Dear Copernicus Publications Editorial Support Team,

In strict accordance with our responses to the two reviews we have received, we have revised our manuscript and provide corresponding responses below, where the reviewer's comments are in black and our responses are in red. A clean version of the revised manuscript and a marked-up version showing the changes we made have been uploaded to the system.

The paper presents well-structured research with appropriate methods, results, and scientific soundness. The combined use of different-wavelength SAR, together with car-based SAR, is an important added value. However, some minor revisions are needed to be published, especially for the quality of the figures and some other minor issues that could be improved.

We thank the reviewer for their positive comments and constructive suggestions regarding our manuscript. In the responses below, we address these suggestions and explain the changes we made to the manuscript.

General comments

Monte mater: EGMS, it could also be possible to also add the dataset 2019 - 2023 (or 2020-2024 if it will be published) to have a long time series for comparison

As detailed in our Reply on RC1, the EGMS data from 2018 – 2022 are better in terms of spatial coverage than those from 2019 – 2023, thus allowing a more conservative estimate of the advantages of L-band versus C-band multi-temporal data. Updating Fig. 4, 12 and 13 with the new EGMS solution does not seem to provide added value and would not change our conclusions. For these reasons, the EGMS dataset 2019 – 2023 is not further included in the revision.

Brienz/Brinzauls: Maybe it is possible to compare the data for DIC/Lidar used in Manconi et al., 2024, as well?

As detailed in our Reply on RC1, a comparison between our latest results (2023 – 2024) and older DIC/LiDAR data (2015 – 2020) from Manconi et al. (2024) is not expected to be accurate given the large changes in kinematics still ongoing at Brienz/Brinzauls. In addition, using the DIC/LiDAR together with SAOCOM-1 and ALOS-2 PALSAR-2 DInSAR stacking results to compute 3-D surface deformation fields would not be considered innovative. For these reasons, we do not include such a comparison in the revision.

There is, however, good overall agreement between the DIC/LiDAR results along the north-south direction in 2018-2020 and the GLSAR interferogram in Figure 11. In Section 5.3 of the revised manuscript, we therefore add a statement about the general agreement of the GLSAR interferogram with DIC data used in Manconi et al. (2024).

It is possible to resume in a table for each satellite and orbit the mean azimuth and incidence angle.

Agreed, the mean azimuth and incidence angles for each satellite and area of interest are included in the Appendix in Tables A1, B1 and C1.

A table or some consideration showing the max (theoretical) velocity (along LOS) that can be detected by each satellite (I suppose based on acquisition frequency and wavelength) could help visualise the different satellites' upper limits.

Agreed, the maximum theoretical velocity for each satellite is provided in Appendix D in Tables D1 and D2.

For the PS density calculation, it would be better to show where the area is in which PS is calculated, and to include a land use map to better understand what is classified as forest and non-forest. For the plot, use the same scale for A B and C, at least using log axis Y to evidence the different order of density from C to L band

To comply with a reasonable number of figures in the manuscript, we agree to provide the percentage and size of forested and non-forested terrain per area in which the PS density is calculated in section 6.1 in Table 3. Additionally, we include maps of the forest cover from the CORINE Land Cover (CLC) and outlines of inventoried slope instabilities in the Appendix in Figures A3, B4 and C5.

We agree to use the same logarithmic scale for the three plots in Fig. 12 and have changed it accordingly.

Specific comments

Line 133: collapse on 27 October 2009 (any reference)

No, unfortunately there are no scientific papers describing this event.

Line 194 : as shown in Fig. 6 is 3 ?

Correct, we have changed the text accordingly. Thank you for spotting this mistake.

Figures

Where possible, add the boundaries of DSGSD and landslide/, especially when interferograms cover the entire area.

The methodologies to classify slope instabilities at the three test sites vary and have not all been published, thus requiring a detailed description of the delineation process if outlines were included in the figures. Additionally, we believe that the moving areas in the interferograms are already clearly visible without the need for additional markings. For these reasons, and to comply with a reasonable number of illustrations and descriptions in the manuscript, we do not include DSGSD and landslide boundaries. Nevertheless, maps showing forest cover from the CORINE Land Cover (CLC) and including outlines of the mapped landslides are provided in the Appendix in Figures A3, B4 and C5.

In some figures (e.g., 6), the aerial photo appears to have a black pixel; is it a shadow mask? If yes, add to the legend.

Correct, in some of the images, layover/shadow is shown in black or dark grey. This has been specified in the caption to the figures in the revised manuscript.

Figure 4 and others: Please add scale and coordinates

Agreed, we have changed the relevant figures to include the geographical coordinates.

Figure 8: Please use the same scale of velocity displacement

Figure 8 shows two distinct analyses; therefore, we use two different scales for the velocity and displacement.

Figure 12: move at the end of par 6.1

Agreed. This figure has been moved to the end of section 6.1.

Reply on RC2

This manuscript describes an extensive comparison among DInSAR measurements obtained by C- and L- band satellite data for landslide monitoring in alpine regions. Data acquired by a car-borne SAR sensor are also presented, complementing the information obtained from satellites.

The paper is very clear in its concept and structure. Several experimental results support the conclusions that are clear.

I have some minor comments that I think could improve the manuscript.

We thank the reviewer for their positive assessment and constructive suggestions regarding our manuscript. In the responses below, we address these suggestions and explain the changes we will make to the manuscript.

a) Table 1-2: it is not clear how to identify Stripmap and ScanSAR data. Please, add this info here or in the corresponding appendix tables.

Agreed. In the Appendix in Tables A1, B1 and C1, the ScanSAR data is now marked in *italics*.

It would also be useful to indicate the mean look angle, which in mountainous regions could be in some case critical.

Agreed, the mean azimuth and incidence angles for each satellite and area of interest are included in the Appendix in Tables A1, B1 und C1.

b) L312: do you mean 2024-2025? In any case, I would detail the temporal span: October 2024-June 2025.

Correct, we have updated the text accordingly. Thank you for bringing this error to our attention.

c) In Fig4a-b, 6, 9, A2, B3, C4 some shadow/black areas are present but no information for their interpretation is provided. Please, explain.

Correct, in some of the images, layover/shadow is shown in black or dark grey. This has been specified in the caption to the figures in the revised manuscript.

d) Fig.5: I suppose Fig. 5 and Fig.4 shares the same view. However, in the case of Fig. 5, the background map is not useful for identifying the area because it is almost completely covered by the interferogram. Therefore, here, and also in general for all the other maps, please add the geographic coordinates to better localize the study area.

Agreed, we have changed the relevant figures to include the geographical coordinates.

As a further suggestion, in Fig. 5b (and in Fig. 11a as well) please consider to make transparent the areas with no phase information, instead of representing them in black.

Here, transparent areas are not without phase information; they are in shadow. In these representations, shadow is not pure black, but has a spectrum of dark grey values. Therefore, it is not possible to set it to transparent, as some pixels would remain and make the illustration look unaesthetic and potentially misleading.

e) Fig 5a and Fig. 7: Please, explain why some part of the wrapped interferograms is masked out.

The part of the wrapped interferograms that is masked out corresponds to layover/shadow. This has been indicated in the caption to the figures in the revised manuscript.

f) Fig 13a,c: SqueezeSAR -> SqueeSAR

Correct, this has been changed in the revision. Thank you for spotting this mistake.

g) Fig. 13d: Caption states GNSS pt. 5001 while panel shows GNSS pt. 800. Please, correct.

Thank you for spotting this mistake, the panel has been changed to read GNSS pt 5001.

h) L486-487: probably "when a significant acceleration detected by the MTS was not recorded by ALOS-2 PALSAR-2"?

Correct, this sentence has been changed accordingly.

i) L535: I would mention the possibility to use the Pixel Offset approach, if applicable, that anyway has its own limitations.

Agreed. We have mentioned the possibility to use Offset-Tracking, with reference to Manconi, A., Bühler, Y., Stoffel, A., Gaume, J., Zhang, Q., and Tolpekin, V.: Brief communication: Monitoring impending slope failure with very high-resolution spaceborne synthetic aperture radar, Nat. Hazards Earth Syst. Sci., 24, 3833–3839, https://doi.org/10.5194/nhess-24-3833-2024, 2024.

j) L536-541: "purely satellite-based analysis". I agree with the main point raised by the authors and I also agree with the authors' final statement at lines 568-570. However, I would mention here that this statement is related to the present satellite landscape.

Agreed. In the revision, we have specified that our findings show the current limitations of a purely satellite-based analysis.

Indeed, future SAR missions acquiring with increased repeat pass and/or from multiple look angles would improve the satellite detection capabilities, even if the use of ground sensors will be probably always required.

In any case, having improved satellite constellations would allow us to optimize in situ monitoring resources.

With reference to Manconi et al. (2024), in line 560 in section 6.3 we have specified that satellite data available with sub-daily revisit times and at sub-metric spatial resolutions can be a viable alternative to manage slope instability operational scenarios.

k) Fig B2-B3: review the figure caption. It should be Val Canaria.

Correct, we have changed the figure caption accordingly. Thank you for spotting this mistake.