February 10, 2026

To: Dr Christos Evangelidis

Journal: Seismica

Subject: Revised Manuscript Submission

Manuscript Title: Did they blow it? Time-lapse velocity variations during an open-pit mine slope failure using seismic noise interferometry

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Dear Editor,

We are pleased to resubmit our manuscript, "Did they blow it? Time-lapse velocity variations during an open-pit mine slope failure using seismic noise interferometry," for your consideration.

We are grateful to the reviewers for their time and valuable comments, which have helped us to significantly improve the manuscript. Our point-by-point response to the reviewers' comments is provided in blue below.

Sincerely,
Tjaart

Reviewer A:

I have read the manuscript "Did they blow it? ..." by de Wit and Snieder. They present a detailed ambient noise coda-wave interferometry monitoring of the flank of an open-pit mine around the time of a flank collapse. Through measurements of relative seismic velocity changes, they characterize the evolution of flank rigidity that leads to collapse and argue, based on their observations, that the collapse has possibly been facilitated by a blast a few days prior, followed by heavy rainfalls. The paper is well-written, clear, and concise. The results clearly support the interpretations and conclusions of the paper. I only have minor comments to help address some technical points.

General Response:

We thank the reviewer for their positive assessment of the manuscript, specifically their comment that the work is 'well-written, clear, and concise' and that the results 'clearly support the interpretations.' We appreciate the technical rigor of the comments regarding the data processing and detailed responses to each comment are given below.

MINOR COMMENTS:

1) Line 86-87: One-bit normalization and spectral whitening. Are they performed in that order? If using MSNoise, I think yes, but I always wonder if this order is the best. If one frequency dominates in the raw signal, then one-bit normalization will enhance it and filter-out the less dominant ones. Then, spectral whitening won't restore the filtered frequencies. On the other hand, if whitening is performed first, then one-bit normalisation shouldn't affect the frequency content much. It looks from Fig. 2 that the final frequency content of the correlation is dominated by frequencies around 20 Hz. You are also performing the MWCS in the 6-30 Hz frequency band. I guess my question is Did you need to whiten in such wide frequency band (5-100 Hz) if you are using only the lower half of the frequency content?

Response to Comment 1:

We followed the processing flow described in Bensen et al. (2007), applying one-bit normalisation followed by spectral whitening. The reviewer raises a valid point that if a single frequency dominates the raw signal, one-bit normalisation could amplify it before whitening can correct it. In this study, we didn’t observe any monochromatic noise sources that would have been problematic for this processing order.

Regarding the bandwidth, we chose to whiten the broad 5–100 Hz band to ensure the Green's functions were reconstructed as broadly as possible. While the MWCS analysis focused on the 6–30 Hz band (where we found the coda energy was most stable), applying the wider whitening window ensured that the cross-correlations were not limited during the pre-processing stage."

Action taken:

We have updated the text in Section 2 (line 86) to clarify the order of operations: "One-bit amplitude normalisation was applied, followed by spectral whitening between 5 Hz and 100 Hz (Bensen et al., 2007).

2) Line 107: Please clarify the reference correlation you are using to measure the dv/v. From this sentence it seems you are measuring dv/v between consecutive 2-days correlations, but from Fig. 3 and 4, because the first point shows 0%, it seems that your reference is the first correlation of the studied period.

Response to Comment 2:

The reviewer is correct. We measured dv/v between consecutive 2-day correlation stacks (moving reference) and then accumulated these changes to produce the time series shown.

We agree that including a '0%' data point at the start of the time series in Fig. 3 and 4 was misleading, as it implied a fixed reference correlation starting at that time. We have updated both figures to remove this starting zero point. The plots now begin with the first calculated relative velocity change."

Action taken:

Fig. 3 and 4 have been updated to remove the initial 0% starting value. The plots now start at the first measured velocity change value.

3) The final results you show are either an average of every cross-pairs and cross-component or an average per station of the dv/v, showing variation on the order of -0.5% max. But Fig. 3 shows a drop of up to 1.5% for one pair and a particular set of components. This means that many other cross-pairs or cross-components exhibit much smaller variations. Is there some systematics on the cross-components that would show little to no variations, or, on the contrary, which show larger than average variations? If yes, could that indicate some constraints on the mode of failure of the slope or the nature of the ambient noise sources and their distribution that are most sensitive to the slope failure? Could you comment on that and perhaps provide an extra figure showing some statistics of dv/v as a function of the components, the azimuth of the considered pairs, etc?

Response to Comment 3:

We appreciate this keen observation. The reviewer is correct that Fig. 3 displays a sensor-component pair (1X-3Z) that experienced a large velocity decrease compared to the average shown in Fig. 4.

To address the reviewer's question regarding systematic changes, we performed a statistical analysis of the velocity changes across all sensor-component pairs, specifically looking for dependencies on component combinations (e.g., X-X vs X-Y) and azimuthal direction of the pairs.

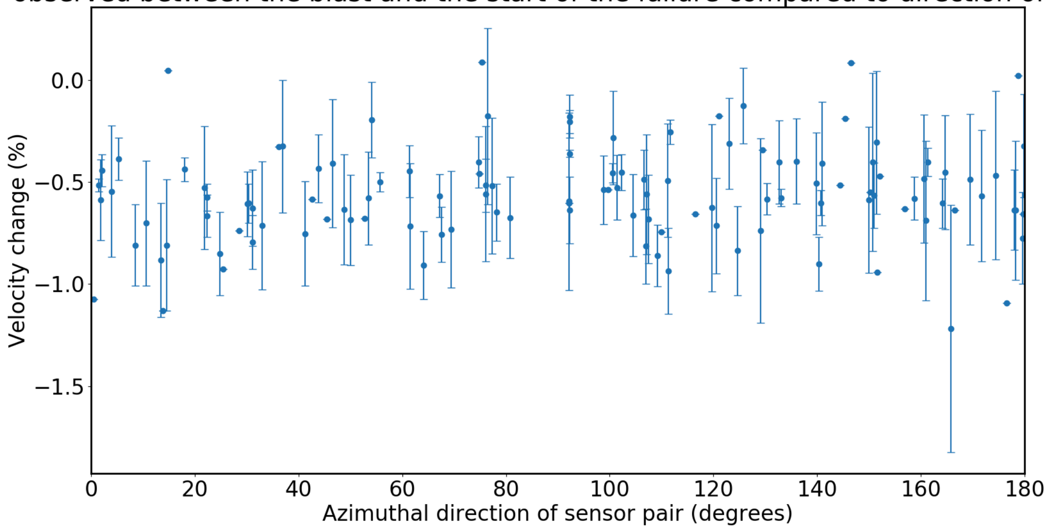
We found that while there is spatial variation in the magnitude of velocity changes (likely associated with spatial variation in the subsurface damage), there is no systematic dependency on the component combinations or the azimuth of a sensor pair.

The velocity changes across different cross-components of a given sensor pair are consistent. For the period between the blast (Feb 11) and the failure initiation (Feb 23), the standard deviation of velocity changes across cross-components was 0.16% (averaged across all sensor pairs). As shown in the table below, no specific component pair (e.g., X-X, X-Z) showed a statistically significant deviation from the mean.

We also plotted the velocity change against the azimuthal direction of the sensor pairs (see figure below) and found no preferential direction associated with the velocity decrease.

While these statistics do not constrain the mode of slope failure, the lack of preferential directionality or component dependence suggests that the coda portion used in this study consists of multiply scattered waves that have lost the original directionality of the direct waves. This confirms that the coda wavefield was sufficiently scattered (diffuse) and isotropic, so the coda regime selected was appropriate for these interferometric observations.

Average velocity change calculated across all cross-components of each sensor pair observed between the blast and the start of the failure compared to direction of pair



Component Combination	Average velocity change (%)
X-X	-0.676
X-Y	-0.674
X-Z	-0.601
Y-X	-0.529
Y-Y	-0.550

Y-Z	-0.482
Z-X	-0.565
Z-Y	-0.586
Z-Z	-0.555

Action taken:

We have added a paragraph in Section 3 (after line 142) summarising these statistics to clarify that the variations observed are likely spatial rather than component-dependent.

"While the average seismic velocity change across all sensor-component pairs was approximately -0.5% for the period between the blast and the onset of failure (Fig. 4), the magnitude of this change varied spatially. Several sensor pairs observed decreases significantly larger than the average; for example, pair 1X-3Z observed a change of approximately -1.0% (Fig. 3).

We analyzed the statistics of these variations to check for directional dependencies. The velocity changes observed across different cross-components of common sensor pairs were consistent (average standard deviation of 0.16%), and no specific component combinations (e.g., X-X, Y-Z) or azimuthal directions of the sensor pairs showed significantly different velocity changes. This lack of directionality suggests that the coda waves used were sufficiently scattered to sample the medium without strong bias from the source-receiver orientation."

Reviewer B:

First please receive my apologies for the unreasonable delay in submitting this review.

The article reads well and presents a very interesting case of applying seismic interferometry to monitor open pit mines, a task that will become more and more challenging in the future.

The structure of the article is good, and presents a short review of past slopes failures and related landslide monitoring techniques.

The analysis presented follows the state of the art of ambient noise monitoring technique, although only using one of the possible dv/v methods (MWCS). Here below, I note a few questions/remarks.

General Response:

We thank the reviewer for their positive assessment of the manuscript and their constructive feedback. We appreciate the time taken to review the work despite the delay. We have addressed the specific questions regarding the geological context, plotting conventions, and physical interpretations below.

Figure 1: the main map would benefit from geographical coordinates (zebra+UTM coordinates). The depth of the isolines could be labelled at a few isolines, to ease the reading of the real depth (large) of the mine.

Response to Comment 1:

We agree that adding coordinates and depth labels improves the readability and context of the map. We have updated Fig. 1 to include a border with UTM coordinates and added elevation labels to the major isolines to better illustrate the scale and depth of the open pit.

Action taken:

Fig. 1 has been updated to include UTM coordinates on the map border and elevation labels on the contour lines.

Elements of Discussion:

- In the introduction, you mention notable slope failures in the past (L67-70) - Is there some litterature about those ? What were the identified (if any) causes of the failures ?

Response to Comment 2:

We have added citations for the specific slope failures mentioned (Bingham Canyon, Palabora, etc.). In most of these cases, the failures were attributed to a combination of geological structural weaknesses and triggering events such as heavy rainfall or seismic activity (natural or induced). We have added a sentence to the introduction to provide this additional context while maintaining focus on the application of seismic interferometry to this study.

Action taken:

We added references for the mentioned failures in the Introduction (lines 67-70) and briefly noted their general causes (structural controls and/or rainfall triggers).

“Slope failures in open-pit mines have occurred regularly over the past two decades, with several notable slope failures including: Bingham Canyon mine in the United States of America in 2013 (Hibert et al., 2014); Palabora Mine, South Africa in 2003 (Brummer et al., 2006); Super Pit near Kalgoorie, Australia in 2018 (Darbritz, 2023); and Gamsberg Mine, South Africa, in 2020 (Pretorius et al., 2025), with the causes of these various failures attributed to a combination of geological structural weaknesses and triggering events such as heavy rainfall or seismic activity."

- The interpretation shows some precursory changes: could you precise the number of "days" (=windows) ahead of the relative changes (maybe add minor gridlines, or a zoom on the different figures). Are your MWCS "days=windows" labelled on the RIGHT of the window (the marker on Feb 15 is calculated from the raw data on [Feb 13 + Feb 14] ? In case it's not well labelled, it could shift the points on Figure 4 (and the red dashed line: the blast could be aligned with the onset of the drop)?

Response to Comment 3:

This is a critical point regarding the causal link between the blast and the velocity drop. Our timestamps correspond to the center of the 2-day correlation stack. For example, the data point plotted on 12 Feb represents the cross-correlation of data from 11 Feb to 13 Feb, whereas the data point on Feb 10th before the start of the velocity decrease represents the cross-correlation of data from 9 Feb to 11 Feb. With this convention, the blast (occurring on the morning of 11 Feb) falls exactly within the window where the sharp velocity drop is observed. The drop is not shifted; rather, the impulse of the blast and the immediate velocity decrease are coincident within the temporal resolution of our 2-day windows. We have added minor gridlines to Fig. 3 and 4 as suggested to allow for more precise reading of the timing.

Action taken:

We clarified the plotting convention (center-of-window) in the captions of Fig. 3 and 4 and added minor gridlines to Fig. 3 and 4 to improve temporal readability.

"Velocity change observations are plotted at the centre time of the 2-day correlation window used to measure the change."

- L107: "the result of this process is the accumulated relative...": so the MWCS is done between one window vs the previous one ? And the dv/v is the cumulative sum of the values ?

Response to Comment 4:

Yes, the reviewer is correct. We calculated the velocity change between consecutive 2-day stacks using the 'moving reference' method and then accumulated these changes to generate the time series. This approach helps maintain high coherency in a rapidly changing medium.

Action taken:

We have clarified the "moving reference" methodology in Section 2 by replacing the sentence on line 107 with:

“We calculate these relative velocity changes between consecutive 2-day stacks (a moving reference) to maintain high coherency. These incremental changes are then accumulated to produce the time-lapse velocity variation series shown in the results.”

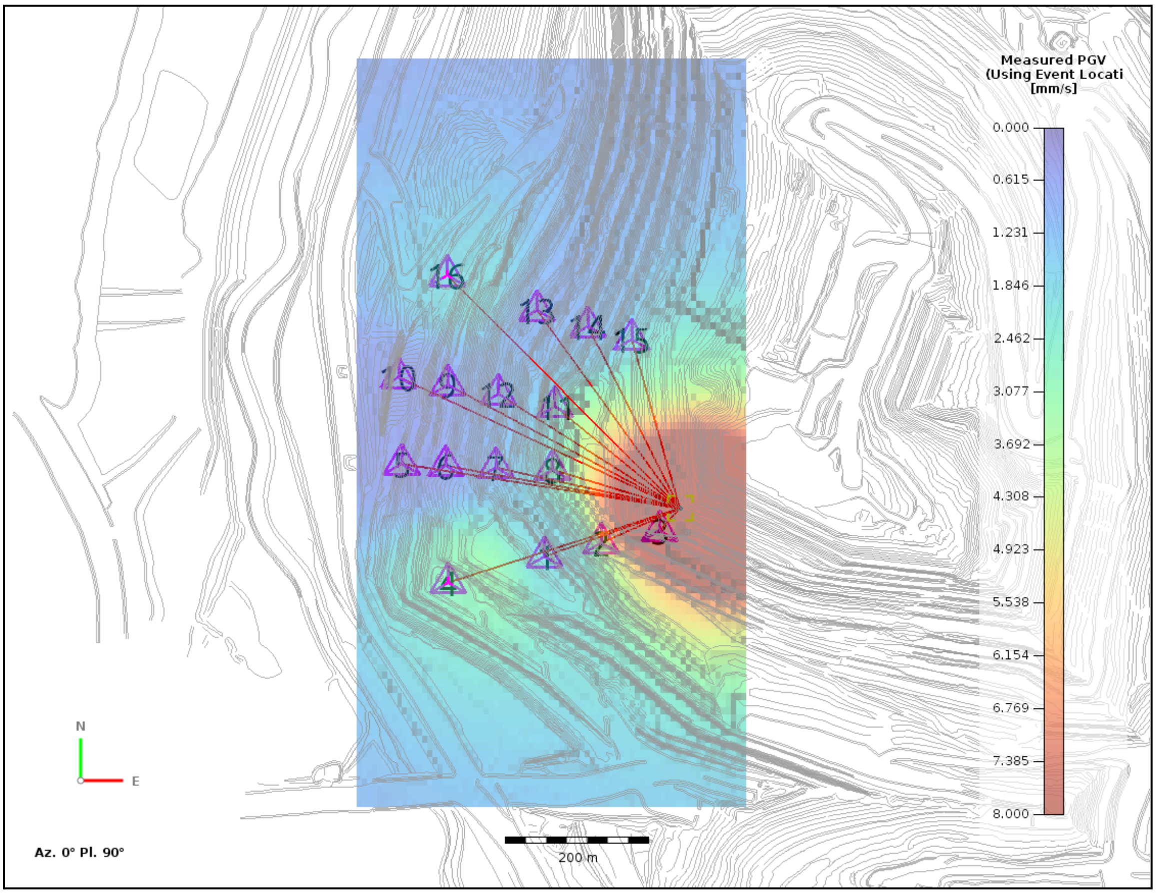
- You mention the PGA of one sensor, it'd be interesting to see a x/y scatter plot of the PGV (or PGA/PGD) values in relation with the velocity changes plotted per sensor on Figure 5

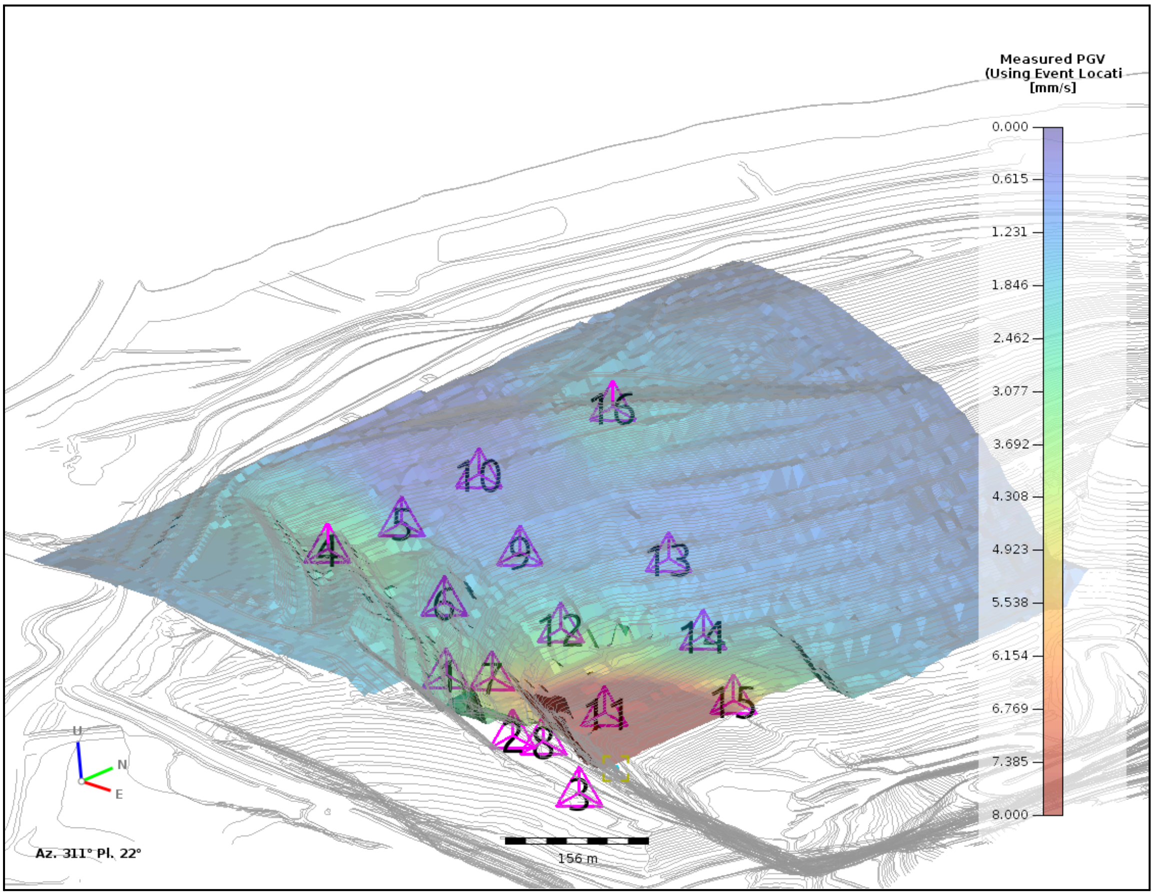
Response to Comment 5:

The reviewer raises a good point regarding the relationship between ground motion intensity and the magnitude of the velocity drop.

We investigated the spatial distribution of Peak Ground Velocity (PGV) from the blast and confirmed that it decays rapidly with distance from the source, spatially correlating very well with the regions of largest velocity decrease (see figures below).

To visualize this relationship directly in the paper without adding a separate scatter plot, we have modified Fig. 5 to include the specific location of the blast. This allows the reader to easily compare the spatial velocity changes with the source of the ground motion, highlighting that large drops occurred near sensors closest to the blast.





Action taken:

We have updated Fig. 5 to include the location of the blast event. This addition clarifies the correlation between the blast proximity (high ground motion) and the regions of largest velocity decrease.

- The relative velocity changes are large, and when compared to Mainsant (L126), you mention the sharpness of the changes in your case. What coud be the reason for it ? Can this be linked to the frequencies chosen for the analysis ? More/less shallow response?

Response to Comment 6:

The sharpness of the velocity drop (compared to the slower creep seen in Mainsant et al., 2012) is likely due to the nature of the trigger. In our case, the drop coincides with a blast, which is an impulsive, high-energy event that causes immediate co-seismic damage. In contrast, many natural landslides act as slow-moving viscous flows.

The reviewer is correct regarding the frequency. By using a 6–30 Hz band, we are sampling wavelengths that are highly sensitive to the shallow, weathered, and fractured layer of the slope. This shallow layer is most susceptible to the immediate shaking damage from the blast.

Action taken:

We added a sentence in the Results and Interpretation (on line 128 of Section 3) contrasting the impulsive nature of the blast source with natural creep to explain the sharpness of the signal, and noting the high sensitivity of the 6–30 Hz band to shallow damage.

“The sharpness of the observed velocity drop, compared to the gradual changes from creep observed in natural landslides (e.g., Mainsant et al. 2012), reflects the impulsive nature of the blast source causing immediate co-seismic damage. Furthermore, the 6–30 Hz frequency band is highly sensitive to changes in the shallow, weathered layer of the slope, which would have been most susceptible to the shaking from this blast.”

- What are actually the walls/slopes composed of ? Hard rock ? On L156 you mention a fault, but it's not mapped on Figure 1 or 5. Also, for this open pit mine, do they have to lower the water table dramatically? In that case, is subsidence or other deformation processes at play?

Response to Comment 7:

We thank the reviewer for prompting us to clarify this. The slope is hard rock. Based on Salvoni and Dight (2016), the slope is composed of Upper and Lower Footwall units dominated by black carbonaceous shales. These shales contain montmorillonite and pyrite, which leads to rapid deterioration and slaking upon exposure. This geological characteristic is crucial for our interpretation: the rock mass is highly susceptible to degradation from atmospheric and dynamic drivers (rain and blasts).

The 'Page Creek Fault' mentioned is indeed a critical structure. It is a sub-vertical structure that creates a local zone of highly sheared and fractured rock, which likely amplifies the susceptibility of this part of the slope to the blast energy. We have indicated its approximate trace on Figure 5 to help the reader visualize the relationship between the structure and the velocity changes.

The mine did manage the groundwater in the pit, but the timeline of this study (2 months) is too short for long-term poroelastic subsidence to be the primary driver of the -1.0% velocity decrease observed. The sharp correlation with the blast and rain events points to immediate damage and infiltration rather than slow subsidence.

Action taken:

We added a description of the geology in Section 2 and annotated the approximate location of the Page Creek Fault on Fig. 5. Additionally, we updated the colormap in this figure to ensure it is accessible to readers with color vision deficiencies.

“The local geology consists primarily of interbedded sedimentary sequences, dominated by shale and sandstone units, with the western slope wall geology consisting of units dominated by black carbonaceous shales (Salvoni and Dight, 2016). While the fresh rock is mechanically competent, the shales have a high clay (montmorillonite) and pyrite content, making them prone to rapid deterioration and slaking upon exposure. The structural setting is complex. A sub-vertical, the Page Creek Fault, intersects the slope, creating a localized zone of highly sheared and fractured rock (Salvoni and Dight, 2016). This combination of lithology and heavy fracturing makes the slope sensitive to external forcing."

- What waves types are in your coda window ? You're starting the window at the theoretical arrival of the S wave (thus including it's "coda", and then surface waves) ? What are the noise sources at the frequencies chosen (high: 6-30 Hz) ?

Response to Comment 8:

As outlined in Obermann and Hillers (2019), the complete Green's function retrieved from the ambient wavefield contains direct body waves and surface waves, reflections, multiples, and multiply scattered coda-waves. Fig. 2 includes dashed vertical lines to indicate estimates of direct P- and S-wave arrival times (from approximate P-wave velocity of 3.6 km/s and S-wave velocity of 2 km/s as calibrated using known locations of several blasts). The peak in the cross-correlation functions coincide with these arrival times, indicating retrieval of direct body wave arrivals. The later arrivals would be associated with surface waves and scattered body waves trapped in the near-surface heterogeneities.

The lag times considered in this study actually start from 1.2 times the direct S-wave arrival time to avoid this direct arrival. We have clarified this in the text.

The noise sources of waves in the 6 - 30 Hz frequency range in this active mine environment are primarily anthropogenic: crushers, haul trucks, and excavation machinery, which provide a rich and relatively continuous high-frequency noise field.

Action taken:

We clarified in Section 2 (line 84) that the high-frequency wavefield is generated by anthropogenic mining activity (machinery, trucks). We also added the comment (line 99) that the coda portion in the 6–30 Hz frequency band consists of "surface waves and multiply scattered body waves" and clarified the specific definition of the coda window used in this study.

"The high-frequency seismic wavefield in this active mine environment is generated by continuous anthropogenic noise sources, including haul trucks, crushers, and excavation machinery."

"In this frequency range, the coda portion likely consists of surface waves and scattered body waves trapped in the near-surface heterogeneities. The coda portion considered in this study corresponds to lag-times in the correlations between 1.2 times the direct S-wave arrival time and 2s for both the causal and acausal sides of the correlation. This window seeks to avoid the direct arrivals while capturing the multiply scattered energy most sensitive to medium changes."

- Figure 4 shows an increase of the velocity before the drop, what could be the reason ? A progressive drying of the material ?

Response to Comment 9:

The slight increase in velocity prior to the blast is an interesting feature. It could indeed be related to the drying (stiffening) of the shallow subsurface, as the period preceding the blast was relatively dry. We have added a brief note acknowledging this trend.

Action taken:

We have added a brief mention in the text describing Fig. 4 (line 124 in Section 3) that the slight pre-blast velocity increase may be attributed to drying of the slope material.

"We also observe a slight increase in velocity prior to the initial velocity decrease. This trend likely corresponds to the drying and stiffening of the shallow subsurface material during the relatively dry period preceding this."

Finally, although I liked initially the short title, I wonder if it's a very good message to send?

Response to Comment 10:

We understand the reviewer's caution. We chose the title 'Did they blow it?' as a question because it reflects the hypothesis we set out to test: Did the blast trigger the collapse? Our conclusion possibly reveals a more complex answer (the blast caused damage, but the rainfall was likely the final trigger). We feel that the title captures the investigation's narrative without being accusatory. However, if the Editor feels it is inappropriate, we are open to changing it.

Action taken:

We have kept the title for now but are happy to defer to the Editor's decision on this matter and would suggest the following alternative: "Seismic velocity changes during mine slope failure"