Thank you very much for the detailed review that helped improve the manuscript. We have addressed all comments below.

The authors' responses are written in blue font.

Lines 15-17: Can authors mention the type and density of vegetation present in the area? It is also an important parameter influencing the accuracy of the measurements and can be correlated to seasonal changes.

Thank you for the comment. You are correct in stating that seasonal changes in vegetation cover can also affect the correlation. It is, however, expected that large changes in the surface appearance due to vegetation would cause the correlation to fail, leading to missing pixels rather than a systematic bias pattern as produced by illumination changes. Our study area is located at high elevations (~2500 - 4500 m asl) and characterized by sparse vegetation cover, mostly shrub and grassland steppe (Angillieri et al., 2020). It is located in the transition zone from the humid foreland to the arid Central Andean Plateau. Seasonal greening is only observed in the lower part of the catchment below 3000 m that receives more moisture, while the upper hillslopes, including the landslide surface at 4000 m, are vegetation-free. The absence of dense vegetation makes the Del Medio catchment ideal for studying the impact of illumination changes, without vegetation effects. If vegetation were to compromise our results, we would expect to see an East-West gradient in the bias distribution, which, however, is not the case (the eastern part of the catchment receives more moisture and has a higher vegetation cover than the higher-elevation, western part). Instead, the measurement bias correlates well with moving topographic shadows.

We have added a sentence in the description of the study area to inform the reader about the local vegetation cover and added a paragraph in the results to explain why the effects of vegetation cover are disregarded:

"The influence of other seasonal variables, such as the changes in humidity and vegetation cover, is disregarded due to the scarcity of vegetation at 4000 m, particularly in the upper part of the Del Medio catchment. We do not observe an East-West gradient of the signal strength that mimics moisture transport and vegetation-cover gradients in the catchment (the lower elevation eastern part of the catchment receives more moisture and has a higher vegetation cover). Dense deciduous vegetation may compromise correlation results of cross-seasonal correlation pairs in other study areas. However, it is expected that changing vegetation cover results in random mismatches as the surface becomes increasingly dissimilar, and not a systematic bias as is the case for illumination changes."

Lines 38-39: Why not mentioning the seasonal variations such as illumination, humidity, and vegetation changes that can create ground displacement measurement biases? Indeed seasonal shadow refers to exactly? Shadows occur because of topography or obstruction, independently from the seasonal conditions; however their pattern is correlated to illumination conditions subject to seasonal changes (then at the origin of the oscillatory pattern in the time series). Please be more specific in the description of this process.

We have refined that sentence to include seasonal variations and make a distinction between shadows and seasonal illumination changes.

Lines 40-41: The sentence “Erroneous measurements will degrade the quality of inverted displacement time series, depending on their magnitude and distribution” reads odd. Erroneous measurements will anyways degrade the inverted displacements. It is the bias in the inverted displacements that will depend on the magnitude and distribution of these erroneous measurements. It is maybe just a matter of rephrasing.

We have removed the sentence to avoid confusion. The problem of erroneous measurements should be clear also from the previous lines.

Lines 47-48: What about humidity and vegetation changes?

See previous point. Vegetation changes are disregarded (a) due to the sparse cover and (b) because of the observed pattern, which suggests a strong link between measurement bias and topography, not to vegetation.

Lines 55-56: This is a great point.

Thank you.

Line 77: It would have been interesting to look at the vertical component from stereo imagery, at least for the cumulative displacement, to be able to infer the possible vertical deformation and rotation of the landslide body as a result of its downslope movement. I know it is not the main focus of this study though.

Agreed, but unfortunately, we did not have high-resolution multi-temporal stereo imagery available, and we also consider this to be outside the scope of this study.

Line 83: The displacement rate was indicated to be ~2.5 m/yr in the introduction. It is indicated 2-5 m/yr here. Which one is the correct one?

Changed to 2-5 m/yr. This referred to the previous paper (Mueting and Bookhagen, 2024) where only pairwise measurements were taken. In the coherent timeseries (after time-series inversion), the rates are, however, closer to 2.5 m/yr, thus the confusion.

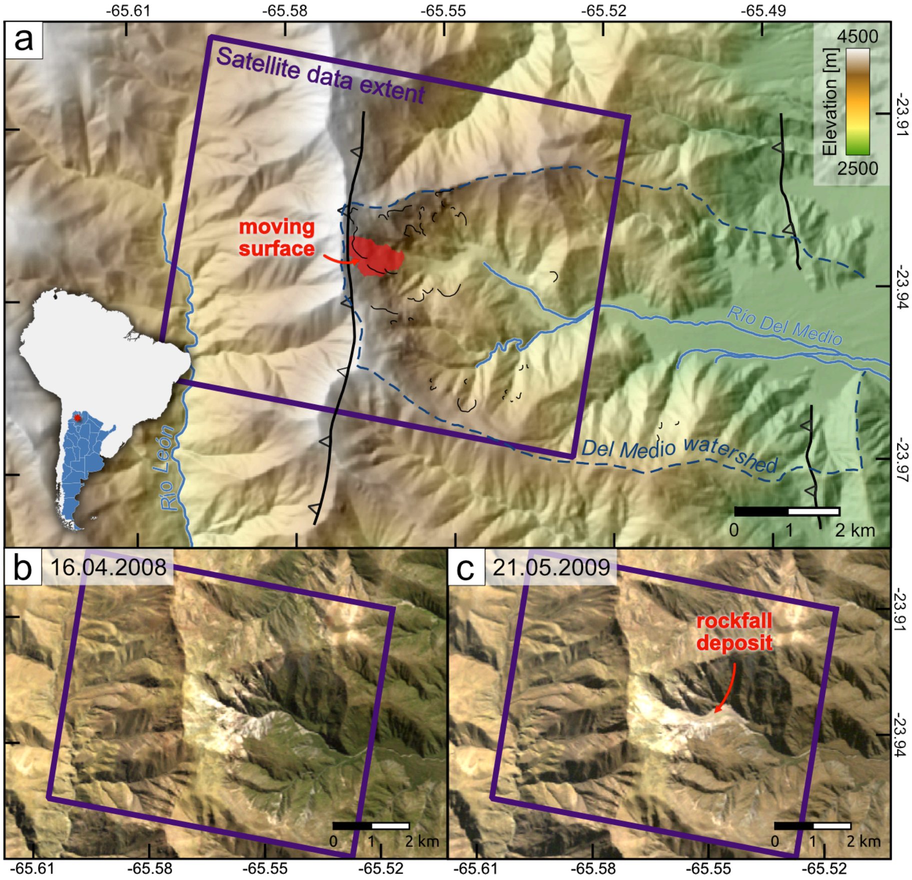
Lines 89-91: I agree that the question of whether the landslide behavior is being affected by the rainfall is an important question. Though the relation between rainfall and landslide have been well studies from observations on other case studies as well as analogue experiments. Authors should raise more specific questions that can be answered using the dense observations they propose, such as: Do the landslide movement reacts seasonally in response to the varying seasonal conditions? If not, what type of response does it show (linear for example) and why (controls of the kinematic of the sliding or bulk creeping)? Can we identify a delay between the start of the rainfall events and the landslide acceleration? Is there a minimum rainfall intensity (or volume) that is required before we start seeing any acceleration in the landslide movement? Further reading the study, it appears that the authors do not aim at studying any seasonal changes in the landslide behavior as it would appear correlated to the seasonal bias that is removed from the inverted time series (and only the inter-annual changes are kept). Authors should be more specific regarding the frequency of the landslide displacement signal they are trying to retrieve (and separate from the seasonal one). This is an unclear point all along the study, that I also raise at other places later in my review.

Figure 1: Please indicate the source of the DEM (SRTM, Copernicus?) and its ground resolution? Black lines showing the scarps and faults should be thicker, and possibly indicated in a legend directly in the figure. Text within panel (a) could also be larger (elevation legend, “moving surface”, and “satellite displacement”). Does the purple bow in (a) is similar to that in (b) and (c)? It seems like the ratio of the landslide size to the box size is bigger in (b) and (c). It could be nice to have an arrow indicated the landslide in (b) and (c). Eventually, can authors mention why the landslide is of a whitish color? Is it because if the lack of growing vegetation and soil on the surface that therefore reflects more light? Surprisingly, it appears more whitish in the “before” image, which could be related to the changing illuminations conditions. To this regard, it would be interesting to have information on the sun elevation and azimuth to visually assess the relation between illumination conditions and shades in the images (as we see there are more shades in panel (c)).

The DEM is the Copernicus DEM (information added to the Figure caption). Lines and text size were enlarged. The purple box in (a), (b), and (c) is identical. Perhaps the ratio looked different because the linewidth was different, which has been adjusted. An arrow has been added to Panel (c) to indicate the location of the rockfall deposit. The landslide’s brighter color is likely linked to the active erosion of the quartzitic rocks that are present in the upper part of the Del Medio watershed, while the vast majority of the catchment is dominated by darker Precambrian phyllites (see Figure 3a in Savi et al., 2016). However, illumination differences also affect the appearance because one scene is from October and the other from May, leading to the September scene appearing much brighter than the post-event image. We have therefore chosen to replace the October scene with a pre-event image that is more comparable in terms of illumination (~1 year difference) and appears less bright. Illumination conditions of all scenes are compared in the table below:

Date	Sun azimuth	Sun elevation
2008-10-09	64.33	54.96
2009-05-21	38.39	34.92
2008-04-16 (new)	46.83	42.31

This is the updated Figure:



Lines 101-102: What sampling strategy (frequency? Acquisition geometry? Illumination conditions?) was chosen to select the scenes when too many are available?

As mentioned in the text, imagery was chosen based on snow- and cloud-free conditions and AOI coverage. Illumination was not taken into account as a parameter (only later as a mitigation strategy), because we aimed to cover a wide range of illumination conditions. Acquisition geometry was only considered for PlanetScope scenes to minimize the effect of orthorectification errors. This pairing strategy is mentioned in the following paragraph on image correlation (Section 3.2). In case too many are available, a random scene was selected to have an acquisition approx. every 1-2 months. We have added a small clarification to the text.

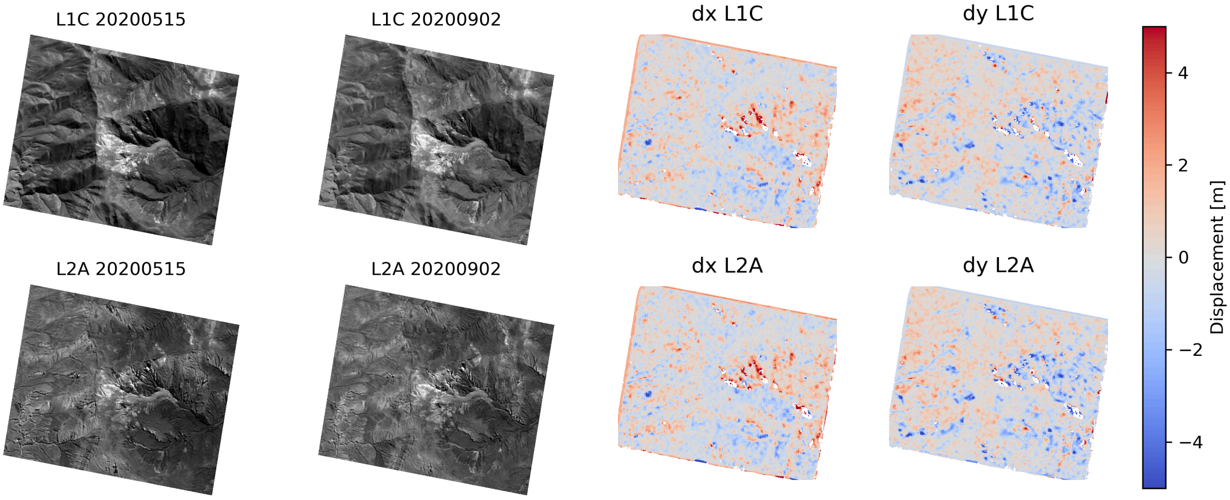
Line 103: I do understand that results from Lacroix et al (2018) show no difference in the results between the difference bands (or too small compared to the overall landslide displacement), though it would be interesting to test it here since the study specifically focuses on the effect of seasonal effects on the displacement measurements. The green band is the most sensitive to moisture changes. Sentinel-2 has a NIR band (band 8) that usually have a higher radiometric contrast, better feature stability, and less atmospheric noise. Even though all bands are correlated and that the results will not strongly be affected, it would be interesting to see how the choice of the band affects the results. See https://custom-scripts.sentinel-hub.com/custom-scripts/sentinel-2/bands/ for a description of the characteristics of all the bands. On the same subject, it would be interesting to mention that one should not cross-correlate a reconstructed RGB image as each band have different geometric distortions which would add up in the reconstructed RGB and so the final correlation product.

We fully agree that a reconstructed RGB image should not be correlated since there may be interband misalignment issues, as e.g., documented for PlanetScope data (Aati et al., 2022). We have added a sentence to highlight this consideration.

A full comparison of the usage of different bands for all four different sensors used in this study is, however, out of scope and covered by previous work (e.g., Lacroix et al., 2018).

Line 105: Why not using the Sentinel2 L2A that have been corrected from atmospheric effects?

For Sentinel-2, L1C data is commonly used across many studies focusing on offset tracking applications (e.g., Lacroix et al., 2018). We made a comparison between the L1C and L2A products for an exemplary Sentinel-2 image pair, and the differences were very subtle (see below).



Line 117: Would it be possible to have an assessment of the effect of resampling (and so of the introduced aliasing) on the correlation results? It would be interesting to see how it affects the geometry of the shadows and of the resulting the displacement bias. Though I agree that upsampling is a better strategy than downsampling for preserving the PlanetScope image resolution and reducing the correlation noise. After further reading, I understand that the goal of upsampling the images in the current study is not to be able to cross-correlate images from the different sensors, but to compare correlation results made at similar ground resolution because of the effect of image resolution on noise (as proposed in Antoine and Liu (2025). Please specify this aspect as, from the current text, the reader is later expecting multi-sensor cross-correlations.

We have clarified in the text that only same-sensor image pairs are correlated, and the upsampling is done to better compare the resulting displacement fields, not the initial imagery. The effect of resampling is shown in Figure 5.

Line 125: Which half-year threshold? Maybe I missed this information earlier in the text.

This is an arbitrary threshold that was set to get an initial assessment of the expected displacement magnitude. It is not the final one. Based on the initial displacement estimate, we inferred which image pairs would be “worth” correlating, considering the image resolution and the detection threshold, assumed to be 1/10th of the native pixel size. This dynamic threshold results in more correlation pairs in times of rapid movement, while in times of relative quiescence, especially before the re-activation in early 2017, the temporal baselines are longer, and we do not expect to see any landslide displacement. This effect is well illustrated in the network plots in the supplementary material (Figures S3 – S5). We have also revised our text to better explain our dynamic thresholding approach.

Line 127: What is the range chosen for the common perspective (±10º around a given looking geometry for example)?

Much less than ±10º – such a difference would introduce severe orthorectification errors given the large vertical changes that occurred at the landslide. The threshold set was ±0.6°, analogous to Mueting et al. (2024). Most scenes, however, have a viewing angle difference less than that. We have clarified the threshold in the text.

Lines 128-130: It is maybe worth clarifying here that the study aims at correlating separately the different sates of images (L8, S2, and PlanetScope) to create three sets of displacement products that can be compared. Correlating all the pairs together is possible (if one focuses on the physical signal and needs as many data as possible); however, this would lead to mixing numerous confounding factors and a more difficult separation of the source different measurement biases (DEM, image and sensor distortions etc).

We have added a clarification that the matching across sensors is not advised, because it introduces further complexities.

Line 133: Do the authors considered a uniform shift over the entire image or was polynomial form fit to the correlation results (with the landslides masked)? Can the authors justify the choice of a uniform shift. Sentinel2 and Landsat8 images are not subject to the same type or image distortions and orthorectification biases than higher-resolution images acquired with more oblique geometries; however, I am wondering whether this uniform assumption is entirely valid.

All postprocessing modifies the original measurements to some degree. When there is no need for more complex geometric corrections, a simple shift is the most rigid transformation that can be applied. Since we did not observe any large-scale ramp or even more complex errors (as is the case for PlanetScope) in the Sentinel-2 and Landsat-8 displacement fields, we believe our choice of a uniform shift is justified and helps to preserve the original measurements as closely as possible. Also note that our study area is small compared to a full Sentinel-2 or Landsat-8 scene. At a larger scale, perhaps a polynomial fit correction would be more appropriate.

Line 136-137: How is the topography accounted for in the polynomial correction. This is not clear. Please elaborate as it is an important point.

The polynomial is fit as follows:

$dx, dy = aX^2 + bY^2 + cZ^2 + dXY + eXZ + f\,YZ + gX + hY + iZ + j$

where $\mathrm{d}x$ and $\mathrm{d}y$ are the estimated displacements in an EW and NS direction, X and Y are the corresponding pixel locations, Z is the elevation (derived from an external reference DEM), and a–j are the coefficients which are estimated using least-squares optimization (Mueting et al., 2024).

We have modified the sentence in the manuscript to clarify the variables taken into account for the fit, but refer the reader to the relevant study for details.

Lines 156-164: This is a very interesting approach! However, I suppose that ASP gives a “correlation score” map at the end of the correlation process as COSI-Corr and MicMac do. In this case, why not using it to weight the different pixels as well? It could also be interesting to see if weighting the displacement maps based on the temporal difference between correlated pairs would have an effect on the inversion result (as in Lacroix et al., 2019). I see that temporal difference is accounted for in the regularization matrix when inverting the PlanetScope correlation results, but this is different and not applied to S2 and L8. I guess in the case of a more complex acquisition geometry, one could use the same principle to weight the displacement maps based on the similarity of image look angles (but this is not the case of S2 and L8, and the authors already put a constraint on the PlanetScope image geometry).

Weighting based on the correlation score would indeed be an interesting approach to assess. However, a detailed quality metric is unfortunately not an output that ASP provides. The output is a 3-band raster containing displacements in the EW and NS directions, as well as a binary “Good pixel mask” which is applied to mask unreliable measurements. This mask typically only removes false matches in areas with large shadows or where the surface has become too dissimilar.

Line 186-187: It is not clear here which signal is preserved to analyze the landslide displacement. In fact, I guess the non-linear inter-annual displacement trend is not “removed” as it contains the landslide displacement signal. It is rather “separated” from the rest of the signal that likely include the seasonal variations that can be fit with a sinusoidal function with a frequency of 1 year, and a possible additional linear trend. In this case, this separated signal (using the 13-month kernel) will smooth out any higher-frequency variations of the landslide displacement. This suggest that the authors do not aim at studying the correlation between seasonal water load changes and landslide displacement, but more likely interannual changes due to longer-period climatic variations (El Nino/La Nina for example). Please rephrase, and add the mentioned specifications regarding the higher-frequency (maybe seasonal) landslide displacement signal that will be overlooked. I also suggest the authors add examples of uncorrected and corrected time series with the different components separated (as for example Figure 3 in Lacroix et al., 2019). This would significantly help the reader understand the post-processing step taken by the authors.

We have rephrased Section 3.5 describing estimation of seasonal biases, as it seemed the initial version had caused a misunderstanding. First of all, please note that this section describes our method to estimate the magnitude of the seasonal bias, not the correction per se, although it works alike. The trend (using the 13-month kernel) is only estimated and removed from the displacement signal to obtain the basis for the sine fitting (i.e., center the displacement at zero). Fitting a linear trend here is not useful because the landslide does not move uniformly over time. The smoothed trend will, as you mention, reflect interannual changes only, which is in fact the objective. When interannual changes are removed from the time series, the remaining signal reflects seasonal landslide changes + seasonal oscillations from the illumination bias. To the detrended time series, we fit a sine function whose amplitude is used to quantify the impact of seasonal bias.

Regarding the correction, this best-fit sine fit is subtracted from the original time series. The smoothed trend is not considered further. Therefore, we can assess seasonal velocity changes even after correction. The assumption here is that, unlike bias from illumination changes, seasonally variable landslide displacement (a) only has one direction (downhill) and (b) may be of variable magnitude, or not observed every rainy season. We have evaluated several synthetic displacement signals, to which degree this correction approach is viable in the presence of a true seasonal displacement component (see our response to this specific matter below.

There is a Figure in the Supplementary Material that illustrates the sine-fitting approach (Figure S8 in the revised manuscript).

Figure 2: In relation with my previous comment, it is not very clear whereas the time series of displacement presented in panels d-f were corrected from the seasonal variations. From the text, it seems like this figure presents the final product (after correction) though the seasonal variations are really visible. Are these residuals after correction? Or maybe the un-corrected time series are presented at this stage?

Figure 2 shows the uncorrected time series, while the corrected time series is shown in Figure 7. We have modified the text to make this clearer.

Line 227: Are the seasonal landslide displacement variations really analyzed or only the inter-annual ones? Please refer to my previous comment regarding the post-processing strategy and the separation of the different components.

It is correct that we look for seasonal landslide variations (and inter-annual ones as well). Please see our response to the previous comments.

Figure 3: Which period of the modeled seasonal signal is currently shown? Please specify. Moreover, there is a seasonal East-West signal at the location of the landslide. How do the authors treat this signal given that it could both be measurement bias but also seasonal changes in the landslide behavior triggered by temperature and humidity changes? Please elaborate on this aspect.

There is only one amplitude of the sine function, which is constant for all periods and plotted in Figure 3. And yes, the separation between illumination bias and potential true responses in landslide motion is the objective of employing different correction methods. This problem is discussed in the dedicated Section 4.4.

Line 236: This is because decreasing the pixel size and therefore the correlation window size will reduce the introduction of larger scale and amplitude seasonal signal. Maybe specify.

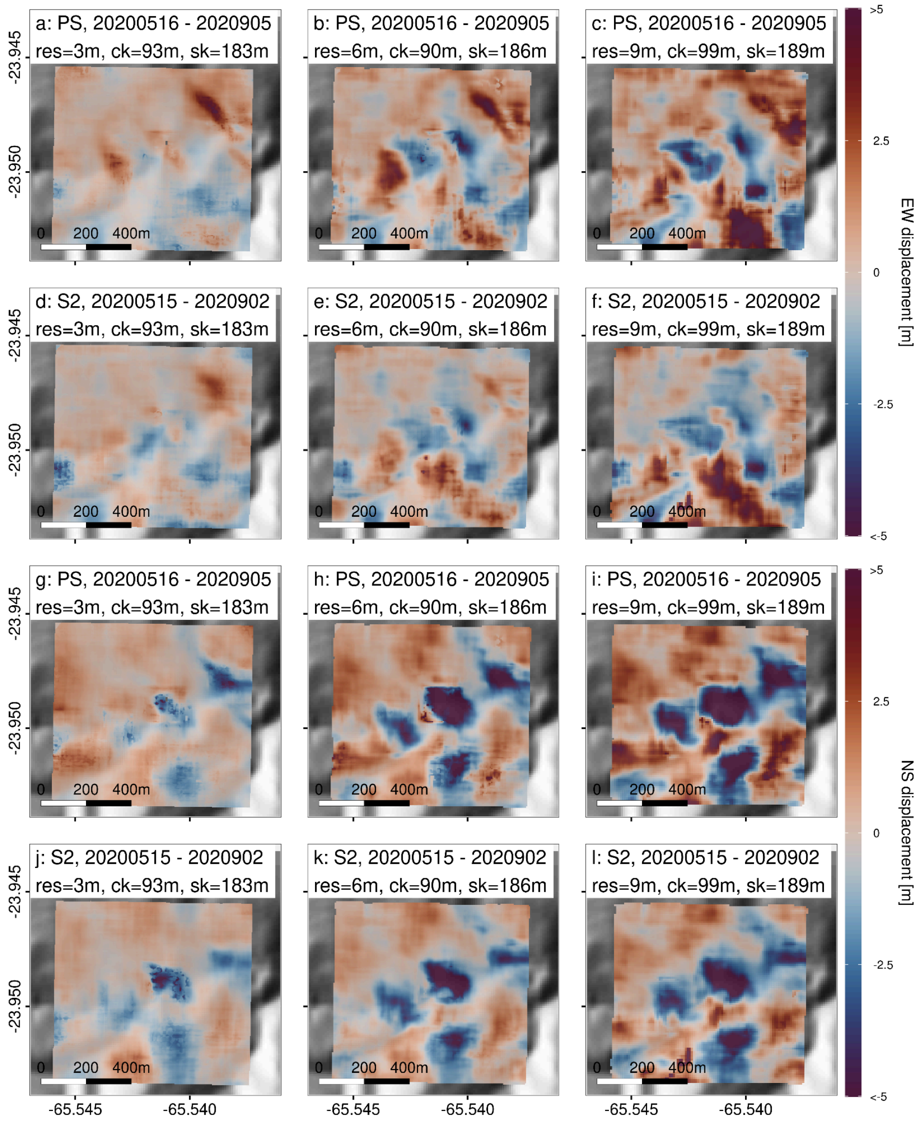
But the effect was also observed when the correlation kernel was kept approximately constant in terms of actual area covered, making sure that the same features are included. Please see the adapted kernel sizes depending on the input resolution in Figure 5, and also our response to the reviewer’s previous point on line 243.

Line 241: The term “however” suggests a difference between the method used and the other ones. Maybe use “also” as the effect of resolution is seen for all methods?

“However” was exchanged with “also”.

Line 242: Of course, the subpixel matching precision depends on the resolution as it is usually around 1/10th of a pixel. However, I am not sure that the matching between upsampled pixels will be more precise as there is redundancy in the information and a greater number of positively matching pixels within a given area. This would need to be studied in more detail.

Indeed, the upsampling does not add additional information. For detecting finer-scale spatial variability in landslide motion, the use of higher-resolution data is therefore essential. However, for comparability across sensors, resampling to a common resolution is useful to ensure that results are produced using common processing parameters (e.g., same kernel size, same number of pixels used for matching). We have updated our analysis comparing Sentinel-2 and PlanetScope resampled to different resolutions to not only include a downsampling, but also an upsampling example. We used images from similar dates over the same area and observed similar effects, with the Sentinel-2 displacement fields (lower native resolution) appearing slightly smoother. The results for Sentinel-2 are now included in Figure 5:

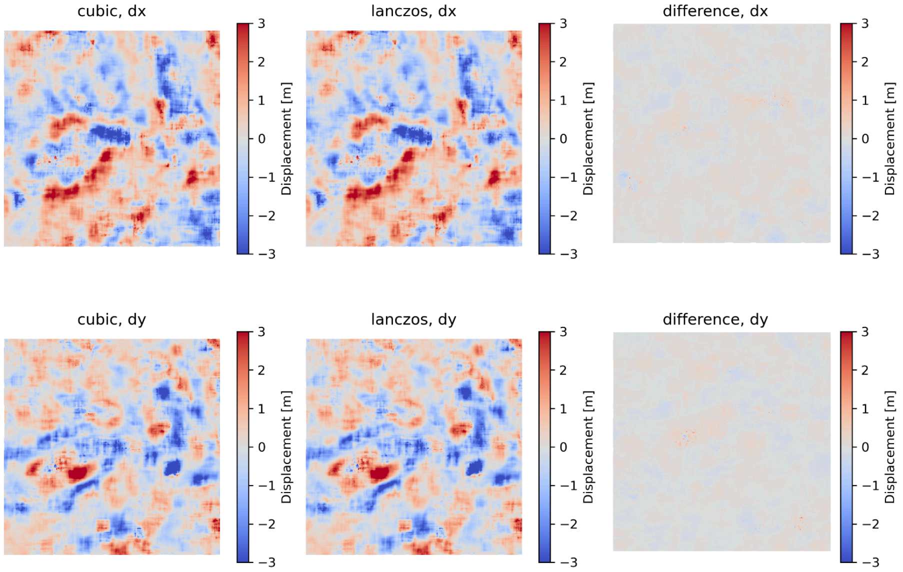


Line 243: Why would the number of pixels be different in the correlation windows if the correlation windows are set in pixel units?

See our response to one of the previous points. The correlation kernel was kept approximately constant in terms of metric area covered, which means fewer pixels at lower spatial resolution.

Line 244: I do agree that the amplitude of the seasonal signal that will be captured is function of the image resolution (as discussed right before), and therefore that it makes sense to correlate the images at similar resolutions if one wants to focus on the seasonal signal. however, there will be aliasing introduced into the correlation results, affecting the subsequent time series. How is this identified?

We did not observe any strong aliasing effects. We did a test repeating the correlation with a clip of two Sentinel-2 scenes over stable terrain (20200515 and 20200902). We upsampled these clips once with cubic interpolation and once with Lanczos resampling, which reduces aliasing artifacts. The displacement fields and the difference between them are shown below. We consider the difference minor, especially in comparison with the magnitude of biases introduced by the variable illumination.



Line 255: Missing “,” between “For the correction” and “Lacroix et al. (2019)”.

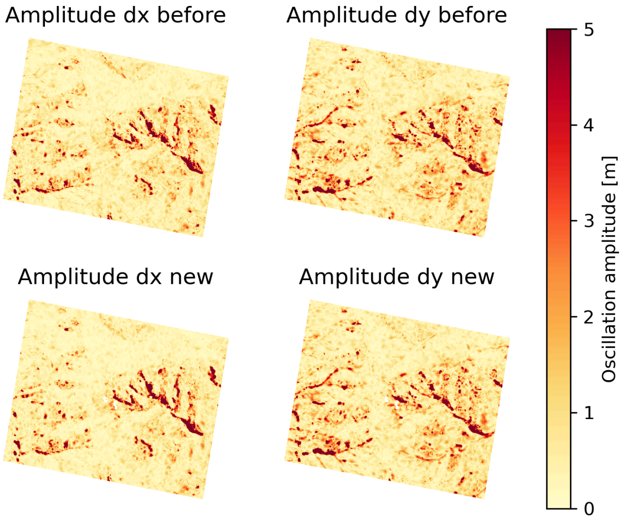
A comma was added.

Lines 265-268: Could a high-resolution DEM be derived from Pleiades stereo imagery for that area to resolve this issue? Maybe even multiple Pleiades DEMs could be derived if more imagery is available. WorldView imagery could also be used but accessing those images is more complicated unless authors have an ongoing collaboration or project with American institutions such as NASA and USGS.

Yes, as mentioned in the text, we do suspect that a higher-resolution DEM depicting topography closer in time could improve this correction approach. We do, however, believe that it is of interest to evaluate how well the mitigation can be performed using more easily accessible and global elevation data, such as the Copernicus DEM.

Lines 272-275: Would it be possible to assess a per-pixel oscillation in stable terrain areas and interpolate over the landslide area, therefore not including the true landslide displacement? This would not be ideal as slip and aspect can be different at the location of the landslide compared to its surrounding, but I am wondering whether it could be an option in some cases.

Yes, this is the approach that was described by Lacroix et al. (2019). We tried to replicate that approach and estimated the seasonal bias signal for stable areas using bins of common slope and aspect and extrapolated these values to the landslide surface (sine fit stable areas). However, we observed that the reduction was not as effective compared to a pure per-pixel approach, likely because of the lower resolution topography and topographic changes that occurred over the landslide since the DEM acquisition. In comparison with the initial version of the manuscript, we refined this approach slightly by updating the topographic groups using slope and aspect calculated on the initial DEM resolution. We also subtracted the median, not the mean, time series instead to reduce the influence of large errors from mismatches in heavily shaded regions. This slightly improved the results for this correction method, but not substantially, see below (also updated in Figure 6):



Line 77: Should it be “responses of the displacement” instead of “responses to the displacement”? What are the arguments for supporting that seasonal response should be variable? If the structure of the landslide has not changed much over one year, its response will also be the same isn’t?

Thank you for spotting this; it should indeed be “responses of the displacement”, or even better, “responses of the landslide motion”. A variable response could be linked to variable precipitation rates in different years – some rainy periods may be drier than others. For the landslide, we postulate that acceleration is linked to single extreme rainstorm events – these may not occur every year.

Line 279: What about other strategies based on ICA decompositions? These are proven quite efficient at separating signal from different source. I think these following studies used

ICA to decompose the signal: Ali et al. (2020), Beaud et al. (2022), and Bontemps et al. (2018).

According to our understanding, these studies did not use ICA decomposition to assess the impact of seasonality. Ali et al. (2020) used differences in solar illumination as a pairing criterion to reduce the impact of seasonal bias, Beaud et al. (2022) used principal component analysis to reduce the noise level, and Bontemps et al. (2018) evaluated seasonal biases as a source of uncertainty, but did not choose a dedicated mitigation approach.

We believe the reviewer may be referring to seasonal decomposition strategies into a trend, seasonal, and noise/residual component. The problem here is that a recurring seasonal signal in the landslide motion would be incorporated into the seasonal component. Our objective is to unmix true seasonality from false seasonality, i.e., illumination bias. We therefore approximate the seasonal bias with a sine wave so that anything deviating from that (e.g., directed response to landslide from rainfall, downhill only) is not removed from the time series.

Line 301-302: Very nice approach indeed! However, I do not understand why the temporal difference is not also accounted for in the image-pair selection. Lacroix et al. (2019) proposed an approach to do so.

We did take into account the temporal difference between scenes; however, we did not set a maximum threshold but a minimum temporal baseline to be able to detect landslide displacement. We did not restrict correlation pairs with large temporal differences because, as argued before, vegetation effects and anthropogenic influence, which would cause decorrelation over time, are limited in our study area.

Lines 303-311: This is a good point. But maybe the best strategy is then to keep all the pairs, but weighting the correlation products based on the temporal, illumination, and viewing angle similarities)

We did discuss and experiment with different weighting approaches. In this study, however, we focused on weighting factors that would address seasonal biases. But yes, to mitigate other effects, a weight based on multiple factors could be considered, e.g. temporal baseline, correlation score (when available), or sun-angle differences.

Line 310: I do not understand this part. One would expect seasonal biases to have different characteristics during different seasons. Moreover, the regular sampling mentioned has a period of 1-month. I do not understand how a seasonal signal would average to zero over a period of 1 month. Please clarify.

For this synthetic example, we introduced errors that were dependent on the temporal baseline between two dates (the highest error for scenes with a half-year difference). But indeed, we did not consider whether specific season matches have more errors than others. Because the impact of temporal sampling is not further explored in the manuscript, we decided to remove this sentence and the corresponding Figure from the Supplementary Material to avoid confusion.

Lines 324-326: The weighting approach seems more efficient (similar to the since correction) for the PlanetScope time series compared to the L8 and S2 ones. Maybe this could be due to the fact that L8 and S2 images are acquired overall in similar illumination and look angle conditions (at the same time in general) compared to the PlanetScope that has a more variable imaging schedule and geometry? Then the weighting would be more impactful. Eventually, there is a shift between the original and the corrected time series derived from L8 imagery. Does this come from additional corrections which are not mentioned here? Please elaborate.

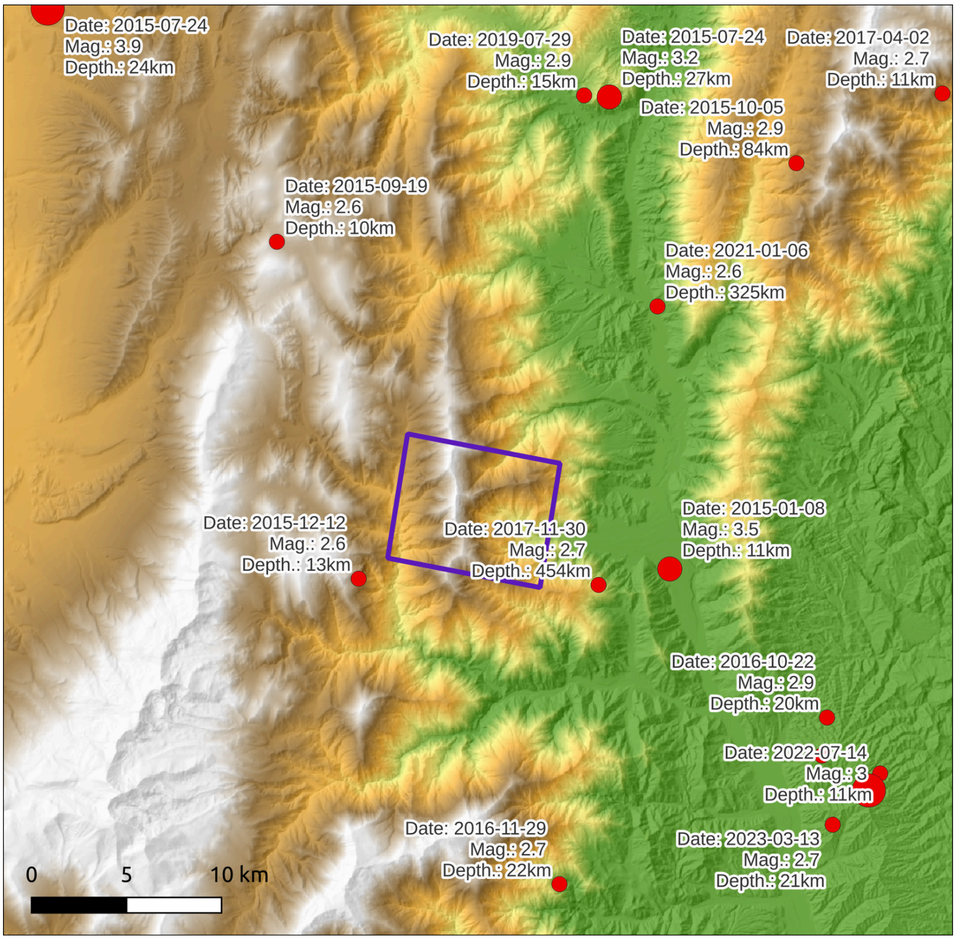
In Figure 6, we only compared the different mitigation approaches for PlanetScope data, not for different sensors. In Figure 7, on the other hand, we compare the two most effective approaches (sine fit per pixel and pair selection based on similar illumination conditions, not weighting) for Landsat-8, Sentinel-2, and PlanetScope. In the revised manuscript, we have updated Figure 7 to include all correction approaches, even when there is very little difference to the original signal. In the initial Figure, indeed, a shift was observed for the Landsat-8 time series. We have investigated this, also concerning a point raised by the second reviewer, and found that this was linked to noisy pixels around the border of the landslide. In the revised manuscript, we have updated our landslide mask by applying an inward buffer of half the correlation kernel size, which excludes pixels close to the transition between stable and moving terrain. All applicable Figures were updated accordingly.

Line 360: The lack of correlation between the landslide movement and the seasonal variations has not been discussed much. This needs more elaborated discussion before assuming there is no string correlation. See also my previous comment regarding the frequencies of the signal that are separated, and how some of the landslide seasonal signal could have been missed.

We have elaborated more on the observed signal and the separation between seasonal bias and true displacement, and supported this through further assessment using a synthetic displacement signal. It is true that by using a sine-fit per pixel, part of a true seasonal component could have been removed. Since no ground control data is available for the landslide, reducing seasonal biases through selecting similarly illuminated scene pairs is likely the safest choice at the expense of increasing the impact of errors linked to other sources due to the lower number of overall measurements. We have adapted our discussion to reflect this.

Line 367: A radius of 100 km is not enough if a large subduction earthquake; however, there is no record of such event in 2017 (Michel et al., 2023). Beside this, authors need to support their statement citing a seismicity catalogue for the given area. All earthquakes are not published on the USGS website or other if they are not large or impactful enough. Maybe small earthquakes occurred locally?

Thank you for the remark. We did check an additional local earthquake catalog from the Instituto Nacional de Prevención Sísmica (http://contenidos.inpres.gob.ar/buscar_sismo). Indeed, this included smaller earthquakes closer to the landslide; however, these events occurred either not on a date matching the acceleration phases, at great depth, or had a very low magnitude (see the map below with the study area marked by a purple rectangle). We have added a clarification to the text.



Line 381: This still remains to be better demonstrated (see my previous related comment).

We have supported this conclusion with additional tests using synthetic data and a more careful evaluation of the effects of different correction approaches.

Line 382: This sentence reads incorrectly. The “periods” are not “documented in landslide motion”. I guess what the authors meant to write is that studies showed that landslide motion can be significantly influenced by periods of extreme draught and rainfall. Please correct the sentence.

The sentence has been corrected.

Figure 10: What is the reference used to calculate the difference maps? This is not indicated in the caption nor in the figure. Did the author do a difference between the landslide displacement map (corrected) and the uncorrected map, and so separately for each sensor? Can authors also indicate the location of the box chosen for plotting the time series in j-k? (see another comment below to this regard). This should also be indicated for profiles from Figures 2, 7 and 9.

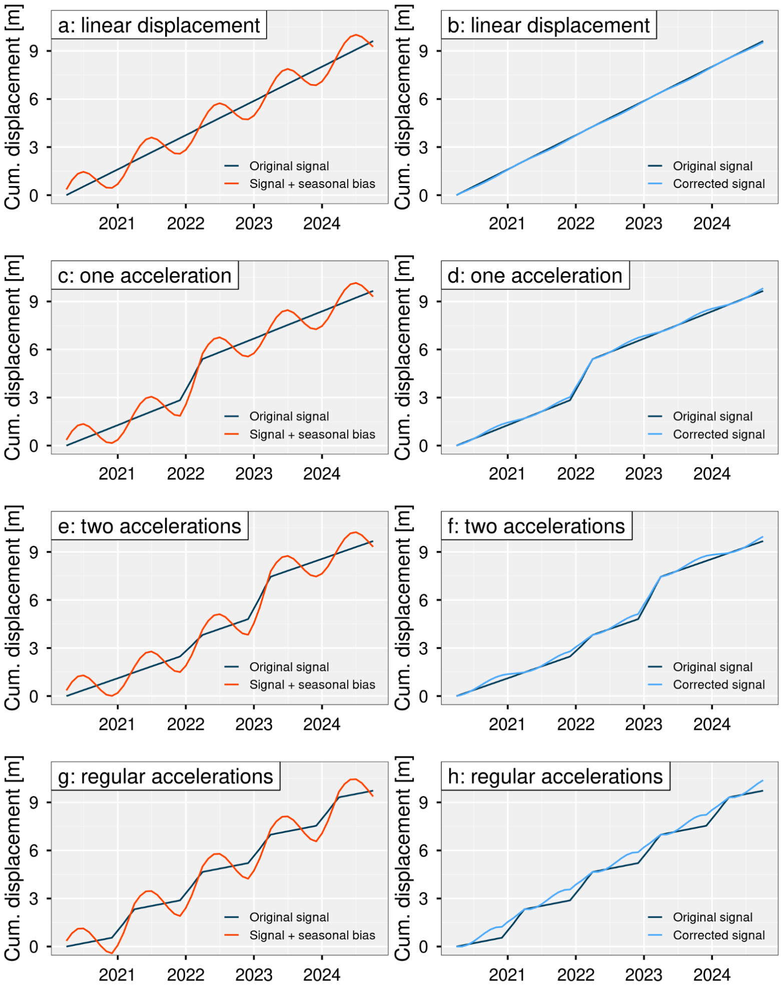
There is no fixed reference; the difference shown is always between the total displacement estimated for two different sensors: PlanetScope - Landsat-8 (a, d), PlanetScope - Sentinel-2 (b, e), and Sentinel-2 - Landsat-8 (c, f). This is indicated in the description of the individual panels. The first row shows the uncorrected displacements, the second row the displacements corrected through a sine fit per pixel. We have added the landslide outline used to extract the displacement time series in panels j-k.

429: I do not understand this sentence. The closest correspondence rather seems to be achieved between the S2 and PlanetScope images. The L8 time series analysis rather shows a deviation towards the later dates with a lower inferred cumulative displacement over the total period (Figure 10 j,k). Moreover, what could explain that less cumulative displacement is detected using L8 images? Can this be related to the image native resolution that is lower and therefore not capturing more local and higher amplitude displacements? Or maybe to a lower image radiometric or geometric (orthorectification) quality? Or maybe this is just a matter of spatial representation of the point chosen for displaying the time series. In fact, in Figure 8, it does not seem that the total displacement inferred from the L8 images is lower than inferred from the S2 and PlanetScope images. Can authors explain better how they select the point for displaying the time series (location, width, and interpolation if choosing a box)? The location and width of this area should also be indicated in all the figures.

We hope Figure 10 has become clearer with the updated Figure labels. With closest correspondence, we refer to the total estimated displacement over the entire terrain, not the landslide time series. For the pixel extraction over the landslide surface, we have updated the relevant mask to exclude border effects (see our related reply to an earlier comment). This mask was added to the maps shown in Panels a-f.

Line 458: Indeed, the correction method used can remove seasonal landslide displacement signal. This is not well discussed int eh manuscript where authors assume several times that the landslide does not present seasonal motion, but maybe it has just been removed during post-processing. This is an important point of this manuscript that needs to be clarified.

We have updated the manuscript with additional analyses on what the effect of the sine fitting approach may have for different signals (linear, individual acceleration phases, regular seasonal responses). The results are shown in the Supplementary Material, Figure S21:



The per-pixel sine correction approach is most effective when the occurrence and magnitude of true seasonality are irregular. For the Del Medio landslide, our interpretation of the signal observed (across different correction approaches) points more towards irregular accelerations linked to extreme rainfall events. True seasonal responses would be expected to also reflect in the time series retrieved from similarly illuminated pairs. However, since there is no ground control data for the landslide, we conclude that the time series based on image pairs with similar illumination is likely the safest choice for the subsequent interpretation of the landslide signal, since this fully avoids any alteration of the initial measurements. Closely assessing the PlanetScope time series from similarly illuminated pairs only, we find that there may even be two more small acceleration phases that were not visible in the sine-corrected time series.

Eventually, all correction methods come with a trade-off and should be chosen based on the study area and available data. We have extended the discussion in the manuscript regarding the benefits and limitations of each mitigation strategy.

Line 468: Indeed, weighting factor based on temporal changes might be more efficient. Please mention it.

We have added this for consideration.

Line 469: Choosing this specific approach implies that authors focus on the inter-annual signal, and do not aim at analyzing the possible seasonal landslide movement variations (removed during the correction using the since function). Please specify.

Please see the earlier clarifications. The smoothed trend is just removed for the estimation of the sine fit, but the actual time series is not smoothed. That is why seasonal changes can still be detected, provided that they exceed a certain magnitude.

Line 479: This remains to be better demonstrated (see all my previous comments related to this point).

Please see the updated interpretation of the displacement time series based on image pairs with similar illumination and the related discussion in the revised manuscript (Sections 4.5 and 5.1).

Line 480: What about fire? Was there any significant fire that would have removed the vegetation and therefore making the landslide more unstable? Just searching in Google, I could find there were significant fires in 2016 and 2017 in Argentina in the provinces of Jujuy (where the study area is) and Salta (just south of it) that were affected. See https://www.batimes.com.ar/news/argentina/argentina-ablaze.phtml. See also interesting references in https://science.nasa.gov/earth/earth-observatory/fire-threatens-rare-forests-in-argentina/. I think this is worth looking into as there is a known correlation between fires and landslides (Kean and Staley, 2021; and https://scienceexchange.caltech.edu/topics/sustainability/ask-expert-sustainability/debris-flows-michael-lamb).

Thank you for this interesting remark. Although both references refer to debris flows rather than slow-moving landslides, we have checked satellite imagery of the Quebrada del Humahuaca from 2017 and 2018 and did not observe any burn scars. In addition, we have consulted the MODIS Fire_cci Burned Area Pixel product (Padilla Parellada, 2018) for the years 2014-2024. This dataset indicates the large majority of burned areas further to the East in the Andean foreland beyond the drainage divide of the Humahuaca basin, where more dense vegetation (potential fuel) and agricultural areas can be found. No burned areas were detected in the Del Medio basin.

Lines 591-592: The link https://doi.org/10.5194/egusphere-2023-1698 in “Mueting, A. and Bookhagen, B.: Tracking slow-moving landslides with PlanetScope data: new perspectives on the satellite’s perspective, EGUsphere, 2023, 1–36, https://doi.org/10.5194/egusphere-2023-1698, 2023.” Takes to the preprint, not the journal article. Please update.

Thank you for catching this. The reference has been updated.

References:

Aati, S., Avouac, J.-P., Rupnik, E., and Deseilligny, M.-P.: Potential and Limitation of PlanetScope Images for 2-D and 3-D Earth Surface Monitoring With Example of Applications to Glaciers and Earthquakes, IEEE T. Geosci. Remote, 60, 1–19, https://doi.org/10.1109/TGRS.2022.3215821, 2022.

Angillieri, M. Y. E., Perucca, L., and Vargas, N.: Spatial and temporal analysis of debris flow occurrence in three adjacent basins of the western margin of Grande River: Quebrada de Humahuaca, Jujuy, Argentina, Geografiska Annaler: Series A, Physical Geography, 102, 83–103, https://doi.org/10.1080/04353676.2020.1744075, 2020.

Lacroix, P., Bièvre, G., Pathier, E., Kniess, U., and Jongmans, D.: Use of Sentinel-2 images for the detection of precursory motions before landslide failures, Remote Sensing of Environment, 215, 507–516, https://doi.org/https://doi.org/10.1016/j.rse.2018.03.042, 2018.

Mueting, A. and Bookhagen, B.: Tracking slow-moving landslides with PlanetScope data: new perspectives on the satellite's perspective, Earth Surf. Dynam., 12, 1121–1143, https://doi.org/10.5194/esurf-12-1121-2024, 2024.

Padilla Parellada, M. (2018): ESA Fire Climate Change Initiative (Fire_cci): MODIS Fire_cci Burned Area Pixel product, version 5.1. Centre for Environmental Data Analysis, 01 November 2018. https://doi.org/10.5285/58f00d8814064b79a0c49662ad3af537.