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

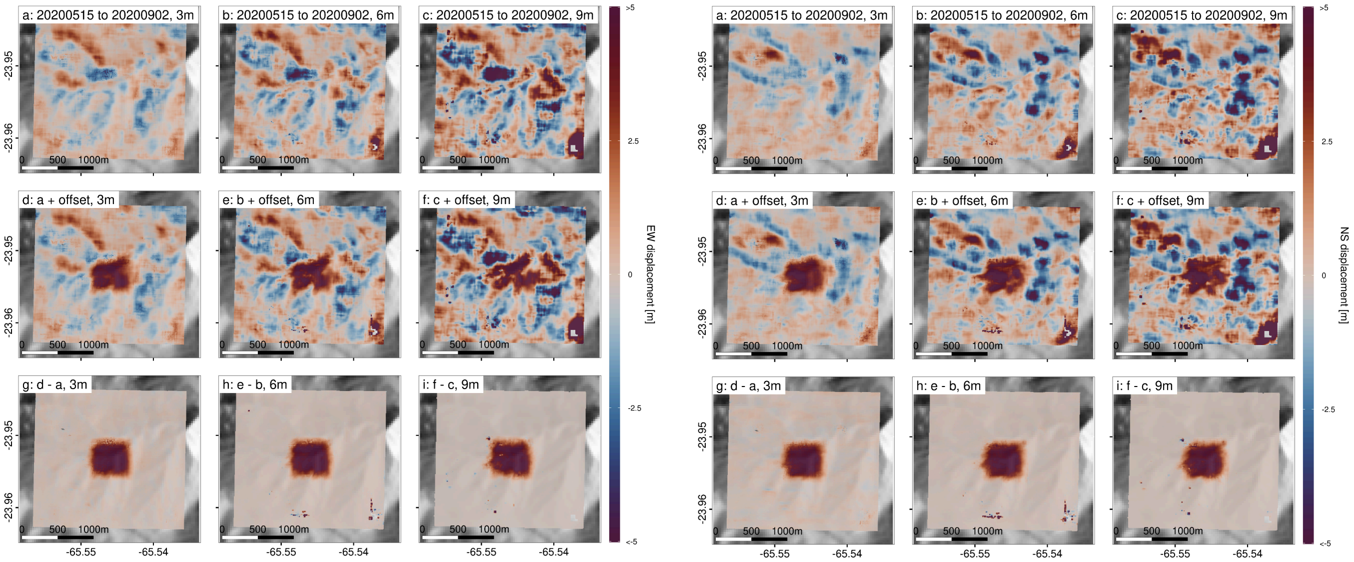
The authors' responses are written in blue font.

The comparison of the different mitigation strategies are valid on the stable areas, but are more questionable on moving areas. Because the correlation operator is not linear, I am wondering what is the combined effect of bias illumination variation and a moving target on the results. Since no validation datasets exist on the Del-Medio landslide, this question is still open.

We believe that the Del Medio landslide is a great case study to highlight the problem that seasonal biases pose for interpreting landslide motion in a seasonal climate. As the reviewer probably knows well, it is unfortunately very difficult to find multi-annual, publicly available GNSS data on slow-moving landslides that are well trackable with optical imagery (meter scale displacements, sparse vegetation, low cloud cover), let alone a landslide matching these criteria that is locally monitored and shows a seasonal displacement component.

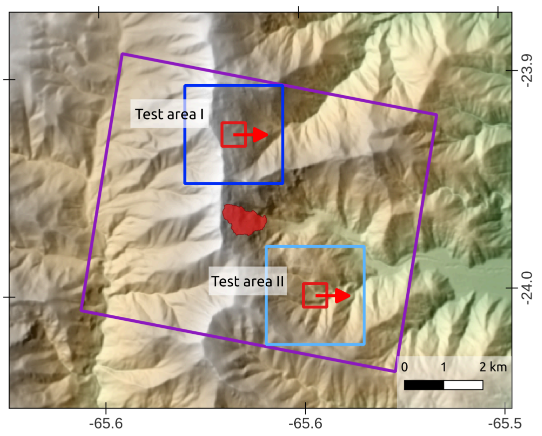
While there is no GNSS data available for this landslide, there are different ways to distinguish between the true signal and a measurement bias: (1) over stable areas, the displacement is expected to be zero; (2) over the landslide surface, we expect mostly (only) downhill motion; (3) estimated displacement rates are (approximately) similar across all datasets used.

Regarding the combined effect of illumination bias and moving targets, we have added results of an experiment to the Supplementary Material, Figures S19 and S20 of the revised manuscript. We correlated two Sentinel-2 images from different seasons (2020-05-15 and 2020-09-02) over stable terrain with different illumination conditions (and also upsampled to test the effect at different resolutions). We then introduced an artificial and known offset to the secondary image (500x500 m patch, shifted by 5 m in both x and y directions in the center of the scene) and ran the correlation again. As expected, the seasonal illumination bias obscured the true signal. However, when the illumination bias is removed (by subtracting the displacement estimated without an artificial offset), the remaining displacement field clearly shows the moving patch and zero displacement estimated over stable terrain. This leads us to the conclusion that (a) with a perfect correction method, it is possible to fully recover the true displacement signal and (b) the success of correction over stable terrain can be transferred to the landslide as well. Furthermore, for our scenario, the bias introduced by the illumination is the dominant source of noise.



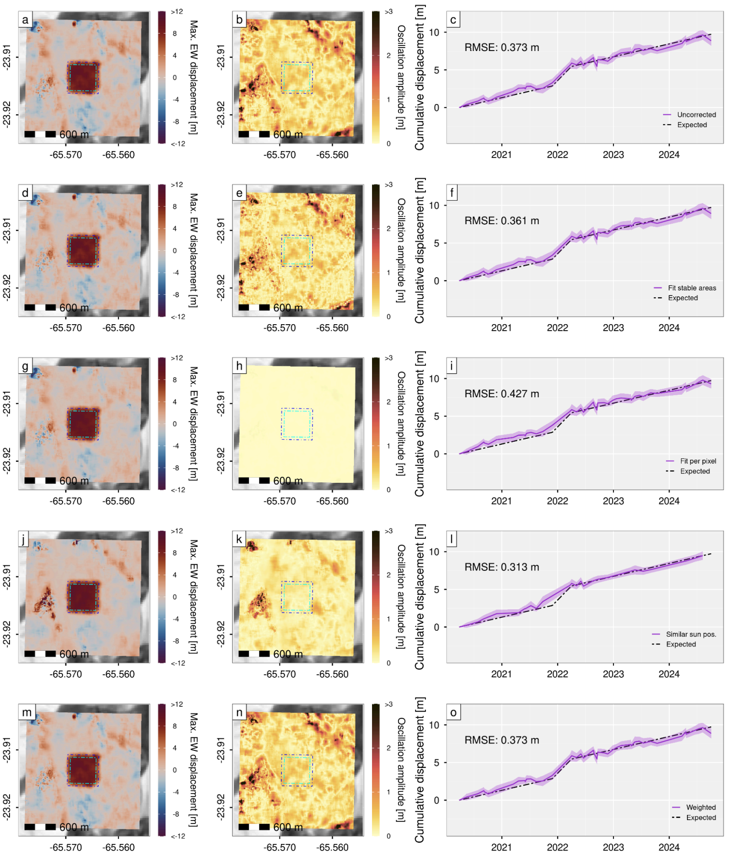
Figures S19-S20: Displacement fields estimated over stable terrain using a cross-seasonal Sentinel-2 image pair (May and September 2020), resampled to 3 m (first column), 6 m (second column), and 9 m (third column) spatial resolution. The first row shows the seasonal biases that affect the displacement estimates when the original scenes are correlated. To simulate the mixing of seasonal bias and true displacement, we introduced an artificial offset to the secondary image by moving a small patch in the center of the scene by 5 m in EW and NS directions. The results of the correlation, including the added displacement, are shown in the second row. When subtracting the pure seasonal bias (first row) from the mixed signal (second row), the true displacement can be reconstructed well (third row).

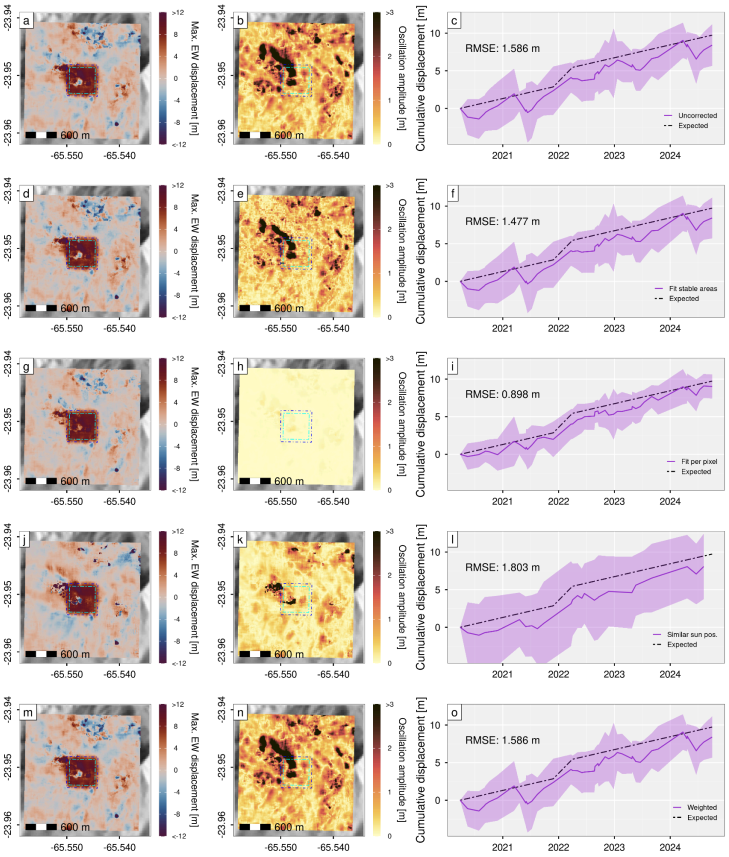
Introducing artificial displacement over stable terrain also opens up the opportunity to evaluate the full processing chain and the effect of seasonal bias mitigation. We did so using all available PlanetScope imagery from the newest generation over two stable areas: one well-illuminated area with a similar orientation as the landslide (test area I) and one heavily affected by shadows (test area II), see map on the right (Supplementary Figure S22 in the revised manuscript). In both areas, we moved a small patch by a known amount, approximately reflecting what was observed over the Del Medio landslide between 2020-2024 (linear displacement (total of 10 m)



with a small acceleration (2 m in 4 months) in the monsoon season of 2021/2022.

The results look as follows for test area I (left) and area II (right) (see previous Figure for the location). Plots show the maximum EW displacement (first column), oscillation amplitude (second column), and the mean displacement extracted over the known moving area (with an inward buffer to avoid border effect, see also our response to the comment regarding the border effects) (third column). The rows show the respective correction approaches: Uncorrected (row 1), sine fit over stable areas (row 2), sine fit per pixel (row 3), similar sun position (row 4), and weighting (row 5):



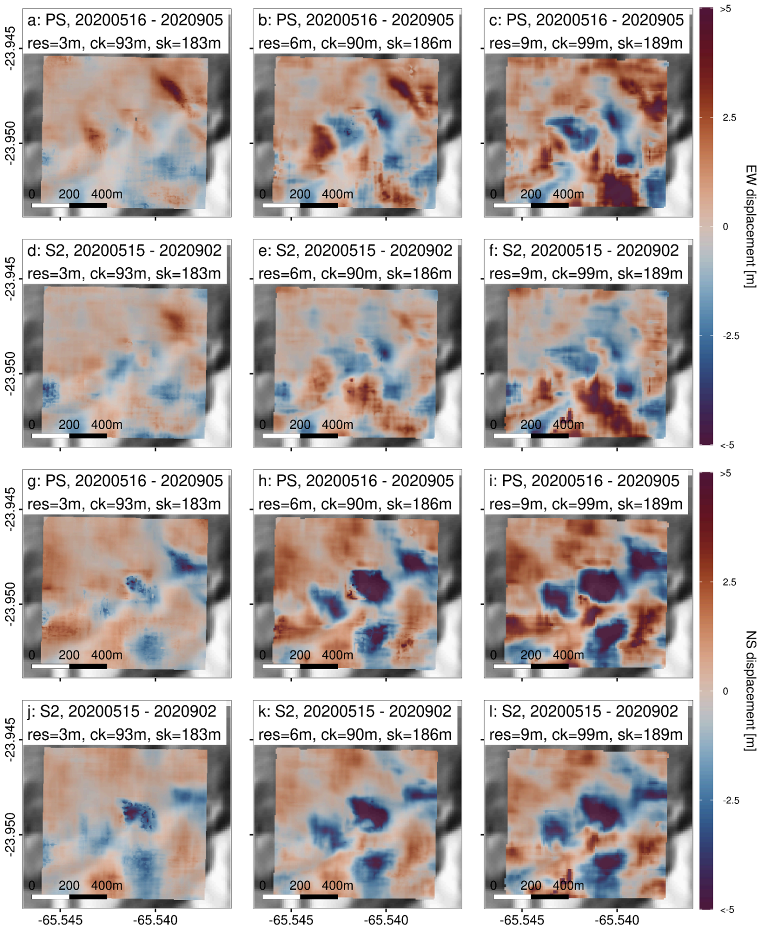


We observe that in the well-illuminated example (area I, left Figure above), the small acceleration is captured very well, and as the seasonal oscillation bias is low, little to no correction is required. In the topographically more complex area suffering from shadow effects (area II, right Figure above), the reconstruction is much more difficult. Here, the best-fit sine per pixel correction achieves the best results but could potentially smooth out a regular seasonal response by the landslide. For the Del Medio landslide, we would assume an intermediate case: the seasonal oscillations are higher than in test area I, but unlike test area II, with a similar aspect and less complex shadows.

We have added these additional analyses to the Supplementary Material (Figures S23 and S24).

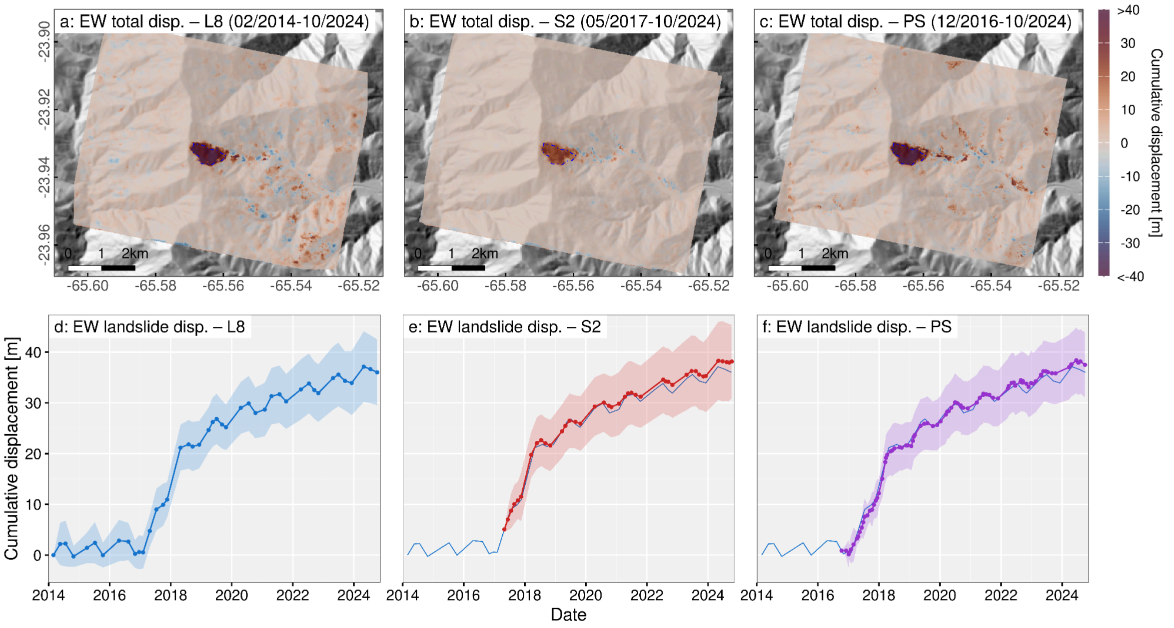
This question also relates to the terrain features the correlation algorithm is sensitive to, depending on the optical sensor (resolution, noise), the parameters of the correlation algorithm and the type of land-cover. My feeling is that for a similar area of study, the high- and medium resolution images are sensitive to different features of the terrain. Your results on the different sensors could perhaps elucidate this question?

We have expanded our analysis of the bias effect at different resolutions to Sentinel-2 as well. This also allows us to compare downsampling a higher-resolution image (PlanetScope) and upsampling a lower-resolution scene (Sentinel-2). We used two scene pairs with very similar acquisition times (2020-05-15 and 2020-09-02 for Sentinel-2 and 2020-05-16 and 2020-09-05 for PlanetScope). The results are comparable and were added to Figure 5 in the revised manuscript:



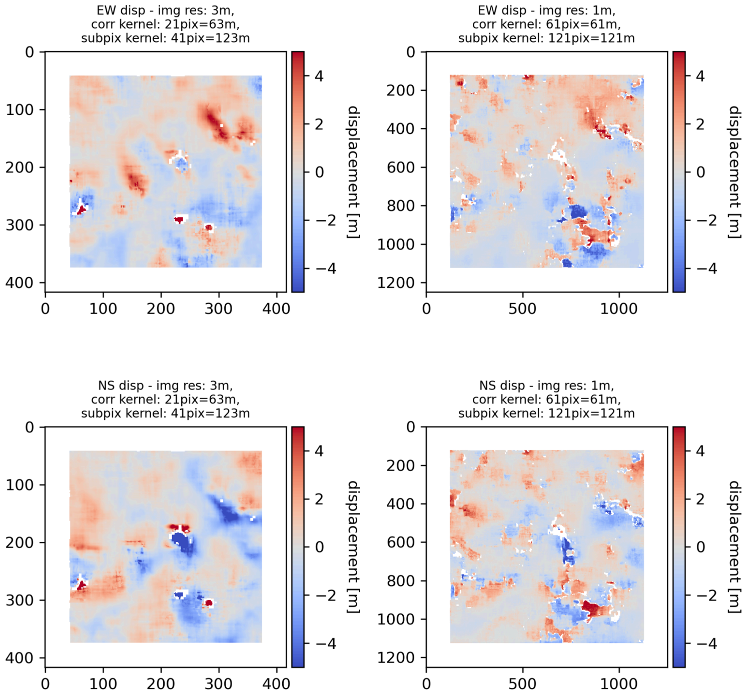
The results obtained with different sensors are really intriguing as S2 and PS present lower displacement values on the Del-Medio landslide than with L8 (Figure 2), despite their higher resolution. This is counter-intuitive (cf e.g. Bontemps et al., 2018 ; Cusicanqui et al., 2025), and questions me a lot on the ground of these results. Is that also an effect of the terrain-feature depending on the sensor type (or resolution)? That would be really interesting to apply your method to another area with validation data (or synthetic data). In the suplementary material, Figure S17 shows a result of synthetic tests, but it is not clear how it has been developed and what are the results for the different sensors. Maybe it could be worth focusing on that aspect?

It seems like the slightly lower velocities for Sentinel-2 and PlanetScope were linked to noisy pixels around the landslide border (i.e., correlation border effects). Following your suggestion (see comment below), we have applied an inward buffer of half the correlation kernel size to the landslide mask to exclude bias from border effects. The results between all sensors are now more comparable. Here is the updated Figure 2:



The results on the impact of the sensors presented in Figure 5 (and section 4.3) investigate the effect of the spatial resolution only, whereas the satellite images have not only their own pixel resolution but also their own pixel noise (radiometric noise). A pixel from a Planetscope image is much noisier than a pixel of S2, so that modelling a S2 image by downsampling a Planetscope image at 9m (~10m) resolution is certainly not valid. To me this paragraph does not allow you to elucidate the differences observed between the different sensors. I would rather try to compare correlation results obtained with different satellite images taken at similar days/hours of acquisitions. Nevertheless, I found this part interesting as it made me also wonder « does a PlanetScope upsampled at 1m lead to even lower seasonal oscillation ? ».

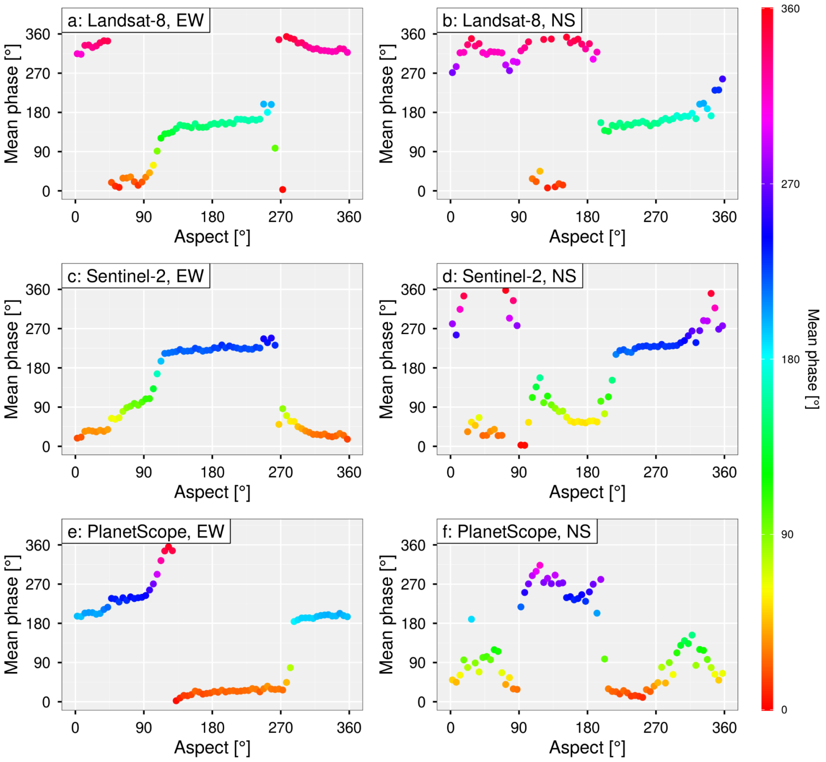
Running the process with 1 m resolution and still keeping the correlation kernel size constant in terms of area is computationally very heavy for even this small area, making it unfeasible to run, especially for larger areas. What we did, however, was to compare PlanetScope 3 m to 1 m resolution, but using a smaller correlation kernel of approximately 60 m. Again, the magnitude of bias from different illumination conditions is reduced, but due to the overall smaller size of the correlation kernel, the results appear less smooth and have more gaps:



We have expanded Figure 5 by adding a comparable analysis using Sentinel-2 data from a similar date and updated the section accordingly.

Some technical aspects must be clarified : (i) no phase delay on the modelling of the seasonal oscillation appears in the Equation 9, despite a map of phase delay is shown in Figure S8 and S10. How is the phase delay taken into account? This phase delay is, by the way, not analyzed at all in your results/discussion. I think that’s a bit of a shame. (ii) the definition of « short-baseline » pairs in relation with inter-annual trend and the choice to not used these « short-baseline » pairs in the inversion strategy are not clear. (iii) how is done the upsampling of S2 and L8 images?

(i) The phase was included, but the term was missed in Equation 9 – thank you for spotting that. We have corrected the equation. We have also included a small discussion on the phase delay together with the following plot to the Supplementary Material, which shows the mean phase delay per aspect bin (5 degrees).



We observe a clear phase shift for south-west facing slopes in the EW component, which is again the direction of shadow extension, while for the NS component, the pattern is more diffuse. Interestingly, phase delay is similar for Landsat-8 and Sentinel-2, while it seems inverted for PlanetScope. This may be related to the higher variability in sun position, particularly sun azimuth in the PlanetScope scenes used.

(ii) We excluded short-temporal baseline pairs when we expect the landslide motion to be below the detection limit. This should not affect the seasonal bias component, since illumination biases are introduced anyway when cross-season pairs from different years are correlated.

(iii) The upsampling is done through cubic interpolation as mentioned in line 117 (initial manuscript).

I found that the section « 4.4 mitigation of seasonal bias » must appear in the methodological section rather than in the results. One key aspect of your study is a comparison of the mitigation strategies of the seasonal bias. This different straegies should first appear in the methods, before showing the results.

We have moved the description of the correction strategies to the methods section in the revised manuscript.

The introduction/title/abstract are too much focused on the Del-Medio landslide. whereas this single-case study is just an example of application of your methods to one specific case-study. To my point of view, the main broughts of your work are elsewhere, on the comparison of the seasonal bias mitgation, including for a multi-sensor approach. I think the introduction should be partly rewritten, to decrease the parts related to the Del-Medio landslide (for instance the text from lines 42 to 48 could be placed in the « site study » section), and to bring more elements on the state of the art on the multi-sensor approach for ground-motion measurements.

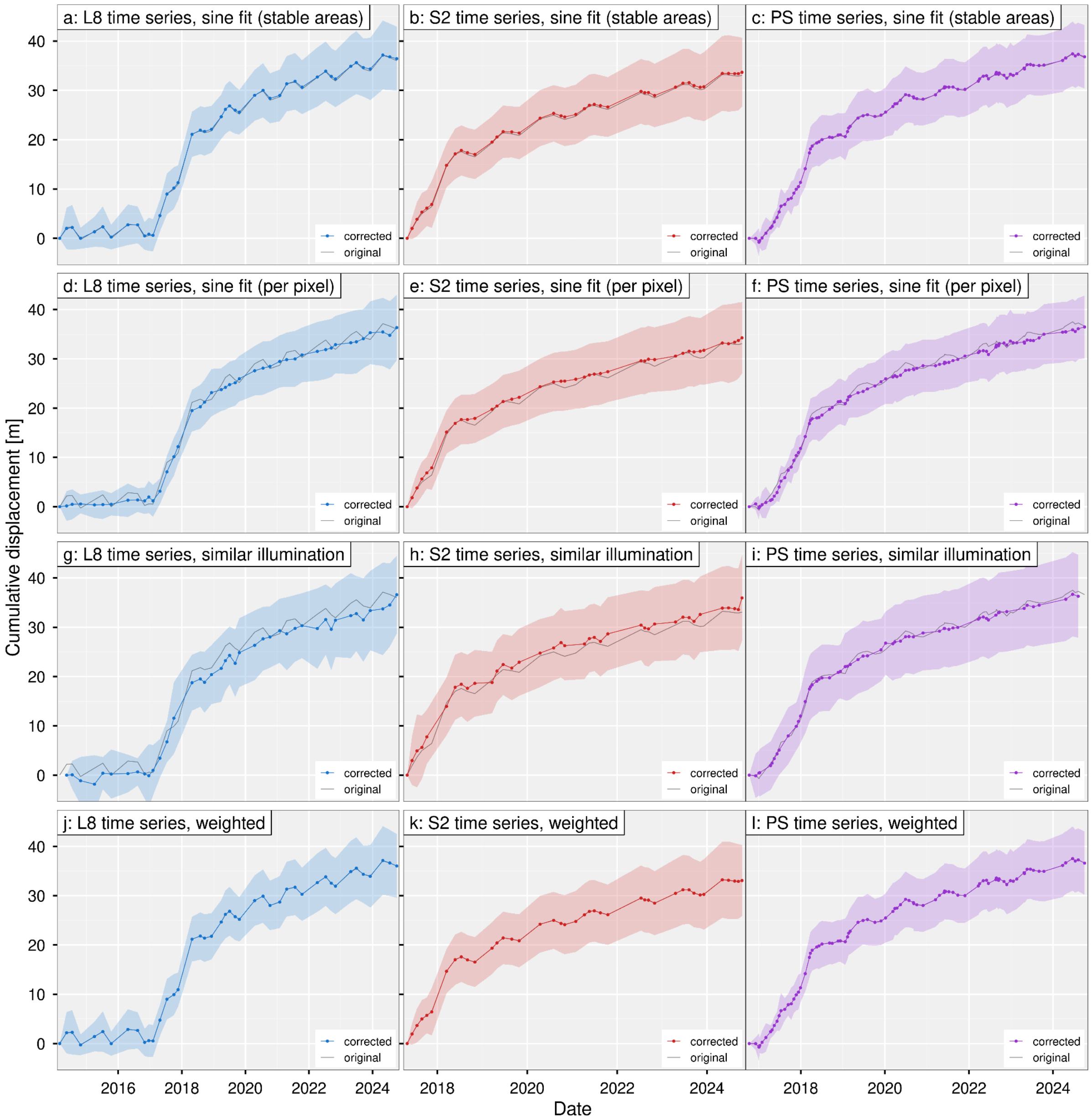
We have restructured the introduction to better separate the general problem of changing illumination for optical offset tracking applications from the case study of the Del Medio landslide. Nevertheless, we do believe the Del Medio landslide is a great example of the problem that the illumination changes introduce: We have a landslide situated in a seasonal environment, making true responses of landslide motion during the rainy season likely. At the same time, seasonal illumination changes and moving shadows from the steep surrounding terrain introduce a severe bias effect. Can these two signals be untangled, and if so, how? – This is the main motivation of this work.

The title/abstract should also reflect this aspect of the study, and provide less importance to the Del-Medio results given that no validation datasets are existing here.

See our response to the previous point. We did not change the title of our manuscript, as we believe it describes well the two aspects that are covered in our study: (a) challenges of and mitigation strategies for seasonal biases, and (b) the Del Medio landslide, which is analyzed in detail. We now include a synthetic analysis that takes advantage of the complex relief of the study area.

The results obtained on the Del-Medio landslide with and without seasonal bias mitigation using the L8 sensor (Figure 7) are not intuitive to me. Indeed removing the seasonal bias leads to a much lower L8 total displacement. This is not the case for S2 and PS. How would you explain it?

This was also related to noisy pixels around the landslide border (see also the other comment related to the border effects). Pixels impacted by border effects were now excluded through an inward buffered mask, which reduced standard deviations and made measurements across all sensors more comparable. Below is the updated Figure 7 with the more restrictive mask applied. We have also added the displacement time series from the two other correction approaches for completeness, although they very closely align with the uncorrected signal:



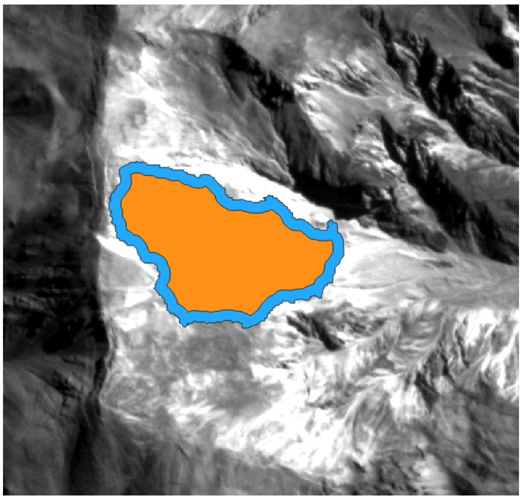
Paragraph 4.5 is not really clear to me:

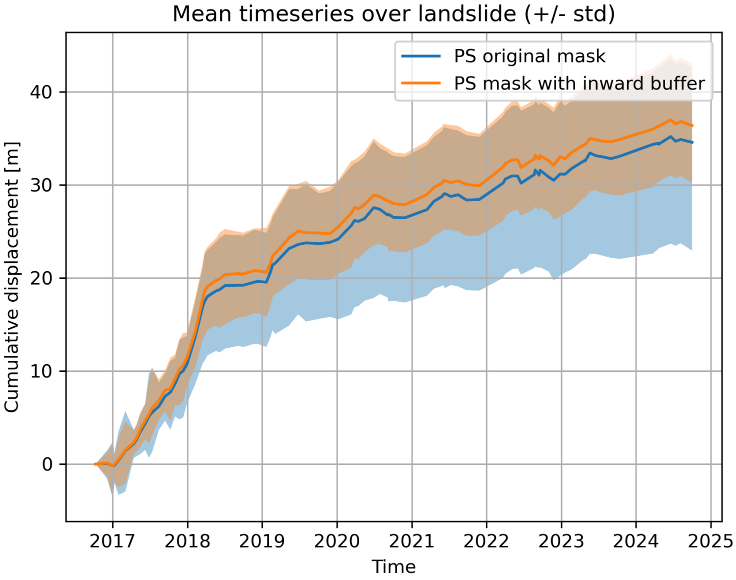
The « high » standard deviation observed in the Figure 7 is not only linked to difference of unit kinematics. If I understand well, the ribbons shown in the Figure 7 reflects the standard deviation of all the pixels. To me this pixel variations is linked both to the kinematics of the different units and to border effects due to size of the correlation windows (See for instance Bontemps et al., 2018).

The standard deviation of all pixels over the landslide is shown. Therefore, if some pixels move faster and others slower, the standard deviation becomes larger.

Also I am wondering the effect of averaging cumulative displacement time-series over areas not showing the same velocities. I don't think this is the best way to highlight the kinematics of the landslide. I would rather pick up some selected pixels in the middle of the different units of the landslide (to limit the border effects very common with medium resolution satellites). This comment could also apply to the time-series of Figure 10.

The reviewer is correct in pointing out potential border effects where the correlation kernel covers both stable and moving ground. We estimate that border effects would concern pixels within a perimeter of half the correlation kernel size from the landslide outline, which is ~ 17 pixels x 3 m resolution = 51 m. We created an inward buffer of the original mask to remove these pixels. The effect on the mean cumulative displacement time series is small; however, the standard deviations and also the lower displacement values are greatly reduced. The measurements between the different sensors are now more comparable. Here is the before and after comparison for the PlanetScope timeseries:





To showcase the spatial variability, we refer to the profile plot where the cumulative displacement along an EW transect is shown.

It is also not really clear what does the term « uniform acceleration » refer to?

With this, we mean that the displacement estimated over the entire landslide body was similar (uniform) for the 2017 event. Later, different parts of the landslide, e.g., areas close to the upper scarp, moved faster than the lower part of the landslide. We have made a small addition to the sentence to clarify this:

“The high standard deviations following the relative spatially uniform acceleration across the entire landslide surface in early 2017 […]”

The word « surge » is used a lot in the glacier community but is not really common for the landslide community. It is sometimes used to describe specific events like landslide-induced-tsunamis. I don’t think it is the right word to use here (and elesewhere in the text).

We have replaced all occurrences of “surge” with “acceleration”.

The accelerations from 2018 and 2021 are not really obvious to see in Figure 7, also given that Figure 7 averages pixels with different velocities (and maybe even different timing of accelerations of the different units). Maybe try to better highlight these accelerations in the mean time-series normalized per-pixel of individual time-series in the fastest parts of the landslide where the signal to noise ratio will be higher?

Yes, averaging over the entire landslide smoothes the 2018 and 2021 responses. However, in Figures 8 and 9, the accelerations are better visible based on the profile line and a smaller kernel in the upper portion of the landslide. The objective of Figure 7 was primarily to show the result of the correction approaches.

The analysis of the link between the landslide accelerations and the rainstorms can be improved (after these 2018 and 2021 accelerations have been better highlighted. See my previous comment). First of all the brought of ERA5, due its low quality in this area, is very limited (as also pointed out by the author). I don’t see the point of showing it. I would keep only the GPM dataset. However, I found interesting that extreme rainfall events could accelerate deep-seated slow-moving landslides. This is not the first case-study showing this. I don’t know the complete bibliography, but I have in mind the Joshimath landslide (Sreejith et al., 2023). A better analysis of the litterature can be done and the mechanics behind this acceleration analyzed.

We have removed ERA5 total precipitation, but kept the ERA5 temperature because these could be relevant for freeze-thaw processes. We have also extended our literature review to include additional references.

The Figure 10 is really confusing to me: Is that a total displacement or the amplitude of the seasonal acceleration ? The caption and the legend says different things. Since I didn’t understand this Figure, I can’t really comment it as well as the section 5.2 that relies on this Figure. Please clarify and I could comment during a second round of review.

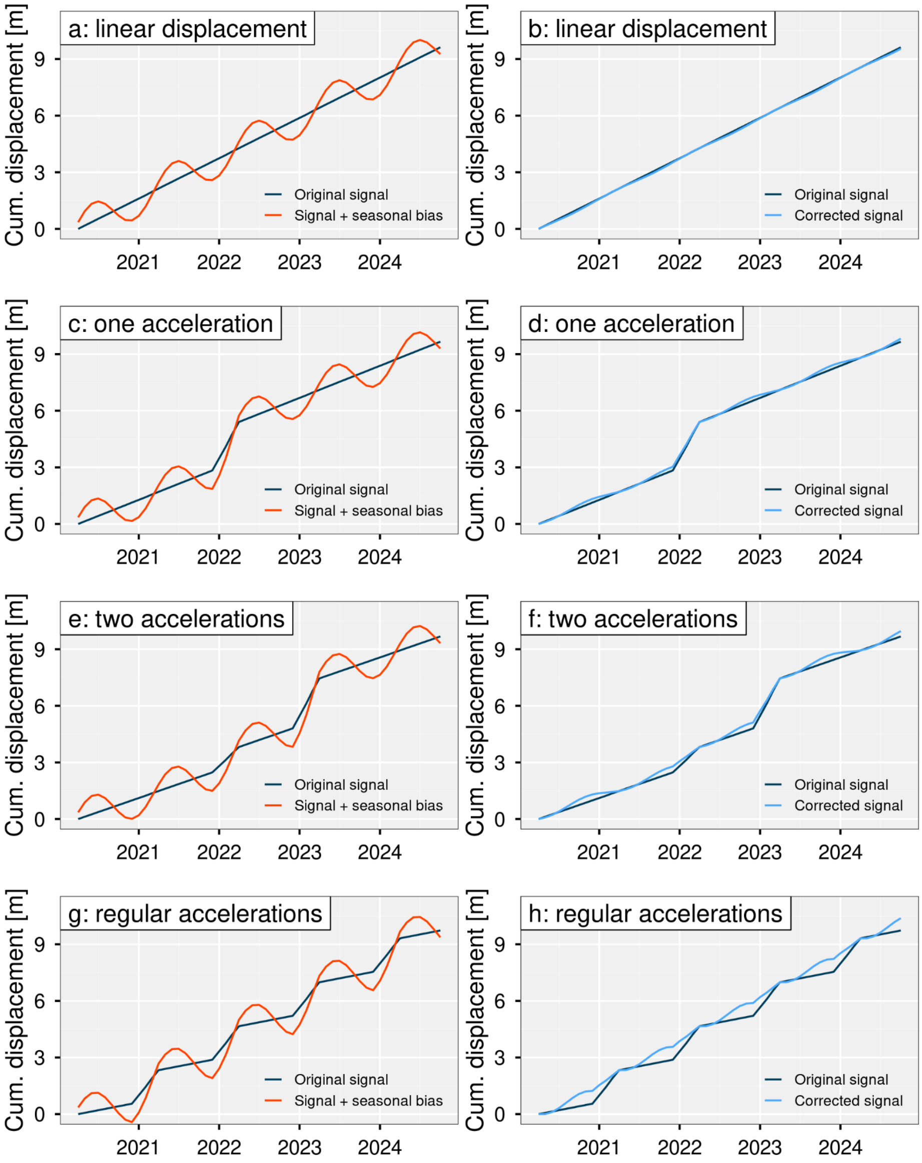
The legend label was wrong and was corrected – thank you for spotting this. What is shown is the difference in total estimated displacement between the three sensors for a common time period for both the corrected and uncorrected results. We also modified the Figure labels to better highlight this.

In Figure 4, it would be interesting to have a probability density function of the slopes (both slope gradient and aspects) and the sun-azimuth differences. Indeed for slopes or sun-azimuth difference appearing few times, the results are certainly too noisy, and the bins must be removed.

A Figure that shows how many measurements fall inside each bin is shown in the Supplementary Material, Figure S12.

If I understand well, for the specific case of the Del-Medio landslide, the seasonal oscillation has been removed per pixel (lines 270-278). As the authors say, this can be applied to the Del Medio landslide only as it seems to not show any seasonal movement (even if the absence of in-situ measurements this assumption is complex to validate). I think it must be even more clearly stated that this method should not be applied to any landslides.

The reviewer's understanding is correct – the fit was applied per pixel. This method is suitable if a landslide does not show a regular seasonal displacement component, but only for more occasional acceleration phases, as we interpret the displacement history of the Del Medio landslide. Having a time series of many years is also helpful in this regard. We have experimented more with different synthetic displacement scenarios to better constrain when this method can be applied and when it introduces a bias of its own: linear displacement and signals with irregular acceleration phases (i.e. not every year) can be reconstructed well, while a regular seasonal landslide response would be smoothed out (we replaced former Supplementary Figure S17 with this updated analysis):



For the Del Medio landslide, because the results of the sine fitting approach largely align with the reduction of seasonal biases by only using correlation pairs with a similar illumination, we assume that the approach is relatively safe to use. However, due to a lack of ground-based data, we cannot conclude this with absolute certainty. We have added this constraint more clearly in the revised manuscript and base our analysis of the landslide displacement history on the data obtained from similarly illuminated pairs only. This indeed revealed two more potential small acceleration phases that were not visible when subtracting the best-fit sign per pixel

Overall, every correction method has its drawbacks and alters the observed signal. E.g. with the sine fit over stable areas with similar exposition, we observe that the oscillations over the landslide were not significantly reduced – is keeping the illumination bias and not being able to correctly interpret the landslide's seasonal behavior better than partially removing it but effectively mitigating bias? This is a tradeoff that has to be considered on a case-by-case basis, which we have stated even more clearly in the revised manuscript.

The choice of not using the short-baselines must be better discussed. Indeed it is certainly a good choice in the case of the Del Medio landslide where the land-cover doesn't seem to evolve much with time (maybe I am wrong?), but this might not be a good choice in other areas with a human presence and modification of the land-cover with time, that can lead to a decay of the coherence with time. This must be discussed.

Indeed, the land-cover changes in the Del Medio catchment are limited as there is barely any anthropogenic impact at that elevation, and vegetation cover is sparse. The only rapid changes occurring are observed in the alluvial river bed, which sometimes can complicate the matching in this part of the basin. We have added a sentence to consider this aspect in study areas with more dynamic landcover changes.

In Figure 8, we still observe seasonal oscillations, despite a per-pixel seasonal signal has been removed (if I understand well). This is not really intuitive to me why these oscillations still remain? Also the transect AA' seems purely EW oriented, and not exactly longitudinal. Wouldn't it be better to use a longitudinal transect of the landslide?

Yes, some oscillations are still present, but their amplitudes are greatly reduced in comparison to the original measurements. The sine fit will always have a regular amplitude, which will not perfectly fit every oscillation especially in the presence of noise.

Considering the additional analyses we did based on synthetic displacement signals and the potential bias introduced to the time series through the per-pixel sine fitting approach, we agree that it is probably safest to use the time series retrieved from the similarly illuminated image pairs for subsequent interpretations, even though the overall lower number of correlation pairs make in more noisy for the Sentinel-2 and Landsat-8 time series. We have replaced the Figure showing the displacement along a profile for the sine fitting approach with the one from the similar illuminated pairs (previously Figure S20 in the Supplement).

We chose an EW transect because we focused on EW displacements only (to not mix seasonal bias effects from EW and NS).

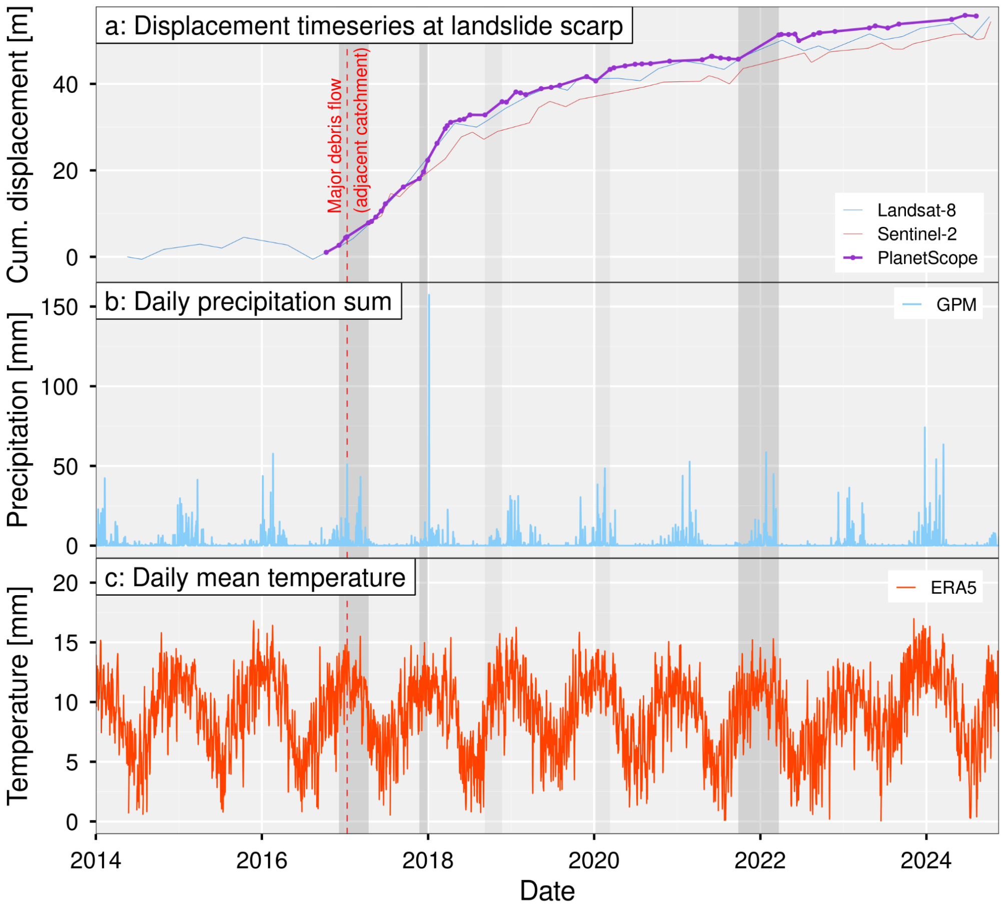
In Figure 10, the standard deviations of the difference of displacement fields on stable areas between L8-S2, L8-PS and S2-PS are calculated. This can be exploited to estimate separately the standard deviation of the displacement fields of L8, PS, and S2. Given the values shown in Figure 10, it will show sigmaS2 and L8 higher than sigmaPS, with sigmaS2 and L8 relatively similar. In this context I am wondering why S2 and L8 do not show similar time-series? Also, adding these standard deviations to the individual time-series (for instance on Figure 9a) would provide a more convincing argument to the analysis of the accelerations period.

We believe the reviewer refers to the possibility and usefulness of investigating the standard deviation of differences between L8, S2, and PS at every time step, not the total cumulative displacement estimated (as currently shown in Figure 10)? However, this would require the time series to be interpolated to a common sampling interval because the acquisitions are not from the same date across all sensors, making it less intuitive and also less of a fair comparison. After all, if the data are linearly interpolated to a regular monthly sampling interval, periods with low actual data coverage (rainy season) would be overrepresented. We therefore kept the initial layout.

Some Figures could be partly improved. In Figure 3, the dashed black contour of the landslide is not well visible. Maybe choose another color or symbol? In Figure 9b the dashed black line indicating the accelerations mask the precipitations. In Figure 9a, indicate more clearly that the major debris flow is not occuring on the landslide but nearby.

We have implemented the suggested changes. For Figure 9, considering the potential bias the per-pixel sine correction method introduced, we changed the time series used for interpretation of the landslide behavior to the results obtained from the images with similar illumination, also with regards to the data shown in the profile plot. The strong reduction of displacement fields also reduces the number of available dates which makes it more difficult to constrain the precise onset of an acceleration phase. We have changed from indicating the increase in displacement rates through a single line to highlighting a wider period between the two (or three if onset is not very clear) closest available satellite images.

Using similarly illuminated pairs only, the PlanetScope time series at the upper landslide scarp appears less noisy than the one of the per-pixel sine fit (inital manuscript). Based on the new plot, one may even argue for two additional small acceleration phases between September to November 2019 and January to March 2020. These are, however, only present in the PlanetScope time series (temporal sampling of Landsat-8 and Sentinel-2 is too coarse with the sun elevation and azimuth thresholds applied). We have therefore added these phases in light gray to the plot and adapted the manuscript accordingly. Here is the updated Figure:


a: Displacement timeseries at landslide scarp
Cum. displacement [m]
0
20
40
Major debris flow
(adjacent catchment)
Landsat-8
Sentinel-2
PlanetScope
b: Daily precipitation sum
Precipitation [mm]
0
50
100
150
GPM
c: Daily mean temperature
Temperature [mm]
0
5
10
15
20
ERA5
2014
2016
2018
2020
2022
2024
Date