Author´s response to referee comment # 2

First of all, we would like to thank the reviewer for such a detailed and constructive review. It has shown us which aspects need to be explained in more detail and has certainly helped us to improve the manuscript. Bevor responding to every comment, we would like to mention, that we adapted the manuscript in the following points to ensure a better readability:

- **We will follow the reviewers recommendation to restructure the current chapter 2 “Methods and Data”.**
- **We fully agree that, based on our tests, automated derivation of surface velocities cannot be ruled out (the manuscript will be adapted accordingly). We would rather point out the high ablation, which overlays the ice dynamic feature change, and thus explain why we choose manual feature tracking.**
- **The reviewer suggested to skip parts of the manuscript by focusing either on historical data or on the interpolated present-day velocity map. As mentioned, we don't want to rule out automated derivation of surface velocities. The focus of the manuscript is more on the historical data sets, whereby we believe that an examination of current ice dynamics is relevant in order to provide a velocity map ready for other studies, e.g. ice flow modelling**

In the following, we quote the reviewer´s comments followed by our replies, which are marked in orange. Since the reviewer posted many comments (i.e. general, major, specific with several subpoints), we tried to organize that with a kind of chapter structure to make cross-referencing easier. His/her general comments receive chapter index 1, while major comments chapter index 2, with every subpoint indexed accordingly. Sub-items within a comment are also numbered with numbers in brackets.

General comments

The manuscript by Dobler et al. analyzes the ice surface velocity of Vernagtferner, as an example of a well-monitored slow-flowing mountain glacier.

Leveraging 60 years of annual observations of stake positions, the manuscript draws a link between the glacier mass balance history and the patterns of ice speed-up and slowdown. After concluding that existing remote sensing products fail to resolve the slow flow field of Vernagtferner, the manuscript presents a map of present-day surface velocities over 2018-2023, compiled by interpolation of multi-source point measurements such as stakes and manually tracked features.

The topic of remote and in situ ice velocity measurements on slow-flowing glaciers is certainly of current interest, and a report on the long-term dynamics investigations at a data-rich site is definitely relevant and holds significant potential to advance knowledge of the ice dynamics of mountain glaciers.

However, the main direction of the manuscript is not fully clear in its current form - the two covered topics (re-analysis of long-term stake data and compilation of present-day velocity map) are only weakly linked. As such, the main message and achievements of the manuscript are somewhat hard to understand. Moreover, the manuscript stops short of advancing the current knowledge on the topic of ice dynamics. The glaciological conclusions from the analysis of such a rich historical dataset are somewhat qualitative and generic, and in some cases are not adequately supported by the collected evidence. Much of the data needed for interpretation of the ice dynamics (such as historical local mass balance, or changes of ice thickness and surface slope) are not adequately presented in the manuscript. Furthermore, the proposed method to compile a velocity map (in part) from manually tracked point measurements is affected by some significant flaws, raising questions about its suitability and advantages compared to state-of-the-art automated methods. Finally, the uncertainty analysis relies on several arbitrary estimations and assumptions, rather than existing, well-established methods to quantify uncertainty in remotely sensed glacier dynamics.

1.1 Thank you for pointing this out. We will explain these points in detail below.

- Redefine research question see response to general comment 1.2.
- Strengthening glaciological interpretation and additional supporting evidence, see response to major comment 2.5.
- Add missing datasets, see response to major comment 2.1(4).
- Address methodological weakness in velocity map construction see response to major comments 2.3 and 2.4.
- regarding uncertainty analysis see response to major comment 2.6.

In light of this, I would suggest to repeat some key parts of the analysis, after reviewing the literature for the most appropriate methods to be applied to the high-quality datasets of Vernagtferner. I would suggest resubmitting the manuscript to reflect these major changes. In particular, I would suggest adjusting the scope to focus more on just one of the two topics - either (i) the compilation of the present-day velocity map, or (ii) the analysis of the historical dataset and its glaciological interpretation. The two topics are quite loosely related and the manuscript would really benefit from a clearer message, answering one or more well-defined research questions.

1.2 We define our research question more clearly. The aim of the manuscript is not to rule out the automated creation of a velocity map (we will reword the relevant sections of the manuscript accordingly), but rather to highlight the problems/challenges associated with high ablation for retrieving glacier velocities on slow-moving glaciers with remote sensing techniques. However, your raised points in this response convinced us to explain in more detail. We try to better explain what we mean by the problem of "high ablation" in the context of feature tracking (detailed information on this can be found in the response to major comments 2.3). Overall, the manuscript focuses more on (ii) the analysis of the historical data set, and we have also expanded the analyses to reflect this. However, we believe that detailed information on current ice dynamics is also highly relevant when analyzing historical data, especially since high-resolution data is available for the current period. Since the dynamics are now only minor, the maximum remaining velocities are particularly relevant. We think, generating a present-day velocity map is a logical consequence based on the data availability in order to provide a velocity map ready for other studies, e.g. ice flow modelling (either used as validation as target for an inversion).

For topic (i), I would specifically suggest application of digital image correlation on the UAV, aerial and/or satellite imagery, with an appropriate pipeline for pre-filtering, post-processing, and aggregation. Multiple studies (e.g., [1], [2]) have shown that data processing specifically optimized to a study site can resolve the ice dynamics of individual glaciers much better than global products. Given the high-quality available data (such as four years of end-of-season, whole-glacier airborne photogrammetry), I expect this processing to resolve very well the

slow movement of Vernagtferner and automatically produce good-quality velocity maps, possibly even at annual intervals and on the whole glacier body, contrary to the current manuscript's conclusion that "It is obviously not possible to (semi-)automatically produce a reliable surface velocity map from aerial or satellite imagery for the slow-flowing Vernagtferner". The stake measurements (and possibly the manually tracked feature) would be a valuable reference for validation and uncertainty estimation. This kind of manuscript might be most suitable for a data-focused journal.
1.3 As mentioned, the focus should not be on excluding possible (semi-)automatic detection methods for the Vernagtferner. This statement will be removed from the manuscript, as we don't test all possible methods. Instead, we would like to point out the special features of (automated) detection with high ablation. More detailed information on this can be found in the response to major comments 2.3.

For topic (ii), I would specifically suggest a more thorough re-analysis of the very interesting historical stake data, possibly including: (1) a more realistic model approach than the shallow ice approximation, for example an IGM inversion, given the high-quality data available over the entire glacier; (2) a better-processed ice thickness map, without the obvious major artifacts visible in Fig. 7; and (3) a more detailed analysis of the interplay of surface slope, glacier thickness, and stake velocity anomaly. The recent measurements shown in the manuscript could still be mentioned to compare spatial patterns over time and to investigate seasonality. This kind of manuscript would be most suitable for a glaciological journal.
1.4 Thank you very much for your valuable suggestions for further analysis. Regarding:

(1): You are right, a more sophisticated model would be appropriate for modelling the response of VF with the observed data for validation/tuning/inversion. However, our intention was to estimate whether the observed dynamic trends in the historical velocity data are connected to mass balance trends (i.e. ice thickness). We think for this rough estimation a simple model (i.e. SIA) is a fair approach, particularly, as our paper is not focusing on modeling. Using an IGM inversion would be a good approach but shifts the focus more to modelling rather than on the observations. The data on ice thickness and surface slope for the individual years are heavily interpolated (more on this under response to major comments 2.1 (1)), making realistic modeling difficult based on the initial data. However, the focus of the manuscript is on showing the velocities over time. Therefore, we only want to use SIA modeling to generate an uninterrupted timeline of the geometry in order to show that the values are realistic in principle. This is therefore not an ice dynamics model in the true sense, but rather a first approximation. We will explain this explicitly in the manuscript.

(2): The artefacts visible in Fig. 7 have been corrected (for result see the response to minor comment of Fig.7) . The calculation of ice thicknesses is explained in more detail under response to major comments 2.1 (1).

(3): Discussion of the analysis of the anomalies of various parameters such as ice thickness and surface slope can be found under response to major comments 2.5.

In both cases, it is important to perform a quantitative, data-driven uncertainty analysis, based on leave-one-out validation and rigorous error propagation, as described in the references provided below.

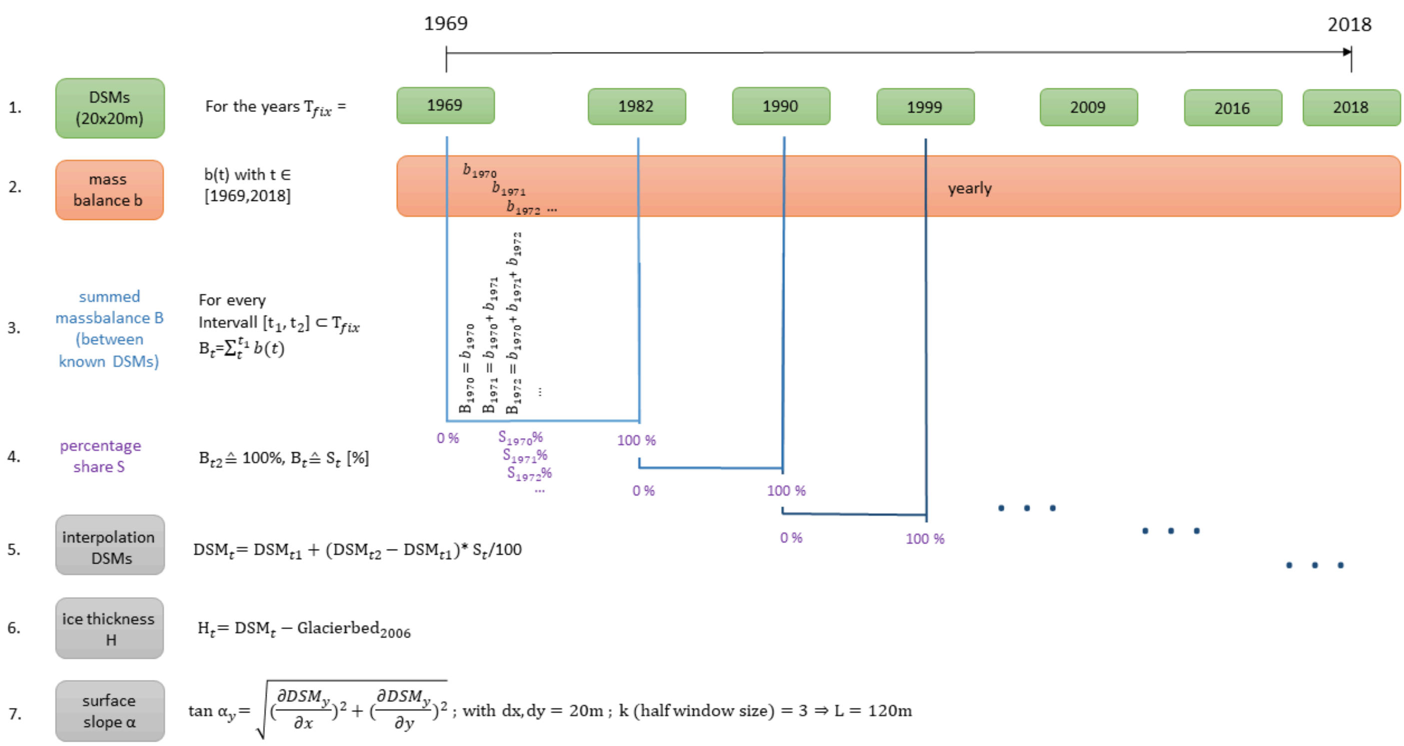
Detailed information under response to major comments 2.6 and specific comments.

Specific major comments:

- Presentation of the Data and Methods needs major restructuring in order to support the analysis. In the current form, it is really hard to understand where some important data come from or how they were processed. In particular, it is important to provide full information on: (1) the data sources used for glacier thickness, as well as all processing applied to (i) extrapolate it to the full glacier area, (ii) compute evolution over different time periods, and (iii) extract it at the stakes location; (2) all the stakes (at the very least, all the stakes whose data are plotted in the figures), with their identification numbers, observation period, and maximum/minimum altitude; (3) the actual calculation of modeled velocities according to the shallow ice equation, detailing the estimation and assumptions made for each variable; and (4) the processing of UAV and aerial data, including georeferencing, the used software pipelines, and the resulting spatial resolution and accuracy. Some relevant pieces of information are already mentioned in the manuscript, but in a rather scattered form across the sections, and should be rearranged.

2.1 Thank you for pointing this out. We reconstructed the Methods and Data section.

(1): A flow chart as well as the following explanation for calculating ice thickness and surface slope for each year was added:



To provide an ice thickness for each averaged period, the surface change between the known periods (DSMs) was scaled using the mass balance data at each date and the known

glacier bed topography was subtracted. The mass balance is described by a single value per year, as area-based mass balance is not available for all periods. The error of non-linear ice transport over time is neglected, as the velocities are relatively low. The calculated ice thickness and surface slope is cut to the least known glacier extent, an overview of the existing extents is in the Appendix.
The extraction on the stake position is carried out through the nearest neighbour.

(2): An overview of the measurement points that appear in the figures will be added to the appendix.

3): A chapter on the shallow-ice approximation modeling, including formulas used and estimated parameters, will be added:

We add a new paragraph to the data and methods section

shallow-ice approximation

Using a simplified shallow-ice approximation, the surface velocity can be derived from the surface slope (α) and ice thickness (H) (e.g. Hutter 1983, Grever and Blatter 2009). This is also referred to as a 'zero-order' model, whereby only vertical shear stress gradients are taken into account. The total velocity (u_{total}) consists of the deformation velocity (u_{deform}) and the basal sliding velocity (u_{basal}). We only want to use SIA modeling to generate an uninterrupted timeline of the geometry in order to show that the values are realistic in principle. This is therefore not an ice dynamics model in the true sense, but rather a first approximation. Since the assumption of uniform basal sliding over a period of more than 30 years is certainly not accurate, and the velocities in recent times are relatively low anyway, making it difficult to distinguish between basal sliding and deformation velocity, a basal sliding velocity of 0 was assumed for simplicity and the modeled velocities were calculated purely on the basis of ice dynamics. Relatively soft ice was used for this purpose, therefore we use for the rheology parameter A a value corresponding to 0 degrees, according to Paterson 1994. We just want to show the link between geometry and ice dynamics.

(u_{deform}) = (2*A / n+2)) * τ^n * H)

, where τ is the shear stress, described as:

$\tau = -\rho * g * H * \sin(\alpha)$

The selected physical flow parameters are summarized in the following table:

parameter	**value**	**unit**
A	2.2e-16	Pa^-3 a^-1
n	3	
ρ	900	kg/m^3
g	9.81	m/s^2

(4): An overview of all available data is added as an appendix:

dataset	date of acquisition	sensor/instruments	resolution [m]	processing	coverd area	source / access	dataprovider
stake measurement	yearly, end of september [1966-2023]	nultiple forward intersections, tachymetric polar coordinate or GNSS	/	remove gross errors	entire glacier, mainly ablation area	https://doi.org/10.1594/PANGAEA.854595	
	2022-08-04, 2022-07-10, 2020-09-21, 2023-08-08	multi-frequency GNSS receiver	/	post-processing with fixed ambiguities	Parts of Taschach and Brochkogel	https://doi.org/10.1594/PANGAEA.854595	
glacier bed topographie	2006 & 2007	Radar transmitter with antenna	20	see Mayer et al 2013	entire glacier	zenodo DOI	Mayer et al 2013
Orthofotos	2022-07-10 and 2022-09-21	UAV DJI with optical RGB camera	0,04	Georeferenzierung with ground control points, orthophotognerierung in Agisoft Metashape	Parts of Taschach and Brochkogel	zenodo DOI	
	2018-09-16	Optical airborne photogrammetry, UltraCam Eagle Mark 2	0,2	Aerotriangulation: Inpho(Match-AT) by Trimble, orthofoto generation: SURE (nFrames)	entire glacier	publication do to legal rights not possible	Geissler 2021
	2020-09-08	Optical airborne photogrammetry	0,2	see tiris Tirol	entire glacier	tiris Tirol online	
	2021-09-25	Optical airborne photogrammetry	0,2	information not available	entire glacier	publication do to legal rights not possible	
	2022-08-23	Optical airborne photogrammetry	0,2	information not available	entire glacier	publication do to legal rights not possible	
DSM	1969	aerial photogrammetry	20	information not available	entire glacier	zenodo DOI	
	1982-09-14	aerial photogrammetry	20	information not available	entire glacier	zenodo DOI	
	1990-08-24	aerial photogrammetry	20	information not available	entire glacier	zenodo DOI	
	1999-09-09	aerial photogrammetry	20	information not available	entire glacier	zenodo DOI	Weber 2013
	2009	Laserscaning	1	information not available	entire glacier	zenodo DOI	
	2016-09-29	aerial photogrammetry, Canon EOS 5D Mark II	1	Photogrammetric analysis: PIX 4D 4.2.27	entire glacier	zenodo DOI	
	2018	aerial photogrammetry, UltraCam Eagle Mark 2	2	Aerotriangulation: Inpho(Match-AT) by Trimble, DSM generation: SURE (nFrames)	entire glacier	publication do to legal rights not possible	Geissler 2021
glacier outline	1969,1979,1990,1999,2009,2016,2018		/	information not available	entire glacier	zenodo DOI	
mass balance	yearly 1966-2024	glaziological mass balance	/		entire glacier	WGMS	
global dataset ice dynamic		ITS_LIVE	/	see Gardner et al 2022	entire glacier	Gardner et al 2022	
		FAU´s glacierportal	/	see Friedl et al 2021	entire glacier	Friedl et al 2021	
		Millan´s dataset	/	see Millan et al 2022	entire glacier	Millan et al 2022	
satellite SAR imagery	see Appendic C	Terra-SAR-X	2	GAMMA Remote sensing Software	entire glacier		

Literature/Links to the table:

- Weber 2013: Dokumentation der Veränderungen des Vernagtferners anhand von Fotografien
- Mayer et al 2013: Vermessung und Eisdynamik
- tiris Tirol: https://lba.tirol.gv.at/public/karte.xhtml
- Geissler et al 2021: Analyzing glacier retreat and mass balances using aerial and UAV photogrammetry in the Ötztal Alps, Austria

Unfortunately, much of the information, such as the software pipelines used or the georeferencing of the source data, is unknown. Where possible, references to the respective source are provided, but particularly regarding the historical surface models there is no known information. As long as there are no legal reasons to the contrary, the data sets are going to be made publicly available via the Zenodo platform. This information can also be found in the table.

- The discussion lacks almost any comparison of the findings of the present study with those of other investigations, in particular those about (1) long-term trends of ice velocities at reference glaciers in the European Alps, which are monitored at several key sites across the Alpine countries (a single publication on Hintereisferner is currently cited); (2) the derivation of velocity maps of slow-moving glaciers from remote sensing (e.g., [2], [5]); and (3) previous examinations of the dynamics of Vernagtferner (ll. 91-92). It is crucial to situate the findings of the current study in the context of published results, in order to highlight advancements and challenges.
2.2 (1): That´s true. We are expanding the discussion:

In the long-term observation of the d’Argentière glacier, Mont Blanc area, Vincent et al 2009 show an ongoing negative net mass balance since 1982, with a direct reflection (delayed by a maximum of 3 years) in the velocity. They describe that the change in velocities in the upper part of the glacier is smaller than in the lower part (ablation area). The velocity trend thus fits the VF timeline quite well. A larger change in velocities in the ablation area suggests that the current status of the glacier can be derived relatively well, especially at the ablation stakes, as we suggest for the VF.

see also expansion due to discussion of possible “direct response”, and compare to studies in response to major comment 2.5.
2): Thank you for pointing that out. We are expanding our discussion regarding the derivation of velocity maps for slowly flowing glaciers, as follow:

Even for relatively slow-flowing glaciers, such as the Griesglacier, surface velocities can be derived using software such as IDMatch, a tool for automated velocity derivation (Gindraux 2019). Compared to the VF, the average velocities on Griesglacier with more than 1 m in just 1 month are significantly higher than the average velocities at the VF of about 1 m per year. Furthermore, a short test of the IDMatch software using the UAV VF datasets from July 2022 and September 2022 does not provide any results, as no correlations can be found. Regardless of the software and the possible detection of identical features, for VF there remains a strong overlap due to ablation (see Appendix A in response to major comment 2.3), thus distorting the results. We do not want to rule out the possibility of automated derivation, but it would be essential to additionally correct for the false values caused by the ablation-induced change in features (a change not caused by ice dynamics). For this reason, we have chosen manual feature tracking. Examples of the differentiation between ablation-induced feature changes and ice dynamics-induced feature changes in manual feature tracking are presented in Appendix A (in response to major comment 2.3).

(3): The two current studies show the changes in velocity up to the respective point in time, with basic analysis. Since this information is already contained in the manuscript, these studies will not be discussed in detail. However, since the data preparation up to the respective point in time and a rather rudimentary display were carried out here, we would like to mention their valuable work at this point. To avoid misunderstandings, we rewrite the sentence to: The derived velocities have been displayed in previous studies up to the respective point in time.

- The manuscript claims that "It is obviously not possible to (semi-)automatically produce a reliable surface velocity map from aerial or satellite imagery for the slow-flowing Vernagtferner", which is the main motivation to propose a manual tracking method (with the resulting downsides for spatial coverage, reproducibility, and labor effectiveness). However, this conclusion is based on observation of some global datasets of glacier velocity, as well as some very poorly detailed testing of automated methods on the available datasets, which are quickly dismissed as an option (ll. 113-119 and 189-199). However, several studies have thoroughly validated the derivation of glacier velocity fields on aerial and UAV imagery with automated methods, such as frequency-domain cross correlation ([3], [4]) and feature tracking ([5], [6]). These methods have been shown to resolve well the ice motion on optical imagery, typically with subpixel accuracy; in particular, the result of [5] (p. 58) clearly shows UAV-based feature tracking fully resolving ice displacements of the same magnitude and time interval as those of Vernagtferner. Thus, a claim of unsuitability of those well-established methods needs to be supported by much better evidence than a quick dismissal, especially given the high-quality available datasets at Vernagtferner (UAV and aerial imagery). The interest of manually pinpointed displacements of individual features is hard to justify without first testing and quantitatively reporting on these state-of-the-art methods.

2.3 You are absolutely right; we cannot rule out automatic methods at this point. We will amend the manuscript accordingly. The aim of the manuscript is not to test all algorithms and rule out the possibility of automation. Instead, we would like to highlight the problems of overlapping due to ablation from a more glaciological point of view and explain why we chose manual detection. We would like to show that detection is not “rudimentary” and cannot simply be carried out using global data sets or standard remote sensing methods. Thank you very much for the reference [5] to a study on the detection of slow velocities. The study shows an average surface dynamics of more than 1 m over a period of about 1 month, with maximum values of up to 3 m. On Vernagtferner, the average velocity is only 1 m per year, with maximum values of 4 m per year. Over a period of one month, Vernagtferner will therefore only reach values of approx. 10 cm to maximum values of approx. 40 cm. This means that the dynamics of the Vernagtferner are once again slightly lower than those of the Griesglacier shown in the study. Furthermore, we only have high-resolution UAV data for the dates 2022-07-10 and 2022-09-21, i.e., for a temporal baseline of 2.5 months (the other datasets have significantly longer baselines). During this period, there was a significant change in surface structures, mainly due to high ablation. Nevertheless, we tested the IDMatch software for the UAV data. Unfortunately, no correlations could be found. This information will be added to the manuscript, and we will also mention further software options. (See also response to major comment 2.2 (2).)

The UAV measurements in 2022 relate to a year of extreme melting. This melting also causes significant changes to surface features. For this reason, we write in the manuscript that “high ablation means that the horizontal melt is even greater than the actual movement.” We did not go into further detail on this. The feedback has shown us that we need to clarify what exactly we mean by changes in features due to ablation. To this end, we would like to include the following examples in the appendix A:

The figures show the same situation at different points in time:

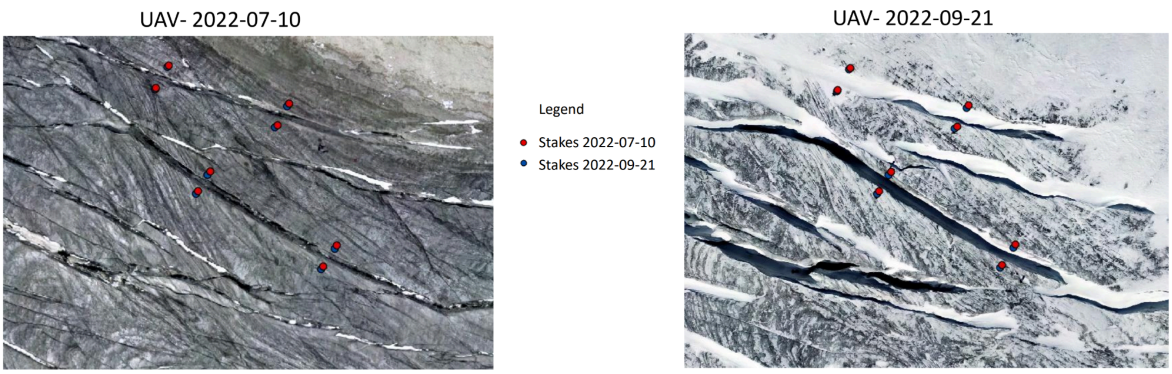


Fig. 2: Melt-induced changes to crevasses. The example is taken from the upper part of the Taschach area at an altitude of approx. 3150 m. Maximum velocities occur in this area at the VF.

Figure 2 clearly shows a change in surface features, in particular a widening of the crevasses. We examined two of the crevasses in more detail using two stakes at the upper and lower edges of each crevasse. The average movement of the eight stakes (shown in Fig. 2) over the two months is approximately 0.73 m (change from the red to the blue points), with a standard deviation across the eight stakes of 0.12 m . The stakes rule out the possibility that a significant actual break-up of the crevasses has taken place. The stakes show an even shift of the upper and lower edges of the crevasses. Thus, the change in the surface is probably largely due to melting. Even if identical features (in this case, crevasse edges) could be identified during the period, the movement would be significantly overlaid by ablation due to the crevasse edges (and their varying melt rates), resulting in erroneous dynamics.

Even with larger temporal baselines, manual feature tracking can be used to exclude features that are likely to have changed due to ablation, as is often the case in 2022. Crevasse trace intersections are particularly well suited for this purpose, as can be seen in Fig. 3. Identifying identical features that have not been subject to high ablation is challenging even for the human eye, especially over longer baselines, such as the example in Figure 4 over a period of two years.

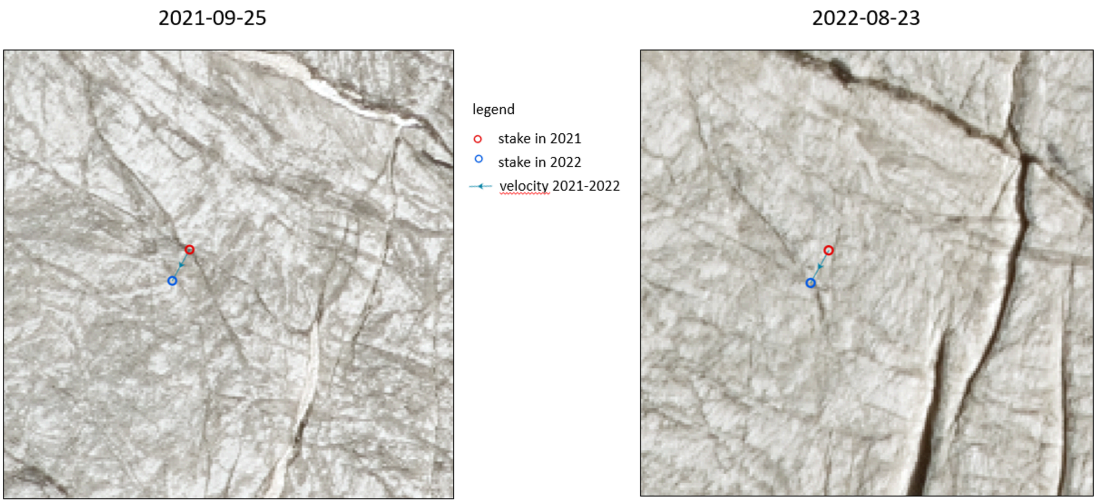


Fig. 3: Melt-induced changes to crevasses. Example from the upper part of the Brochkogel area at an altitude of about 3200m.

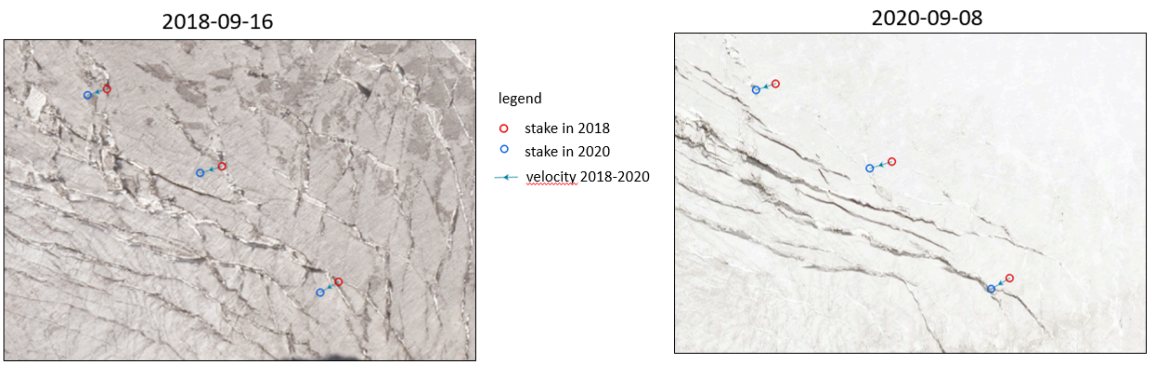


Fig. 4: Crevasse pattern in a period without significant melt. Example from the highest part of the Brochkogel area at an altitude of about 3250 m.

- The 2018-2023 velocity map is aggregated from data collected over 5 years, on the assumption that ice dynamics would "not change much" over that period. However, Fig. 4 and Fig. 5 show ice velocity changes by more than 50 % taking place over 5 year periods at several stakes. Such a change is larger than (for example) the 30 % correction factor applied to convert summer to annual stake velocities in the data used within the velocity map. As such, the validity of the aggregation of such heterogeneous data is questionable and should at least be discussed in the uncertainty budget. Moreover, the assumption of zero velocity at the glacier edges is questionable - a contribution from transverse stress coupling could potentially be significant on such a slow-flowing glacier. See for example [7]. A rigorous analysis and discussion of these uncertainties is required to support the presented results, especially when the manuscript claims that other existing methods and results are not suitable for the study site.

2.4 Thank you for pointing that out. The assumption of an “unchanged” velocity for the period 2018-2023 needs to be explained in more detail and substantiated with data. We used the stake series (measured over at least three epochs in the period 2018-2023) to calculate whether there was a significant average slowdown in velocity from one epoch to the next, taking into account the measurement uncertainty. As explained in the manuscript, the measurement uncertainty of the stakes is +/-10 cm or, taking into account the law of error propagation, +/- 14 cm for the calculated velocity.

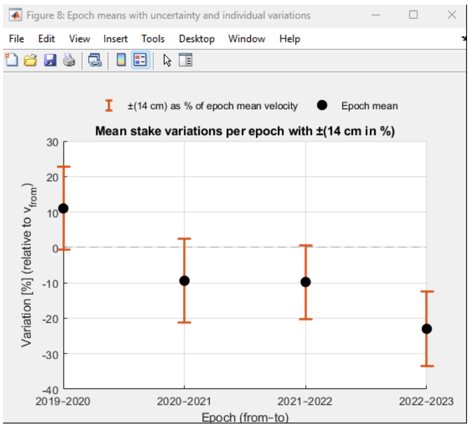


Fig. 5: Average variation in velocity between the periods 2018–2023, for stakes with at least three measured values during this period.

There are no measured values for the period 2018-2019. The variation from 2019 to 2022 is within the measurement uncertainty. Only the period 2022-2023 shows a decrease of approximately 10% (excluding the measurement uncertainty), but there are only three relatively widely scattered measurements for this period. Therefore, it cannot be assumed that there is a significant decrease. However, this uncertainty should be taken into account in the uncertainty budget by including the maximum variation of 10% in the measurement period 2019-2023. We will add the maximum variation in the uncertainty chapter of the manuscript.

We agree that transverse stress couplings are not taken into account when assuming a velocity of 0 m/yr at the glacier margin. Nevertheless, the dynamics at the margin of the glacier are significantly slower than in the center. Since the VF itself flows relatively slowly even in the center, the velocities at the margin will be close to 0 m/yr. The reference to transverse stress couplings will be included in the manuscript.

- The manuscript mentions a "strong sensitivity of velocity to mass balance", claiming that "even the small effect of slightly positive mass balance years on the glacier geometry, results in very pronounced changes of ice flow". While this "effect [...] on the glacier geometry" is not further described, this conclusion suggests that changes of glacier geometry due to positive glacier-wide mass balance would be reflected as faster ice flow in the ablation area already during the same mass balance year, with little to no lag. Such a conclusion is somewhat at odds with the notion of glacier response time and would need to be backed up by some evidence (such as an actual analysis of the mentioned "effect on glacier geometry"). As such, a more thorough analysis of the interplay between glacier mass balance, geometry changes (especially thickness and slope), stake location within the glacier, and stake velocity, is needed.

2.5 We agree that a more detailed analysis of anomalies in, for example, glacier geometry and stake velocities could reveal the sensitivity to the mass balance more accurately. The parameters surface slope and ice thickness for each year are derived from an interpolation, whereby a surface model is only available approximately every 10 years. The scaling of the change in surface area between years is performed using the mass balance (as described in more detail under response to major comment 2.1(1)). However, a continuous stake series extends over a maximum of two of these known surface models before the stake melted out. The remaining surface models and ice thicknesses are derived from interpolation. Accordingly, an investigation of the anomalies is strongly influenced by the interpolation, and this in turn is influenced by the mass balance, among other factors. In order to avoid generating a false dependence of the mass balance on the glacier geometry (slope and ice thickness), we focused on the mass balance parameter and did not carry out any investigations into the glacier geometry.

Furthermore, we fully agree that the effect we describe as “strong sensitivity of velocity to mass balance” suggests the conclusion that changes in glacier geometry due to mass balance are reflected in the ablation area with little or no delay. As already mentioned, we are unable to perform more detailed analyses due to the rough interpolation of the geometry data. The effect can be seen particularly clearly in Figure 4 of the manuscript for the year 1983. Already in the year following a strongly negative mass balance, the velocity appears to decrease significantly. Unfortunately, very little stake information is available for the years 1983-1985, which are particularly relevant here. However, this information suggests that the stakes in the ablation area react relatively directly to changes in mass balance. We will now describe this effect in more detail and discuss it with the help of further literature:

Stocker-Waldhuber et al 2019 have found evidence that ablation stakes can be well suited to reflecting the current status of a glacier. They show that the Kesselwandferner (also in the Ötztal valley) shows relatively direct response of ablation-area ice dynamics to changes in mass balance, regardless of the geometry of the glacier.

We have found evidence that the ablation stakes at VF also react quickly to a change in the mass balance. Only with a slight decay,the strongly negative mass balance in 1983 led to a decrease in velocity in the ablation area. This could be attributed to the fact that ice thicknesses at the tongue (in the ablation area) are usually already very low. Even small changes in ice thickness can have a noticeable effect on velocity here, as the driving stress is highly sensitive to ice thickness. In a year with a strongly negative mass balance, the strong melt in the ablation area directly decreases ice thickness, which represents the current status of the glacier. On VF, there is insufficient data to substantiate this adequately.

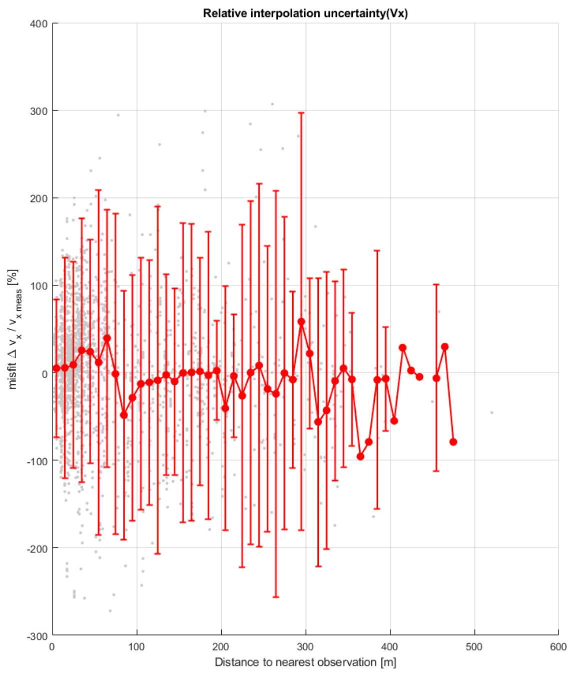
Further studies (including in other areas) are necessary to investigate this effect in more detail.

- Most uncertainty estimations (Sect. 4.4) appear to be qualitative, arbitrary or statistically inaccurate. A more rigorous uncertainty analysis is needed, since an extensive literature exists on relevant methods, specifically concerning glacier dynamics (e.g., [8], [9], [10]). The availability of a large number of data points suggests application of a leave-one-out method for robust uncertainty estimation. Finally, the formulas used to calculate and transform uncertainties should be shown.

2.6 Thank you for pointing that out. In this section, we also address your minor comments on line 229 and 235.

As suggested in reference [14] (minor comment on line 235), the interpolation uncertainty can be calculated using a bootstrap technique. We tried this technique on our interpolated velocity field consisting of 177 measurement points. To do this, we selected 10 random datasets from 20, 50, and 80% of the points and interpolated each of them. At the skipped points, the misfit to the measured velocity was plotted (separately for velocity in the X and Y directions). The relative misfit (the absolute misfit (Δ v) in relation to the measured velocity ($v_{x\ meas}$)) in percentage is shown as gray points in Figure 90. The median is shown in red, as well as the 2σ-confidence interval, grouped for distance intervals. Sufficient measurements are only available for the range up to approx. 50 m distance to the closest observation. A significant trend, as could be described by a linear regression, for example, cannot be found.

This is most likely due to the inhomogeneous distribution of the points and the fact that there is a high point density with a high variation in magnitude in the crevassed areas, whereas there is a low variation with a low point density on the glacier tongue. Due to the structure of these data, no statement can be made about a possible dependence of the interpolation uncertainty on the distance to the nearest observation point.



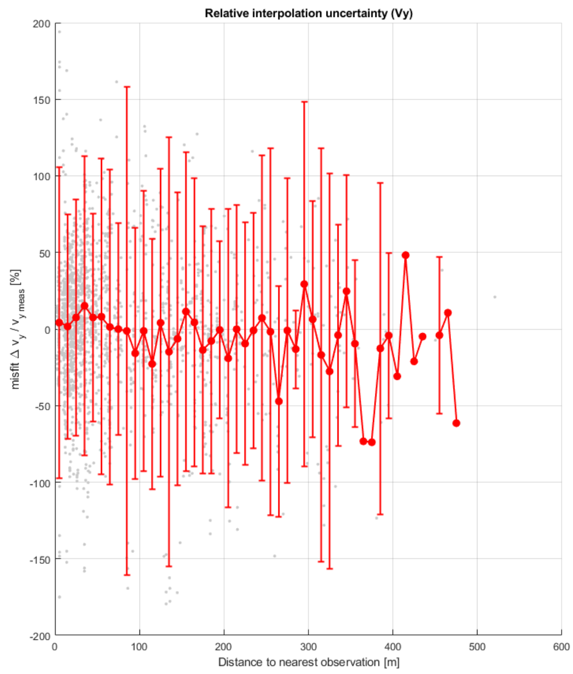


Figure 90: Misfit between modeled and measured velocity (Δ v) in relation to the measured velocity (v_{meas}) versus distance to the nearest observation as grey points. Red points indicate the median for a distance interval with its 2σ-confidence interval.

Furthermore, we performed a leave-one-out method. The results (Figs. 91, 92, and 93) and a description, will be added to the manuscript as Appendix B as follows:

A further uncertainty analysis can be performed using a leave-one-out method. For this purpose, an interpolation was performed for each point without using this point and the misfit of the interpolated velocity to the measured velocity was determined. The generated misfits are shown as relative and absolute errors with respect to the distance to the closest observation in Figures 91 and 92. The figures show that there is no significant correlation between the misfit and the distance to the nearest observation. This is most likely due to the inhomogeneous distribution of points and the fact that there is a high point density with a high variation in magnitude in the crevassed areas, whereas there is a low variation with a low point density on the glacier tongue. This can be seen in Figure 93. The individual misfits for each point are shown, with the mean average being formed in case of an overlay. The percentage uncertainty does not appear to be significant in specific areas. Due to the structure of the data, no statement can be made about a possible dependence of the interpolation uncertainty on the distance to the nearest observation point. Instead, as can be seen in Figures 91 and 92, there is a normal distribution around the value 0, so a 1σ-confidence interval can be derived as the mean uncertainty of the interpolation (in x- and y-direction). The relative error is estimated at 50.4% in x-direction and 28.3% in y-direction, while the absolute error is 0.6 m/yr in each direction.

The interpolation uncertainty ($u_{interpolation}$) for the velocity magnitude can be calculated from the 1σ-confidence interval in x- and y-direction as follow:

$$u_{interpolation} = \sqrt{u_{interpolation\ x}^{2} + u_{interpolation\ y}^{2}}$$

resulting in an interpolation uncertainty of 57%, respectively 0.8 m/yr.

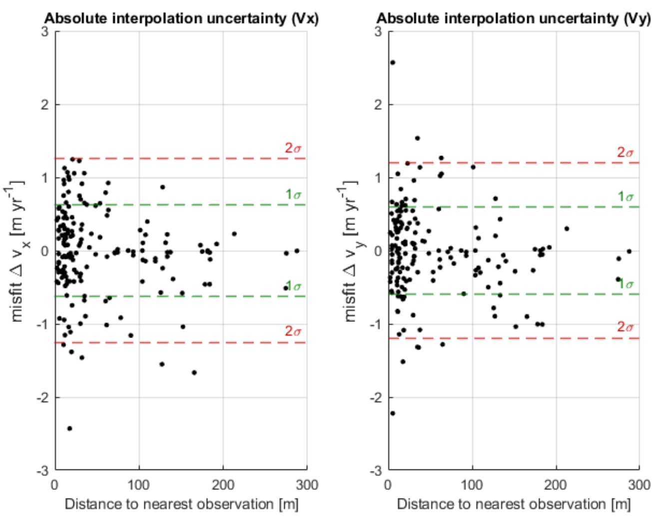


Figure 91: Misfit between modeled and measured velocity (Δ v) in relation to the measured velocity (v_{meas}) versus distance to the nearest observation according to a leave-one-out-method, referred to as $u_{interpolation\_x}$ and $u_{interpolation\_y}$.

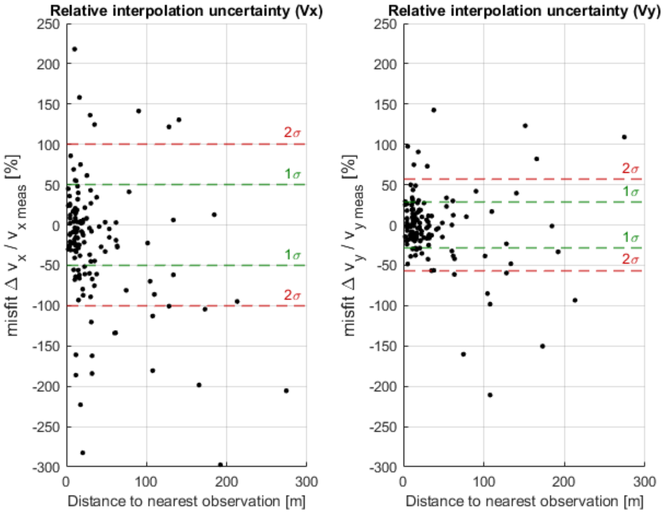


Figure 92: Misfit between modeled and measured velocity (Δ v) in relation to the measured velocity (v_{meas}) versus distance to the nearest observation according to a leave-one-out-method, referred to as $u_{interpolation\_x}$ and $u_{interpolation\_y}$.

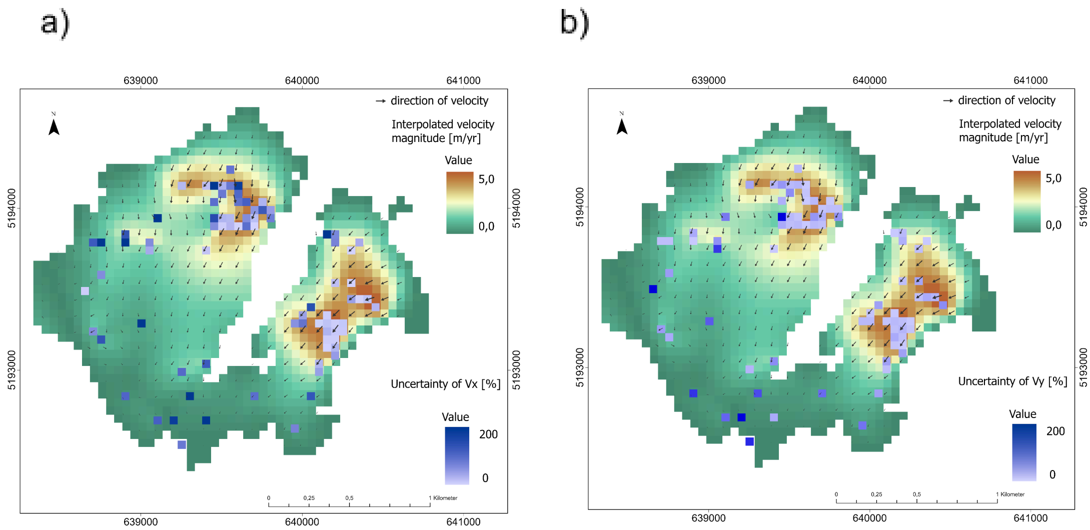


Fig. 93: Interpolation uncertainty ($u_{interpolation\_x}$, $u_{interpolation\_y}$) as misfit of a leave-one-out-method, with the misfit between modeled and measured velocity (Δ v) in relation to the measured velocity (v_{meas}) for measuring point, averaged in case of overlaps. a) for velocity in x-direction, b) for velocity in y-direction .

Furthermore, a position uncertainty (referred to as $u_{measured}$) of the measured points can be calculated for the velocity, by following the covariance propagation law. Where t is the temporal resolution in years and $u_{startdate}$ the position uncertainty of the startdata, as well as

$u_{enddate}$ for the position uncertainty of the enddata, from whom the velocity of each point is calculated. The start and end date, the temporal resolution as well as the resulting velocity uncertainty for each set of data is reported in Tab.3.

$$u_{measured} = \sqrt{(u_{startdate}^{2} + u_{enddate}^{2}) / t}$$

Regarding the utilized datasets, different measurement uncertainties arise. An approximate total measurement uncertainty $u_{measured\ total}$ across all observations can be estimated:

$$u_{measured\ total} = \sqrt{\sum_{i=1-5} (n_i * u_{measured\ i}^{2}) / n_g}$$

with i indicates a row in Tab. 3 and n_i being the number of points of the respective dataset, n_g the total amount of points. This leads to an approximate total measurement uncertainty of 0.27cm.

For clarification in the main manuscript we:

-delete line 228-230, as the mean uncertainty is not the mean of individual uncertainties.

- will change the column name of column 5 to: position uncertainty $u_{measured}$

- add in line 226: The resulting velocity uncertainties represent one standard deviation (1σ) and are listed in Tab. 3, a detailed description to the calculation is in Appendix B. It is also demonstrated that an approximate total measurement uncertainty of 0.27 m/yr can be estimated.

- add in line 233: To examine the uncertainty of the interpolation more closely, a leave-on-out method was performed. This shows that there is no significant dependence of the misfit (between modeled velocity and measured velocity) and the distance to the next observation. However, a relative interpolation uncertainty of 57% and an absolute interpolation uncertainty of 0.8 m/yr can be estimated from the standard deviation. Detailed calculations and explanations can be found in Appendix B.

Specific minor comments:

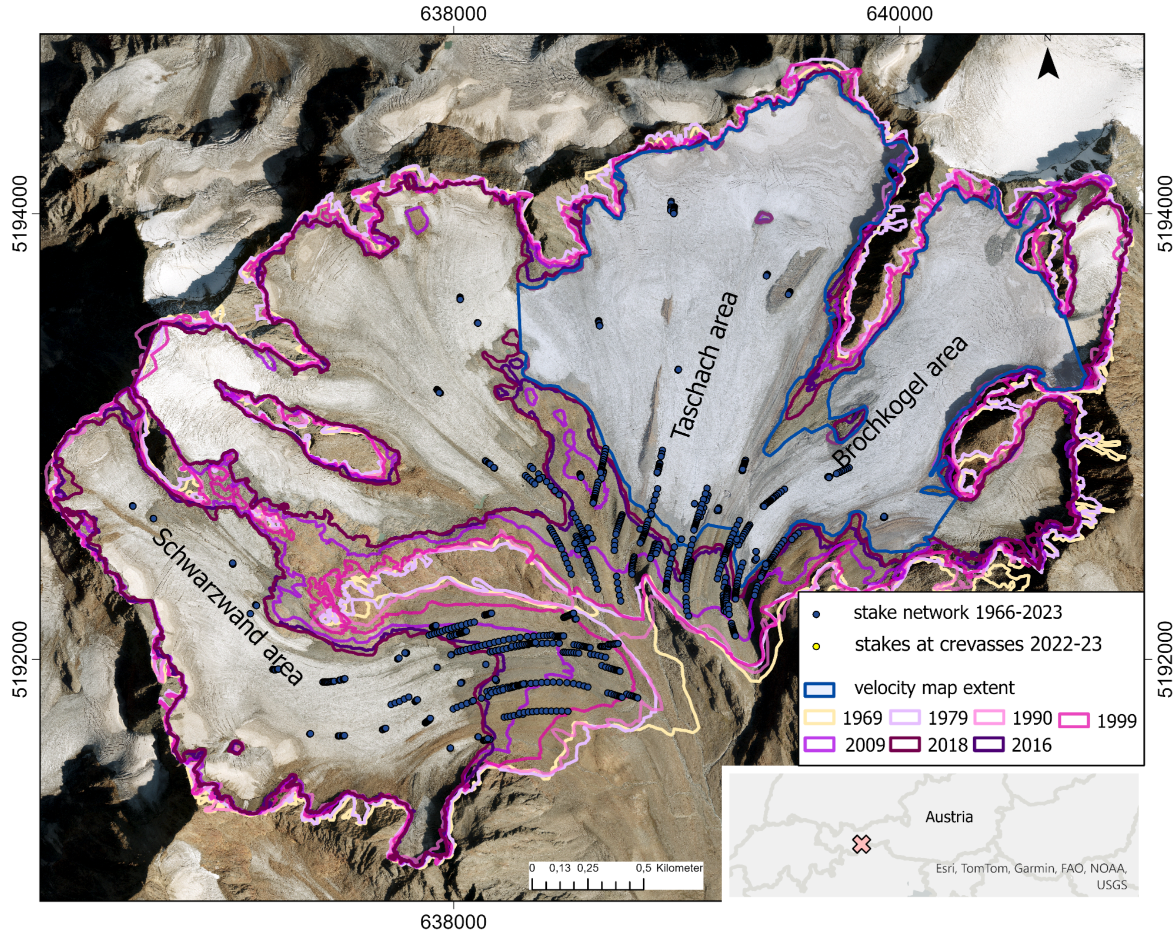
- The Introduction needs to provide more focused context on the specific topic of the manuscript, citing more literature on the monitoring of slow-flowing mountain glaciers, on long time series of ice velocities, and on the creation of glacier velocity maps for single glaciers; at present, it is somewhat meandering over broad topics of ice dynamics (basics of glacier flow, glacier hydrology, global ice velocity products).

That´s true. We provide further specific context in the introduction:
to current line 27:
Therefore, long-term measurements are essential for detailed analyses. A long time series of measurement make it possible to investigate the flow processes of a glacier and their changes due to the glacier response to climate change, taking into account the glacial processes. Vincent et al. 2016 were able to show that for the Argentière glacier in the Mont Blanc area, no correlation can be found between changes in surface velocity and subglacial water runoff. A change in mass balance, on the other hand, has a direct influence on the ice dynamic behavior of this glacier, as Vincent et al. 2009 find a direct or delayed reaction of maximal three years. However, phenomena are also found that deviate from a correlation between surface geometry and velocity change. For example at glacier de Saint Sorlin, France, velocities around the year 2000 are still greater than in 1960, despite a negative cumulative net mass balance since 1957 (Vincent 2000). This shows the complexity of the processes.
Spatial high-resolution velocity information as well as long term monitoring allow a detailed and precise analysis of glacier behavior.

We add in line 50: For individual glaciers, better results can be achieved with specific optimized data processing than with global data sets (Mattea 2025). Even relatively low ice velocities can be detected, as Gindraux 2019 shows for Griesglacier. However, slow-flowing glaciers need a specific temporal baselines between image pairs to capture recognizable flow. This implies a potential loss of coherence, as surface features (e.g. crevasses) can change considerably during this period (van Wyk de Vries and Wickert, 2021).
- l. 29, Nye (1959) was most definitely not the first rigorous investigator of ice velocities; see e.g. [11]
That´s true. We change it to “A method established by Nye(1959)...”.
- l. 48, what are "sensors such as Sentinel-2" compared to "other optical sensors"? Define the groups or reword for clarity.
We reword for clarity: “Millan et al (2019) showed that Sentinel-2 data can produce more precise results than Landsat-8.”
- l. 69, "continuous monitoring" is unclear given the present-day availability of automated, sub-hourly monitoring sensors. Consider using "systematic" or similar
Thanks for the hint, we will use “systematic” instead of “continuous”.
- l. 72, what makes the site "unique"? Explain concrete reasons for uniqueness if possible, otherwise consider rephrasing.
We rephrase to: “Together, with VF they form a well-monitored site of glacier-related variables.”
- Fig. 1, why show the glacier extent from 2016 and not the present-day? Especially since high-quality whole-glacier imagery is available (Table 1). Also, the 2022/2023 stakes installed and surveyed specifically for the present study should be clearly marked as such and distinguished from the historical archived stakes.
We update Fig. 1, showing the glacier orthoimage from 2022 (newest available). Regarding the glacier extent we now show all available glacier extents, showing the change in the glacier extent, since we also have historical data here. The available extents are listed in the Appendix (see response to major comment 2.1(4)) (1969,1979,1990,1999,2009,2016,2018).

Furthermore, we mark the stakes installed especially for the survey 2022/2023 with a different colour, to distinguish the data from the historical stakes.

updated Fig.1:



- l. 82, maybe it should be mentioned that the stakes were never repositioned at a fixed location, rather they were re-drilled at or close to their last location?

Thanks for the hint. We add in Line 86: “The measurement series are interrupted due to melt out of the stakes. The re-drilling took place close to its last measured position so that the stakes do not describe a fixed position.

- l. 90, "terrestrial polar connection" yields zero results on Google Search - is the method also known by different names? Please check and possibly rephrase or provide a reference

True, change to: “Polar coordinate methods”.

- ll. 91-92, the two "previous studies" analyzing velocity at the site appear to be unpublished and unaccessible diploma theses. However, their findings should probably be (1) quickly presented and (2) discussed and compared with the ones of the present study.

As already mentioned in response to major comment 2.2 (3), information contained in these diploma theses are already included in the data. These studies focused on data preparation up to the respective point in time, which is why we would like to mention their valuable work here.

- l. 102, "image pair velocity fields" - I think ITS_LIVE and possibly Millan rather provide annual or biennial velocity composites?

That´s true. We change to: “ Figure 2 shows exemplarily the ITS_LIVE (for the period 2017-2018) and Millan data base (annual for 2018) mosaic, …

- l. 113, please provide more details about the TerraSAR-X data and methods used - at least the following: (1) acquisition dates and pairs tested, (2) software used for feature tracking, (3) tracking window sizes used, (4) any post-processing and filtering steps

We provide more information and therefore change line 113-118 to:
Since the user-ready products do not provide usable results, we tested the suitability of high-resolution TerraSAR-X stripmap imagery (~2 m spatial resolution) for obtaining glacier surface velocity fields. Therefore, we applied feature tracking using various tracking window sizes (32x32, 64x64, 128x128, 256x256) and temporal baselines ranging between 11 days and up to about 2 years to pairwise co-registered images. The SAR processing was carried out using GAMMA Remote Sensing Software. Dates and orbit information of the employed acquisitions are summarized in Appendix C. All possible image pair combinations were tested using an automated processing pipeline (e.g. Seehaus et al., 2015, 2018), including an filter algorithm based on a comparison of the magnitude and the alignment of the displacement vector with surrounding values to remove the unreasonable displacement estimates (Burgess et al., 2012). Coherence tracking or InSAR-based displacement measurements were not feasible to carry out at VF, because the InSAR coherence was not maintained between subsequent acquisitions, which we attribute mainly to the pronounced surface lowering rates in summer and snow accumulation in winter.
Appendix C:

date	orbit dir. (decending/accending)	rel. orbit number	strip number
2011-07-03	D	78	6
2011-07-14	D	78	6
2011-08-08	A	131	10
2013-07-01	A	131	10
2014-06-07	A	131	10
2017-07-11	D	78	6
2017-08-13	D	78	6
2017-08-24	D	78	6
2018-07-31	D	78	6
2018-08-22	D	78	6
2019-07-29	D	78	6
2019-08-31	D	78	6
2020-01-02	A	131	7
2020-01-30	A	55	1
2020-01-30	A	55	1
2020-02-21	A	55	1
2020-02-21	A	55	1
2020-02-26	A	131	7
2020-05-02	A	131	7
2020-05-08	A	55	1
2020-05-08	A	55	1
2020-07-04	D	78	6
2020-07-26	D	78	6
2020-09-11	A	131	7
2020-09-17	A	55	1
2020-09-17	A	55	1
2020-11-11	A	55	1
2020-11-11	A	55	1
2020-12-25	A	55	1
2020-12-25	A	55	1

date	orbit dir. (decending/accendi	rel. orbit number	strip number
2021-01-27	A	55	1
2021-01-27	A	55	1
2021-02-01	A	131	7
2021-02-07	A	55	1
2021-02-07	A	55	1
2021-02-23	A	131	7
2021-03-25	D	78	5
2021-03-28	A	131	7
2021-04-08	A	131	7
2021-04-30	A	131	7
2021-05-11	A	131	7
2021-06-13	A	131	7
2021-06-24	A	131	7
2021-07-05	A	131	7
2021-08-18	A	131	7
2021-08-26	D	78	6
2021-09-09	A	131	7
2021-09-17	D	78	6
2021-10-12	A	131	7
2021-11-14	A	131	7
2021-11-25	A	131	7
2022-03-04	A	131	7
2022-03-26	A	131	7
2022-04-06	A	131	7
2022-05-09	A	131	7
2022-05-20	A	131	7
2022-05-31	A	131	7
2022-06-11	A	131	7
2022-07-03	A	131	7
2022-07-25	A	131	7
2022-08-02	D	78	6
2022-09-18	A	131	7
2022-09-29	A	131	7
2022-10-21	A	131	7

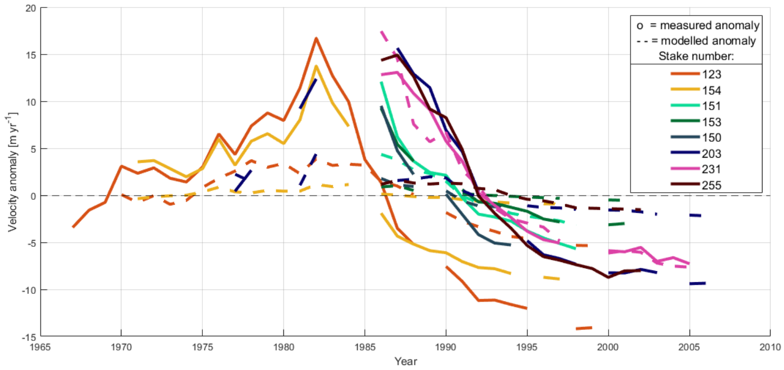
- Table 1, please provide details about the "UAV" and the "Optical airborne photogrammetry" - which platform, camera, flight altitude, spatial resolution?

Information on spatial resolution is added. Details on the platform, camera and flight altitude used in optical aerial photogrammetry are only partially known. Where available, the relevant literature is cited for more information. The UAV data was obtained through manual flights, meaning that no uniform flight altitude was used, as it varies.
updated Table 1:

Name of dataset	Sensor/Instrument	Date	Res. [m]	Covered area	Data provider
Airborne 2020	Optical airborne photogrammetry	2020-09-08	0.2	Entire glacier	© Land Tirol t i r i s 2020
Airborne 2018	Optical airborne photogrammetry	2018-09-16	0.2	Entire glacier	Geissler (2021), Withheld for legal reasons
Airborne 2021	Optical airborne photogrammetry	2021-09-25	0.2	Entire glacier	Withheld for legal reasons
Airborne 2022	Optical airborne photogrammetry	2022-08-23	0.2	Entire glacier	Withheld for legal reasons
UAV 07/2022	DJI UAV with optical RGB camera	2022-07-10	0.04	Parts of Taschach and Brochkogel	10.5281/zenodo.17590764
UAV 09/2022	DJI UAV with optical RGB camera	2022-09-21	0.04	Parts of Taschach and Brochkogel	10.5281/zenodo.17590764
Stake network (since 1966)	Total-station polar method / GNSS (annual surveys in Sept)	1966–present (annual)	—	Entire glacier, mainly ablation area	doi.org/10.1594/PANGAEA.982940
Stakes at crevasses 07/2022	GNSS	2022-07-10	—	Parts of Taschach and Brochkogel	doi.org/10.1594/PANGAEA.982940
Stakes at crevasses 08/2022	GNSS	2022-08-04	—	Parts of Taschach and Brochkogel	doi.org/10.1594/PANGAEA.982940
Stakes at crevasses 09/2022	GNSS	2022-09-21	—	Parts of Taschach and Brochkogel	doi.org/10.1594/PANGAEA.982940
Stakes at crevasses 08/2023	GNSS	2023-08-08	—	Parts of Taschach and Brochkogel	doi.org/10.1594/PANGAEA.982940

- l. 130, please provide more details here already about this seasonal correction - is it a single multiplication factor? How is it calculated? This information cannot be postponed to the second half of the manuscript.
We add to the manuscript (as later also suggested): " At VF, summer velocities are approximately 30% higher than the annual average. This variation was determined from stake measurements taken at different times (detailed information see chapter 4.2 seasonal variation). However, we consider the derived seasonality to be representative for the entire VF and assume its applicability to other years. The calculated seasonal variation is used to standardize displacement observations recorded at different times, converting them into annual velocity values. A seasonal increase of 30% relative to the annual velocity is assumed for the summer months (July through September)."
- l. 150, first introduce Fig. 3 and what is displayed there, then add specification such as which stakes are included
Done, changed to: "For this purpose, the stake network data, which has been measured annually at VF since 1966, is analyzed. Figure 3 displays the stake positions during the years. The focus lies on evaluating the temporal evolution of the ice movement. Therefore, only time series of at least six consecutive years are considered in the Figure. The data clearly demonstrate …"
- l. 157, the modeling of velocities should be described in detail in the Data and Methods, not just passingly in the Results. In particular, it should be explained (including formulas): (1) Where do the values for local ice thickness come from? (2) How are thickness and surface slope calculated for each stake to evolve for each year? The resulting ice velocity is a high power of both variables, thus it is highly sensitive to their precise values and variations.
Information has been added, for a more detailed description see response to major comment 2.1.

- l. 163, strictly speaking, in the modeled values, we see the sensitivity of velocity to ice thickness, not directly to mass balance.
That´s true, changed to: “ A strong sensitivity of velocity to mass balance (shown in Fig. 4) is evident in the measured values, while the modelled values provide the same dynamic trends as the measured. Offsets occur, …”
- l. 165, what is "the temporally fixed choice of flow parameters"? This has not been described before, it has to be fully explained in the Methods subsection about the modeling.
Thanks for the hint. See response to major comment 2.1 (3).
- Fig. 4, this figure is very interesting. However, the "average elevation" of each stake is possibly not the most informative value here, since all elevations are clustered within less than 100 m altitude. If possible, provide also the maximum and minimum elevation of each stake (supposedly corresponding to the earliest and most recent observation dates)
Done.
- Fig. 5, to better highlight interannual changes of ice velocity, it would likely be more informative to display stake velocities not all together as absolute values, but rather as anomalies (additive or multiplicative) compared to the long-term mean at each stake
That's true, we add a subfigure to Fig. 5, showing the anomalies.



- l. 168, no data above 3000 m a.s.l. has been introduced in the text before Fig. 6 - please give a quick introduction to those stakes, are they all stakes with < 6 consecutive years of data? Are they stakes in the accumulation area?
We do not understand this point of the reviewer. Figure 6 shows the whole dataset of measured velocities; no differentiation was made e.g. on length of time series. We updated the Figure caption accordingly.

Regarding the point “no data above 3000m”: We basically have velocity measurements in the ablation zone, since the stake network was designed to measure ablation rather than the velocity pattern of VF (see lines 82-84 of current version of manuscript). To clarify, we added in line 84 of the current version of manuscript: “Note that the stake network was adapted over the years to capture the ablation area which changed over the years due to the propagation of ablation to higher elevation in response to global warming. Therefore, the elevation range captured by the stake network varies.

- l. 172, these are all methodological details that belong to the Methods section, as they are necessary to understand most of the results presented so far. Also, more details are needed on the calculation of ice thickness change: was it computed only from local mass balance, fully neglecting ice advection? This would introduce a major, systematic, elevation-dependent bias in the ablation area (too fast thinning)

We agree and move these methodological details to the Methods section. Details on the calculation of ice thickness are explained in response to major comment 2.1. Both, mass balance and mass transport are taken into account in the surface models. We interpolate between two surface models. The mass balance is only used to scale the geometric change. The geodetic height change includes ice advection, so at least the average ice transport is taken into account.

- Fig. 6, this interesting plot is hard to read, especially since all point measurements from all stakes are shown without distinction. At this stage, the reader does not know how many stakes are visualized on this plot, and how many stakes exist at each given point in time. Thus, it is not clear what is the time evolution here, apart from a general trend of "slowdown at all altitudes". It could make more sense to connect the points of single stakes, possibly aggregating in multi-annual intervals (even decadal aggregation) and/or excluding stakes with very few years of observation, in order to reduce the complexity of the data shown.

We are not sure whether we have correctly understood the suggestion regarding Fig. 6. Representing stakes with a long observation period and connecting the points of single stakes would lead to a Figure close to Fig. 4 of the original manuscript. We agree that it is not possible to distinguish between the individual stakes in Fig. 6 in the current way, but we intent to show that the glacier velocity slow down occurs in all height levels. Therefore, we keep this Figure in the manuscript.

- Fig. 7, the thickness maps exhibit obvious major interpolation/processing artifacts, which would be strongly reflected in any calculated ice velocity. The thickness data need to be made available and/or re-examined in the light of these artifacts, if any conclusion about changes in glacier geometry is to be drawn. Also, if possible, please show simultaneous extent and thickness of the glacier rather than inconsistent dates; the 2016 extent is already in Fig. 1. Finally, the arrows indicate the flow direction, how was it determined? Please provide methodological details in the relevant section.

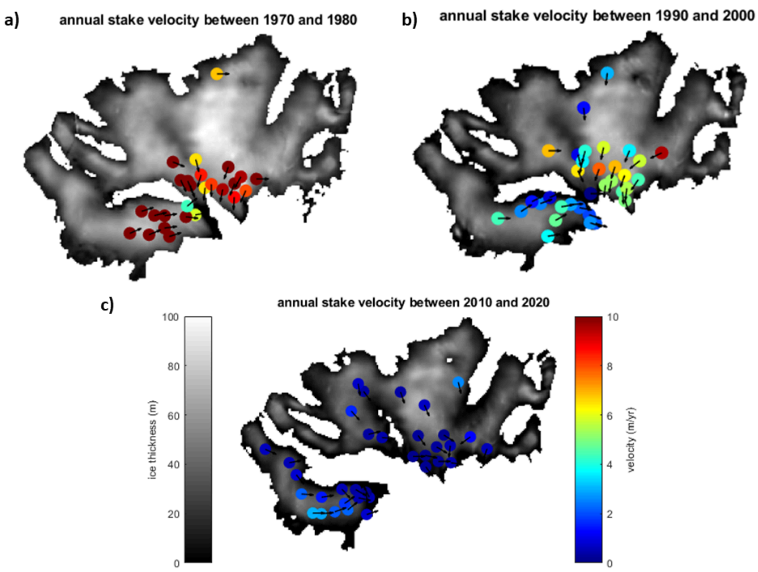
That is true, thanks for the hint. We re-examined these artifacts (see new figure 7 in this comment).

In addition, the publication of the DGMs (see response to major comment 2.1(4)) allows the calculation of ice thickness data to be traced.

In Figure 7, we show the average stake velocities for three different periods (1970–1980, 1990–2000, and 2010–2020). In order to represent the average ice thickness for each epoch, we decided to show the ice thickness for the respective average year (1975, 1995, 2015). Unfortunately, we do not have glacier extent data for these exact years, therefore the ice thickness is clipped to the last known glacier extent, as it is described in more detail in the response to major comment 2.1(1). To avoid misunderstandings, and since the clipping is already described, the glacier extent is removed from Fig. 7.

The arrows indicate the average direction of movement of the respective stake. This information is added to the figure caption: “The arrows indicate the determined flow direction, which is calculated from the mean angle of movement of the respective stake.”

updated Fig.7:



- l. 184, here is a description of a methodological choice and would fit very well around l. 130 if it is the same seasonality correction that is described here.

Done, thanks.

- Fig. 8, what is the standard deviation of this 30 % summer speed-up? If I understand correctly, the monthly (in summer) and annual velocities are available for each of the 8 + 4 stakes mentioned here, thus it would be quite important to show how much these stakes deviate from the 30 % estimate (and thus, how uncertain the estimate is) - especially if the correction factor is to be considered "representative and applicable" anywhere.

Velocity data used to calculate seasonal variations is available at PANGAEA. An additional table is added to explicitly show the individual vales and the calculated seasonal variation.:

Table 2. Exact values of the seasonal variation of glacier surface velocity for Taschach (8 stakes) and Brochkogel area (4 stakes).

Area	Number	$v_{Jul-Aug}$ Juli 22–Aug 22 [m yr^{-1}]	$v_{Aug-Sep}$ Aug 22–Sep 22 [m yr^{-1}]	v_{annual} Aug 22–Aug23 [m yr^{-1}]	$\frac{v_{Jul-Aug}}{v_{annual}}$ seas. var.	$\frac{v_{Aug-Sep}}{v_{annual}}$ seas. var.
Brochkogel	1	3.37	5.05	3.14	1.07	1.61
Brochkogel	2	3.44	4.74	3.03	1.14	1.56
Brochkogel	3	4.52	4.16	3.22	1.40	1.29
Brochkogel	4	4.62	3.00	no measurement		
Brochkogel	**mean**	**3.99**	**4.24**	**3.13**	**1.20**	**1.49**
Taschach	1	4.54	2.21	2.38	1.91	0.93
Taschach	2	3.78	2.39	2.27	1.67	1.05
Taschach	3	3.57	3.85	3.09	1.16	1.25
Taschach	4	4.12	3.97	2.17	1.90	1.83
Taschach	5	1.27	1.52	1.74	0.73	0.87
Taschach	6	1.82	3.84	2.11	0.86	1.82
Taschach	7	2.24	3.49	2.17	1.03	1.61
Taschach	8	2.08	1.95	1.77	1.18	1.10
Taschach	**mean**	**2.93**	**2.90**	**2.21**	**1.30**	**1.31**

We add to the manuscript:
in current line 221:
On average, seasonal variation is 1.30 (130%), with a standard deviation across all observations of 0.37 (37%).
and in current line 311:
At Vernagtferner, summer velocities are approximately 30% higher than the annual mean, with a standard deviation across all observations of 37%. The relatively high uncertainty results most likely from the fact that the absolute measured values over a month (July-Aug or Aug-Sep) have maximum values of about 50 cm. However, these values have a relatively high measurement uncertainty of +/-14cm (see chapter Measurement Uncertainty).
- l. 218, a claimed 3 cm measurement error corresponds to a good-quality dGNSS / RTK survey, whose method (instruments and protocol) should be described in the Methods section.
We will name the method in Table 1. In our opinion, GNSS measurement is a state-of-the-art method. Therefore, we do not describe this method explicitly in the “Methods” section and do not submit any protocols.
This was a classic static GNSS measurement using a multi-frequency GNSS receiver, with our own base station in operation. The post-processing evaluation, which we had to rely on due to a lack of data connection for RTK processing, consistently delivered solutions with fixed ambiguities. This resulted in an accuracy of approximately 3 cm. We are adding this information to Table 1.
- l. 222, the data resolution should be introduced in the presentation of the UAV / aerial data.
Done, see response to major comment 2.1 (4).
- l. 229, the statistical basis of this calculation of overall uncertainty is unclear: the mean uncertainty is most definitely not the average of individual uncertainties. See e.g. [12], [13]. Another possibility would be to use leave-one-out validation of each available measurement.
That´s true. We change the uncertainty analysis, a new version can be found in the response to major comment 2.6
- l. 233, the dataset contains maps of ice velocity and of surface slope, it should be easy to calculate proper metrics (such as a correlation coefficient) of the agreement between surface aspect and flow direction, rather than a qualitative "overall alignment of the calculated flow directions with the glacier topography".
That´s true. We calculate the angle deviation of the flow direction of the topography to the flow direction of the velocity in the median at 43 degrees. Larger angular deviations may occur, especially at lower magnitudes.
- l. 235, there exist published methods to estimate interpolation error in areas of heterogeneous point data coverage. See for example [14].
This must be a misunderstanding; we don't interpret here the quality of the interpolation. We just say the ice flow map is most trustworthy in areas of high data density. We slightly rewrite the sentence for clarity: “Despite the estimated interpolation error, the velocity map becomes less reliable in regions with limited data availability. In zones where observational data is lacking, there is insufficient information, making the reliability of the velocity map for understanding ice flow dynamics in these areas questionable.”
However, we think the reviewer comment also belongs to lines 231-234 where we estimate the interpolation error. More information, including testing of the method described in the literature suggestion [14], can be found in the response to major comment 2.6.

- l. 253, is this "wide range of notable differences between the summer and winter seasons" shown anywhere? So far the Authors have presented only three spatially-aggregated, monthly data points from a single summer season (Fig. 8).
As shown in Table 2 above, the datapoints are represented now. We agree, the description as “wide range of notable differences between summer and winter seasons” is not sufficiently justified based on the initial data, we change it to: “with a notable difference between summer and winter seasons”.
Information on the restrictions regarding the summer year and the spatial extent can already be found in lines 256-267.
- l. 315, this conclusion can only be taken if proper state-of-the-art methods are tested and the results are shown, the well-established methods validated by several studies (including on slow glaciers) cannot be so quickly dismissed.
We agree. We don´t want to rule out the possibility that there could be a successful tracking of the features. At this point, we would like to draw more attention to the overlap caused by ablation. Even if a modern method enables feature tracking, the challenges associated with high ablation remain. We would like to clarify this statement as follows:
“With the general tendency of strong negative mass balances and the associated possibility of high ablation, changes in surface features may be caused in parts by ablation rather than ice dynamics. This may also make it difficult to detect ice dynamics in other Alpine glaciers under climate change.
- l. 323, I would caution against using a map with manually tracked features as benchmark for validating other datasets, since the computed quality of such datasets would then inherit the reproducibility issues of the manually-compiled map.
We agree and rewrite: …. allow a detailed modeling of glaciological processes as well as plausibility assessments of future remote-sensing results.”
- l. 329, data availability: most of the datasets mentioned and used within the study are actually missing, including the ice thickness data, any digital elevation models, and the UAV and airborne data. Only the historical stake data are provided, the other assets are rather results from the study such as the manually tracked displacements and the five-year velocity map. Unless there are legal restrictions, for both review and reproducibility purposes it is important to provide access to the actual data (not just the results) used in the study.
We will publish all data that is not subject to legal restrictions. An overview can be found in the new created table, see response to major comment 2.1 (4).

Additional Literature used in the response:

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