Reviewer Comments

Reviewer B

For author and editor

This is a very interesting look at how coseismic shaking and wave propagation can directly influence the occurrence and distribution of landslides, with application to the specific case of the 2005 M7.6 Kashmir earthquake. I think this combination of earthquake and landslide physics is an exciting way to interpret what happened during/after this event. I also think that interdisciplinary approaches like this – and especially with the comparison of physics-based models to observational data not included in the models – are the way forward for hazard assessment in general, especially when physical processes are as interrelated as seismic shaking and landslide occurrence.

I'm coming at this review from the perspective of an earthquake source modeler. I fully admit that I do not have much background in landslide science! Most of my comments are related to clarification of some of the more landslide-specific aspects of the paper, and for why you made some of the specific choices that you did about model setup and parameters; I don't think there's anything that needs to be changed or redone about the simulations/comparisons themselves for this to be ready for publication. The manuscript itself cleanly-written and well-organized, with clear and readable figures, so I have very few comments regarding that.

I've broken my comments down into more broad/general things and line-specific things below.

GENERAL COMMENTS

Please find a different way to name your source models other than S1 and S2. Considering you have a lot of information in the supplement, and that you use S1, S2, etc. to number the different supplementary sections as well, it can get confusing exactly what you're referring to (particularly in cases like lines 319-320, where you're talking about source models and supplement sections in the same sentence). I'd consider coming up with some more descriptive names/abbreviations, so your readers don't have to keep flipping back to see which model was which.

You are likely to have a mixture of landslide people and earthquake people reading this paper (especially since you submitted it to an earthquake-focused journal). With this in mind, it would be helpful to add a little more detail/explanation of why certain parameters are important for landslide hazard modeling, and what specifically goes into those considerations.

For example: I am an earthquake researcher, and I was unclear on what was significant (and what was included) in your closeness-to-river term. I initially assumed this was related to the water in the river, but as I kept reading, I was became less and less sure what went into this. Does it have to do with the slope leading down into the river channel? Or the unconsolidated sediments deposited by the river? Please clarify!

I was also unclear on what went into the closeness-to-faults term. Does this separate out closeness-to-source-fault from closeness-to-other-faults? It seems to me that those are two different issues, though I may have been reading this wrong. In terms of closeness-to-other-faults in particular, is this largely related to how faults damage surrounding rock? I initially assumed this would also consider wave reflections off fault planes, but I became unsure after you stated that you don't model faults as discontinuities in your mesh.

Related to the above: can you please explain a little bit in the text why you chose not to treat your faults as actual discontinuities within your mesh? Most dynamic earthquake source simulations do discretize faults as discontinuities/interfaces. This shouldn't matter so much for initial seismic radiation away from the fault, but having no discontinuity there could affect how waves propagate in the intermediate/far field. One of the major hanging-wall effects is waves getting trapped between the free surface and the source fault below. Additionally, faults other than the source fault can reflect and focus waves (as was the case with the damage in Santa Monica during the 1994 Northridge earthquake); it seems to me like this could be a consideration for how seismic energy interacts with slopes. I don't think you necessarily need to change anything in the model setup to include this factor, but I think the paper would be stronger if you wrote in why you chose not to involve it.

I really liked how you compared your model results against so many other observational factors/variables that weren't part of the models themselves! This did leave me wondering why you didn't include a term/factor/comparison for recent rainfall/soil saturation, though. Isn't the rainfall history around the Kashmir earthquake region something you could look up? (And would it be possible to have dry/moderate/wet terms in more generalized models for landslide likelihood?) I know I may be totally off base with this comment, since again, I'm an earthquake person and not a landslide person. But given that moisture content (and weight from it) is something people are taught can affect landslides, I still think it might be worth adding a sentence or two about why you chose not to include this factor (especially if it's too difficult to actually incorporate here, or if it doesn't actually matter much for what you're calculating).

This is a very thorough look at this specific earthquake and its associated landslides. I think your discussion and conclusions could be even stronger, though, if you included a paragraph or so about how the method you used here may be applicable/useful for coseismic landslide hazard assessment or forecasting beyond the specific case of the 2005 Kashmir event.

LINE-BY-LINE COMMENTS

Lines 81-82: How/why does seismic wave amplification lead to landslides initiating higher on hillslopes?

Line: Cite some of the many studies here.

Line 86, and lines 116-118: Please clarify/specify whether you're modeling just wave propagation, or are also modeling the whole on-fault rupture process.

Line 141: Please consider changing this to "previous major earthquake," since the last one was the one in 2005!

Lines 145-146: This sentence seems out of place in a paragraph that is otherwise about rock type. I'd consider giving the uplift/incision/erosion discussion its own paragraph here – which can also include some of that elaboration on what the closeness-to-river term includes/considers.

Line 148 and elsewhere: Please spell out "formation" (at least the first time you use it, so you can introduce what "Fm" stands for).

Lines 182-183: Consider moving this sentence about communication loss earlier, when you're first introducing the impacts of the earthquake and its triggered landslides.

Lines 188-190: If you have pre- and post-event imagery, why is it difficult to determine which landslides were vs. were not caused by the earthquake? Or am I misreading this sentence?

Lines 205-207: Is the landsliding near the Kashmir earthquake still continuing in 2024? The paper you cite for this is from 2012; please clarify whether it is truly ongoing, or if it just lasted as long as the publication date for that reference.

Line 227: Finite fault models can simulate ground motions well above 1 Hz! (And you even outright say that you're going above this frequency in lines 237-238.)

Line 244: Consider putting a paragraph break before "Rise times typically increase…"

Lines 270-272: Please clarify what you mean by "naturally incorporates high-resolution topography." Do you mean that SEM makes it easy to include this?

Lines 280-281: What is your element shape?

Lines 280-281: "with elements that increase in size with depth" makes it sound like they keep getting bigger as it gets deeper into the mesh. I don't think this clause is necessary, considering you explain the element size thing in the next sentence.

Lines 308-310: Most ground motion simulations focused on seismic hazard in and of itself use PGA, not PGV. Please explain why PGV is better when assessing landslide hazard.

Lines 326-328: I'm a little confused here. Since the Kashmir earthquake was a surface-rupturing event, the top of the fault should reach the top of the mesh regardless of elevation, shouldn't it? Or do all the meshes have a fault plane that extends to the same depth, meaning the higher elevation ones have a larger fault? Or is this more of a topographic stressing thing?

Line 329: Why are you trying to decrease the effects of geometrical spreading? This is a real factor that affects seismic wave propagation.

Lines 331-332: This is an awkward sentence. Please consider rephrasing it to something like, "We conduct a 1D interpolation to the elevation of our topography…"

Lines 334-335: Why does the fault only come close to the free surface, as opposed to reaching it?

Line 348: Which seismic parameters?

Lines 349-350: What do you mean by "landslide polygons"? The outlines of slides?

Line 369: "Hypocenter," not "epicenter."

Lines 373-374: Do you just mean reaching the free surface? In general, "terminus of the fault" means the along-strike end, not up-dip.

Lines 399-402: I'd move this explanation of why you're using PGV up to earlier in the paper (like around lines 308-310).

Lines 422-424: Cite some of the previous studies here.

Lines 439-443: This phrasing makes it sound like something about the ridge topography itself is being amplified, and not the seismic waves interacting with this topography. Please rephrase a little.

Line 464: You’re missing another close parenthesis after the Sato et al. citation.

Lines 500-506: I’d consider moving this to earlier in the paper, when you’re explaining the reasoning for which parameters you consider in your landslide hazard assessments.

Lines 540-542: This sentence is a little unclear. Do you mean that topographic amplification is the dominant mechanism for landslide triggering on the hanging wall and not the footwall? Or that amplification really only happens on the hanging wall?

Line 626: You left out a “with” between “align” and “previous.”

Line 628: Consider using “which all” instead of “additionally” here.

Line 631: You’ve spent so much time talking about the physics of seismic wave focusing that using “focusing” in this sentence seems awkward. Consider using “located” instead.

Lines 730, and 733-734: Please change “is experiencing” to “experienced.”

Line 743: Please clarify what you mean by “horizontal and vertical ridges.” Do you mean fault parallel vs. fault-perpendicular? Or ridges that are longer than they are tall vs. taller than they are long? Or something else altogether?

Reviewer D

For author and editor

I've thoroughly reviewed the article on the 2005 Kashmir earthquake-triggered landslides, entitled " *The influence of ground shaking on the distribution and size of coseismic landslides from the Mw 7.6 2005 Kashmir earthquake*" finding it insightful in exploring factors like seismic activity and topography affecting landslide distribution. While the study contributes significantly to understanding landslide hazards and mitigation strategies, addressing some limitations would enhance its impact. Overall, it's a valuable resource for researchers studying landslides and seismic events.

Comments

1-The introduction underscores the significance of earthquake-triggered landslides as a secondary geotechnical hazard, highlighting the challenges in determining ground motion distribution in high mountain regions like the 2005 Kashmir earthquake. However, it could benefit from a clearer articulation of the study's objectives and contributions for enhanced impact.

2-The legend for Figure 1 inaccurately states that the Muzaffarabad Formation is of Pre-Cambrian age; however, it is actually of Cambrian age. For accurate geological mapping, please refer to sources such as Basharat et al. (2021) and Riaz et al. (2023).1. Basharat, M., Riaz, M. T., Jan, M. Q., Xu, C., & Riaz, S. (2021). A review of landslides related to the 2005 Kashmir Earthquake: implication and future challenges. Natural Hazards, 108, 1-30. and 2. Riaz, M. T., Basharat, M., & Brunetti, M. T. (2023).

Assessing the effectiveness of alternative landslide partitioning in machine learning methods for landslide prediction in the complex Himalayan terrain. Progress in Physical Geography: Earth and Environment, 47(3), 315-347.

3- Incorporating comparisons with previous studies on similar seismic events or numerical modeling approaches could provide additional context and validate the findings. Highlighting similarities or differences in results would contribute to the robustness of the analysis.
4- Acknowledging and discussing any uncertainties or limitations in the methodology or data interpretation would enhance the credibility of the findings. Addressing factors such as model assumptions or parameter uncertainties would provide a more nuanced understanding of the results.

5- Conclude the discussion by outlining potential avenues for future research to further investigate the mechanisms of landslide initiation and propagation during seismic events. Suggesting specific research questions or methodologies could guide future studies in this field.

6- Incorporating field photographs of co-seismic landslides, particularly the Hattian Bala rock avalanche, would significantly enhance the visual understanding of the landslide phenomena discussed in the paper. Including images of the affected areas, such as before-and-after shots or snapshots capturing the scale and impact of the landslides, would provide valuable context and illustrate the real-world implications of the study's findings.

Thank you to the reviewers for their helpful and insightful comments and the editor for their thoughtful and thorough summary of these reviews. I believe that the suggested revisions have significantly improved the manuscript. Below are my responses to the comments from both the reviewers and editors in blue, along with line numbers of where I have changed the text based on the comments. Line numbers refer to lines in the document without track changes. The most significant change to the manuscript is the addition of a discussion section (6.4) outlining potential future directions for using simulations in real-time co-seismic landslide hazard assessment to put the results of this study into broader context.

Dear Audrey Dunham, Eric Kiser, Jeffrey Kargel, Umesh Haritashya, C. Scott Watson, Daniel Shugar:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "The influence of ground shaking on the distribution and size of coseismic landslides from the Mw 7.6 2005 Kashmir earthquake". Thank you once again for submitting your work to Seismica.

Based on reviews I have received, your manuscript may be suitable for publication after some relatively minor revisions. Reviewer B has a detailed and constructive review, while Reviewer D is more general. However, the general gist of both reviews is that this is a very detailed case study that is suitable for publication in Seismica, but it could be a stronger paper if the context of the case study and broader implcations are better spelled out.

I find that the introduction nicely reviews controlling factors for coseismic landslide initiation, and also the 2005 Kashmir earthquake and limitations of past studies. It is clear to me what is new in this study, but, I can also see where Reviewer D is coming from in asking for clearer objectives. A basic research question or hypothesis is a bit hidden in the details of new data and new conclusions, and this is also a reason the study may come across as a very detailed case study that is somewhat hard to apply elsewhere (see request from Reviewer B for a paragraph on application to other events). So – I think it would help a broader readership if the introduction had some clearer, broader contextual questions/hypotheses, that can then be explicitly revisited in the discussion. This would also address the general comment from Reviewer D asking for comparison to other events.

To address these suggestions, I have modified the final paragraph of the introduction to include clearer objectives of the study, which are to use simulations to better understand the relationships between ground shaking, topographic amplification, and landslide initiation for the Kashmir earthquake, that can then be applied to future earthquakes and potentially rapid landslide hazard assessment. I have added a paragraph to the discussion for implications of these types of simulations to rapid landslide assessment and reflections of what we need in order to consider these types of simulations to evaluate hazard (section 6.4 in the discussion). I have also included an example of a landslide from the Wenchuan earthquake that could be a candidate to study for better understanding topographic amplification within a ridge kink.

Reviewer D would like to see some more discussion of uncertainties and limitations. I think generally the paper does this quite well, with detailed methodology and comparison of results for two source models (which I agree with Reviewer B could have better names). The discussion, however, does not really discuss limitations, so I can see how, in such a multidisciplinary work, readers may do with a few more reminders about possible limitations within the discussion. For example, the cartoon in Fig 15 is nice, and there are examples in the text of other landslides related to such amplification at a ridge, but one could potentially discuss (and discount or not) other alternative explanations.

I have added a sentence to remind readers that there was pre-existing deformation along the source ridge of the Hattian Bala landslide to make the limitation of this conclusion clear, that topographic amplification is not the only contribution to landslide initiation.

L740-743: ***While it is clear that this ridge was experiencing deformation prior to the earthquake that led to failure (Dunning et al., 2007), we can use these simulations to conclude if topographic amplification of the causative ridge occurred and therefore could have contributed to landslide initiation***

I also found that the discussion on landslides along the rupture trace relies on an assertion in the text that slope does not vary substantially – is there a way to show this graphically on Fig 13?

While this could be added to figure 13, I think this would make the figure too complicated/busy. In the text (L660), I describe the modal slopes for (I) and (II), which are within 1.5° of each other, showing clearly that these regions have similar slope distributions without including it in a figure.

Reviewer D suggests adding some field photographs – I do not know how easily that may be obtained, but I can see the value in visualizing some aspects, for example the Hattian Bala landslide adding some context to Figure 15. I can understand if this is outside scope though.

Because we did not conduct any field work for this study, I believe including field photos is outside the scope of the work. There are great photos of this landslide in many of the work cited in this manuscript that readers can refer to.

Overall, as the reviewers both point out, this is a detailed case study using a range of techniques, it is well written and the figures are great. I think Reviewer B has made some very constructive and detailed comments that should be quite easy to address and will make the paper both more accessible and the presentation clearer. Reviewer D has broader comments, I've tried to add some of my perspectives on those above, but also for these I think some minor modifications will make a big difference – personally I found the discussion and conclusions a bit disappointing after the detailed study; ok, landslides are affected by PGV, slopes, and lithology, locally amplified by topographic detail, but that is quite basic. I think the comments on clear objectives and discussion beyond the case study are quite important – I urge the authors to add a discussion section that steps beyond the specific earthquake, and makes some comments on what can be learned here for other earthquakes in mountainous terrain (and there is also scope then to include suggestions for further study).

Thank you for your detailed summary of the reviewers comments and your perspectives on the manuscript. I have addressed all of the reviewers comments and some of yours above as well. I have added an additional section on implications to landslide hazard assessment and suggestions of future research, both in terms of hazard assessment and in better understanding topographic amplification of these large landslides.

When you are ready to resubmit the revised version of your manuscript, please upload:

- A 'cleaned' version of the revised manuscript, without any markup/changes highlighted.
- A pdf version of the revised manuscript clearly highlighting changes/markup/edits.
- A 'response-to-reviewers' letter that shows your response to each of the reviewers' points, together with a summary of the resulting changes made to the manuscript.

Once I have read your revised manuscript and rebuttal, I will then decide whether the manuscript either needs to be sent to reviewers again, requires further minor changes, or can be accepted.

If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

Please note that Seismica does not have any strict deadlines for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly, and please let me know of any expected delays.

I wish you the best with working on the revisions. Please don't hesitate to contact me with any questions or comments about your submission, or if you have any feedback about your experience with Seismica.

Kind regards,

Ake Fagereng
Cardiff University
fagerenga@cardiff.ac.uk

Reviewer B:

This is a very interesting look at how coseismic shaking and wave propagation can directly influence the occurrence and distribution of landslides, with application to the specific case of the 2005 M7.6 Kashmir earthquake. I think this combination of earthquake and landslide physics is an exciting way to interpret what happened during/after this event. I also think that interdisciplinary approaches like this – and especially with the comparison of physics-based models to observational data not included in the models – are the way forward for hazard assessment in general, especially when physical processes are as interrelated as seismic shaking and landslide occurrence.

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GENERAL COMMENTS

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Thank you for this comment and you are completely right, those names are confusing! I have changed the abbreviations to ORIG and HF, meaning Original source and high frequency source, respectively.

You are likely to have a mixture of landslide people and earthquake people reading this paper (especially since you submitted it to an earthquake-focused journal). With this in mind, it would be helpful to add a little more detail/explanation of why certain parameters are important for landslide hazard modeling, and what specifically goes into those considerations.

For example: I am an earthquake researcher, and I was unclear on what was significant (and what was included) in your closeness-to-river term. I initially assumed this was related to the water in the river, but as I kept reading, I was became less and less sure what went into this. Does it have to do with the slope leading down into the river channel? Or the unconsolidated sediments deposited by the river? Please clarify!

I was also unclear on what went into the closeness-to-faults term. Does this separate out closeness-to-source-fault from closeness-to-other-faults? It seems to me that those are two different issues, though I may have been reading this wrong. In terms of closeness-to-other-faults in particular, is this largely related to how faults damage surrounding rock? I initially assumed this would also consider wave reflections off fault planes, but I became unsure after you stated that you don't model faults as discontinuities in your mesh.

This is a great point and I have added clarification to these terms. For the normalized distance to river term (Dst), this is a metric of the location of the landslide along the hillslope where 0 is at the river/valley and 1 is at the ridge. It has been used by previous studies to show if there is statistically significant clustering of co-seismically triggered landslides at ridges, ie are they being caused by topographic amplification or potentially hydrologic effects downslope closer to rivers.

I have updated a sentence within the 4.2 Landslide Catalog processing section to clarify this parameter:

L362-365: *We also calculate the landslide location along the hillslope* ***(D_{st}, normalized river distance (Rault et al., 2019)) to determine if coseismic landslides are preferentially located along ridges or within valleys*** *and frequency-area distribution (FAD) curves for a subset of landslide parameters to demonstrate their control on landslide size.*

The closeness-to-fault term is used to compare to past studies of the Kashmir earthquake that found landslides preferentially clustering around faults in the region. Explained in lines 505-509 in the main text, this is likely due to the more highly fractured rocks in these tectonically active zones and the proximity to the causative fault, which has been seen in other landslide studies as well (Massey et al., 2018). So, how we are using this metric does not separate out the ruptured fault from the active faults in the region. We do not include the fault plane discontinuities in our models so this is an interpretation based on the fault locations, not a result of the modeling.

Related to the above: can you please explain a little bit in the text why you chose not to treat your faults as actual discontinuities within your mesh? Most dynamic earthquake source simulations do discretize faults as discontinuities/interfaces. This shouldn't matter so much for initial seismic radiation away from the fault, but having no discontinuity there could affect how waves propagate in the intermediate/far field. One of the major hanging-wall effects is waves getting trapped between the free surface and the source fault below. Additionally, faults other than the source fault can reflect and focus waves (as was the case with the damage in Santa Monica during the 1994 Northridge earthquake); it seems to me like this could be a consideration for how seismic energy interacts with slopes. I don't think you necessarily need to change anything in the model setup to include this factor, but I think the paper would be stronger if you wrote in why you chose not to involve it.

For kinematic ground motion simulations, we typically do not include the discontinuity of the fault. Rather, our modeled "fault" is a densely spaced series of point sources along the fault plane, rupturing in space and time to represent a coherent wavefield. In a perfect world, we would incorporate the fault as an impedance contrast using a 3D seismic velocity model but one does not exist for this region. This would definitely impact the wavefield and likely increase our hanging wall effect but unfortunately cannot be done in this region.

I really liked how you compared your model results against so many other observational factors/variables that weren't part of the models themselves! This did leave me wondering why you didn't include a term/factor/comparison for recent rainfall/soil saturation, though. Isn't the rainfall history around the Kashmir earthquake region something you could look up? (And would it be possible to have dry/moderate/wet terms in more generalized models for landslide likelihood?) I know I may be totally off base with this comment, since again, I'm an earthquake person and not a landslide person. But given that moisture content (and weight from it) is something people are taught can affect landslides, I still think it might be worth adding a sentence or two about why you chose not to include this factor (especially if it's too difficult to actually incorporate here, or if it doesn't actually matter much for what you're calculating).

You are not off-base here, it is very common to compare rainfall/soil saturation when looking at coseismic landslide catalogs. I did not include this in my paper because it was shown to be unusually dry/unsaturated prior to this earthquake. I have added a sentence to my description of landslide catalog processing:

L366-369***: While rainfall and soil saturation prior to an earthquake are important factors controlling overall coseismic landslide distributions, we do not include this in our analysis because of the dry conditions documented before this event (Petley et al., 2006).***

This is a very thorough look at this specific earthquake and its associated landslides. I think your discussion and conclusions could be even stronger, though, if you included a paragraph or so about how the method you used here may be applicable/useful for coseismic landslide hazard assessment or forecasting beyond the specific case of the 2005 Kashmir event.

Thank you for these comments, I believe that these will make the work more applicable and impactful. To address this, I have added a section to the discussion outlining how these simulations/methods fit into landslide hazard assessment. I have also added a line in the discussion (L636-639) that relates my findings to that of other earthquake induced landslide studies. And finally, I have outlined an avenue for potential future research of the "ridge kink" phenomenon that has been described in detail in this manuscript.

LINE-BY-LINE COMMENTS

Lines 81-82: How/why does seismic wave amplification lead to landslides initiating higher on hillslopes?

The increased amplitude/duration of seismic waves focused at the tops of ridges causes coseismic landslides higher on the hill slope. This sentence has been modified:

L81-83: *Coseismic landslides have been shown to initiate higher on hillslopes than rainfall-induced landslides, likely due to* ***the increase in seismic wave amplitudes at ridge tops*** *(Meunier et al., 2007; Rault et al., 2019, 2020).*

Line: Cite some of the many studies here.

Unfortunately, I am not sure what line this is referring to, perhaps L83, where the citations for that claim are referred to in the remainder of the paragraph.

Line 86, and lines 116-118: Please clarify/specify whether you're modeling just wave propagation, or are also modeling the whole on-fault rupture process.

For these simulations, we are only modeling wave propagation using a kinematic (not dynamic) description of the source. I have modified L116-118 to convey this.

L117-120: *We model the wavefield* ***from the 2005 Kashmir earthquake using a kinematic source description*** *in a domain with a topographically complex free surface to quantify the role that topography plays in amplifying the wavefield and initiating landslides.*

Line 141: Please consider changing this to "previous major earthquake," since the last one was the one in 2005!

As suggested, I have changed the text to *previous major earthquake*.

Lines 145-146: This sentence seems out of place in a paragraph that is otherwise about rock type. I'd consider giving the uplift/incision/erosion discussion its own paragraph here – which can also include some of that elaboration on what the closeness-to-river term includes/considers.

This sentence has been removed. I have opted not to include an additional paragraph on uplift/incision/erosion and I have added clarification of the closeness-to-river term within the 4.2 Landslide Catalog Processing section.

Line 148 and elsewhere: Please spell out "formation" (at least the first time you use it, so you can introduce what "Fm" stands for).

I have modified Line 147 to include Fm: *The Miocene Murree* ***Formation (Fm)*** *makes up most of the study area (Fig.* ***Error! Reference source not found.****).*

Lines 182-183: Consider moving this sentence about communication loss earlier, when you're first introducing the impacts of the earthquake and its triggered landslides.

The comment about communication loss is already stated earlier in L95-96.

Lines 188-190: If you have pre- and post-event imagery, why is it difficult to determine which landslides were vs. were not caused by the earthquake? Or am I misreading this sentence?

In a perfect world, you would have imagery from the minutes before the earthquake and the minutes after, so you could truly document which landslides were in the region just before the earthquake. However, that is not the case and pre-earthquake imagery can be from months or even years before, so there will likely be some pre-earthquake landslides even if you use pre-earthquake imagery. This is mentioned in the maintext in L186: "*While the authors acknowledge the potential for some pre-earthquake landslides in their database due to the timing of the imagery*"

Lines 205-207: Is the landsliding near the Kashmir earthquake still continuing in 2024? The paper you cite for this is from 2012; please clarify whether it is truly ongoing, or if it just lasted as long as the publication date for that reference.

Thank you for this comment, I have added an updated reference from the same authors that find the landslide mass to be currently stable.

Removed line 205-207 and updated with information from a more current reference:

L203-205: ***Since the dam failure in 2010, the landslide mass has not experienced significant changes and likely does not pose an imminent hazard to the region (Sattar & Konagai, 2023).***

Line 227: Finite fault models can simulate ground motions well above 1 Hz! (And you even outright say that you're going above this frequency in lines 237-238.)

Due to the use of teleseismic waveforms and large subfault spacing, finite fault models without modifications i.e. directly from an inversion, generally do not have energy greater than 1 Hz in ground motion simulations (and it's typically even lower than that). Modifications can be made, such as systematically decreasing the rise times as I've done in this study, or generating a more heterogeneous slip model that matches the overall pattern of slip. In order to model the wavefield using a finite fault model, it is imperative to decrease the subfault size to approximately the size of the elements within the mesh. Our mesh is resolved to ~3Hz but the energy from our original source (S1/ORIG) has considerably less energy, to about 0.5Hz.

Line 244: Consider putting a paragraph break before "Rise times typically increase…"

Added a paragraph break here.

Lines 270-272: Please clarify what you mean by "naturally incorporates high-resolution topography." Do you mean that SEM makes it easy to include this?

Other numerical methods can only use a flat free surface and cannot implement surface topography. The SEM uses the weak formulation of the equations of motion which has a feature of a stress-free surface boundary condition that does not need to be imposed explicitly (ie my use of the wording "naturally incorporates"). This means that a free-surface with topography is straightforward to implement and has more accurate surface waves than other methods. This is quite in the weeds of the SEM and I do not think needs to be explained in this manuscript but can be found in the cited references.

Lines 280-281: What is your element shape?

Our elements are hexahedral which is required by the SEM. This is stated in L276 within the meshing and synthetic waveform generation subsection.

Lines 280-281: "with elements that increase in size with depth" makes it sound like they keep getting bigger as it gets deeper into the mesh. I don't think this clause is necessary, considering you explain the element size thing in the next sentence.

Yes, the elements within our mesh are smaller at the surface and larger at depth but only have two sizes that are separated by a doubling layer, so I see how this sentence is confusing. I have removed this and modified the sentence:

L278: *The mesh is 71 km x 78 km x 18 km in depth,* ***with 250 m elements*** *in the upper 1 km buffer layer, doubling at 1 km depth to increase the element size to 500 m for the remainder of the model (Fig.* ***Error! Reference source not found.****).*

Lines 308-310: Most ground motion simulations focused on seismic hazard in and of itself use PGA, not PGV. Please explain why PGV is better when assessing landslide hazard.

I have moved L399-403 to this paragraph to emphasize why we have chosen PGV earlier on in the manuscript and modified it to include other possible metrics that have been used in the literature.

Now L308-313: There is debate in the literature about the best ground motion metric for understanding coseismic landslide initiation, ***including peak intensities (peak ground acceleration (PGA) and PGV), duration, and cumulative metrics such as Arias Intensity (Jibson et al., 2000; Jibson & Tanyaş, 2020; Nowicki Jessee et al., 2018).*** *We have chosen to use PGV as our ground shaking metric due to the relatively low frequency content of these simulations and because it has been shown to have significant control on landslide initiation and size (Dunham et al., 2022; Massey et al., 2018; Nowicki Jessee et al., 2018).*

Lines 326-328: I'm a little confused here. Since the Kashmir earthquake was a surface-rupturing event, the top of the fault should reach the top of the mesh regardless of elevation, shouldn't it? Or do all the meshes have a fault plane that extends to the same depth, meaning the higher elevation ones have a larger fault? Or is this more of a topographic stressing thing?

All of the meshes have the same fault plane/seismic sources regardless of their elevation. This method of calculating the wavefield in meshes with difference elevations and interpolating between them helps us to curb the effects of geometrical spreading say between a mesh with 500m elevation vs. 5000m elevation. The impacts of this are shown in Supplementary Fig S4.

Line 329: Why are you trying to decrease the effects of geometrical spreading? This is a real factor that affects seismic wave propagation.

With topographic amplification, we want to be able to attribute differences in amplitude to topography alone, rather than the differences in distance between topography and a flat surface. This means we want to remove the effect of geometrical spreading between say a flat surface at 0m and the topography at 4000m elevation to just capture topographic effects. This effect is shown in supplementary fig S4.

Lines 331-332: This is an awkward sentence. Please consider rephrasing it to something like, "We conduct a 1D interpolation to the elevation of our topography…"

Modified this line to:

We then conduct a 1D interpolation of these values to the elevation of our topography mesh at each point *on the surface to match the expected distance traveled by the wavefield for both the flat and topography ground motions, generating elevation corrected flat PGVs.*

Lines 334-335: Why does the fault only come close to the free surface, as opposed to reaching it?

This is because we ran into some numerical issues when the fault ruptured to the surface. To curb this, we set a cut off to have the fault be 1 km below the mesh with topography. Because we are representing the fault kinematically and defining the rupture properties (not initiating them dynamically) this does not make a major difference in our wavefield simulations.

Line 348: Which seismic parameters?

This is primarily referring to intensity measures. Modified to include:

To investigate the possible causes of landslide initiation from the 2005 Kashmir earthquake, we determine topographic, lithologic, hillslope position, and seismic parameters ***(i.e. PGV and topographic amplification****) at each landslide location from the Sato et al. (2007) catalog.*

Lines 349-350: What do you mean by "landslide polygons"? The outlines of slides?

Yes, landslides can be mapped in different ways, the most typical are either as a point at the top of the landslide, a polygon around the entire landslide, or more granular, a polygon around just the head scarp region of the landslide and the runout portion of the landslide.

Line 369: "Hypocenter," not "epicenter."

Changed from epicenter to hypocenter.

Lines 373-374: Do you just mean reaching the free surface? In general, "terminus of the fault" means the along-strike end, not up-dip.

Removed terminus and changed to "surface"

L380-381: The breaking of this asperity (between 8-10 s) is coincident with the rupture reaching the ***surface*** *(Fig.* ***Error! Reference source not found.****, line with red triangles),*

Lines 399-402: I'd move this explanation of why you're using PGV up to earlier in the paper (like around lines 308-310).

Done based on previous comment.

Lines 422-424: Cite some of the previous studies here.

Modified sentence to include previous studies:

L424-427: Positive topographic amplification is mostly concentrated at ridges, and negative amplification in valleys, as expected from previous studies ***(e.g. Dunham et al., 2022; Harp et al., 2014; Hartzell et al., 2017)****.*

Lines 439-443: This phrasing makes it sound like something about the ridge topography itself is being amplified, and not the seismic waves interacting with this topography. Please rephrase a little.

Modified this sentence to include "the wavefield" :

L442-444: *From the snapshots of the wavefield, we can see much shorter wavelength phases for S2 compared to S1, contributing to the fact* ***that the wavefield*** *within shorter wavelength (smaller) ridges is being amplified.*

Line 464: You're missing another close parenthesis after the Sato et al. citation.

Done

Lines 500-506: I'd consider moving this to earlier in the paper, when you're explaining the reasoning for which parameters you consider in your landslide hazard assessments.

I have decided to leave this explanation of fault distance within the results section as this is where I have gone into the most detail about distance to faulting.

Lines 540-542: This sentence is a little unclear. Do you mean that topographic amplification is the dominant mechanism for landslide triggering on the hanging wall and not the footwall? Or that amplification really only happens on the hanging wall?

Here, I mean that topographic amplification is really only within the hanging wall of the fault, and does not seem to be related to the crest clustering of landslides. I have modified these sentences to clarify:

L542-546: *This indicates that, at this scale and frequency content (<2.7 Hz), crest clustering* ***of coseismic landslides does not signify the initiation mechanism*** *of topographic amplification. It* ***also*** *shows that, on average and at this macrocell scale, topographic amplification is dominant on the hanging wall but not on the footwall.*

Line 626: You left out a "with" between "align" and "previous."

Done

Line 628: Consider using "which all" instead of "additionally" here.

Re-reading this sentence, I don't think this change fits here. To clarify, I have modified it below:

L630-634: These findings align with previous studies that investigated the geomorphic and geologic controls on Kashmir earthquake landsliding (e.g., Kamp et al., 2008; S. F. Khan et al., 2013; Khattak et al., 2010; Owen et al., 2008; Shafique, 2020), ***and expand upon these investigations using*** *simulated ground motions to show the influence of high PGVs on coseismic landslide initiation.*

Line 631: You've spent so much time talking about the physics of seismic wave focusing that using "focusing" in this sentence seems awkward. Consider using "located" instead.

Done

Lines 730, and 733-734: Please change "is experiencing" to "experienced."

Done

Line 743: Please clarify what you mean by "horizontal and vertical ridges." Do you mean fault parallel vs. fault-perpendicular? Or ridges that are longer than they are tall vs. taller than they are long? Or something else altogether?

Thank you for pointing this out, those terms were coming from the cited paper but do not make sense in the context. I have revised this sentence below and related it to the terminology I am using to describe the ridge:

L754-757: *By modeling topographic amplification of ridge features in California, Asimaki & Mohammadi (2018) also showed this phenomenon by demonstrating that topographic amplification is higher* ***where ridges with differing azimuths intersect (i.e. at a kink)****.*

Recommendation: Revisions Required

Reviewer D:

I've thoroughly reviewed the article on the 2005 Kashmir earthquake-triggered landslides, entitled " *The influence of ground shaking on the distribution and size of coseismic landslides from the Mw 7.6 2005 Kashmir earthquake*" finding it insightful in exploring factors like seismic activity and topography affecting landslide distribution. While the study contributes significantly to understanding landslide hazards and mitigation strategies, addressing some limitations would enhance its impact. Overall, it's a valuable resource for researchers studying landslides and seismic events.

Comments

1-The introduction underscores the significance of earthquake-triggered landslides as a secondary geotechnical hazard, highlighting the challenges in determining ground motion distribution in high mountain regions like the 2005 Kashmir earthquake. However, it could benefit from a clearer articulation of the study's objectives and contributions for enhanced impact.

Thank you for this comment. I have modified the final paragraph of the introduction (mostly the last sentence) to clarify the objectives of the study and the overall expected contributions:

L122-125: ***By using ground motion simulations compared to traditional methods, we can better understand the relationship between ground shaking, topographic amplification, and coseismic landslide initiation, therefore offering crucial insights into past and future coseismic landslide distributions, including implications for the largest triggered landslides.***

2-The legend for Figure 1 inaccurately states that the Muzaffarabad Formation is of Pre-Cambrian age; however, it is actually of Cambrian age. For accurate geological mapping, please refer to sources such as Basharat et al. (2021) and Riaz et al. (2023).1. Basharat, M., Riaz, M. T., Jan, M. Q., Xu, C., & Riaz, S. (2021). A review of landslides related to the 2005 Kashmir Earthquake: implication and future challenges. Natural Hazards, 108, 1-30. and 2. Riaz, M. T., Basharat, M., & Brunetti, M. T. (2023).

Assessing the effectiveness of alternative landslide partitioning in machine learning methods for landslide prediction in the complex Himalayan terrain. Progress in Physical Geography: Earth and Environment, 47(3), 315-347.

Thank you for this correction, I have updated the legend for the geologic map and added the Basharat et al., 2021 reference to the figure caption.

3- Incorporating comparisons with previous studies on similar seismic events or numerical modeling approaches could provide additional context and validate the findings. Highlighting similarities or differences in results would contribute to the robustness of the analysis.

To address this comment, I will cite a few examples throughout the manuscript where we use comparisons to other studies to strengthen our results.

- Throughout the manuscript, we refer to previous modeling and observational studies of topographic amplification to put the simulation results into context within the literature. We find similar features such as amplification at ridge tops, on the leeside of ridges, and de-amplification within valleys. We also find the highest PGVs focused at the surface rupture and on the hanging wall, associated with the cited hanging wall effect.
- Added L636-639 to highlight other earthquakes that experienced landslides concentrated along the surface rupture and the highest PGVs.

  L636-639: ***Examples of this have been shown for the 2002 Denali earthquake (Gorum et al., 2014), 2008 Mw 7.9 Wenchuan earthquake (Xu et al., 2014), the 2016 Mw 7.8 Kaikōura earthquake (Massey et al., 2018), and most recently the 2023 Türkiye earthquake sequence (Görüm et al., 2023).***

- L733: Compared results for landslides not located along the surface rupture to findings from Rault et al., 2019 where they found topographic amplification to be most prominent in regions with little lithologic variability.

- Sect 6.3 Hattian Bala landslide discussion – I highlighted the similarity between the Langtang Valley landslide triggered by the Gorkha earthquake and the Hattian Bala landslide triggered by the Kashmir earthquake. I use previous modeling work by Asimaki & Mohammadi (2018) to show that amplification is stronger in ridges with a kink/intersecting azimuths.

4- Acknowledging and discussing any uncertainties or limitations in the methodology or data interpretation would enhance the credibility of the findings. Addressing factors such as model assumptions or parameter uncertainties would provide a more nuanced understanding of the results.

As the editor stated, I believe that we have done a good job addressing limitations throughout the manuscript. I have added a sentence in the Hattian Bala discussion section to again highlight that this landslide was likely primarily influenced by pre-existing conditions.

L740-743: ***While it is clear that this ridge was experiencing deformation prior to the earthquake that led to failure (Dunning et al., 2007), we can use these simulations to conclude if topographic amplification of the causative ridge occurred and therefore could have contributed to landslide initiation.***

L706 is another example of limitations outlined in the discussion, higher ground motions could occur in river valleys due to soft sediments, which are not included in our model due to limitations in the knowledge of the 3D velocity structure.

5- Conclude the discussion by outlining potential avenues for future research to further investigate the mechanisms of landslide initiation and propagation during seismic events. Suggesting specific research questions or methodologies could guide future studies in this field.

I have added the following lines to the discussion of the Hattian Bala landslide as a candidate for future research:

L757-764: ***The Daguangboa landslide, the largest landslide triggered by the 2008 M_w 7.9 Wenchuan earthquake, was similarly positioned to the Hattian Bala landslide, both on the hanging wall, close (~6.5 km) to the surface rupture, and with the slope failure facing in the direction of rupture propagation (Huang et al., 2012). It has been speculated that topographic amplification was a factor in initiating this large landslide but there were no seismic recordings to confirm this (Huang et al., 2012). The pre-earthquake imagery of the topography shows ridges with multiple azimuths converging at the head scarp, making it a potential candidate to investigate this phenomenon further.***

I have also added a section (6.4) to the discussion to outline implications of simulations for landslide hazard assessment.

6- Incorporating field photographs of co-seismic landslides, particularly the Hattian Bala rock avalanche, would significantly enhance the visual understanding of the landslide phenomena discussed in the paper. Including images of the affected areas, such as before-and-after shots or snapshots capturing the scale and impact of the landslides, would provide valuable context and illustrate the real-world implications of the study's findings.

I agree, the field photographs of the Hattian Bala landslide are astounding and definitely underscore the impact of this landslide. I do believe, however, that since we did not conduct these field surveys, that it is out of the scope to include other's field photos in this work. Field photos can be found within the many cited references for the Hattian Bala landslide.

Recommendation: Revisions Required