2nd round of review

Response to comments by Handling editor Prof. Christian Hauck

General comments

- Thank you again for your author comments and revisions of the manuscript. The reviewers of the first round re-evaluated your manuscript and your changes, and both found that the manuscript improved substantially. However, they both found that there still remain several open questions that were not addressed sufficiently. I would like to especially mention the points 1 and 2 of referee 2, as they address potential errors or unclear conclusions in the manuscript. But also the other points mentioned by the reviewers need to be addressed and improved before publication is possible.

Response > We sincerely thank you for your feedback and for coordinating the re-evaluation of our manuscript. We are pleased to learn that both reviewers recognized a substantial improvement in the revised version. We also appreciate the continued critical assessment and fully acknowledge that several aspects still required clarification or further elaboration.

In particular, we have carefully addressed the concerns raised in Points 1 and 2 by Referee 2, as highlighted in your message, by providing additional explanations, data support, and revised interpretations where necessary. Furthermore, we have systematically responded to all remaining comments from both reviewers to ensure that all outstanding issues have been resolved. The main revisions incorporated in this second version of the manuscript include:

- Equation 2 and related calculations have been updated, which required a revision of the section discussing velocity changes (Section 5.5).
- Figures 8 and 9 have been moved to the supplementary material. This decision was based on the fact that these figures were infrequently referenced in the manuscript and provided limited added value for the reader. In contrast, we have added a new Figure 9 (see our response to Referee 2), which better illustrates the velocity changes and associated uncertainties derived from the L7/8 dataset for those PMAs where such changes could be detected.
- In order to be more clear about the validation of PMAs using high resolution GoogleEarth imagery, we replace the ‘other’ geomorphic class by ‘unclassified’. Please refer to Section 5.1.
- We acknowledge the reviewers’ comments regarding the writing style and clarity. Although we had already reduced the manuscript length by 15% in the first revision, we conducted another thorough review focused on simplifying complex sentence structures, particularly those in the past tense. This led to an additional 10% reduction in manuscript length, without compromising scientific clarity or coherence. We believe that further reductions would risk omitting essential content. In addition, language and style issues have been addressed throughout the text to ensure greater precision and consistency.

We believe that the revisions made have substantially improved the clarity, coherence, and overall robustness of the manuscript. All changes are clearly highlighted in the revised version, and detailed point-by-point responses to the reviewers' comments are provided below.

Response to comments by Dr. Jan Henrik Blöthe

We sincerely thank Dr. Jan Henrik Blöthe for the thoughtful comments and constructive suggestions, which have greatly contributed to improving the clarity and overall quality of our manuscript. Below, we provide a point-by-point response to each comment.

General comments

- This is a revised version of the original submission that I commented on earlier. I will quote my summary of the original submission, before mainly focussing on the replies to my earlier comments but also taking into consideration the revised manuscript in general.

"In their manuscript, Cusicanqui et al. use freely available Landsat 7 and 8 imagery to derive velocities of rock glaciers (and other detectable movements) in the Andes of Chile and Argentina over the course of more than two decades. Checking their derived velocities against a complementary data set using InSAR approaches as well as high-resolution data sets and DGNSS measurements for two selected field sites in Chile, the authors demonstrate the feasibility of using medium resolution imagery for the robust quantification of rock glacier surface movement."

In their revised manuscript the authors have adjusted the text and figures largely in line with the comments made by one anonymous reviewer and me. Overall, I am convinced that the manuscript has significantly improved in overall quality. More specifically, the inclusion of a larger section that specifically addresses the uncertainties associated with the analysis has added to the scientific rigour of the work.

Regarding my previous comments and suggestions, I will outline a few specific comments below (Line numbers refer to the document "egusphere-2024-2393-ATC1.pdf") that should in my view be addressed before accepting the manuscript for publication.

Apart from this, I recommend to once again critically go through the manuscript and check for inconsistencies and clarity of writing. Though the clarity has improved, there are still some statements that could be formulated with more precision.

In general, the authors followed most of my suggestions, in some cases more literally than I would have imagined. The authors put a lot of effort into adjusting the figures of their manuscript and this clearly helped to improve the quality of their work. Especially Fig. S15, the adjustments to Fig. 5 and the inclusion of the new Fig. 8 and Tab. 2 are important to mention here.

- Even though the authors adjusted parts of the text, I am not fully convinced that the terms gravitational mass movements, landslides, and rock glaciers have been defined in a sufficient clarity. I commented on this aspect extensively (GC-1, SC-2, SC-12, SC-19), and I acknowledge the decision by the authors to keep the landslide movements in their analysis. However, following a clear and consistent terminology would be desirable for the clarity of the text. In this regard, "gravitational mass movements" (L232, 403; "gravitational slope mass movements" (L22) ◊ is this a typo?) seems a fitting term, though most geomorphology textbooks would not list "rock glaciers" under the term "mass movements". I would therefore recommend to separate the identified landforms, e.g. in L22, into 153 rock glaciers and 229 gravitational mass movements. In the introduction, I want to recommend to include a clear definition of what is considered a “mass movement” and a “landslide”, as these are terms that are not used consistently throughout the manuscript.

Response > We agree with the reviewer regarding the inappropriate use of the term “gravitational mass movements,” which does not accurately encompass all the landforms identified in our study. This imprecision likely stems from common usage within the landslide research community, where “mass movement” is often employed as a synonym for “landslide.” To eliminate this ambiguity, we have added a clear definition of “landslide” in the Introduction. Furthermore, we have removed the term “gravitational mass movement” throughout the manuscript and now explicitly refer to the specific landform types identified. The revised text now reads as follows:

> *[…] (Cusicanqui et al., 2021; Haberkorn et al., 2021). These changes also favour landslides (**the downslope movement of soil, rock, and organic materials under the force of gravity**). **For instance, recent warming induces an increased frequency of landslides (Pei et al., 2023).** However, warming affects mountain permafrost differently […]*

On the other hand, we also corrected term “gravitational mass movements” within the lines L22, L232 and L403. Now you can read:

> L22: *[…] quantification of surface kinematics of **1153 rock glaciers, 124 landslides and 105 unclassified landforms** over a 24-years, across a 2250 km² area. […]*
>
> *L232: […] Due to the limited spatial extent of the VHR dataset, **we used raw Sentinel-1 wrapped interferograms to validate the classification of the L7/8 surface displacement products** (cf. Section 4.3). […]*
>
> L403: *[…]* These confirmed PMAs correspond to rock glaciers and mostly large landslides (Tab. 1) *[…]*

Specific comments

SC-1. L329-330: Please rephrase this sentence. Geomorphology interpretation = geomorphic interpretation?

Response > We modified the sentence to be more specific. Now you can read:

> *[...] When no clear interpretation about the movement and **geomorphic interpretation** could be assessed on either InSAR or Google-Earth basemaps, [...]*

SC-2. L348-349: Maybe rephrase and elaborate this slightly, my comment was rather thought as a suggestion, not for literal use;-) In my view, natural behaviour/reasons would be more fitting than causes.

Response > We modified the sentence to be more specific. Now you can read:

> *[...]* due mainly to natural **behaviour of rock glaciers—**increased friction and low/no ice content in lateral margins—as well as *[...]*

Technical corrections

SC-3. L308: “river banks erosion” ◊ river bank erosion

Response > Modified as suggested

SC-4. L860: “recent deglaciated” ◊ recently deglaciated

Response > Modified as suggested

SC-5. L866: better: Material and water supply?

Response > Modified as suggested

SC-6. I realize that the authors changed the unit from m yr-1 to m a-1 throughout the manuscript. This might have been in reaction to a comment by the anonymous reviewer? Looking at the guide for authors from TC, I would think that m yr-1 is more appropriate:

o “Ma and Myr (also Ga, ka; Gyr, kyr): "Ma" stands for "mega-annum" and literally means millions of years ago, thus referring to a specific time/date in the past as measured from now. In contrast, "Myr" stands for millions of years and is used in reference to duration (CSE, p. 398; North American commission on stratigraphic nomenclature).” ◊ https://www.the-cryosphere.net/submission.html

Response > Thank you for your suggestion. We carefully reviewed the International System of Units (SI) Brochure available on the Copernicus website. The document does not explicitly state a preference between the use of $m\ yr^{-1}$ or $m\ a^{-1}$ for expressing velocity. In light of this, and to maintain consistency and clarity, we have standardized all relevant notations in the manuscript and figures using $m\ yr^{-1}$.

Response to comments by Anonymous Referee

We sincerely thank the Anonymous referee for the thoughtful comments and constructive suggestions, which have greatly contributed to improving the clarity and overall quality of our manuscript. Below, we provide a point-by-point response to each comment.

General comments

SC-7. I am grateful to the authors for the detailed response and the various improvements to the manuscript. In particular, the method description has improved considerably, and the added subsection on uncertainty estimates in the results also strengthens the manuscript. However, I continue to have major concerns about the uncertainty analysis of the velocity change and the evidential basis of several statements in the conclusion section. In addition, the presentation warrants further improvement, as language issues such as incomplete sentences detract from the content.

Velocity change analysis I continue to have major reservations about the correctness of Equation 2 and am concerned about the absence of quantitative estimates and uncertainty estimates in Section 5.5. a) Equation 2: Equation 2 looks wrong and the assumptions and derivation remain unclear.

Response > We sincerely thank the reviewer for identifying this error. We acknowledge that there was a mistake in the derivation of Equation 1 related to the calculation of relative uncertainty. This has now been corrected, and the updated Equation 2 is presented as follows:

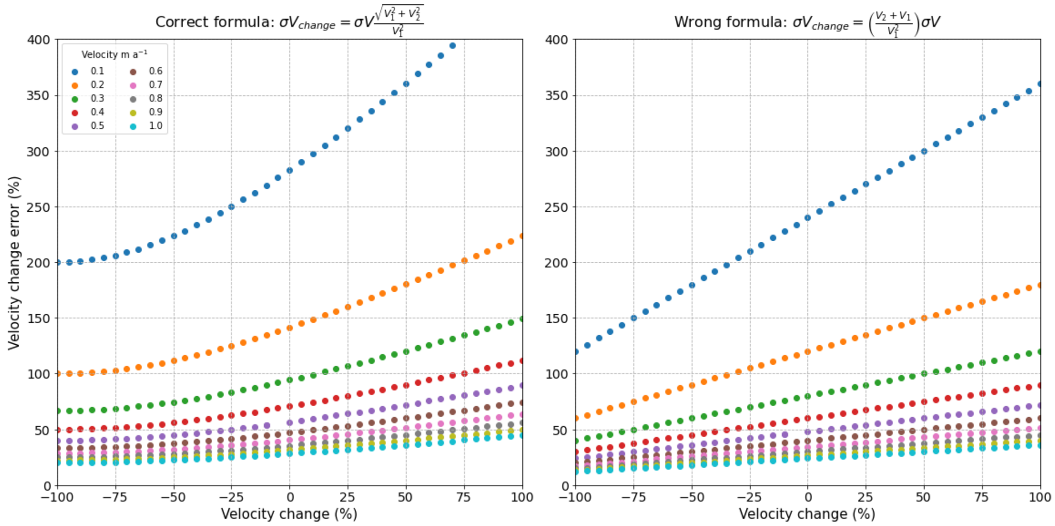
$$\sigma V_{change} = \sigma V \frac{\sqrt{V_1^2 + V_2^2}}{V_1^2} \quad (2)$$

This error led to an underestimation of the relative uncertainty. To illustrate the implications of this correction, we compared the uncertainties in velocity changes obtained using both the incorrect and corrected formulas (Figure A). Figure A displays the percentage of velocity change on the x-axis and the corresponding uncertainty on the y-axis for both formulations. As shown in the figure, the previously incorrect derivation resulted in a consistent overestimation of uncertainties. This overestimation reaches up to 57% for PMAs with smaller velocity magnitudes (0.1–0.3 m yr^{-1}), and decreases significantly for velocities above 0.3 m yr^{-1}. For PMAs with an average velocity of 1 m yr^{-1}, the overestimation between the two formulas is reduced to approximately 6%.

Figure A. Modeling of relative velocity changes (ranging from -100% to 100%; Eq. 1) and their respective uncertainties (Eq. 2) using correct and wrong derivation (respectively) for various velocity magnitudes (0.1 – 1 m yr^{-1}).

Accordingly, we have updated all affected calculations. However, the overall results remain largely unchanged, particularly regarding the number of PMAs for which velocity changes exceed the uncertainty threshold (σV change). The number of PMAs meeting this criterion decreased slightly, from 11 (9 rock glaciers and 2 landslides) to 8. Among these, we identified 3 rock glaciers, 2 landslides, and 3 unclassified PMAs.

We have revised Section 5.5 to reflect these updates and included a new figure to present the results more clearly. The updated Section 5.5 now reads as follows:



[…] Using the 24-year surface displacement dataset, decadal velocity changes (Eq. 1) and velocity change uncertainties (Eq. 2) were computed using Top 50% average velocity over two periods: 2000–2014 (V1) and 2013–2024 (V2), across all PMAs. However, since relative velocity changes depend on the initial velocity magnitude (Eq. 1), velocity changes on PMAs with smaller magnitudes (<0.3 m yr^{-1}) exhibit higher uncertainties. According to our calculations, only 2% (n = 8) of the entire PMA dataset exhibits velocity changes greater than their respective uncertainties (σVchange;Fig. 8). Among these, 3 rock glaciers, 2 landslides, and 3 unclassified PMAs, were identified. These 3 rock glacier PMAs have an average size of 6,075 m^2 (~27 pixels) with a Top 50% average velocity of 0.59 m yr^{-1}. Two (one) of them, accelerate (decelerate) with a mean value of 198% (-46%). Landslide PMAs have an average size of 15,412 m^2 (~69 pixels) and a Top 50% average velocity of 2.5 m yr^{-1}. However, only 2 cases exhibit acceleration with a mean of 214%. PMAs in the 'unclassified' class have an average size of 7,050 m^2 (~31 pixels) and a Top 50% average velocity of 0.44 m yr^{-1}. One (two) accelerates (decelerates) with a mean value of 70% (-42%). […]

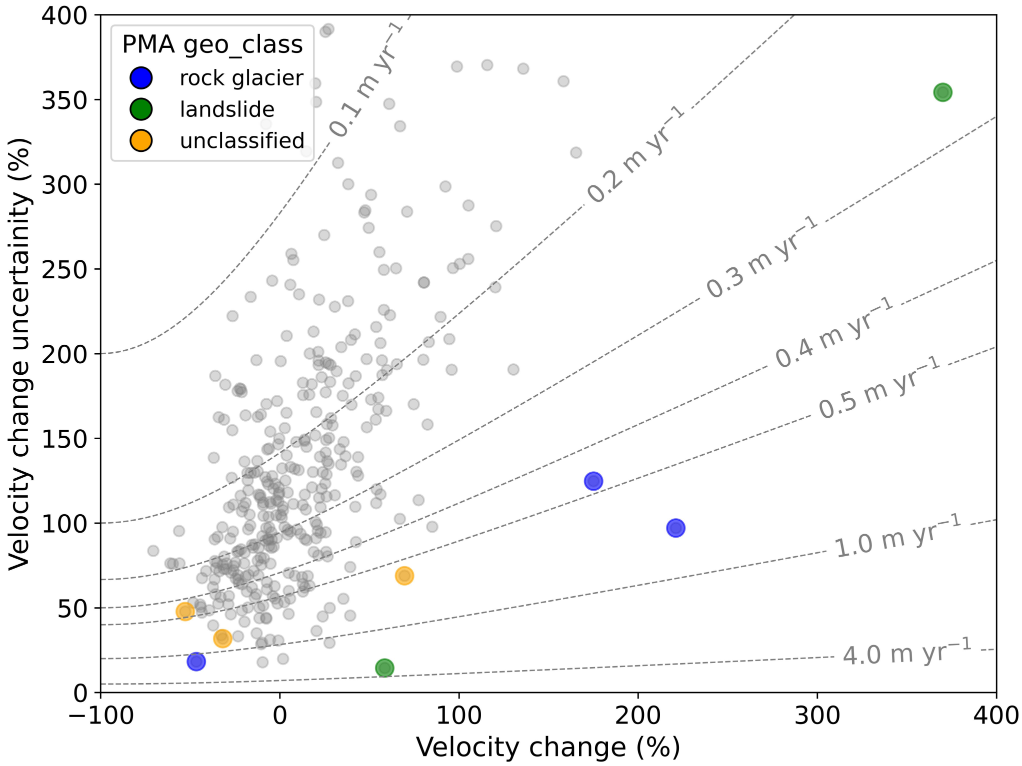


Figure 8. Modeling of relative velocity changes (dashed lines; Eq. 1) and their respective uncertainties (Eq. 2) for various velocity magnitudes (0.1 – 4 m yr^{-1}). Grey dots represent the entire PMA dataset. Blue, green and orange dots highlight PMAs where velocity changes exceed their uncertainties.

SC-8. Assuming the errors in V1 and V2 are uncorrelated and their variances identical and equal to σ2V (none of these assumptions is invoked in the manuscript), ...

Response > The assumption of uncorrelated error is mentioned two times in the manuscript. The first one is mentioned in <u>Section 4.4. Average spatial velocity and relative velocity changes</u> in line 322:

> *[…] assuming that the NMAD for both periods is **similar and not correlated** (σV; cf. Section 5.4). Finally, […];*

and more explicitly in <u>*Section 5.4. Reported uncertainties*</u>*,* In line 451:

> *[…] The NMAD is **0.21 and 0.19** m yr^{-1}, for 2000-2014 and 2013-2024 periods, respectively. […];*

SC-9. Conversely, the authors' equation (2) replaces the numerator with V1 + V2. I mentioned last time that their equation can give meaningless negative results for negative values of V1 or V2.

Response > We acknowledge that there was an error in the derivation of Equation 1, which led to some misinterpretations. Equation 2 has now been corrected and is presented as follows:

$$\sigma V_{change} = \sigma V \frac{\sqrt{V_1^2 + V_2^2}}{V_1^2} \quad (2)$$

As mentioned in our previous response, since we are computing velocity changes relative to the first time period (V_1), negative values are expected. These negative values indicate deceleration between the two observation periods, which is a meaningful outcome in the context of slow-moving landslides and rock glaciers. Such landforms may experience deceleration or transition into a deactivation phase over decadal timescales, making negative velocity changes not only plausible but also physically consistent with known geomorphic behavior.

SC-10. b) Results: Section 5.5 The second half of the only paragraph states selected relative velocity changes, but no comprehensive presentation of quantitative estimates or their uncertainty is provided. While Fig. 5 is referenced, neither the relative velocity change nor its uncertainty is directly shown. Given the claims in the conclusion about the estimation of velocity changes, I feel the reader needs to be presented quantitative information to appraise the authors' claims.

Response > We have updated section 5.5 accordingly. Please refer to response of comment GC-1 in this manuscript to look at the modifications.

SC-11. Unclear basis for conclusions The conclusion sections continues to contain statement for which I cannot identify solid evidence.

In particular, a) A sentence I highlighted in the same context in the first round "Although underestimations occurred due to pixel size, temporal data gaps and velocity field heterogeneity, decadal velocity changes were observable under certain conditions, particularly for features exceeding 1 m a-1". What is the evidence that pixel size and temporal data gaps caused the observed underestimation? What is the precise meaning of observable, and how was this conclusion established? I assume you mean features whose velocity exceeds 1 m/a on average.

Response > The evidence supporting the underestimation of velocities is already presented in Section 5.3, and is illustrated in Figures 6 and 7, as well as in Table 2 (see the columns titled "Difference in velocity"). This issue is discussed as follows:

> *[…] Quantitatively, the average velocity differences between VHR and GNSS points is 0.01 ± 0.05 m yr-1 (Tapado complex) and 0.38 ± 0.3 m yr-1 (Largo rock glacier). Meanwhile, the average difference between L7/8 and GNSS points is 0.18 ± 0.24 m yr-1 (Tapado complex) and 1.35 ± 0.84 m yr-1 (Largo rock glacier; Figure 7). The good agreement on slow surface velocities on the Tapado complex could be explained by the homogeneous surface velocity field in both datasets (Fig. 6a). However, this consistency is not observed on the Largo rock glacier, where large differences are likely due to the heterogeneity of its surface velocity field. Figure 6c shows a single PMA that could be either divided in two, splitting Largo rock glacier in two different units, with likely independent dynamics. This is not the case for the VHR velocity field, showing rather a more homogeneous spatial distribution of velocities (Fig. 6d). […]*

The heterogeneous surface velocity fields derived from the L7/8 dataset over the Largo rock glacier (Figure 6) are primarily attributed to the size of the correlation windows used, along with the limited image texture and contrast. Although this rock glacier displays characteristic ridge-and-furrow geomorphology, the velocity patterns obtained from L7/8 imagery do not correspond well with the pseudo-ground control points (pseudo-GCPs) or with the surface velocity fields derived from the very high-resolution (VHR) dataset. These discrepancies are further addressed in Section 6.1.

[…] In contrast, Largo rock glacier presents greater complexity. Despite its ridge-and-furrow morphology, its homogeneous texture (Fig. 2d) reduces contrast, potentially explaining observed discrepancies between the L7/8 and VHR results (3–4 m yr-1; Figure 6b). L7/8's lower resolution, which capture less surface detail, affects velocity estimates in landforms with high internal variability. Therefore, correlation parameters are key when performing image correlation (Kääb & Heid, 2012; Leprince et al., 2008; Rosu et al., 2015). As L7/8's smallest matching window (3x3 pixels, covering 2025 m²) differs substantially from the VHR window (7x7 pixels, covering 49 m²) leading to an averaging effect. This difference contributes to the observed variability in features such as Largo rock glacier. *[…]*

Based on these observations, we believe that sufficient evidence has been provided to support our interpretation. Furthermore, the validation of surface velocity fields using GNSS data reinforces the reliability of our interpretations and conclusions.

b) "Below this threshold, velocity changes detected with L7/8 data were not statistically significant." I am not aware of any formal statistical test that was conducted (see 1) and hence cannot identify the empirical basis for this claim

Response > We acknowledge that no formal statistical tests were performed in this study. Instead, we considered velocity changes to be significant only when they exceeded their respective uncertainty values. For further details regarding this approach and the modifications made, please refer to our response to comment GC-1. The corresponding sentence has been revised as follows:

[…] Although **some** underestimation occurred due to pixel size, temporal data gaps and velocity field heterogeneity, decadal velocity changes were **for 2% of PMA dataset (n = 8). Among these PMAs, we find acceleration (deceleration) in 2 (1) rock glaciers, 2 landslides, and 1 (2) unclassified PMAs, all exceeding their respective uncertainties. According to our calculations, detecting decadal velocity changes smaller than 0.4 m yr^{-1} involves relatively high uncertainties when using L7/8 data.** *[…]*

SC-12. Presentation I encourage the authors, especially the senior authors, to make further improvements to the presentation. Language issues include missing articles (e.g. "Average velocity field" in l 296 [or plural]), incomplete sentences ("By considering PMAs with velocities exceeding 1 m a-1, nine rock glaciers and 2 landslides." in line 467); or sentences such as the following from line 312: "Stable areas were defined using TanDEM-X DEM and slopes lower than 35°, without taking into account neither glacier outlines with a buffer of 500 m for each glacier (RGI Consortium, 2017) nor all PMAs, also not confirmed ones produced in this study." Do you mean that glaciers and their surroundings (500 m buffer) and PMAs were excluded?

Response > We modify the text as suggested. Now you can read:

[…] over stable areas,defined using TanDEM-X DEM and slopes ***below 35°****.* ***Glaciers outlines from RGI consortium (2017) and surroundings (with a 500 m buffer) and*** *all PMAs****—both confirmed and unconfirmed—were excluded****. Stable areas account for 53% of the study area (45x45 km²; Fig. S4). […]*

SC-13. This sentence is a representative example of the verbose style that I find difficult to follow. Adopting a more concise style and reducing the word count by ~20% could make this manuscript easier to follow.

Response > We agree with the reviewer that the writing style and wording required further improvement. Although the manuscript had already been reduced by 15% during the first revision, we conducted an additional thorough review, focusing on simplifying complex sentence structures—particularly those written in the past tense. This effort resulted in an additional 10% reduction in length, achieved without compromising clarity, coherence, or scientific content. We believe that further reduction would risk omitting essential information. In addition, language issues have been systematically addressed throughout the manuscript to ensure consistency and improve overall readability.

SC-14. Consider including a quantitative statement on the uncertainty estimate in abstract and conclusion

Response > We added an small assessment about the uncertainties in the abstract and the conclusions. Now you can read:

Abstract:

> *[…]* Nevertheless, decadal velocity changes were observed in 2% of PMAs, where two (one) rock glaciers show a significant acceleration (deceleration) over two decades. **Our calculations show that decadal velocity changes < 0.4 m yr^{-1} are associated with high uncertainty when using L7/8 data, with sensitivity depending on the reference period.** Our results highlights *[…]*

Conclusions:

> *[…]* respective uncertainties. **According to our calculations, detecting decadal velocity changes below 0.4 m yr^{-1} (two times decadal NMAD values) using L7/8 data involves high uncertainty, depending on both velocity magnitude and the length of the reference period.** The results of this study *[…]*