Review of - *Morphodynamics of the Mont Blanc glaciers and their recent evolution*

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

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 analysis, 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.
- 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.
- 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. 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.
- 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).

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.

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

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

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.

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

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.

L29 – Do you mean in the field?

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.

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.

L41 Again not a proxy

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.

L86-91 Not sure this paragraph has much useful information

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.

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.

L123 Just add this to the methods and remove the section

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?

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.

L154 What do you mean an 'active' glacier? Try and use precise language.

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.

L168 Could you show a map, here or in sup mat, of the included/excluded 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?

L186 Did you consider using other thickness estimates, e.g. from Farinotti 2019?

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.

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.

L211 Missing some key info. What did you use to evaluate coherence – was it using the snr output from GIV? Some other measure?

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.

L218 'averaging' through what operation. Taking 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.

L225 Why is the median named 'GlobalAvg'? This is misleading.

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?

L231 The max in a given year or over the entire period?

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?

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.

L249-260 This is methods not results.

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.

L273 Coming back to this again, but why is Miage mostly excluded? Were you excluding debris-covered regions?

L278 You describe elongation ratio as length divided by area, so I guess it has units of metres?

How precise is your length? Worth rounding to the nearest 10m?

Same for the mean ice thickness, do you really have cm precision? Even to the nearest m seems to be pushing it.

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.

L283 What data led you to identify this glacier in particular? Could you show a plot of summer/winter vel ratio for all glaciers?

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.

L296-299 This seems like methods not results.

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

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.

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.

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.

L355 What do you mean 'less statistically significative'? Presumably, it is or is not statistically significant for a given threshold.

L368 What is a 'gentle' glacier?

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).

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).

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

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.

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?

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

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

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

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

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?

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

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

L501 / Table 2 Please label 'Mean difference' instead of just 'Mean', and include the error bars here too.

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.

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.

L532 See comments above- reasons for 5 groups not clear.

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.

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.

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