Reviewer #2 Comments

We appreciate the reviewer's time and thoughtful feedback. We agree that the readability of the manuscript would benefit from modifications to whether particular items are described in the results, discussion, or conclusion sections. Additionally, we are happy to both expand on the discussion of network science itself, clarifying the unique value this method brings to the problem, and further elaborate on the monitoring implications of our analysis. These revisions will significantly enhance the clarity and overall impact of the work. Below, we respond to each of the comments and questions in detail.

This manuscript explores the use of network science to analyze landslide failure potential along the Big Sur Coast, with a focus on the concept of "community persistence" across subregions. While the use of network science is innovative and the figures are visually appealing, I have significant concerns regarding terminology, scale, and the clarity and coherence of the manuscript's structure.

Major Comments

> The use of the term *vulnerability* is bizarre. In the risk assessment framework, *vulnerability* has a well-defined meaning —people, property, infrastructure, and resources, or environments that are particularly exposed to adverse impact from a hazard event. The authors appear to conflate this with *hazard*, which would be more appropriate terms in the context of slow-moving landslides. This issue persists throughout the manuscript and should be addressed comprehensively.
>
> > We had introduced the term 'vulnerability' since we had created a new metric and it seemed appropriate to name it. Given the concerns of both reviewers, we will replace `vulnerability' with `hazard' as the context requires, based on the following definition:
> >
> > > "Landslide hazard maps indicate the possibility of landslides occurring throughout a given area. An ideal landslide hazard map shows not only the chances that a landslide might form at a particular place, but also the chance that it might travel downslope a given distance." - USGS (https://www.usgs.gov/faqs/what-a-landslide-hazard-map)
>
> The analysis is conducted at a subregional scale of 5 km², yet the landslide processes typically affect areas of a few 0.1 km². This mismatch raises questions about the sensitivity and appropriateness of the method for capturing relevant slope dynamics. Additionally, the definition of subregions is not clearly

justified—especially the inclusion of “varied terrain” within single units. From a monitoring perspective, this scale is difficult to reconcile with actionable insights, and no clear rationale is provided for not working at the scale of slope units or other, more geomorphologically meaningful divisions.

This should as well form a clear part of the manuscript discussion.

> We agree that this choice of length scale is important to establish on firm footing, and did some tests of subregional areas ranging from 25 km² to 5 km². The key argument for conducting our analysis at 5 km² instead of a couple of 0.1 km² lies in the possible application of this technique as a monitoring tool that is computationally efficient (a 250x larger set of grid points is a significant increase in computational time). We considered smaller subregion areas but this would have reduced the square footage of stable slopes -- an essential factor for identifying the slope dynamics that signal a transition to catastrophic failure.
>
> A potential future direction could be to expand this approach at a smaller spatial scale to determine if the method would improve in sensitivity and specificity. This could improve the applicability of this work for monitoring, at the cost of additional computing time. Were this technique to be under development as a monitoring tool, a careful analysis of the tradeoff between increased spatial resolution and increased computational time would be important to tackle before deployment.

From my perspective, this methodology making use of network science/community presence/nodes/etc. seems overly complex without delivering clear added value. Especially since, ultimately the Z-score based on the ‘community persistence’ metric only considers slope and surface velocity... Why not include the other factors introduced in the paper ? The exclusion is neither explained nor justified.

> In our original (and successful) application of this method (Desai et al 2023), we were drawn to the ability of network science in reducing a complex problem to a description in terms of spatiotemporal relationships derived from readily available remote sensing data. This approach allows us to study slow-moving landslides and identify the transitions between stable and unstable using minimal inputs.
>
> The value of simplifying the system to a network is demonstrated in the Multivariate Analysis (Section 4.1), which connects the results of community detection to known physical drivers of slope activity.
>
> Regarding your question on including other factors: the two inputs to the network -- surface velocity and slope -- were selected deliberately. The

inclusion of precipitation, which we also thought would be beneficial, is addressed in the Supplemental Material: we instead found that its inclusion did not significantly improve the community persistence metric. As such, we determined that it did not add value in this analysis. The other factors -- InSAR Coherence, Mean Displacement, Max Displacement -- were not used as inputs but rather served as benchmarks for evaluating the community detection results. Specifically, we used them to assess how well the Z-score aligned with these known geophysical measurements. Including these variables as dynamic inputs would introduce redundancy: coherence was already applied as a mask to the InSAR data, and velocity was one of the network inputs. Therefore, they were excluded from the network inputs to entangling cause and effect.

The goal of this method is to identify key transitions using the most reliable and widely available data. Our results suggest that network-based analysis using only slope and velocity is sufficient to capture the essential dynamics of landslides: its simplicity is one of its strengths.

We will revise the manuscript to better highlight the rationale for the use of network science, as we did in Desai et al., 2023, and also to better-highlight how we came to the conclusion that many factors could be excluded without significant loss of predictive power.

The manuscript would benefit from significant restructuring. Results and discussion are overly interwoven, and there is actually little discussion of the results/methods. Also, the Conclusion introduces new analyses rather than synthesizing findings. This undermines the clarity and scientific rigor of the narrative. Moreover, several key analytical steps—such as the multivariable analysis—are presented in the Results rather than the Methods section, reducing transparency.

We will separate the discussion from the conclusion, and the results of including precipitation with velocity and slope will be included in the results section instead. The multivariable analysis will be split to incorporate the first paragraph of 4.1 under the methods section instead and the rest will stay under results.

It must be made clearer from the abstract and throughout the manuscript that the methodology targets large, deep-seated, slow-moving landslides. The focus on four specific landslides (e.g., W22-3) is not well-motivated in the Introduction.

The focus of the 4 landslide events is that they were recent landslide events for which we had InSAR data for and could test the method on. We will add language to identify the 4 events as true positives. We will add in more language to clarify this throughout and to highlight the motivation of studying these landslide events.

Section-specific comments

Abstract

What do you mean by ‘which provides a natural way to incorporate spatiotemporal dynamics’?

We were trying to state that the network analysis provides an intuitive way of studying the spatiotemporal dynamics. We understand how that is not clear and will remove ‘natural’ from the sentence.

Introduction

The introduction could be more comprehensive, particularly in contextualizing the relevance of network science to landslide analysis.
Paragraph 30: Clarify what network science techniques entail and justify their application here.

We had aimed to keep this paper shorter, since the original paper (Desai et al 2023) had already covered the relevance of network science. Since both reviewers would like to have that information repeated in this paper, we are happy to add it to this paper as well. Among the key points are that network science has applications in many fields including granular/amorphous materials (Refs Bassett et al., 2011; Kivela et al., 2014; Mucha et al., 2010; Papadopoulos et al., 2016; Porter & Gleeson, 2016; Porter et al., 2019 in the paper).

The landslide material underlying a hillslope is made up of grains, and due to the success of applying network science in granular systems, we were attracted to the ability of network science to reduce this complex system to a description in terms of spatiotemporal relationship in our original and successful application (Desai et al 2023). This is done by providing an overview of the state of the system in terms of a set of nodes connected by edges, where an edge contains quantifiable data about the relationships between the nodes, information which (in the case of landslides) is available from remote sensing data. In our revision to the paper we will emphasize this context (as was done in our earlier paper, but we hadn’t originally repeated it here.)

Again, reconsider the use of *vulnerability*.

Instead of the term vulnerability, we will use hazard, as defined by USGS.

Clarify the rationale behind focusing on the W22-3 landslides.
It is not clear in the introduction why you focus on the 4 landslides of W22-3 and not the 44 others.

We look at all 44 landslides, but only 4 of them failed catastrophically and as such are true positives. Therefore, we will reword within the paper to 4 true positives and 40 true negatives in this study for the W22-23 period.

Data

Paragraph 50: The initial detection of 44 active landslides is mentioned twice and then largely ignored. Either omit or incorporate this information meaningfully in the analysis—e.g., assessing their impact on community persistence in other subregions.

As described above (comments on Introduction), we will be making a change to include both populations explicitly.

Paragraph 60–65: Rather than stating volumes removed, provide basic information about landslide size. Paul's Slide, for example, appears much larger than others, which likely impacted the analysis. This should be discussed in the discussion section as well.

Due to a lack of clear classification by landslide type, we decided to focus on providing information which was available for all four landslides, and one of the available metrics was the volume removed. There are no published studies of these recent landslide events, except for Paul's Slide, and we are not sure if all 4 of these events are associated with slow-moving landslides. We will define that these landslides occur deeper than 1 m below the surface soil. We will add other references and clarify that this study encompasses both slow-moving and debris slide processes.

We agree that Paul's Slide is much larger than the other in terms of volume removed. The initial surface area is roughly similar to those of the other three landslides that failed. Since only the moving surface of a hillslope is being detected in the network analysis, the volume removed has no influence on the results. This is a good discussion point and we will add the above explanation to the manuscript.

It feels premature to present the number of landslides detected before explaining the InSAR methodology.

We agree that moving Sec 2.2 before Sec 2.4 could be helpful and will implement this change when writing the revision.

Figure 1: Increase the size of panel (a), reduce legend clutter, and adjust scale values in panel (c) for cleaner presentation.

> We will take this into consideration when we modify this image to improve its presentation based on both reviewer's comments.

The term "InSAR snapshots" is unclear—consider replacing them with "deformation maps" or similar.

> Our goal is to keep terminology as easy for the reader as possible and agree that deformation maps (or something akin) could help achieve this.

How did you defined you subregions, is it relevant to include 'varied terrain' in a subregion? Also, in a monitoring perspective, how to deal with information at a 5km² scale?

> We defined the subregions as areas of (5 x 5) km^2 that were along the coast of the defined area, in which landslides had been identified by co-Author Alexander Handwerger and InSAR data had been processed.
>
> It is relevant to include regions of varied terrain as it is important for community detection to have areas of stable hillslopes to compare to hillslopes within the same subregion that are moving and transitioning from stable to unstable. They are the control that we measure against.
>
> By quantifying the results of each subregion, this method has the ability to be used in a monitoring perspective by identifying areas that have a high likelihood of experiencing a landslide..

Paragraph 115: Why are only slope and velocity used in community persistence? This choice should be justified more explicitly.

> The inclusion precipitation does not enhance or improve the results, an observation that is discussed in the both paper and Supplemental Materials. See responses above under 'Major Comments' for further details about how we will clarify and emphasize this during the revisions.

Paragraph 130: "We use peak Z to quantify differences between sub-regions to better classify the slow-moving landslides as stable (peak Z < 2.5) or vulnerable (peak Z > 2.5)." Clarify whether you are classifying subregions or landslides based on peak Z.

> In paragraph 130, we will clarify the sentence to state that while peak Z is used to classify sub-regions, it is an indicator of the landslides within that sub-region.

Landslide susceptibility is approximated using the sole slope. Why not using susceptibility models – I guess there are many available for the region?

> No, while there are many susceptibility maps available in this region, susceptibility maps do not quantify the probability of a near-real-time

landslide occurrence for a given area. Through the inclusion of velocity along with slope, we provide near real time monitoring of landslides in the region.

Results

Figure 2: Clearly show the correspondence between Zt point size and value. Make the color scheme and value thresholds more intuitive (e.g., emphasize the vulnerable vs. stable divide).

We will consider this and see if we can make the interpretation more intuitive while still keeping the colors accessible in monochrome and for folks who are colorblind (this colormap was chosen to achieve both of these aims).

How do you rule out that Paul's Slide is detected so clearly simply because it is much larger than the others? This point deserves to be discussed.

While the volume removed in Paul's Slide is much larger than in the others, the initial surface area is roughly similar to those of the other three landslides that failed: the signal we are detecting comes from the same combination of factors collectively to identify it as a hazard. Since only the moving surface of a hillslope is being detected in the network analysis, the (eventually-known) volume removed has no influence on the calculation we do. This is a good discussion point and we will add the above explanation to the manuscript.

Discussion & Conclusion

Results, discussion and conclusion are overly interwoven, and there is ultimately little discussion of the results/methods.

We agree that the results, discussion, and conclusions sections could benefit from clearer separation. In the revised manuscript, we will reorganize these sections to distinctly separate the discussion of the results from the conclusion. Additionally, we will expand the Discussion section to more thoroughly interpret the results and contextualize the methods used, especially incorporating the specific discussion points raised by both reviewers. We will place emphasis on how the network science techniques simplifies the system and the value it adds to identifying the transition from stable to unstable.

How did you define the subregions? Is it relevant to include 'varied terrain' in a subregion?

See response above under ‘Major Comments’.

Also, from a monitoring perspective, how to deal with information at a 5km² scale (e.g., to ‘potentially allowing for preemptive monitoring and mitigation measures’)?

See responses above under ‘Data’.

How does changes in slope velocity (of a few cm/yr) over a few 0.1 km² influence velocities max/averaged over subregions of that size? Why not working e.g., over slope units?

The average velocity is taken for any two nodes connected by an edge, which spans roughly 20 m, not for the entire subregion. Each edge has a slope value which accounts for the slope at a higher resolution at roughly 20m than a slope unit would.

paragraph 205: The Conclusion should not introduce new results; instead, it should synthesize key findings and implications.

We will move the results of the precipitation inclusion into the results section.

Final Recommendation

This manuscript addresses an important (and complex) aspect of landslide risk, aiming at combining satellite remote sensing and network science framework to provide a comprehensive monitoring technique. However, I see fundamental issues with terminology, scale, methodological clarity, and manuscript structure that significantly limit its current impact.

Thank you for your time and thoughtful review. We appreciate the feedback and believe this paper will improve on its clarity and impact once we implement these changes to the structure, vocabulary, and expansion on the decisions we made above.