though, how well does L-band data work in the dense, closed canopy forests that characterize this region? The 24-cm wavelength of L-band SAR is similar to the diameter of large tree branches, which could be problematic for deriving coherent returns. In addition, the geometry of SAR acquisitions relative to the landscape limits our ability to detect movement. The authors acknowledge this potential limitation but can they use topography and show where slides will be undetectable owing to the censoring of orthogonal look angle-deformation orientation on landslides? The scale and orientation of major ridges could prevent many slides from being detectable. I'm somewhat more compelled by the use of sequential lidar to track both vertical and horizontal changes although reliance on this tool precludes the ability to confidently detect seasonal signals without a massive change in the frequency of lidar acquisition. The ability to use WGS landslide inventories to focus the analyses is a major advantage in that landslides in these areas have been comprehensively mapped with high accuracy. Other approaches are worth exploring, as well. The Lahusen et al. (2020, Science Advances) approach of using roughness as a first order estimate of landslide activity is perhaps not a bad place to start this analysis given that trends in roughness can serve as a decent first-order indicator of relative surficial age.	The authors acknowledge that published examples of using L-band data for landslide detection and characterization specifically in the PNW is limited. However, numerous published research articles demonstrate the use of L-band data for landslide mapping and characterization in densely vegetated terrain including, for example, by Yang et al. 2023, and Dai et al. 2022. Most recently L-band InSAR data aided in the identification of the large St. Cyr landslide in British Columbia as published by BC Hydro (see Figure 4: https://www.bchydro.com/content/dam/BCHydro/customer-portal/documents/corporate/safety/Dam-Safety-Report-F22-Q2.pdf). However, while the potential to map landslide activity beneath dense vegetation canopies is possible, several disadvantages of using L-band data for this purpose has also been documented. While a reduction in measurement point densities are expected due to interaction with large tree branches, the main limitation of using L-band data for the purpose of detecting landslide movement and characterizing landslides based on their velocities, is due to the reduced sensitivity of L-band data to smaller-scale movements (also documented in Dai et al. 2022 and mentioned in Section 5.2.3). A very large number of images are needed in L-band stacks to achieve a high level of precision and has a reduced sensitivity to small-scale movements when compared to X-band or C-band wavelengths for example. Therefore, very slow creeping landslides (< 15 mm/year for example) may be hard to detect with a high level of precision. In that sense, L-band will likely not provide a fully 'comprehensive' classification scheme unless it is comprised of a very long timeseries and a very large number of images in the stack (>30) and the reviewer is correct in stating that the emphasis on velocity could result in a systematic bias (i.e. toward landslides moving faster than approx. 15 mm/yr). The ideal scenario to overcome this potential bias is for multi-frequency analysis (using L-band in addition to say C-band, similar to the approach by Dai et al. 2022) although C-band will have reduced returns (potentially no returns) in the presence of dense vegetation. Perhaps the best approach then is to aim towards a first pass estimate of landslide activity based on the proposed analysis that can then be used to further refine the inventories. The second limitation affecting SAR data, irrespective of wavelength, is geometric distortions due to the side-looking geometry of SAR data as alluded to by the reviewer. We have added some text explaining this phenomenon and implications in Section 5.2.3. In steep terrain, geometric distortions can make InSAR measurements infeasible, depending on the incidence angle of the sensor and the geometry of the slope (aspect angle and slope gradient). Usually, combining data acquisitions from both ascending and descending orbits ensures that most slopes can be observed in at least one geometry. However, in some cases, geometric distortions will prevent the extraction of InSAR measurements, especially if only a single orbital direction is used. With knowledge of the InSAR sensor geometry and using terrain information (derived from an external elevation model), areas that will be affected by geometric distortions can be identified and strategies identified to maximise coverage (by using different incidence angle ranges or orbital directions for example). However, for regional assessments like we propose here, both geometric distortions and the impact of radar orthogonal slope movements will surely be a problem in some locations. Ultimately, the publication of InSAR results and the accompanying inventories will need to be supplemented by datasets describing and documenting areas that could not be fully assessed due to these limitations. The reviewer asks if we "can use topography and show where slides will be undetectable owing to the censoring of orthogonal look angle-deformation orientation on landslides?" The answer is yes and that is what was recommended in Section 5.2.3.2. The authors have now also included a bullet stating that the areas affected by geometric distortions can also be extracted and provided as supplementary datasets to identify areas that may have been subject to biases due to the geometric affects.		AE: Response accepted, especially as these roughness techniques are valuable, but possibly less relevant to the timescales of interest in this project. Nonetheless, such an approach could a valuable datapoint used in the interpretation of landslide activity (or lack thereof).
Q3b. Is it clear how data will be evaluated for inclusion in the proposed analyses?	**2**	N/A	Some portions of the proposal reveal well-laid out descriptions of how data will be analyzed and evaluated while others are less specific. In particular, the InSAR and Lidar datasets are well-described in terms of availability. By contrast, how the project will integrate many of the other datasets into the analyses could benefit from a much more detailed description of the methods.	Thank you for this comment. This is mostly because we are not sure yet how best to integrate. There is a necessary research component to this project, as I am sure you are aware, and the incorporation of these descriptors is in that bucket. In the Intent of this Document section, we attempt to lay out this limitation: "...detailed statistical analysis procedures following the development of a landslide inventory, activity, and velocity time-series database are difficult to prescribe at this time. Following development of the database, evaluation will require a significant exploratory component, as with most statistical evaluations. " We've significantly bolstered Section 5.4.2 to address this comment.		AE: Response accepted.
Q4. Is the methodology/workflow clear, reproducible, and capable of achieving the project goals?	**2**	N/A	My sense is that this document serves more to lay out the principles and concepts rather than serve as a workflow. From my reading, it wasn't clear how an investigator would tackle this work without extensive sensitivity analyses, algorithm testing and selection, parameter sensitivity tests, and more. In that sense, I could imagine a wide array of approaches being taken in order to accomplish the work. For example, how will the rainfall be processed? Will it only consider antecedent rainfall? Do storms matter? How will storms be parsed from rainfall data? And at what scale will the rainfall and reanalysis datasets be processed? Most generally and perhaps most importantly, what statistical models and approaches will be used to pull together the various datasets and determine controls on slide behavior? There has been an infusion of new tools, including machine learning, to decipher patterns with multi-variate datasets. More traditional tools, like multiple regression, are very useful as well. Although section 8.0 states that the statistical analysis will be addressed in a future project, from my experience it can be problematic to generate a database without a plan on how it will be used.	Thank you for your comment. You are spot on with this assessment. We discussed this with the project team early on and determined it would entirely too burdensome (for the authors, and the readers!) to provide a truly step by step manual. The current study design is indeed intended to be a guidance document to steer technically experienced and capable individuals to accomplish the study objectives. We completely agree that database design can make or break future modeling efforts. We prescribe the study executors to make every attempt to build a flexible database suitable for modern modeling frameworks (Section 4.2.3). Further detail should be provided in a proposal to perform a pilot/regional study. We have provided a major rework of Section 5.4 (Assessment of Landslide Sensitivity) that will hopefully address this comment.		AE; Response accepted.
Q5. Do the literature citations include the latest applicable information and represent the current state of scientific understanding on this topic?	**2**	N/A	As noted above, the project could better capture the relevant literature and tools available to assess landslide behavior. In addition to the work by Adam Booth and Alex]Handwerger, the proponents might dig into work by Pascal Lacroix, P. Malet, and others working on geodesy for landslides as they've pushed the envelope in terms of extracting mechanical controls on sliding from time series. For example, work in Northern California by Handwerger shows that slow-moving slides of vastly different scale (and depth) show a very similar response timescale to seasonal rainfall, which contrasts with the study assumption that response times vary systematically with landslide geometry (section 5.4). Most generally, the use of various datasets, including climate information, land cover, geology, and topography to correlate with landslide behavior is a burgeoning scientific endeavor and this project design could benefit from a more comprehensive survey of the literature in order to inform the data analysis.	Thank you for this comment. We have investigated the literature more, including the works you've cited here and added several references to the document that have also been discussed above. The foundational aspect of the project will be to first build a reliable displacement time series and map to landslide types and kinematic models. Based on this, we will be exploring work by the authors that you've noted, among others that have been referened in prior responses on this document.		AE: Response accepted, although I'd encourage the authors to further explore relationships between landslide geometry/depth/recharge proxies and seasonal (and more importanrtly, sub-seasonal) precipitation forcing should this move forward. This seems apparent from section 5.4.
Q6. Are uncertainties and limitations of the proposed work stated and described adequately?	**2**	N/A	These questions are discussed above in various contexts.	No comment.		N/A
Q1. Are rigorous, transparent, and sound research and statistical methods proposed?	**2**	N/A	The research plan is well detailed, providing a comprehensive overview. While generally well-explained, certain sections of the report may be slightly ambiguous. For instance, in Section 9, the term "potential targets" lacks clarity regarding whether it pertains to study areas, specific localities, or individual landslides. A technical review and editing process would significantly enhance the report's transparency and overall coherence. I believe the landslide classification proposed by Cruden and Varne (1996) is outdated. "Hungr et al. (2013) [The Varnes classification of landslide types, an update] presents a significant improvement over the 1996 system. This system incorporates explicitly an evaluation of the landslide process, which is crucial for assessing the relative risk associated with deep-seated slides. Moreover, it consists of a much more Geo technically robust material classification system. I strongly recommend that the contractor consider adopting the Hungr system over Varnes's The research plan exhibits a robust foundation. It adeptly identifies various candidate statistical methodologies (e.g., principal component analysis) that are well suited for the research.	Thank you for this comment. We would love to use the Hungr update, however, we are not convinced the added detail could be sufficiently gleaned for this population of thousands of landslides in a meaningful way. Varnes 1996 does indeed provide both a material type and a landslide process, though as you state, with less specificity. We've not made any changes to the manuscript as a result of this comment, but we will consider this further if the study moves to implementation. CRF Comment: In addition, Hungr et al was develped to build in more resolution as to more rapid flows that were "lumped" in Cruden and Varnes. For this point Cruden and Varnes is still the primary classfication utilized in the international landslide community and the most appropriate for classifying DSL's.		AE:This is likely fine as these classification schema are somewhat arbitrary anyways and the quantitative data produced from this study, if successful, would be a more valuable means of categorization. Response accepted.
Q1a. Is there enough detail in the Study Design to understand and implement the statistical methods and workflow needed to conduct this study?	**3**	N/A	The report offers sufficient detail to understand the planned workflow. It's important to note that while the report discusses several candidate approaches—like principal component analysis, independent component analysis, and cluster analysis—a specific statistical method has not yet been determined.	Thank you for your comment. You are correct, the early results will greatly influence the downstream decisions, specifically around statistical modeling methods. We just won't know how large of a dataset we have until we start interrogating the data (we mention this in Section 7 - Technical Limitations). No changes made as a result of this comment.		AE: Response accepted.
Q2. Is there sufficient detail in the document to conduct the study as written?	**3**	N/A	The report is sufficiently detailed to undertake a more comprehensive study.	Thank you for this comment!		N/A
Q3. Is there sufficient detail to support consistent, reasonable, and reproducible data interpretations?	**3**	N/A	The research plan is well described and reproducible.	Thank you for this comment!		N/A

Q3a. Are the proposed uses of different data for classification and statistical analyses, including InSAR and lidar change detection, reasonable given the data limitations?	**3**	N/A	The report thoroughly documents the research approach and data collection strategy. A study of this magnitude clearly demands substantial resources, both in technical expertise and computing power. A more comprehensive literature review demonstrating the effectiveness and viability of the chosen research approach before initiating such a study would be ideal. Specifically, including or pointing to results from previous research studies conducted by others (in different locations) would have been beneficial. This would instill greater confidence in achieving the objectives through the chosen approach. Some of my thinking (and slight reservation) stems from the work of Meretto and colleagues, among others. While they were able to use SAR to effectively delineate areas of slope instability (i.e., movement) at several more localized study locations, they also highlighted significant challenges using SAR due to (1) data temporal frequency and (2) phase ambiguity associated with large displacements at some locations. Are the contractors aware of other successful regional-scale efforts to identify reliable time series velocity trends in individual landslides? If yes, what is the spatial resolution? In other words, what size landslides can be identified using SAR time series? Providing practical feasibility demonstrations or offering examples from others would increase my confidence in the worthiness of this large-scale effort. Reference: Moretto,S.;Bozzano,F.; Mazzanti, P. The Role of Satellite InSAR for Landslide Forecasting: Limitations and Openings. Remote Sens.2021,13,3735. https://doi.org/ 10.3390/rs13183735 The report extensively discusses the work of Xu et al. (2021). This study focused on slow-moving landslides with phase coherence. The accompanying USGS inventory of slides is notably sparse and seems to significantly underrepresent slides in the western part of the state. The scarcity of slides, coupled with their limited occurrence in very slow-moving events, implies that the proposed approach may present challenges. Once again, additional citations of previous studies demonstrating the proof of concept and effectiveness of this procedure would bolster my confidence in the feasibility of achieving the study objectives.	Thank you for this comment. We have reviewed the Moretto et al reference. We understand that large displacements cause incoherence between SAR scenes and InSAR is not useful for identifying rapid landslide occurrences. Regarding temporal frequency, it is our understanding that Moretto et al.'s primary concern with revisit times was regarding the ability of InSAR to estimate time of failure (ToF). We do not recommend trying to estimate ToF in the present study because as is consistent with Moretto et al., this is an extremely challenging problem and would likely be beyond the state of the science for a population of thousands of landslides. A new reference (Cignetti et al, 2023) was published since the original submittal of this study design and is now included. Cignetti et al. describe a use case of InSAR for activity state classifications of a population of 279 deep-seated landslides. They do this via Sentinel-1, which is rather surprising given the C-band wavelength and the vegetation cover in Italy, so it is encouraging. We are still recommending primary reliance on L-band in Washington. The newly added reference is: Cignetti, M., Godone, D., Notti, D., Giordan, D., Bertolo, D., Calò, F., et al. (2023). State of activity classification of deep-seated gravitational slope deformation at regional scale based on Sentinel-1 data. Landslides. https://doi.org/10.1007/s10346-023-02114-7 Can the reviewer clarify their comments around the Xu et al (2021) study? Xu et al. compared their results to the existing multi-agency and multi-sourced landslide inventory of Jones et al., 2019, which included Washington Geological Survey inventories. In Western Washington, Xu et al. identified similar densities of landsliding, and address those they may have missed: "Comparison with the US Geological Survey (USGS) landslide inventory (Fig. 1) shows that our InSAR-based mapping captured much fewer active landslides in northwestern Oregon and the Sierra Nevada in California. One most likely reason is that the USGS inventory comprises many small and/or catastrophic landslides which are challenging to be detected by InSAR. Another potential reason is that some landslides were no longer active during our InSAR observation periods from 2007 to 2011 and from 2015 to 2019." (page 8 of the manuscript). Is it possible these explanations allay the reviewer's comments? - The Cignetti et al. (2023) framework has been added through Secton 5.2.4. - Extra consideration and context around the Xu et al. (2021) results have been added to Section 3.1.2.4		AE: Response accepted. The focus of this work is not on landslides that fail catstrophically (typically shallow landslides) and further, the authors' acknowledge the challenges with landslide inventories in this comment and the study plan - these inventories are subjective, mapped using different data and criteria and mapping professionals. Consequently, I don't see this as a major obstacle compared to some of the larger uncertainties posed by the authors and reviewers.
Q3b. Is it clear how data will be evaluated for inclusion in the proposed analyses?	**3**	N/A	I found it challenging to determine the relative number of landslides in the study area. Table 3–1 provides a summary of available landslide inventories for the study region, but it lacks information on individually identified landslides. It would be beneficial to reproduce this table, taking into account the number of landslides in each inventory. I understand that the Land use/land cover datasets of Google Dynamic World are of excellent temporal resolution— but relatively poor spatial resolution. What is the spatial resolution, and is it too coarse to meet the study objectives? The study design heavily relies on LiDAR data collected from various regions across Washington. The quality of this airborne LiDAR data varies significantly from region to region. Moreover, the ground filtering quality is poor in many locations. How does the contractor plan to address the disparities in LiDAR data quality across the study region? Often, quality differences can be quite significant when differencing lidar.	We believe this would be quite misleading as these inventories, in entirety, would not be used for the proposed study. Instead, in Table 5-1, we list the number of mapped deep-seated landslides in Proposed Study Areas 1 (>3,000 mapped DSLs) and 2 (>4,350 mapped DSLs). Google's Dynamic World is based on Sentinel-2 imagery and has a corresponding resolution of 10-meters. We do not believe this is too coarse for the proposed study. Since the initial study design submittal, BGC Engineering has completed a lidar change detection project for Washington DNR and included evaluating data from 2006-2017. The older 2006 data was indeed deemed suitable for high-quality lidar change detection. This project has now been referenced in the updated manuscript.		AE; Response accepted.
Q4. Is the methodology/workflow clear, reproducible, and capable of achieving the project goals?	**3**	N/A	The research methodology is clearly articulated and designed for reproducibility. I am generally confident in the project plan's potential for achieving its objectives. Nevertheless, as previously mentioned, I recommend a more robust proof of concept to substantiate the efficacy of the research methods prior to initiating a large-scale effort.	Thank you for this comment. We suggest the next step of the project is a proof of concept as well, as detailed in Section 5.1 Selection of Area for Proof-of-Concept Execution.		AE: Response accepted.
Q5. Do the literature citations include the latest applicable information and represent the current state of scientific understanding on this topic?	**3**	N/A	As mentioned previously, I suggest including more literature citations that validate the utility of utilizing remotely sensed data in producing time series deformation data specific to landslides. Section 2.1.1.1 is most likely distilled from a series of references, which should be cited.	Thank you. We have updated references accordingly and utlize these references to support discussion in above responses.		N/A
Q6. Are uncertainties and limitations of the proposed work stated and described adequately?	**3**	N/A	The uncertainties and limitations are well described.	Thank you for this comment!		N/A
Q7. Are assumptions stated and described adequately?	**3**	N/A	The assumptions are adequately described.	Thank you for this comment!		N/A
Q8. Is the information presented in an accurate, clear, complete, and unbiased manner and in a proper context?	**3**	N/A	The document would benefit from professional editing to enhance the technical writing, which was occasionally challenging to comprehend. My review primarily addressed the technical aspects of the work, rather than delving into the specific details of the writing. Consequently, I won't provide a section-by-section breakdown for editing. However, as an illustrative instance, I'd like to mention a sentence on page xiii that was particularly challenging to comprehend: "There is an inherent level of nuance embedded in the study design that stems from the history and experience of the report authors in similar studies and from conversations and feedback from DNR and its affiliates on early versions of the design document." The report includes many acronyms; therefore, I appreciate the section defining these. Nevertheless, I was baffled by Figure 4 – one to understand what SME refers to. This should be clarified. The results of this work hold significant implications for public safety in Washington. Therefore, in section 8, I would prefer to see a plan for openly disseminating findings and the database. As per the solicitation's requirements, this work was explicitly focused on deep-seated landslides. However, it's important to note that many forested regions in Washington state are prone to producing shallow yet dangerous landslides, such as debris flows originating from colluvial hollows. In future work, it would be valuable to contemplate broadening the scope of landslide hazards to encompass shallow events as well.	Thank you for these comments. We will review the manuscript with an eye for readability. SME has been added to the acronym list, thank you for catching this one. The work will be performed for DNR, who has a rich history in making landslide related information available to the public. It is unclear to us if that level of data disemmination should be included in the technical study design, but we will discuss with DNR. We agree on shallow landslide hazards.	To ISP Reviewers: All CMER research is public domain, through the Washington Department of Natural Resources. Study designs do not routinely have a dissemination plan, and we don't think one is appropriate or necessary. The final approved study design will be posted on the document section of the DNR Adaptive Management Program Website, which is publically accessible. And UPSAG/CMER has a separate research track focused on shallow landslide hazard, the Unstable Slopes Criteria Project.	AE: Response accepted.