Answer to the reviewers - *Morphodynamics of the Mont Blanc glaciers and their recent evolution*

By *Fabrizio Troilo, Niccolò Dematteis, Francesco Zucca, Martin Funk, Daniele Giordan.*

First of all, we would like to thank both the editor and the reviewers for their very accurate revision of our work. Thanks to many of the observations and the criticalities that have been pointed out, we believe that our work has undergone great improvements thanks to a deep review of both its structure and its methodologies. We briefly present hereby the major changes, and we also provide in the following point-by-point answers to the specific questions of the referees:

A first improvement that we made based on the indications of the reviewers, was a restructuration and rearrangement of the sections of the manuscript and the reorganization of the methodological section description. As far as a major weakness of the paper was highlighted to be a poor analysis of the uncertainties, we reworked this aspect. At first, we changed our method in the analysis of stable ground shifts, and we extracted those, not relying on points anymore, but analysing the stable areas surrounding the glaciers of the massif.

Secondly, we discretized our analysis not only on the whole period of study but producing error estimates for every single monthly velocity map that we produced, hence giving the possibility to add error estimates to our monthly time series and the derivate analysis.

In order to improve the repeatability of our study, we reprocessed the whole dataset to introduce automated outliers spatial filtering and introduce an objective way of defining the areas for time series extraction. In order to do so, firstly we applied a 3x3 median smoothing filter to all the single monthly velocity maps. Subsequently, we applied a LOESS regression to the time series of velocity, thus avoiding the need to manually reject outliers. Moreover, we redefined the sampling areas for the time series extraction by individuating the glacier flowlines and by cropping those to the altitudes of the minimum and maximum ELA (with a 40 m buffer) that was mapped in the study period. The results obtained with the abovementioned modifications are coherent with the results highlighted with the previous methodology, confirming its robustness, but individuating an easily replicable procedure to extract data.

Considering that the conclusion of the study period (end of 2022) is already slightly outdated, we decided also to analyse the images updated to February 2024 and this gave the chance to observe new insights on the phenomena that we described.

In fact, what would have looked like an ongoing velocity increase since 2020, can be now described as an anomaly appearing during 2020-2021-2022 in some of the time series, that suggests to analyse the trends on a single period rather than dividing the study period in two distinct phases. Nonetheless, the presence of the 2020-22 anomaly, as well as the presence of some overall accelerating trends that are confirmed and on some of the glaciers, remain something counterintuitive and that we cannot fully explain as of now and that will need further research.

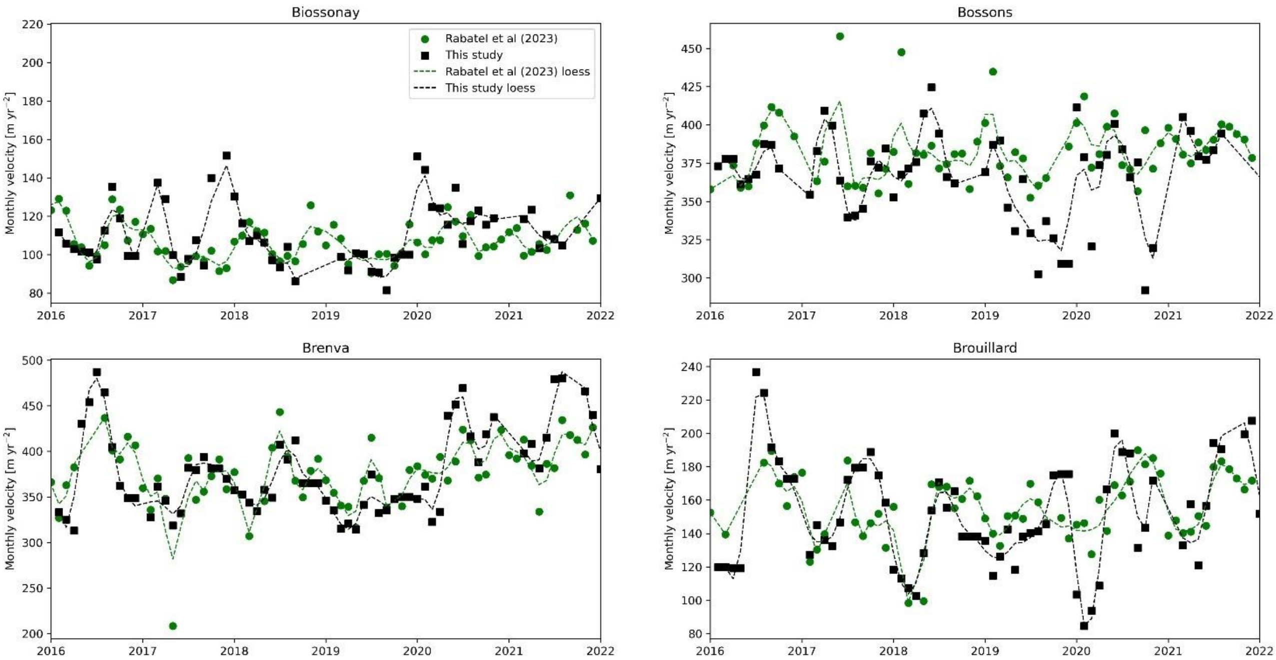
Finally, we decided to simplify greatly the glacier classification part, for the reasons highlighted by the reviewers and for an easier interpretation of the presented data by the readers. In particular, we identified three classes of glaciers based on their size and three classes of glaciers based on their "seasonality". Then, we observed that small glaciers present in general more pronounced and regular seasonal fluctuations, while large glaciers have a more constant behaviour, or irregular velocity variations.

Reviewer #1 comments:

The paper by Troilo et al., titled "Morphodynamics of the Mont Blanc glaciers and their recent evolution," presents a study on the evolution of glacial dynamics in the Mont Blanc massif. The authors used Sentinel-2 images to examine the monthly changes in flow velocity in this massif, as well as morphological characteristics of the glaciers, in order to classify the observed dynamic behaviors in this area.

General comment:
One point of concern that needs to be more highlighted is the results from a study published in 2023 (Rabatel et al., 2023). Indeed, this study shows trends that differ from the study by Troilo et al. For instance, in Rabatel et al., the Brenva glacier appears to be accelerating since 2016, whereas the Bossons and Argentière glaciers have significantly reduced their speed. One possible explanation for these differences may arise from the method used to observe these variations. The authors, by looking at an average speed over several points distributed across the glacier's surface, may tend to smooth out dynamic changes. Indeed, Rabatel et al. (2023), as well as the authors of this paper, have specifically noted that the strongest speed changes are not uniformly distributed along alpine glaciers, which can be polythermal in the Alps, and have generally much lower changes in accumulation regions. Thus, the method used to calculate speed changes in this paper will impact the amplitude of the seasonal cycle calculated in this study. Furthermore, important details of methodologies are still missing from the paper to determine whether the calculated trends here are robust (see below).
Thank you for indicating us the work by Rabatel et Al. (2023) (in MDPI "Data"). During the preparation of our manuscript, we found a "twin" publication by the same authors Rabatel et al. (2023) (in Journal of Alpine Research), which we included into the discussion. However, the former article contains more information that allowed us to perform a more detailed comparison, even though we had to contact the authors directly to get the data, since not all of them are publicly available (e.g., the time series).
We point out that the time series shown in Rabatel et al. (2023) have not been extracted at the same location as ours: they shown the velocity of the Bosson Glacier very close to his front; in Argentiere, they analysed the lower flat valley tongue, while in Brenva they considered a point very close to the right margin of the glacier. Neither the overall period is the same, in particular the period they considered to calculate the trend; in fact, they only estimated the trend over the interval 2016-2021, while we split the analysis into two periods: 2016-2019, 2020-2023.
A comment should be made on the specific behaviours observed over these 3 points of interest:
On Brenva Glacier, we observed this general acceleration that we observed on other glaciers in the study over the 2020-2022 period. The particular location of the other 2 sites gives a possible explanation of the different trends observed: both sampling points are located on the terminuses of the lower glacier tongues, far lower than the ELA. Bossons sampling location in particular lies on the very far reach of the tongue at a particularly low altitude where extreme mass loss occur and a strong thickness loss is expected, thereby a subsequent strong velocity decrease on this section is plausible. Regarding Argentière, the slightly (but not statistically significative)negative trend is confirmed by the GNSS data from the GlacioClim data repository and being located in an area at a higher elevation than the Bossons sampling point, suffering less acute ice thickness losses. Nonetheless, the area is far lower than the mean position of the ELA, possibly justifying a negative velocity trend at this section of the glacier. The absolute values of velocity at this position fit well between the two datasets but we believe that the upper bound of the velocities could be unrealistic on both datasets and represent outlying values possibly linked to both strong shadow movements and a lack of surface features over this area (as observed at the Sentinel2 resolution). GNSS data from this area show the surface velocity variability to fall typically in the 35 - 60 m/yr range during the different seasons (2018-2020 data, Vincent 2022, https://doi.org/10.1029/2021JF006454) and mean yearly values around 45-50 m/yr (2016-2017,2017-2018,2018-2019 data Vincent 2021 https://doi.org/10.5194/tc-15-1259-2021). Different authors have highlighted difficulties in obtaining good optical satellite feature tracking results on the Argentière glacier main tongue (Millan 2019

doi:10.3390/rs11212498, Van Wyk de Vries 2021 https://doi.org/10.5194/tc-15-2115-2021) The extraction of the time series over a single pixel of the velocity maps can also exacerbate the presence of outlying values.
We decided to include this comparison in the supplementary material, because the locations from Rabatel et al (2023) do not satisfy the criteria we have defined to select the area where extracting the time series of velocity. Such criteria are now described at lines 280-285. We added a reference to this comparison into the discussion.
Because of the abovementioned comments, we decided to ask the authors to extract data on more representative zones of glaciers, and extracting them on the same locations individuated in the present revision of the work, located around flowlines at min-max ELA altitudes. The two datasets are compared in the following plots:



On the 4 glaciers that we analysed both the trends, the variability of the velocities and the absolute values of velocities find good agreement. There is also good agreement on the seasonal variability. This comparison will be described in detail in the paper revision.

One critical point is a precise description of the repeat cycles (and their distribution through time) used to calculate speeds, which can significantly impact the errors associated with time series.
Since our focus was to detect and analyse monthly to seasonal velocity changes, we considered time gaps spanning between 10 to 120 days (35 days on average). We provided a table in the supplementary reporting all the repeat cycles used to produce monthly velocity maps and added a statement in the manuscript to explain this point.

Millan et al., 2019, demonstrated that for repeat cycles of 5 days for Sentinel-2, errors associated with speeds can exceed >40 m/yr (Figure 4 from their paper). Errors of this magnitude must be taken into consideration, especially considering the relatively small changes in speed observed on the glaciers. Using the error on each repeat cycle employed, Millan et al. notably compiled maps of the Mont Blanc massif, with the minimum cycles required to observe dynamic changes (Fig. 9 from their study). On these maps, we can see that the minimum cycle ranges from 10 to 30 days in the fastest parts of the glaciers (Bossons, Brenva, serac fall of Mer de Glace, Miages tributary glaciers). At all other sites, return cycles of over 300 days are necessary to observe dynamic changes with a satisfactory signal-to-noise ratio. Hence, I think the authors should filter out all their selected sampling points located in those slow-moving areas when deriving their monthly speed changes. At the moment, almost 70% of the glacier considered in Figure 7 does not have a sufficiently high speed to monitor monthly speed changes.

Thank you for this comment, which allows us to explain better this point.

We are aware of the work of Millan et al (2019) and, according to their findings, we did not consider images separated by 5 days, since they can be affected by large uncertainty, and we focused on pairs between 10 and 120 days. However, we did not use images with larger gaps, because we aimed at identifying short-term behaviour of velocity. Of 218 pairs analysed in our study, in 22 cases (~10%) the time gap was 10 days, while on average it was 35 days. Furthermore, considering the specific methodology of GIV in producing monthly velocity maps, which performs a weighted average of results obtained by overlapping pairs with possibly different repeat cycles, in many cases we further reduced the uncertainty related to short time gaps.

Besides, according to your suggestion, we estimated the uncertainty for each monthly velocity (we calculated the normalised median absolute deviation of the velocity in the stable areas following the method of Millan et al, 2019 obtaining a median value of 10 m/yr (from 8 m/yr to 12 m/yr, 25^{th} - 75^{th} percentiles respectively), which is in line with our previous estimate of 11 m/yr.

Since the variations between minimum (in winter) and maximum (in summer) velocities are usually >20 m/yr (save in few cases), we are confident that we could correctly detect these changes, considering an uncertainty of ~10 m/yr.

The new time-series (produced considering a buffered area around the ELA at the centre line) show in most cases well-defined seasonal cycles even in glaciers with average velocity <100 m/yr (e.g., Rochefort, ANeuveC, Charpoua, Grandes Jorasses, DesGlaciers). However, in other (few) cases (e.g., PierreJoseph, NantBlanc, BionassayFR), the seasonal cycle is less evident, because probably its magnitude is below the measurement precision, or it is basically absent. In such glaciers, we are close to the limit of what we can detect with the presented methodology due to the glacier slow velocities and small sizes. We did not know their typical velocities before the analysis, so we believed that it was worth showing those results. We in fact stated that into the discussion (L445) "The velocity seasonal cycle is modest or even non-detectable since the velocity in winter is close to the measurement uncertainty (besides the Grandes Jorasses, which has a relatively pronounced seasonal cycle). It is worth highlighting that signals of potential velocity fluctuations could exist but remotely-sensed data are not currently suited for the analysis of such small glaciers."

All considered, we believe that in most glaciers that we show, we have been able to detect seasonal/monthly velocity changes.

Hence the monthly variations that are shown are likely due to noise in the ice velocity estimates. See later comment for more details.

According to our previous answer, we are confident that in most glaciers that we show we have been able to detect seasonal/monthly velocity changes. Moreover, the evident seasonal cycle that is present also in "slow" glaciers is very unlikely due to noise, thus reinforcing our outcomes.

Figure 7 of the paper absolutely needs to include error bars on these time series. An important piece of information that is currently missing would be to provide the raw time series with the monthly average overlaid on top. This provides essential information about the signal-to-noise ratio magnitude on these time series, as well as the robustness of the data used.

Thank you for this constructive suggestion. We produced new time series with error bars for every monthly velocity and applied a smoothing via robust locally-weighted quadratic regression (LOESS) over a moving window of 12 months (see attached figure), thus avoiding the manual identification and removal of the outliers, which was a criticality raised by the reviewer 2. Thereby, we could better highlight the glaciers velocity trends. The extraction of time series has been performed according to comments coming later in the text. We extracted *data from an area defined by cropping RGI 7 flowlines at the upper and lower altitudinal limits of the ELAs mapped between 2016 and 2023, plus a buffer of 40 m. We included a figure showing the extraction area in the supplementary. Besides, at this stage, we also decided to apply a 3x3 median filter to the velocity maps to reduce the occurrence of velocity artefacts. We explained better this point at lines 280-285.*

Furthermore, methodological details need to be provided regarding the calculation of monthly velocity maps, specifically to understand whether the authors calculate a simple median or if they compute a weighted average.
We applied the weighted average included in GIV, where the weights are proportional to the fraction of time included in a given month over the total time gap between the image pairs. Quoting the GIV paper by Van Wyk de Vries & Wickert (2021): "The weighting parameter is determined by the proportion of the individual map contained within a given month. For instance, a velocity entirely within 1 month will be weighted 1, while a velocity spread evenly over 4 months will be weighted 0.25". We explained better this point in the new version.

Data reduction methods have been described in Mouginot et al., 2023, and accounting for the precision of each pair of images is critical for maximizing the signal-to-noise ratio in monthly averages. Currently, description of error estimation comes way too late inside the manuscript and should one of the first points that opens the results section.
In the original version, we included the uncertainty analysis into the discussion section according to the suggestion of the Editor. However, following your comment, we moved it into the results section.

Similarly, it would be necessary to show (at least in supplementary material) speed maps over stable regions throughout the entire area (similarly to Figure 5 but everywhere), and on a logarithmic scale, to provide a better understanding of the noise associated with the time series. It is particularly important when examining really small glaciers such as Charpoua, that are in the limits of what we are able to measure with Sentinel-2.
We prepared an additional figure with the velocity visible across the entire area (i.e., not masked over the glaciers), following the example of Figure 2 presented in Millan et al (2019).

Finally, the paper's structure needs revision because numerous methodological elements are currently located in the "Results" section of the paper. This comment should really be considered for the clarity of the paper. For example. you have two sections both the Methods and Results that are entitled "Selection of Sampling Points" and "Sentinel-2 image selection." This really gives the impression that the author did not put effort into the submission and structuring of the paper.
Thank you for this comment. Actually, the structuring of the manuscript took a lot of effort for us, because, since the workflow of the study is rather complex, we thought about a structure that would have helped the reader to follow the text. Accordingly, we decided to create, for every methodological section, a corresponding section in the results (e.g., in 4.1 we explained the criteria that we followed in selecting the satellite images, and in 5.1 how many images were selected and how were they distributed across the study period). Therefore, we thought that using the same titles would have been clear. Nevertheless, we probably failed and, considering your comments and those of reviewer #2, we restructured the manuscript.

Please provide a proper Data and Methods section. For example, all details related to Sentinel-2 imagery (e.g., 3.2 and 4.1 and 5.1) should be within the same section. All things related to the data and methods for morphometric analysis should be gathered in one single section (3.2 and 4.3). Finally, you should have one big part with ice velocity calculation and time-series analysis. Even the name of the section looks random: section 4.5 is "Glacier surface velocity mapping," and section 5.2 is also "Glacier velocity mapping."

Thank you for this comment. We decided to split Data and Methods into two sections and create corresponding sub-sections between methods and results (see also the previous answer). The title of section 5.2 is "Glacier velocity mapping and selection, outline delineation and morphometric analysis" because it includes results about the generation of glacier surface velocity, the update of the glacier outlines of RGI and the morphometric analysis. We omitted the term "surface" (which was present in the method section) from the title to shorten it. However, according to your suggestion, we restructured the manuscript, thus fixing this issue.

Specific comments:

L27-29: The transition between sliding and surface velocities is a bit odd at that place since you are not discussing sliding velocities in the paper. Rephrase the sentence to say that measurements of sliding velocities are extremely difficult and rare, and that measuring surface flow velocities can be used as a strong alternative to invert for sliding speeds using ice sheet numerical models.

Thank you for this comment. We have rephrased the text accordingly: "Because continuous monitoring of sliding velocities is extremely difficult and rarely achieved (Vincent and Moreau, 2016), measuring surface flow velocities can be used as a strong alternative to invert for sliding speeds using ice sheet numerical models. Nonetheless, the continuous monitoring of surface velocities of Alpine glaciers is complex on specific study sites, and very rarely has it been performed on a spatially distributed scale."

L42: Specify that it is a measurements of surface flow velocities made on the ground.

Thank for the comment. We specified that the studies the identify the occurrence of glacier surge have been done from satellite remote sensing, while the studies that analysed the accelerations have been performed with ground-based sensors.

L57: ITS_LIVE (please correct accordingly). Furthermore, specify that the cross-correlation is derived at a resolution of 240 m and statistically downscaled to 120 m, which has major limitations for small mountain glaciers.

Thank you for this helpful comment. We have added this detail into the manuscript: "The availability of such datasets is very relevant globally, but its application to Alpine glaciers is limited due to its relatively coarse spatial resolution - e.g., 300x300 m (GOlive), 120x120 m (ITS_LIVE) - which can provide data on just few of the largest Alpine glaciers. Moreover, for the ITS_LIVE dataset, the cross-correlation is derived at a resolution of 240 m and statistically downscaled to 120 m, which has major limitations for small mountain glaciers.".

L57: You are never mentioning the Millan et al., 2022, which is the first dataset that covers all glaciers on earth (outside of the ice sheet), with a pixel size of 50 m, hence significantly gaining in resolution.

Thank you for the comment. We focused on products that provide updated glacier velocities and/or time series, while Millan et al (2022) created averaged velocity maps over a period of two years. However, according to your suggestion, we have added the reference.

L107: This entire section and the following one needs to be better organized. See general comment.

We have revised the manuscript according to your suggestions.

L118: Millan et al 2019 on thousands of Sentinel-2 image pairs from the same orbits, in several mountain ranges, that errors on geolocation are closer to a value of 0.52 pixels, which corresponds to the absolute geolocation specification by ESA.

Thank you for the comment. Millan et al (2019) calculated the mean offset on Sentinel 2 images over the years 2017-2018, but Kaab et al (2016) estimated offsets of 1-3 m in the Sentinel 2 images in 2015 and 2016. We have cited both works into the text.

This step is accounted for in the calibration procedure. Furthermore, you do not provide details about the co-registration scheme that you are using. Please add more description on that part.

Thank you for the comment. We mentioned the misregistration correction at lines 200-201 (old version). However, in the revised manuscript, we moved this description and explained it better at lines 216-220: Correction of stable ground shifts of images is implemented in the GIV process chain using a stable area where potential shifts are estimated. GIV calculates stable-ground shifts to correct for georeferencing errors without the image degradation which co-registration can introduce.

In order to do so, we create a stable ground mask composed of non-glacierized terrain surrounding the massif, and fit a 2D second degree polynomial to the residual velocities over stable ground in the x and y directions.

L208: Please show in the supplementary material all the sampling points that are used on the glaciers.

We added a figure into the supplementary where we indicated the areas adopted to extract the time series. Besides, we described the improved approach, based on buffering the glacier flow line in correspondence of the ELA, at lines 232

L255: The number of images used per year seems really low compared to other work from Rabatel et al., 2023, and Mouginot et al., 2023. For example, Mouginot et al. assemble 5000 different pairs of images for years 2018-2019, which seems much higher than what the authors are showing here. Is it because you are limited to cycle of 20 to 40 days? Why not considering longer cycle for slow moving region and inter-annual trends?

Thank you for this comment. The different number of image pairs depends on the approach and on the aim of the study. In Mouginot et al (2023), we could not find the number of image pairs in 2018-2019, but only in the period 2016-2020 (which is indeed ponderous: 18000 pairs). However, it must be noted that they used all the possible combinations considering temporal gaps between 5 and 400 days and that they did not discard the images with clouds or in general they did not select suitable images, like we did (we manually selected 123 images). Besides, since we focused on short-term velocity variations, we did not consider gaps >120 days (and neither gaps of 5 days, since these pairs may be affected by large uncertainties). At the end, we produced >170 image pairs.

In the case of Rabatel et al (2023), they too considered all the possible pairs between 5 and 200 days, and they also correlated images of different orbits.

L261: This entire section 5.2 should go inside the Data/Method section, and I would suggest to call it “Study region.”

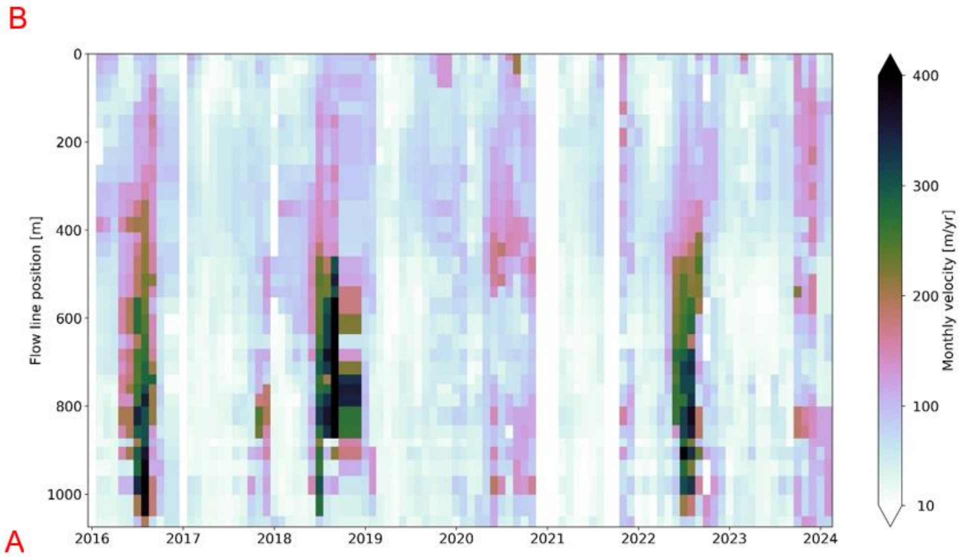
We have moved and renamed this accordingly.

Figure 3: Please add the names of the main glaciers that you are studying on the map rather than just numbers. You have plenty of space to do that.

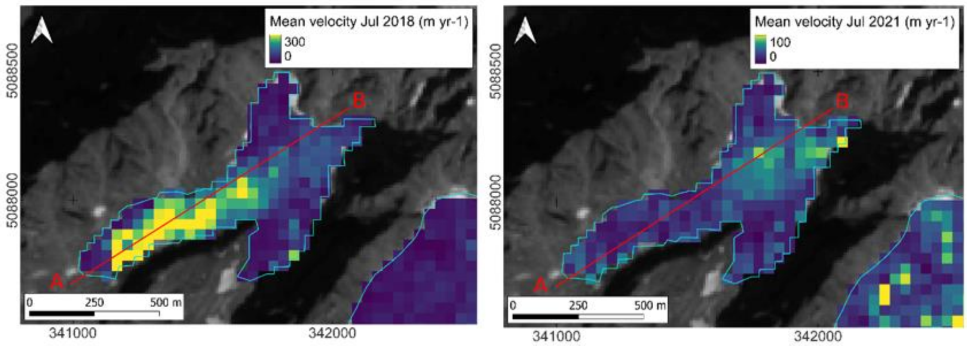
Thank you for this suggestion, we have added the glacier names.

L284: Here I think that the discussion and figures are really limited to conclude about the “surge” behavior of Charpoua. Can you please provide a full raw data along-profile time series over Charpoua glacier since 2016? This would allow us to differentiate between seasonal signal vs abnormally high speeds. From Figure 7, it is difficult to conclude that the speed up of Charpoua is abnormal, but it looks rather like a seasonal cycle.

We agree that our figure is not yet very effective at showing the particular trends of the Charpoua Glacier. We prepared a new presentation of the data, according to your suggestion (see attached figure). In this image, one can notice a seasonal cycle in the higher part of the glacier, with summer values between 80-100 m/yr. This behaviour is less marked in 2017. On the other hand, winter velocities are usually well below 50 m/yr. In 2016, *2018 and 2022, the velocity in the lower area of the glacier was much higher, reaching more than 400 m/yr. Contemporary, the summer acceleration seems to begin slightly earlier than in the other years.*



Charpoua Glacier monthly velocities along A-B longitudinal profile



Have you looked at thickness changes over this area (see Hugonnet et al., or papers from Berthier et al.) to conclude on typical mass changes patterns that would suggest a surge?

Unfortunately, the studies that you suggest did not cover the analysed period (e.g., Berthier et al) or integrate across too long periods (e.g., Hugonnet et al. provided elevation changes at 5 years intervals, which cannot confirm/negate shorter processes, like those we are analysing)

As a general remark from both datasets suggesting a hypothesis that both strong accumulations occur at this particular location and a strong mass transfer mechanism towards the frontal part of the glacier could exist, resides in altitudinal changes that appear to be significantly lower in respect to nearby glaciers.

L286: In general, for land-terminating glaciers, we would expect the ice velocity to be maximum close to the ELA.

Thank you for this notable remark. Accounting for this, coupled with comments regarding the distribution of the sampling points, we decided to re-extract data with a different strategy regarding the selection of sampling points. We extracted data from an area defined by cropping RGI 7 flowlines at the upper and lower altitudinal limits of the ELAs mapped between 2016 and 2022. Around this section of the ELA, we buffered an area of 40 m. The obtained polygons were used for the extraction of new time series.

What is the elevation of this glacier compared to others?

The elevation range (2650-3479 m asl) is comparable to other glacier bodies, as reported in Table 1.

Can you differentiate this pattern from the glacier surface slope?

Please apology, but we are not sure if we understand correctly this question. If the question refers to the fact that we checked or not if the acceleration occurs or have a relation to zones of higher slope, we checked that the acceleration takes place in an area with less slope compared to the rest of the glacier.

Here you also have a really small glacier, that is only a few pixels large, hence you are reaching the limits of what we can measure with Sentinel-2. Having more information on the level of noise in the data (see general comment) will provide more strength to the conclusions.

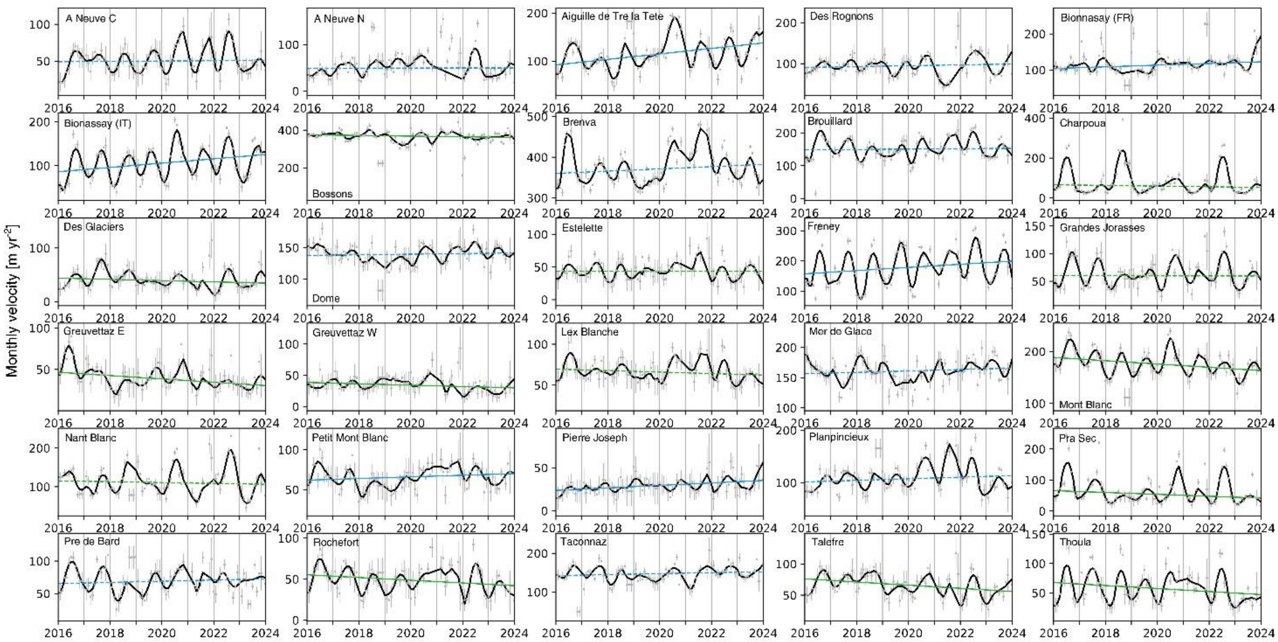
We have provided a map with indicated the motion in the off-glacier areas in the supplementary and added error bars to our time series.

L293: What does this do inside the Results section? Please homogenize and merge this with the similar part in the Data/Methods.

Done.

Section 5.5: Please provide error bars on these estimates (see general comment)? Do you include what you call surge-type behaviour in here? If yes, you have to differentiate that from "seasonal" velocity variations. Figure 7: Adding error bars to these time series is critical. Furthermore, please adjust the range of y values so that it fits the max/min speeds calculated for each glacier. Add error estimates on all of the velocity values that are discussed

We have added the error into the text and error bars in Figure 7. We also adjusted the y-axis limits based on the velocity range of each glacier to better highlight its behaviour. Sorry, but we do not understand the question about the surge-type vs seasonal behaviour.



L340: What do you mean by "robust" linear interpolation?

A robust interpolation (or regression) is the common name referring to a family of interpolation methods that are less affected by the presence of outliers. We adopted a quadratic Huber loss function (Huber, Peter J. (1964). *"Robust Estimation of a Location Parameter"*. *Annals of Statistics*. **53** (1): 73–101). *We added some details and a reference into the new version.*

Section 6.1: This will need to be revised after considering the general comment made on the methodological aspect of the paper. Indeed, everything in the paper lies in the methods that are being conducted to derive the trends in ice velocities, which will affect the discussion.

Thank you for this comment. The new uncertainty analysis and time-series corroborate the conclusions of the first version. However, we slightly modified the conclusion to better explain our results.

L401-402: The authors should provide a more detailed description of the differences in trends with Rabatel et al., and more specifically over Brenva, Bosson, and Argentière.

Thank you for the comment. We have responded to this question in the answer to the general comment regarding the comparison to the Rabatel et al (2023) data.

L429: A critical piece of information that would be needed to classify a glacier as a "surge type" is the pattern of changes in ice thickness. This would be drastically different if a surge occurs, showing a net mass transfer across the glacier. Hugonnet et al., 2022 provide changes in ice thicknesses since 2000 which is a key information to account for.

Thank you for this comment. We agree that the ice thickness is a relevant information in the definition of a surge. Actually, we referred to surge-like behaviour, since the kinematic activity was similar to a small surge, but we indeed presented this as a hypothesis, which should be conformed acquiring more data (like the thickness change). Unfortunately, the studies that you suggest are not suitable to evaluate possible thickness changes in the period of interest, because some do not overlap it and other integrate the thickness variation across too long periods. We also point out that surges of small glaciers have never been observed, since this process is more often related to large valley glaciers. Therefore, it is not obvious that the same dynamics should occur, and neither that is caused by the same processes. Besides, in Charpoua, relatively large ice break-offs from the front usually occur during these periods of speed-up, thus possible contributing to weak the thickness rise.

L442: Changes in ice thickness are available over this area. See the following papers:

Hugonnet, R., McNabb, R., Berthier, E., Menounos, B., Nuth, C., Girod, L., Farinotti, D., Huss, M., Dussaillant, I., Brun, F., and Kääb, A. Accelerated global glacier mass loss in the early twenty-first century. Nature, 592, 726-731, doi: 10.1038/s41586-021-03436-z, 2021

As referred in previous comments, the considered time intervals are not appropriate to describe the observed phenomenon. Only one analysed interval from Hugonnet et Al. partially overlaps with the study period of the present work (2015-2020 vs 2016-2022).

Berthier, E., Vincent, C., and Six, D. Exceptional thinning through the entire altitudinal range of Mont-Blanc glaciers during the 2021/22 mass balance year. Journal of Glaciology, in press, doi: 10.1017/jog.2023.100, 2023.

The data presented by Vincent et Al. is very interesting but has two major criticalities from our point of view for the comparison with our data: i) the periods of the analysis are 2012-2021 and 2021-2022. The first time interval covers a much larger time interval compared to the one in our study and as stated for the Hugonnet dataset, such long-time intervals are not suited for the observed recurrence of the "surge-like" behaviour of the glacier. ii) The second period covers a one-year interval and falls into the time coverage of our study, which could be relevant for the proposed analysis, but unfortunately, as stated in the paper's title, the exceptionally negative mass balances and thinning documented across the massif are so large to probably overcome a possible observation of the phenomenon.

As a general observation, we observed substantial stability of the altitudinal data of Charpoua Glacier in the period 2012-2021, which would suggest the possibility of higher than normal accumulation rates or downslope mass transfer in the area.

Berthier E., Cabot V., Vincent C. & Six D. Decadal region-wide and glacier-wide mass balances derived from multi-temporal ASTER satellite digital elevation models. Validation over the Mont-Blanc area Frontiers in Earth Sciences, 4, doi:10.3389/feart.2016.00063, 2016

Berthier E., Vincent C., Magnússon E., Gunnlaugsson Á. Þ., Pitte P., Le Meur E., Masiokas M., Ruiz L., Pálsson F., Belart J. M. C. and Wagnon P. Glacier topography and elevation changes derived from Pléiades sub-meter stereo images, The Cryosphere, 8(6), 2275-2291, doi: 10.5194/tc-8-2275-2014 (open access), 2014

The analysed periods of these works do not overlap the one presented in our study.

Section 6.3: This section is coming way too late inside the manuscript. Part of this section should go inside the methods, and the description of the errors should be the first paragraph of the results! Furthermore, the error calculation here should be used throughout the entire results section to put error bars on the trends estimates and inside the figures.

Thank you for this comment. We modified the manuscript accordingly.

Why are the authors selecting 155 points on stable terrain? How are these points selected? Do you keep a uniform spatial distribution of these points? Millan et al. calculated error on all available stable terrain and the same should be performed here.

We selected 155 points on stable areas distributed around the Mont Blanc massif. Since we manually selected the points to extract the time series, we followed the same approach in selecting the stable points to maintain a homogeneous methodology. Since in the new version we extracted the time series with an area-based approach, we also performed the uncertainty evaluation considering stables areas. However, as expected, the uncertainties are very similar, the point-based one was 11 m/yr, while the area-based one is 10 m/yr.

Another comment I have is the use of repeat cycles of 20-40 days. Millan et al., 2019 have shown that if you want to observe a speed change of 10% with good SNR, the 2-sigma precision of the velocity maps should be smaller than 1/10 of the magnitude of the ice slow locally (Figure 9 of their paper).

We respectfully point out that the criterium proposed by Millan et al (2019) is based on a heuristic and subjective assessment, as the authors state "Finally, we have tried *to establish the required minimum repetition cycle for Sentinel-2 that would allow to detect a 10% change in glacier surface velocity, which generally corresponds to the* expected variation *in glacier velocity with the seasons.* We assume *that this requirement would be met when the 2-σ precision of the velocity maps (Figure 4C) is smaller than one-tenth of the magnitude of the ice flow, which would therefore provide a sufficient signal-to-noise ratio to capture the variation", but they did not demonstrate this formally. Besides, in their conclusions, Millan et al stated that "Our estimation of Sentinel-2 precision indicates that only glaciers with ice velocity fluctuations greater than 10 m/year can be monitored on a seasonal basis". We have shown that the seasonal variations are usually >10 m/yr, almost always >10% and >50% in many cases. Therefore, our findings suggest that repeat cycles of 20-40 days are sufficient to detect monthly velocity variations of the considered glaciers.*

Considering this, the sampling points that were chosen for the velocity trends, you should filter them out, otherwise they will bias the monthly velocity estimate. This is the case, for example, of Argentière, LexBlance, PetitMontBlanc, DesGlacier, Talefre, Taconnaz, Bionnassey glaciers (and lots of others ones from Fig 7), that have a speed typically <100 m/yr. With a 2-sigma precision of 22 meters/year, the monthly speed change observed in Figure 7 is mostly just noise in the data.

We respectfully disagree with this comment. We have shown that, using our approach, we could detect seasonal/monthly changes in glaciers with mean velocities <100 m/yr. A further empirical confirmation is given by the new Figure 7, where the seasonal cycle is clear with maxima in summer and minima in winter for most glaciers. Indeed, in a few glaciers, such seasonal behaviour is not evident, which could be due to a real absence or an amplitude lower than the measurement uncertainty, thus is true that the fluctuations can be caused by noise. But the absolute velocity can be detected correctly (net of the uncertainty), since we can definitively measure velocities of ~50 m/yr or less, considering that the estimated uncertainty is 10 m/yr. Thus, it is worth including also these glaciers into the analysis, since this possible lack of seasonal behaviour, together with other morphodynamical features, allowed us to characterise some of the glaciers of the Mont Blanc massif.

L482: A more important comparison would be the trends in surface flow velocity with Rabatel., 2023. You could both compare the amplitude of the seasonal signal and the multi-annual trends with their data, which are freely available.
As stated for the comment at L401-402, we provide a comparison with this data as we requested the exact location of the time series extraction to the authors (the data is available upon request) first and later we added a comparative analysis based on our chosen data extraction areas.

L516-518: Can you better explain how you calculated these 40 m/yr regarding the uncertainty, the resolution of the images, and cross-correlation parameters (grid spacing)?
Thank you for the comment, this helps us to correct for this repo. It should be 22 m/yr, which is twice the estimated uncertainty of 11 m/yr. However, we modified this statement according to the new analysis.

Reviewer #2 comments:

The authors have created a new multitemporal velocity map for all glaciers in the Mont Blanc Massif using Sentinel-2 imagery. They make a number of derivative analyses, both classifying the glaciers based on velocity and morphology into new 'categories/types' and investigating temporal trends. Overall, the paper is well written, the analysis is robust, and the topic appears relevant to this journal. I do, however, have a number of questions and comments about the paper structure and certain of the follow-up analyses, and so propose that this paper undergoes major revisions at this stage.

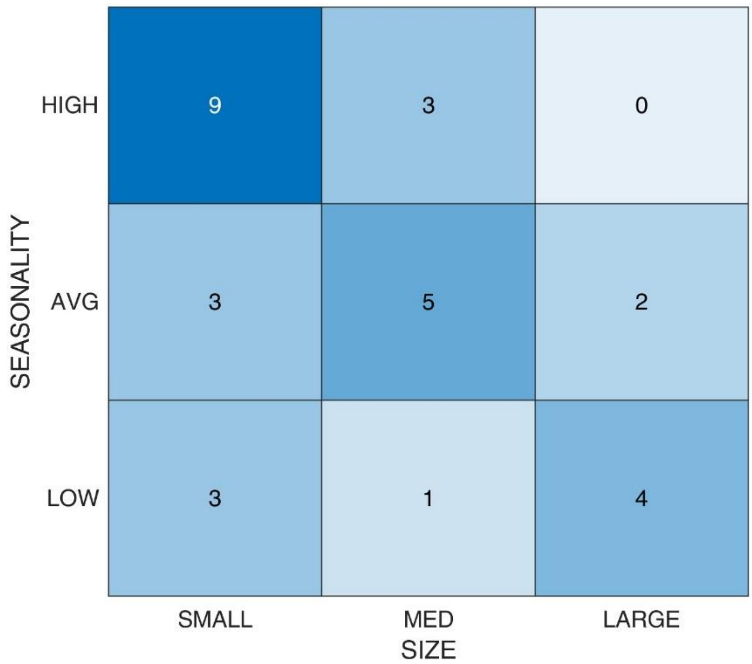
A few specific comments or concerns are:

One of the motivations which you present is that the glaciers of Alpine regions, and even the Mt Blanc massif specifically, have been understudied. However, they have been studied quite intensely for many years and are actually one of the most data rich glaciated regions on the planet. This does not diminish the usefulness of your study (this is not a direct repeat of a previous study) but this needs to be better recognized so that the previous contributions are built on.
Thank you for this comment. We agree with you and we did not want to underrate previous studies with our statements, a lot of our work builds up on other works that have been undertaken specifically on the Mont Blanc Massif. We have reworked our text to communicate this correctly.

For the timeseries analysis, I am not sure whether you have really located a breakpoint in 2020 since this is a date you chose yourself. Showing that the trend pre- and post-2020 is different is not the same thing as

determining that this is the specific year that something changed (e.g. if you chose 2019 or 2021 as the year, would it not likely still show different trends either side?). If you want to show this I would perhaps expect to see some automated breakpoint analysis technique applied so that you can remove the a-priori assumption of 2020 being a change point. I don't necessarily see a justification for a 2-piece rather than a single fit for many of the timeseries. Many of the resulting trends seem to be related to truncating the seasonal cycle more so than a major shift in glacier dynamics.
In the new version, we provided an analysis of the global behaviour of all glaciers, where is clearly evident the 20-22 anomaly. Therefore, the break-point analysis is no longer necessary.

The glacier classification was not very convincing as presented in the current manuscript. I think the method itself you are using (PCA + K-means clustering) is fine, but I am not sure you have sufficiently explained why it might be useful. I definitely don't think you should give these names as you currently have ('surging', 'energetic', etc) which imply more understanding of processes or already have specific meanings which are not necessarily met here (e.g. 'surge'). I'd just call them 'Group/Class 1', 'Group/Class 2' etc to avoid this and clearly decouple data and interpretation. You need to clearly explain why you chose 5 categories, and make sure the wording clearly reflects that this is a parameter choice rather than a fundamental property of the data.
Thank you for the comments. The number of categories was evaluated with an expert-based visual analysis. We applied the K-means to discriminate between glaciers that lie close to the boundaries of different classes. However, we profoundly reworked this part and did not apply the PCA+Kmeans.
The categories are presented, but you do not show that they represent a particular difference in the glacier processes in different types or that they are likely to respond in different ways. With some thought and engagement with the local literature you could probably do the latter – i.e. frame it as the categories providing a convenient frameworks to identify similar glaciers and compare their evolution.
In this version, we propose a classification based on glacier size and velocity seasonality (see the heatmap below), which shows that small glaciers are more susceptible to seasonal velocity changes than large glaciers. This new analysis should be easier to understand and be more generalised.



The structure is a bit mixed with pieces of methods in the results and discussions, etc. Better getting this in one place will improve the readability. You should add some more information about the specific parameter choices used for the feature tracking so that we have the full info (even knowing GIV well I cannot figure it all out).

Thank you for the comment. We have restructured the manuscript according to your comments and those of reviewer #1. We also added full information about the adopted parameters of GIV.

As I mentioned, none of these issues are fundamental and should be addressed with a round of major revisions. I'll provide some more specific comments for given lines:

L1 / title: I think the title could be more closely linked to the study, it is a bit vague. Mention ice velocity in there. E.g. 'Velocity and dynamic change of the Mt Blanc Glaciers, 2016-22'. I've not seen the term 'morphodynamics' used for glaciers before and am not sure it quite applies here.

Thank you for the suggestion. Since we have modified the analysis, we also decided to change the title: Velocity of the Mont Blanc glaciers and their recent evolution, 2016-2023. However, morphodynamics is a common term used in glaciology and geomorphology, see for example Deline et al (2012). The morphodynamics of the Mont Blanc massif in a changing cryosphere: a comprehensive review. Geografiska Annaler: Series A, Physical Geography, 94(2), 265-283.

L9 Velocity is not a parameter, it is a physical property.

Correct

L10 Glaciers don't really 'adapt' to climate change. Perhaps 'their sensitivity' or 'their response' would be better.

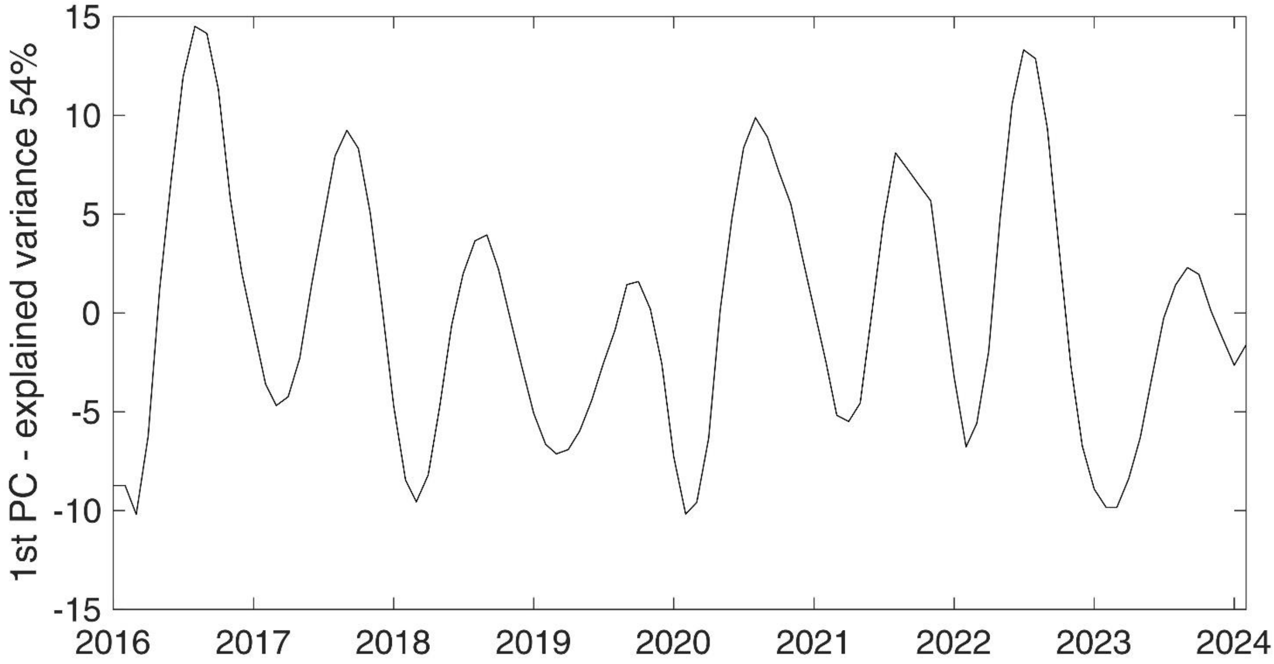
"Their response" sounds like a better option.

L13-14 'Few studies have been performed in alpine regions' – this is just not true. There are certainly tens, and likely hundreds of papers on ice velocity in Alpine regions. They are also relatively better studied than many other area (the Mt Blanc massif in particular has many datasets, some unique in the globe). I'd just remove this sentence.

Thank you for the comment. We have revised the introduction accordingly.

L19 See my comment about the '2020 breakpoint' – since you chose this date yourself, I am not sure you have shown this

In the new version, we provided an analysis of the global behaviour of all glaciers, where is clearly evident the 20-22 anomaly (see figure below). Therefore, the break-point analysis is no longer necessary.



L21 Needs some info about what these classes show, or why the classification is valuable. Otherwise the readers will think ‘so what?’. Also, since you chose the number of classes (why 5?) this phrasing is slightly confusing.
In this version, we propose a classification based on glacier size and velocity seasonality, which shows that small glaciers are more susceptible to seasonal velocity changes than large glaciers. This new analysis should be easier to understand and be more generalised.

L29 – Do you mean in the field?
Yes, we clarified that in the text.

L30-36 This paragraph could use a revision for clarity. The information is mostly OK, but is presented in a messy and somewhat confusing manner.
We have rephrased the paragraph more clearly.

L37 I don’t understand what you are saying. Surface displacements are not a proxy for ice flow, they are the result of ice flow.
Thank you for the comment, we have adjusted the statement. We wanted to stress that the measure of surface velocities is often the only physical process that we can measure regarding ice motion with a relatively small effort. Rarely we have the chance to perform field measures in order to measure motion from the surface of glaciers, along the ice column and down to the ice-bedrock interface, which would be really interesting but requires an enormous effort compared to obtaining surface velocity data.

L41 Again not a proxy
We have changed “proxy” to “indicator”.

L65-66 What about Millan et al 2019 (https://doi.org/10.3390/rs11212498), Rabatel et al. 2023 (https://doi.org/10.3390/data8040066), etc? to just name a couple of recent studies. It is fair to say more work is needed, but what you have written here is not true.

Since we are focused on velocity variations, we added the reference to Rabatel et al (2023), who analysed the surface velocity across a period of 5 years. But Millan et al (2019) just performed an analysis considering a single time interval: 2017-2018.

L86-91 Not sure this paragraph has much useful information
Thank you for the comment. It is true that this information is not fundamental to the study, but it provides some spot details for readers who may not be familiar with the area of the Mont Blanc.

L107 I do not think this section is needed at all. I would just merge one or two sentences about what S2 images you used into the methods and leave it at that.
Thank you for the comment. We have merged Sections 3 and 4 into a single Materials and Methods.

L111 A lot more background info about very basics of the mission than needed. We need to know what band you used, how many images, date limits, but not all the rest.
We have omitted superfluous details.

L123 Just add this to the methods and remove the section
Done

L128 I tend to call this optical feature tracking, though DIC is also widely used (and PIV, pixel offset tracking, etc etc). Could be worth an 'also known as' parenthesis?
Indeed, the technique has many nouns. We usually use the acronym DIC, but non-expert may not be familiar with it, therefore we decided to expand digital image correlation throughout the manuscript (it occurs just a few times). We added a "(also known as feature tracking)" in parenthesis.

L133 I wonder if an 'inset' here giving a little more detail about the 'Digital image correlation' procedure would help? I know the details of the model, but this won't necessarily be the case for most readers. The timeseries processing and so on in GIV in particular is not necessarily 'standard' procedure.
Thank you for the comment. We prefer to maintain the focus on the results of our study, rather than on the specific methodology of image correlation. Therefore, we decided to not include an inset into the flow chart to leave it clearer. Contemporary, we have expanded the description of the methodological application of GIV.

L154 What do you mean an 'active' glacier? Try and use precise language.
We have modified "active" with "showing strong kinematic activity".

L160 The orientation filter pre-processing in GIV is generally quite good at handling shadowed areas so long as features remain visible to some extent.
Thank you for the comment. The orientation filter actually improves a lot the performance of satellite image correlation. Nevertheless, in the areas where the cast shadows vary significantly the correlation may fail even using the orientation filter.

L168 Could you show a map, here or in sup mat, of the included/excluded glaciers?
Figure 1 shows the outlines of all the glaciers of the massif, while Figure 3 shows only the selected glaciers.

L173-175 I am not sure if it is a problem with your workflow or with your description of it but this sounds very subjective here. On what basis did you split / merge glaciers? I would say you need an objective criteria

(e.g. separated by at least x pixel of exposed rock) or stick to the RGI for consistency. For instance, why was the main branch of Miage glacier not analyzed?
We divided the Miage complex into different individual tributaries according to their names and outlines that appear on both Italian and French topoghraphic maps.
We did not analyse the main Miage tongue because it is entirely below the ELA and its velocity too small to evidence monthly fluctuations. On the contrary, we have considered as a unique glacier the upper part of the Mer de Glace Glacier, which lies entirely above the ELA.

L186 Did you consider using other thickness estimates, e.g. from Farinotti 2019?
We have analysed the thickness of Farinotti, and found them similar to those of Millan et al (2021), which are indeed more recent. However, the morphological analysis has largely changed.

L200 Technically GIV does not coregister images, it instead calculates stable-ground shifts to correct for georeferencing errors without the image degradation which coregistration can introduce.
Yes, we unproperly used the term coregistration here as the normalization to stable ground is a different procedure as implemented in GIV. We have rewritten the sentence accordingly.

L203-205 You are missing some key information to understand your feature tracking results here. What were your min and max temporal bounds? What was your 'target resolution' (used by GIV to set the pixel matching scale)? What was your overlap between matching windows (was it the default of 0.5?)? Did you adjust any other parameters from the defaults? State that all else was at default, and include your parameter file in the sup mat.
We have added full details of the processing setup of GIV.

L211 Missing some key info. What did you use to evaluate coherence – was it using the snr output from GIV? Some other measure?
The presence of areas of higher velocities compared to the rest of the glacier and showing very little outlying pixel values was a first indicator together with the persistence of the velocity patterns on the velocity maps over time. Moreover, the analysis of the flow direction standard deviation was really useful to individuate areas where glacier velocity has been successfully tracked by the algorithms.

On the whole I don't really understand the idea of manually selecting points. This seems like it would inevitably introduce some bias, and the location of each point is not justified. It would be better if you instead used (i) a glacier centreline or (ii) a zone of the glacier x km from the front / x% between the front and ELA or something of this type. It is very risky to manually select points you perceive as having lower noise levels as this will almost certainly bias outputs. I would therefore like to see this analysis redone with one of these other criteria, or much stronger justification of the point distribution.
To cope with this source of subjective choice that you correctly pointed out, we have performed a new data extraction procedure:
We extract data from an area defined by cropping RGI 7 flowlines at the upper and lower altitudinal limits of the ELAs mapped between 2016 and 2022. Around this section of the ELA we buffered an area surrounding the linear feature by 40m. The polygons obtained were used as surfaces for the extraction of new time series. Performing all the derivate analyses such as the trend fitting, the PCA and the glacier classification we obtain very similar results compared to the previous ones, but sticking to a more robust method for the definition of the sampling areas. We carefully revised the discussion of results according to the newly extracted datasets.

L218 'averaging' through what operation. Taking the arithmetic mean?

Yes, we calculated the arithmetic mean.

L219 ‘Outliers removal was manual’ …based on what? Again, I would much prefer an automated filtering of some kind, or at least much better explanation of what was done.
Manual outlier filtering is a common procedure, which relies on an expert-based visual analysis. However, during the review, we applied a 3x3 median filter to the velocity maps, which allowed us to automatically remove many outliers. To process the time series, we did not filter them anymore, but we applied a loess quadratic interpolation, which automatically weights the raw data based on their statistical consistency, thus avoiding the need to apply a manual rejection.

L225 Why is the median named ‘GlobalAvg’? This is misleading.
We have modified with MedVel.

L228-230 This is confusing – are the reference period always the same or different then? Please tell us exactly what they are, is it 01 Dec-end April and 01July – end Sept?
Thank you for the comment. The reference period is the same for all glacier. The intervals are December-April and July-September. We did not report the day of the month because we calculated monthly averaged velocities. However, we rewrite the statement for clarity.

L231 The max in a given year or over the entire period?
We refer to the maximum monthly value in the entire period. In the new version, this datum is no longer considered.

L241 Why did you select 5 classes? This seems like a very fundamental question given the following analyses with this. How do the results differ with 2, 3, 4, 6, 7 classes? Given that you manually selected this, do we expect the classes to have any physical meaning? Would a clustering mechanism which does not require pre-selection of the number of classes be more appropriate?
Thank you for the comments. The number of categories was evaluated with an expert-based visual analysis. We applied the K-means to discriminate between glaciers that lie close to the boundaries of different classes. However, since the application of automatic clustering is not fundamental and may appear too sophisticated, we decided to eliminate this part. Besides, we experimented some automatic clustering algorithms (e.g., affinity propagation, DBSCAN, mean shift) and the number-parametric Gaussian mixture, but the results were always unsatisfactory. However, as said previously, in the new version we motivated the glacier classification based on manual analysis.

There are a lot of questions you have to answer here, and they affect the interpretability of a large portion of the following manuscript so require some serious attention.

L243-248 Not sure this text is needed.
Thank you for the comment. However, we prefer to leave this paragraph which introduces the reading of the results section.

L249-260 This is methods not results.
Thank you for the comment. The method to select Sentinel 2 images was described in section 4.1, while here we are providing the list of images and how they are distributed across the years. However, according to your suggestions, we have reviewed the manuscript structure.

L270 Not clear, given the lack of description, whether 40m is the native resolution of the GIV output here or whether it was resampled to a new resolution.
Thank you for the comment. Yes, 40m was the native resolution of the velocity maps. We did not perform resampling of the velocity maps from GIV.

L273 Coming back to this again, but why is Miage mostly excluded? Were you excluding debris-covered regions?
We separately analysed the tributary of the Miage Glacier because the lower flat tongue is all below the ELA. We could have chosen a single trunk as representative of the whole glacial complex, but we preferred to increase the number of analysed glaciers.

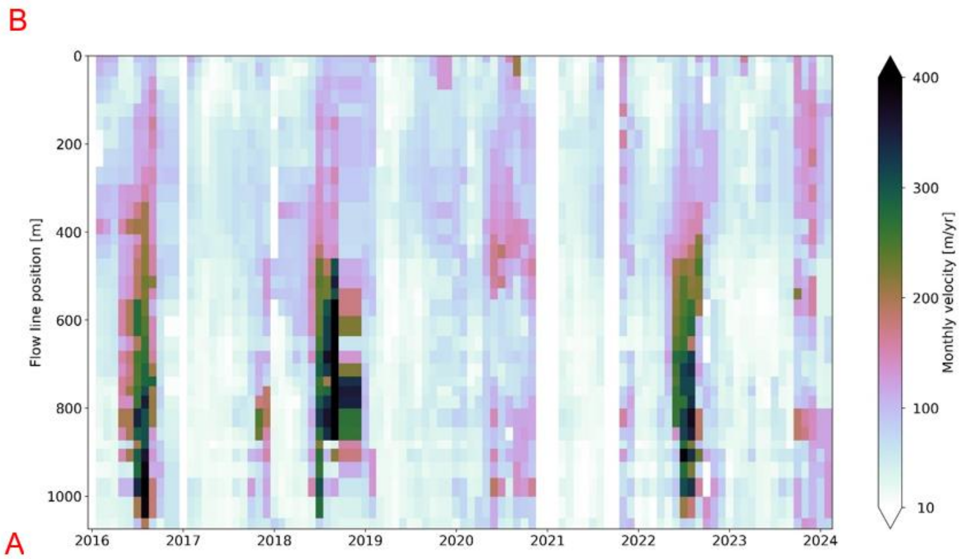
L278 You describe elongation ratio as length divided by area, so I guess it has units of metres?
Correct, we have added the units.

How precise is your length? Worth rounding to the nearest 10m?
We agree that probably the precision is not so high, however, in the RGI6 the length is expressed rounded to the metres.

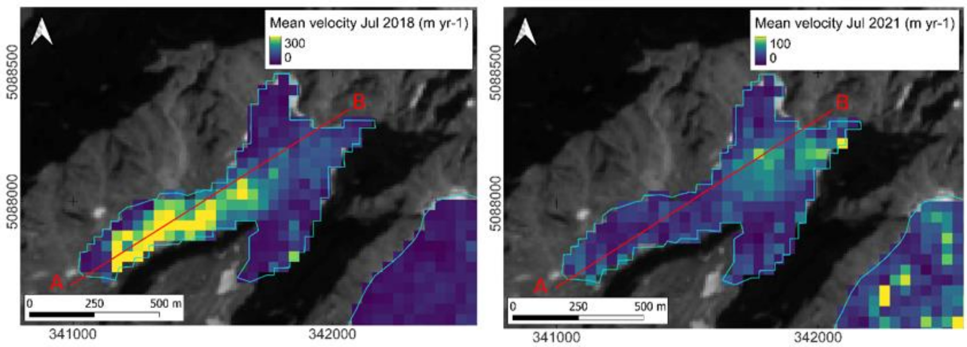
Same for the mean ice thickness, do you really have cm precision? Even to the nearest m seems to be pushing it.
The ice thickness is obtained from the data of Millan et al (2022), which has metric accuracy.

For all of these, could you include uncertainties. You mention the thickness is from Millan's paper, they have an error estimate. Same for area, length slope, etc.
The morphological data belong to the RGI6, which is provided without uncertainty. Regarding ice thicknesses, we will add uncertainties referring to uncertainties found in "Results from the Ice Thickness Models Intercomparison eXperiment Phase 2 (ITMIX2)", Farinotti (2021).

L283 What data led you to identify this glacier in particular? Could you show a plot of summer/winter vel ratio for all glaciers?
At first, the Charpoua Glacier was one glacier in particular that showed surprisingly high variability of the summer velocities in different years. Generally, the summer velocities are in the 70-100 m/yr range (2017-2019-2020-2021), around +100% higher than winter, when always fall in the 25-50 m/yr range, while in other years (2016-2018-2022), the velocities are in the 200-300m/yr range which is in between 4 to 7 time higher than the winter velocities. This can be noticed on the velocity maps (fig.4) as well as on the velocity time series (Fig. 7). The 2016 velocity peak of Charpoua Glacier in Fig. 7 on the velocity time series is remarkably reaching well above 250 m/yr, while the subsequent winter deceleration falls to around 25 m/yr. This is the most abrupt velocity change found in the velocity time series.
We prepared this figure to better highlight this behaviour that we included in the new version.



Charpoua Glacier monthly velocities along A-B longitudinal profile



L284-286 This does not match the typical definition of a surge, this looks more like ‘typical’ uneven seasonal variability for a glacier with high sensitivity. Consider if the language is appropriate.

A surge is a process that usually concerns large valley glaciers and it has never been observed/attribute to a small steep glacier. We referred to those glaciers as “surge-like” to indicate an anomalous acceleration with a certain regularity, but not occurring every year.

L296-299 This seems like methods not results.

Correct.

L310-317 Can you report these values with error bars?

We have redrawn Figure 7 by adding error bars for each monthly velocity calculated independently. Plus, we have included the uncertainty analysis into the text.

L328 / Fig 7 I am concerned looking at some of these linear fits that you have not fully considered the implications of a seasonal cycle on your results. Many of the ‘significant’ results (e.g. PraSec, Dome, Freney, Planpincieux, Brouillard, Mt Blanc) seem to be at least partly related to the fact that your timeseries is starting during the low part (winter) of the seasonal cycle. In this case the assumptions you are using to test for statistical significance are probably not valid.

Thank you for the comment. The linear fits that we used to determine trends always start in January and end in December in both periods of interest (i.e., 2016-2019 and 2020-2022). Plus, such fits are calculated with a *robust regression method (i.e., a Hubert loess linear function), which gives low weights to possible outliers. However, with the extension of the analysis to the February 2016-February 2024 period, we decided to perform a full period fit only.*

I would recommend doing one of the following:

- Subtract the seasonal cycle from each timeseries and calculate the linear trends of your residuals
- Instead of fitting only a linear term, fit a linear term summed with a 1-yr period sinusoidal term.

There are decent toolboxed in python/matlab for doing the latter if you want to do that, and the former is easy to set up.
Thank you for the comment. We respectfully believe that the first method that you are proposing is not suitable to perform our analysis. In particular, we do not understand how we should calculate the seasonal cycle. Plus, the linear fit of the residuals should have slope zero and the residuals should be symmetrically distributed around zero, provided that the interpolation was successful and net of possible outliers. The second method appears more promising, but it would entail estimating a fixed amplitude of the sinusoid (or one that varies monotonically with the time), which is an assumption hardly satisfied.
In the new version, we adopted a loess quadratic function evaluated over a sliding interval of 1 year, which is an effective way to highlight the seasonal behaviour. Subsequently, we applied a robust linear fit on the loess-interpolated values.

L340-342 Considering trends for only summer/winter does help, but it does not solve the problem above as the plots clearly show that there are regular seasonal trends within these subdivisions still.
Please, refer to the previous answer.

L355 What do you mean ‘less statistically significative’? Presumably, it is or is not statistically significant for a given threshold.
Statistical parameters like p-value or t-statistics, as in our case, are a measurement of the statistical significance level. For clarity, we reworded it as “they have a lower significance level”. However, the threshold of 2 of the t-test is not a convention.

L368 What is a ‘gentle’ glacier?
We meant that a gentle glacier is a glacier with a gentle slope.

L371 This seems a bit misleading to say, it was not the model that identified 5 classes but rather you that set 5 classes as a parameter (and it is not clear why).
In the new version, we did not use this classification anymore.

L373 / Fig9 The PCA plot here is interesting, but as I have mentioned there is not enough info to judge why the clustering is being done as you have done it (or even why it is being done at all).
Please, see the previous answer.

L377-398 A lot of this seems like it might fit in the intro or be greatly shortened rather than be here.
Correct

L398-399 As mentioned, the issue of timeseries splitting in a seasonal cycle needs to be considered in more depth before we can be confident about these results.
We are confident that the more exhaustive explanation and content that we gave here and in the new version will sustain our results and conclusions.

L418 I would like you to elaborate in more detail if the trends remain robust after addressing the issue of seasonal cycle. What mechanisms do you envisage for a climatic regime change to affect regional glacier velocities? A more intense early summer melt pulse driving high basal pressures and more sliding before efficient drainage is established?

Regarding the hypothesis for the mechanism driving a regional glacier velocity change, we suppose that an enlargement of the temperate areas towards higher, once cold based, areas of glaciers could be one of the drivers of this change, as suggested by Rabatel et. Al.(2023)

The hypothesis of enhanced sliding by high pressure build up by increased melt in early summer season is another possible cause of the change.

The first hypothesis seem to be plausible, but the fairly rapid change in velocity trend that we observe in this study does not seem to fit well with the slow multi-decadal basal warming trend that is observed at ice-bed interfaces for example at Dome and Taconnaz Glaciers from the measures of Vincent et Al.(2007) https://doi.org/10.1029/2007GL029933 , and modelling from Gilbert et Al. (2015) https://doi.org/10.1029/2007GL029933.

The latter hypothesis is accountable for the changes in the early season but not really in the peaks of the mid-summer season. Still the same process could explain late season (autumn) speedups as well.

At this stage we think that more research is needed to address more in detail these questions together with possible thickening of accumulation areas of the glaciers and a change in the circulation of melt water in accumulation areas as the main (but not all) possible causes of this phenomenon. Nonetheless, we must account that probably not a single factor is playing a role into this change but more likely a combination of those.

L423 ‘Detachment’ is usually the term used for this glacier collapse process as at Aru

We will correct accordingly.

L428 Again, quite misleading to say this since you parameterized to model to identify 5 groups.

Correct

L429-444 These are not meeting the typical glaciological definition of a surge. It would be better to not label them this way.

We have modified the labels of the groups.

L445 Since they are moving still, ‘stagnant’ is also not really accurate

We have modified the labels of the groups.

L429-471 This whole section goes into a lot of detail about the different glacier classes/groups defined by the K-means clustering, but is really missing information to understand why these might be useful. As far as I can tell from this manuscript it seems to be a fairly artificial separation which doesn’t necessarily reflect different underlying processes or other commonalities within the clusters. Are you suggesting that there might be some generalizable rule about these different types of glaciers? That these classes might be found in other Alpine, or even non-alpine areas?

In the new version, the glacier classification has substantially changed.

Where does the number 5 come from? How do we know there shouldn’t be 4, or 6 classes?

We did not perform automatic clustering anymore.

L472 It would be good to move the error analysis and evaluation to the results, it is necessary to understand the quality of the subsequent work.
Thank you for the comment. We did it accordingly.

L497 / Figure 10 It would be good to plot this figure with error bars from your data and from Millan et al.
Done

L501 / Table 2 Please label 'Mean difference' instead of just 'Mean', and include the error bars here too.
Thank you for the comment. We modified the label. However, we could not understand which kind of error we should include here. We are already indicating statistics of uncertainty.

L509-510 It is unclear what you mean by 'anomalous' here, how can this be determined for a given velocity increase? I agree about you point that a better understanding of baseline velocity variability is necessary to identify precursory changes to a detachment.
We went a bit too far by stating "anomalous" at this stage, as it would be speculative. At this point we can just remove it. It is interesting to note that this was one of the main research questions that drove us into this study: while some glaciers are under specific, high rate monitoring, such as the frontal part of the Planpincieux Glacier, and there is quite a lot of data on this specific sector of the glacier that shows signs of a possible destabilization (Giordan 2020), not much is known about the baseline velocity variability of the main glacial body. So we agree that we cannot define "anomalous" a possibly detected acceleration, but we stress on this point on which you agree, as one important finding of our study and a starting point for future research.

L519-520 If we include both short and long baselines it is possible to improve resolving power over slow-moving areas but still capture short events. The post-processing can be more complex however.
Correct. To our opinion, the approaches of Millan et al (2019) and Mouginot et al (2023) do not allow to detect short-term fluctuation effectively.

L532 See comments above- reasons for 5 groups not clear.
We refer to previous comments as well on this topic.

L550 This data availability statement is not really aligned with the FAIR (Findable, Accessible, Interoperable, and Reusable) principles which is expected for this journal. Unless there is a particularly compelling reason, I would expect you to upload the glacier outline shapefiles to an online repository and link to them here.
Thank you for the comment. We have added those data to the supplementary.

You say that the glacier velocities are available as a supplement, but I cannot access them. There only seems to be the pdf supplement. Since they are fairly large files, perhaps they would be better hosted in an external repository (e.g. Zenodo). You could, perhaps, add a section to the pdf supplement with your GIV parameter choices to enable easy reproducibility.
We did not think we could add data yet in the supplementary material so we sticked to the PDF supplementary, we can both add to a repository the glacier velocity maps and the GIV parameter choices.

Overall, this is an interesting paper and the basic data looks robust, but changes and clarifications to some of the follow-up analyses and restructuring of the text is needed before it is ready to publish here.
-Max
Thank you for your very accurate review of our work. We believe that significant improvements can be made to the paper thanks to your suggestions. We hope that our proposed integrations will meet your expectations.