Five decades of Abramov glacier dynamics reconstructed with multi-sensor optical remote sensing
Enrico Mattea *et al.*

Reply to reviewer 1

The authors compile a 50+ year dataset of kinematic changes of Abramov glacier, filling in gaps in the in situ observational record using a variety of remote sensing datasets. Overall, the manuscript is well-written and demonstrates how more detailed datasets of glacier kinematics can reveal novel dynamic behavior that may complicate mass balance studies. I applaud the authors for the thoroughness of their data processing and presentation of the methodology. I recommend a minor revision of the manuscript with the specific comments listed point by point below.

We would like to thank the reviewer for the positive and constructive review of our manuscript. Below, we provide point-by-point answers to the comments. Any comments which are not mentioned here are considered accepted and fully implemented in the revised manuscript. The review text is reported in *black italic*, while our responses are in blue.

Specific comments:
Abstract:

- *L9: It would be helpful to mention which "archives" are used in this study, especially since your results show a newly-discovered pulsation not resolved from certain products (mentioned L11).*

We agree with the reviewer on this point. However, we think that the full list of archives (with more than a dozen different sources) would be too long for an abstract. In the revised manuscript we mention examples of the most significant archives – Key Hole (KH), SPOT and RapidEye.

- *L15: The results you present throughout the rest of the abstract suggest that the dynamics are quite active. I recommend adding a sentence prior to this with some results related to the transition to stable dynamics.*

In the revised manuscript, we are rephrasing as "However, we also find a decreasing magnitude and increasing duration of the pulsations, suggestive of a potential ongoing transition towards more stable dynamics."

Introduction:

- *L29, L34-35: State specific years instead of "recent years". 2011 is not that recent and "recent" will be even less applicable if this paper is read years from now.*

We agree with the reviewer on this point. However, at L34-35 (the list of recent remote sensing studies over Central Asia), the years of the cited studies are already provided in the citations themselves, therefore we are dropping the mention "In recent years" but we are opting to not repeat the year specification.

- *L88 and 93: State specific years rather than "present-day".*

We are dropping the sentence at L88 entirely, since the study actually includes analyses of all sub-periods. At L93, we are rewording as “the build-up to a third one during the 2010s and early 2020s”.

Methods:

- *L99-100: Change “surface ice velocities” to “ice surface velocities” and state here what techniques are used to derive them. Feature-tracking? InSAR?*

In the revised manuscript we are adding this information (frequency-domain correlation).

- *L103: How many 30 m DEMs in the set?*

In the revised manuscript we are adding this information (11 DEMs, although – as stated in the subsequent line – only 4 were usable).

- *L106-107: How much lower is the error compared to NASADEM? Were the reference measurements made in situ? In what time frame?*

The studies we cite in this section (Fahrland, 2022; Li et al., 2022; Okolie et al., 2024) report a large number of numeric values for the comparison of the Copernicus DEM to NASADEM – including various terrain types and reference data. Here, we just convey that the overall precision and accuracy of the Copernicus DEM are in most cases considered superior to NASADEM; in the subsequent sentences of our manuscript we explain why this is not the case in the Abramov glacier region. Thus, we believe that providing detailed numbers or additional information from the papers comparing these two DEMs goes beyond what is relevant in this section.

- *L207: Please clarify units for the 11 x 11 window (pixels or meters).*

In the revised manuscript we are adding this information (pixels).

- *L223: Please justify the time separations of 5-100 and 300-430 days. Why were 100-300 day separations removed?*

In the revised manuscript we are adding this information (in order to minimize the variability of surface characteristics and solar illumination).

- *L228: What was the typical variance in velocity values across the four bands? It would be convincing to report the value here.*

In the revised manuscript we are adding this information (1.1 m yr^{-1}, computed as standard deviation of velocity within the four velocity rasters of one year, averaged over all cells within the Abramov glacier outline).

- *L292-295: Clarify what you mean by “consistency” of remote sensing data here. From looking through your Appendix D, it seems as if you are assessing both the accuracy (by confirming that velocity ratios are within physically reasonable values) and the temporal* *variation in velocity / volume changes. Should these values be temporally consistent considering the dynamic pulsation just prior to those years (2000-2003)?*

In Appendix D we are checking consistency of our results only via the ratio of depth-averaged to surface velocity – this ratio is computed from the estimated velocity and thickness changes. The verification is that such a ratio falls within physically reasonable values, in particular, as expected, we obtain a high ratio for the late stage of the pulsation (2003 to 2004) and a lower ratio for the two subsequent one-year periods. As such, the occurrence of the pulsation until 2004 offers the possibility to check our results under a wide range of values of the ice flux (see Appendix D). In the revised manuscript we are clarifying this sentence and the first paragraph of Appendix D.

- *L299: A length change uncertainty of 4 m (0.03%) seems unrealistically small. User error uncertainty in manual delineations is typically at least one pixel (Paul et al., 2013). For the all images other than the 0.5 m resolution images, the uncertainty should be the GSD at minimum.*

We agree with the reviewer that in general the uncertainty in manual delineations should be at least one pixel. Indeed, the length change under question (-1106 ± 4 m over the period 1968 to 2023) was computed from a CORONA image at 1.8 m and a Pléiades image at 0.5 m (Table 2): the uncertainty of 4 m is more than double the value of both GSDs. We also note that the glacier length change is computed with the rectilinear box method (Sect. 2.3.1), which performs aggregation of the changes (and thus their uncertainties) over the full width of the glacier terminus: thus, in some cases it could be possible to achieve a smaller uncertainty of length change than the pixel size of either image in the pair.

- *L303: Was the wave of active ice observed through velocity datasets? If so, I recommend moving this down to that section.*

The wave is indeed visible in velocity datasets (Fig. 4a). In the revised manuscript, we are moving this description to the section on velocity results.

- *L306: Oscillation in glacier length of what magnitude? It would be helpful to list a typical range here.*

In the revised manuscript we are adding this information (30 to 50 m).

- *L345-346: You have not yet defined what a "reservoir region" is to your readers. Similarly, you have not defined active versus quiescent phases. Please define these terms to readers in the introduction.*

In the revised manuscript we are defining "reservoir region" in the Introduction. We note that "active phase" and "quiescence" are already introduced, at L44-45. We are expanding their definition.

Discussion:

- *L372: Ice redistribution due to the pulsation is a relationship of note between ice thickness and velocity. Perhaps you mean "no positively-correlated relationship" rather than "no* *direct relationship"*

In the revised manuscript we are updating the text as suggested.

- *L384-385: Would like to see the 2022-23 velocities plotted alongside the older SPOT- and IRS-derived velocities where they overlap on the glacier to better show the attainment of a new velocity peak.*

Unfortunately, the 2022-23 velocity peak (Fig. 4c) is occurring in a region where SPOT and IRS velocities are missing due to sensor saturation and lower snowlines in the early 2000s (L329).

- *L426-427: List the value, error range, and uncertainty in this sentence.*

An exact calculation of the error range and uncertainty is unfortunately not possible in our case, because there is no information on the distribution and accuracy of the measurements of ice thickness used in the calculations by Emelyianov *et al.* (1974, from radio-echo surveys performed in the 1960s) and for the bed DEM used in our study (from radar surveys of 1986). Ice thickness measured by radar, later interpolated into a map and subsequently converted into a DEM from the contours of such a map has several poorly-constrained sources of uncertainty: among them performance of the early radar systems, used wavelength, manual picking of reflectors, horizontal distance from the measured point, re-interpolation between contour lines. Thus, here we simply did a rough estimation of overall uncertainties from a literature-based value of 20 % for the ice thickness uncertainties (L636; Grab *et al*., 2021). We also note that the statement by Emelyianov *et al.* (1974) about the evolution of ice volume during the first pulsation is provided without any uncertainty or absolute values; in particular, the uncertainty in the calculation of total ice volume by the authors is not known but probably quite high, since the interpolation from measured points was likely performed manually. The authors simply report "a doubling of the total ice volume over the first 8 months": L426). By introducing the estimated 20 % uncertainty in all volume calculations, we obtain a range of 70 to 160 % volume increase (over January-August 1973) in the results of Emelyianov *et al*. (1974), and of 40 to 80 % volume increase from the remote sensing data.
In the revised manuscript, we are including a summary of these considerations to explain our reasoning and the uncertainty estimates.

Conclusions:

- *L550: Where will the DEMs and ortho images be made available? A data repository?*

The DEMs and ortho images are already available to reviewers through the review platform. Upon publication, they will be made available via Zenodo.

Figures and Tables:

- *Table 1: Recommend converting all spatial resolutions into meters for ease of comparison*

In this table, we are reporting the original resolution of the datasets as they are provided. The global NASADEM and Copernicus DEMs are provided in equirectangular projection (EPSG:4326) for which the actual resolution can only be expressed in arc-seconds (resolution in meters is not spatially constant), while all other products use projected coordinate systems whose resolution can only be expressed in meters. As an alternative, in the revised manuscript we are adding to the Table caption information about the metric resolution which is commonly used at the mid-latitudes when re-projecting global DEMs to projected coordinate systems (30 m for 1”, 90 m for 3”).

- *Figure 2: The cyclic colormap makes the most recent (e.g., 2023) traces and the oldest traces (e.g., 1968) difficult to distinguish. Please change to a sequential colormap, keeping in mind what color schemes are colorblind friendly.*

We agree with the reviewer that cyclic colormaps can sometimes make visualization difficult. However, in this case the large number of colors would make a sequential color map even harder to read than a cyclic one. Moreover, in the figure, lines with similar colors (the most recent and oldest traces) are also separated in space by more than 1 km, and the annotated legend on the left of the figure also describes the direction of the changes, making it clear which line corresponds to which year. We tested several alternative color schemes for the figure, but could not find a more satisfactory solution for visualizing all the digitized outlines.

Appendices:

- *Appendix D, L636: Are there existing data to compare with to comment on whether these ice influx values are reasonable?*

There are some Soviet-era estimates of ice discharge at Abramov glacier in the years surrounding the 1970s pulsation, but they refer to flux gates located several km upstream of the region where we have remote sensing data for our estimates. Thus, even though the order of magnitude is the same as our results (between 1 and 10 million m^3 per year), we see limited value in reporting such a comparison.

References

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