Reviewer #1

This is quite a thorough revision of the manuscript, and it's clear the authors have made a commendable effort to address many of both reviewers' previous comments — which is much appreciated. The use of more appropriate terminology, a clearer structure, and improved explanation of network science concepts have all significantly strengthened the manuscript.

That said, I have a few remaining suggestions. As a general comment, I would recommend, in the discussion section, to link the findings more explicitly to relevant literature to better position the study within the broader context and help readers understand how it advances current knowledge.

Thank you for your review. We appreciate the recommendation to link our discussion to relevant literature as it would greatly improve the context of the study. We have linked the findings to relevant literature in the most recent manuscript in the Discussion section, as well as added more citations in our introduction to link our study to recent research. Below, we have addressed the more specific comments, and believe that this manuscript has a clearer flow and improved impact.

Below are some more specific, paragraph-by-paragraph comments:

• L25–30: This paragraph would benefit from further rephrasing. For example, simply stating that these are deep-seated landslides would suffice. Also, four deep-seated landslides were recorded by USGS, but others may exist. I don't think it is necessary to mention that the specific landslide process has not been studied. Also, the term "shallow acceleration" is unclear — please clarify or rephrase.

We have rephrased this paragraph to simply state the landslides are deep-seated. We replaced shallow acceleration to "reactivated where some material from the surface failed and engulfed the highway''.

• L35–40: As pointed by the question from the other reviewer, network science is not a familiar concept for many geomorphologists, and further details in the intro are welcome. Yet I believe it could be made more useful – not by expanding it but by improving how the concept is introduced. Briefly explain what network science is, why it is potentially useful in this context, and how it adds value compared to more traditional approaches (e.g., early warning systems, other monitoring techniques). Also, I remain skeptical about the connection drawn between the granular nature of hillslopes and the predictive power of network science in granular materials. The method's strength seems to lie more in its ability to simplify systems by reducing the number of required parameters, rather than in any link to grain-scale processes.

We have added additional details in L35-40 to improve on the introduction of network science. The granular nature of hillslopes and the studies reference show that granular failure does not occur spontaneously, but that there is a precursory signal for the transition from stable to unstable. This signal can be identified using network science. We have rephrased some of the language as well as added a sentence in L35-45 to clarify this importance.

• L55: Consider revising “satellite coherence” to “InSAR temporal coherence,” as the former is typical for SAR data.

We replaced “satellite coherence” to “Insar temporal coherence” to be consistent.

• L60: You mentioned in your rebuttal that ‘coherence was already applied as a mask to the InSAR data’, so can you write here that you ‘include InSAR temporal coherence as a variable to measure satellite reliability’?

Line 60 already states this verbatim.

• L90: The Copernicus DEM has a native resolution of 30 x 30 m. If you downsampled it to match the multi-looked Sentinel-1 data, it’s better to state that explicitly (avoid phrasing like “provides elevation data at 40 x 40 m,” which could be misleading).

We corrected this part of the sentence to be “downsampled to 40×40 \unit{m}^{2} resolution (matching to InSAR resolution) \citep{hyp3} ”.

• L120: I’m still unclear about the rationale for using the volume of material removed during reconstruction works, instead of (even rough) estimates of landslide size volume — which would presumably be a more direct and consistent proxy. The quantity of removed material likely varies based on many context-specific factors (e.g., road design, access needs), making it a less reliable comparative metric.

We agree that estimates of landslide size volume would be a more direct and consistent proxy, but that information is not available. Therefore, using Google Earth, we measured the surface area of the landslide and have added that information on L120 instead. We have removed the volume of material removed to keep the text clear and focused.

• L135: This point may not have been clearly communicated in my previous comment, so I’ll rephrase: why use slope as a proxy for landslide susceptibility when actual landslide susceptibility maps are available? While this may not significantly alter your results, using such maps could provide a more comprehensive view, as they integrate multiple contributing factors beyond just slope.

Landslide susceptibility maps can provide a more comprehensive view than slope maps alone, but looking at the USGS susceptibility map, we observe that

most of the area has high susceptibility, which would provide many false positives. By using a slope map, we have a data-driven approach that utilizes gravitational driven stress by integrating how steep a slope is, distinguishing between hillslopes, and therefore allowing the algorithm to identify patches of area that have a higher chance of experiencing a landslide due to recent climatic conditions rather than identifying whole acres of area. In other words, a slope map will output a spatial location on the scale of a landslide.

Reviewer #2

The research proposed by Desai and co-authors is an interesting contribution to the study of (slow moving) landslide hazard with network science. I was not involved in the previous round of review. However, for assessing the current version of the manuscript, I have looked at the previous comments and the revised version.

In their responses to the other two reviewers, the authors have made an important revision of their work, addressing a large part of the concerns/suggestions of the first round. In its current version, the manuscript is to me almost ready for publication, pending a few “minor” points that I have listed below:

• In the introduction, I would welcome a few extra words on network science (what is it, why using it here in the specific case of landslide hazard?) since most readers may not be familiar with this concept.

We have added additional details in L35-40 to improve on the introduction of network science.

• Lines 25-30 and lines 120_130, and also elsewhere in the manuscript, the term landslide events is used. In the literature, one commonly have in mind that landslide events are made of several landslide features. However, here; for the landslide event of Paul’s slide, it seems that there is only one landslide. And in figure2c, there are 4 landslides (1 per event) that are pictured. Somehow, that could be said/formulated in a clearer manner and say in a more explicit way how many landslide are in the event inventories and how many have been studied. .

We have removed the term “landslide events” and instead cleanly stated that there are 4 landslides that are studied throughout the paper.

• Line 134. Slope is used as landslide susceptibility. Why not using a landslide susceptibility map instead? Maybe this methodological choice could be better justified.

Landslide susceptibility maps can provide a more comprehensive view than slope maps alone, but looking at the USGS susceptibility map, we observe that most of the area has high susceptibility, which would provide many false positives. By using a slope map, we have a data-driven approach that utilizes gravitational driven stress by integrating how steep a slope is, distinguishing between hillslopes, and therefore allowing the algorithm to identify patches of area that have a higher chance of experiencing a landslide due to recent climatic conditions rather than identifying whole acres of area. In other words, a slope map will output a spatial location on the scale of a landslide.

• One aspect that can still be improved is the discussion section where, to my opinion, reference to the broader literature/context is somehow missed.

We appreciate the recommendation to link our discussion to relevant literature as it would greatly improve the context of the study. We have linked the findings to relevant literature in the most recent manuscript in the Discussion section, as well as added more citations in our introduction to link our study to recent research.

Overall, well done to the co-authors for this interesting research on landslide hazard.

Thank you for your time and review. We have addressed your comments above and believe that this manuscript is much better for it.