

Author's response on manuscript 'Atmospheric and cryospheric observations at the high-altitude Zarafshon River Basin and the Hydrographic Party Glacier (GHP), Tajikistan, 2018-2025' by Svensson et al.

First, we would like to thank both reviewers for their efforts in reviewing our manuscript. We are pleased to read that both referees see the value of these diverse data sets, and that it would be a valuable contribution to ESSD. In this comment we first answer the general comments given by referee #1, followed by detailed answers to specific comments. Our response to referee #2 is given subsequently after the referee's comments are given. Our author responses are highlighted with different font (Times New Roman), which is italicized, while new or modified text inserts from the revised manuscript will not be italicized.

Anonymous Referee #1

The authors present an interesting overview of different types of data collected on and around the GHP glacier in Tajikistan in multiple campaigns since 2018. The data also includes continuous records from an automatic weather station near the glacier. These datasets can certainly be of use for a range of further studies and I commend the efforts to compile such a diverse collection of data from an understudied region. I believe this can be a valuable contribution for ESSD with some revisions. Most of my comments are related to manuscript structure.

The manuscript covers multiple datatypes, from snow chemistry to UAV photogrammetry. In the introduction, general comments on data scarcity are presented as motivation for the monitoring activities, with a focus on glacier mass loss. I suggest providing bit more detail here on the motivations for each type of measurements. Why were snow chemistry, aerosols, and surface reflectance assessed? This is partially hinted at in the conclusion but, in my opinion, would be more helpful to the readers in the introduction.

I believe the structure of the manuscript could be further improved by more clearly differentiating between 1) explanations related to the methods and data 2) results and 3) interpretations of the results. The content in Section 2 "Material and methodologies" and Section 3 "Description of datasets and key results" currently overlaps in some subsections. In other cases, the methods subsections seem short on details, which are then partially provided later in the corresponding "results" subsection. The authors make a few comments related to interpretation of the various datasets. I think these provide valuable context but should be separated from the results/data description. An additional "discussion" section would be useful for this. I would also suggest adding a subsection to the discussion where the known limitations of the datasets are summarised (such as comparability between surface reflectance and remote sensing reflectance, positional accuracy of the UAV and in situ data, challenges related to AWS operations, and similar).

The motivation of this work was surely focused on generating datasets relevant for the different processes affecting glacier loss, but also to gain more insights into general glacier characteristics with year-to-year differences. Since glacier mass loss is driven by many factors, such as temperature and precipitation, but also the absorption of shortwave radiation (for which reflectance plays a key role), it is essential to measure a variety of variables. For the absorption of shortwave radiation, it is important to characterize the light-absorbing particles (both in air in the snow surface), along with additional surface reflectance measurements (for verification/comparison). This specifically relates as to why these parameters were measured here, but we can appreciate that one could make arguments that other variables should also be included when characterizing glacier mass loss on a general basis. For our revised manuscript, we have tried to make the motivation for the different measurements clearer in the introduction, by modifying paragraph 2, which now reads:

The net change in glacier mass balance, and its consequent environmental impacts, is governed by processes operating across a wide range of spatial scales, from microscale processes within the snowpack to synoptic-scale precipitation systems and the long-range transport of particles deposited onto the snow surface. Consequently, observations must be conducted over different scales and a multitude of variables. Precipitation is a key variable for determining the mass balance albeit notoriously difficult to measure in cold and mountainous regions where it can be underestimated on the order of 50-100% (e.g., Beck et al., 2020; Pritchard, 2021), and precipitation amounts are poorly quantified throughout CA and the greater HMA region (Chen et al., 2016). In addition, nearly all long-term observations of precipitation are typically from weather stations that are below 3 000 m. In CA precipitation, however, there is a strong spatial variation, with the general pattern of higher precipitation in the west and more arid conditions towards the east (e.g., Aizen et al., 1995). Absorption of solar radiation and ambient temperatures are the key drivers for ablation and atmospheric warming in HMA, with temperature increase that is proceeding at a rate that is twice as fast as the global average (e.g., You et al., 2021). Ablation is also strongly modulated by trends in the albedo (Marks and Dozier, 1992). Across HMA, glacier albedo has declined over recent decades, with important implications for enhanced melt rates, particularly on the Tibetan Plateau and in adjacent regions (Zhang et al., 2021). Untangling the albedo reduction requires a characterization of snow and ice properties on microscopic scale, including the characterization of so-called light-absorbing particles and snow chemistry, while monitoring the combined effect using remote sensing.

Regarding the structure of the manuscript, we can see that differentiating between methods and results more clearly would be beneficial. In the revised manuscript, we will therefore remove the overlapping content from our original sections 2 and 3. We appreciate the referee's comment that the few interpretations on the data provide valuable context and can see the idea of a discussion section. However, since our interpretations are few and quite limited, we do not feel that it warrants its own discussion section in this specific paper. The limitation of the datasets is a welcomed addition and can be suitable text in the summary/conclusions section. Based on these comments, we have renamed section 3 into results and discussion in the revised manuscript to inform readers that we are presenting the results and have some initial discussion of the data. In order to distinguish the results from the discussion in the results sections, we have separated the two entities by placing

them in different paragraphs. This means that first the results will be presented, followed by a new paragraph where the results are briefly discussed (for the sections where this is applicable as some results sections do not contain any discussion).

Please see below for specific line by line comments.

Data: The data are accessible in the stated link. They are arranged by data type and each “data subset” has a readme file that explains the contents. The AWS data is provided in hourly intervals. The manuscript also mentions 10 minute data. A note might be added that the available temporal resolution in the data download is hourly and that 10 minute data was averaged or summed (I assume) to obtain the hourly values. This is currently not explained in the readme. It can be deduced from the manuscript but could be stated more obviously, preferably in the methods section. I am struggling to open the DSM tif files from 2018 and 2019 in Qgis. They do not load or load only partially and produce the following error: “band 1: lreadBlock failed at X offset TIFFReadEncodedTile() failed”. I am unsure if this is a problem with the files or my Qgis installation. The 2023 and 2024 DSMs and all orthomosaics load normally. Visually comparing the mosaics and DSMs from different years suggests they have some spatial offsets and are not perfectly aligned with each other. This is not unusual and of course the authors do not claim perfection, but it might be mentioned in a discussion section how to improve this either at the data acquisition stage or during post-processing.

The readme file for the AWS data has now been updated to reflect that the temporal resolution is hourly, while in the manuscript text we explain that the AWS data is originally 10 min resolution. There was indeed a problem with 2018 and 2019 DSM files. For some unknown reason these files did not work, and so, the files have now been re-created and uploaded again to the data repository.

Regarding the offsets, we thank the reviewer for this helpful observation and agree that small spatial offsets between the orthomosaics and DSMs from different years may occur, and further, that these should be explicitly discussed. In our case, the offsets are likely mainly related to the limited number, and in some cases absence of ground control/check points available for parts of the area surveyed. This limitation was partly due to the logistical complexity of field operations on the glacier, where different teams were responsible for ground control point placement and UAV mapping. In some cases, the marking of ground control points was not synchronized with the UAV surveys, which reduced their usefulness for subsequent georeferencing and independent accuracy assessment.

We have added a note to the discussion explaining that future surveys would benefit from improved coordination of field activities, including ensuring that well-distributed ground control points or check points are marked and surveyed before UAV data acquisition. Additional improvements could include using more clearly visible and stable targets, increasing the number and spatial distribution of ground control points, and applying consistent post-processing co-registration procedures between annual datasets. These steps would help reduce interannual spatial offsets and improve the comparability of DSMs and orthomosaics.

Introduction L31 *periods of draught* drought (spelling)

Fixed.

L34 *both spatial and temporally* —> spatially

Fixed.

L38 *changes on cryospheric variables on* —> of

Fixed.

L63 *Being a small temperate cirque-valley glacier with typically winter accumulation, GGP has a northward facing orientation and is situated at the elevation range...* Consider simplifying the phrasing. (e.g., “GGP is a small, north-facing, temperature cirque-valley glacier with mainly winter accumulation. It spans an elevation range of ...:”

Fixed. The suggested phrasing is better and has now been adopted in the revised manuscript.

Fig. 1: Panel b: In the legend, why is GGP glacier shown as a black square? I assume the blue outline on the google earth image indicates the outline of the glacier. I suggest changing the legend to match this, and indicating the year of the outline in the legend. Try to adjust the yellow labels so they do not overlap the white lines. Not sure I understand the meaning of the years in the AWS labels, particularly for the lower AWS (“AWS 2019 ->” ..?). I find the comment in the image (panel b, top right corner) a bit odd – I think this would work better in the caption. The caption can be adjusted to avoid explaining the abbreviation “AWS” multiple times. Consider adding a panel label for the photo in the lower left. For the Google image, the copyright may need to be included in the image (unsure), check journal guidelines.

These are good comments regarding the figure, and we have adopted the suggested changes in the revised figure. The lower AWS label (AWS 2019->) refers to the second location that the AWS was moved to. The outline of GGP is from 2024, and a panel label has been added to the photo. Our understanding is that copyright does not need to be included in the image. The legend has been revised along with the figure caption to reflect these changes.

The updated figure and caption now reads:

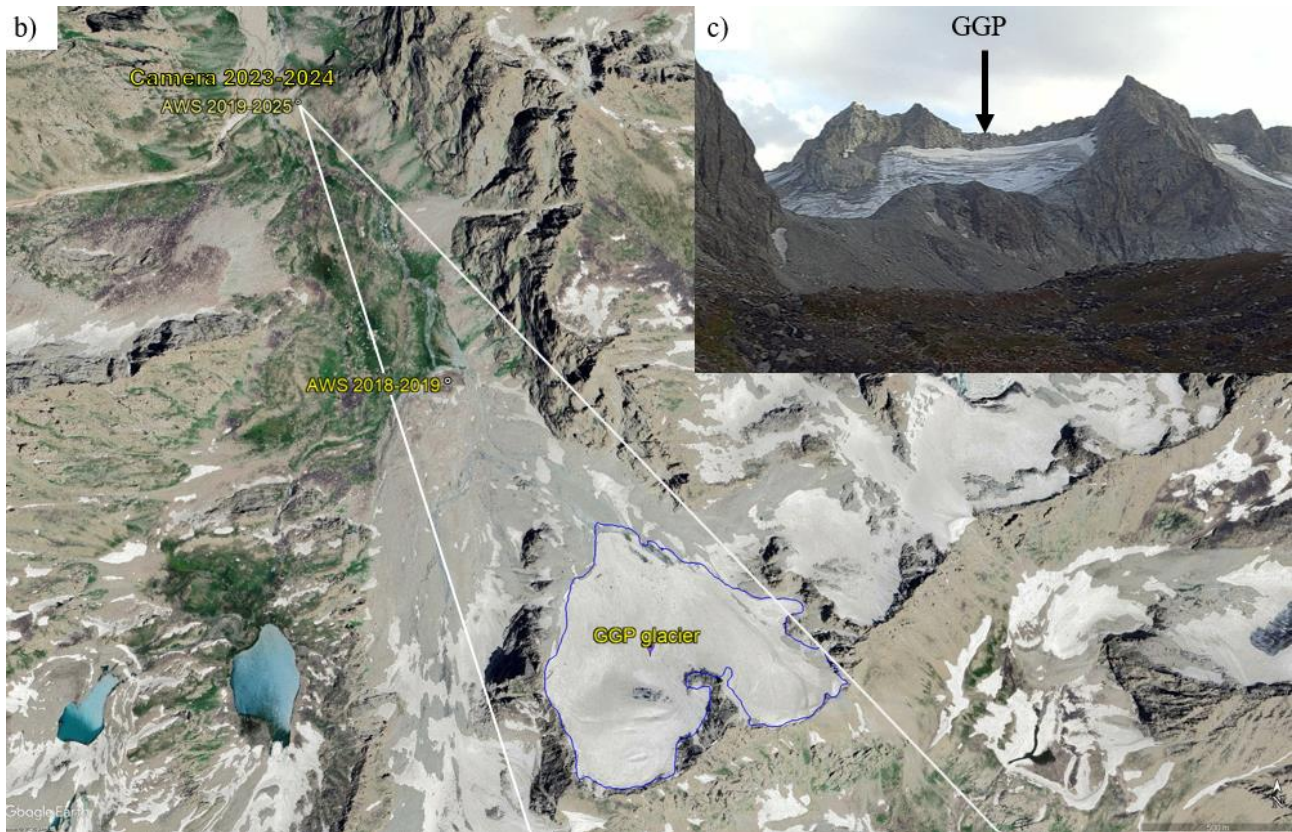


Figure 1. b) Satellite imagery of the study area. The outline of GGP is from 2024. AWS stands for automatic weather station. Field of view of the camera installed in 2023 is indicated with white lines. c) Inserted image of GGP (top right) as seen from the weather station camera 2023-09-04. Image source Google Maps (© Google 2025).

Section 2.1. L86 *the systematic Sentinel-2 satellite constellation* What do you mean by systematic here? I suggest citing standard references for the Landsat and Sentinel datasets, such as: Crawford et al, 2023, The 50-year Landsat collection 2 archive <https://doi.org/10.1016/j.srs.2023.100103> and Drusch et al, 2012, Sentinel-2: Esa’s optical high-resolution mission for GMES operational services <https://doi.org/10.1016/j.rse.2011.11.026> Where did you obtain the images? Did you use RGB composites of bottom of atmosphere reflectance? If so, state that. Otherwise please explain what exactly you used.

While reviewing the text it became evident that the use of the word ‘systematic’ is not necessary here, as it does not refer to anything specific for this section that is essential for the reader. We have therefore removed systematic from the sentence in the revised manuscript. The suggested references are a welcomed addition and have been added to the revised manuscript. The details regarding the images (where obtained; RGB composites of bottom of atmosphere reflectance) have also been added to the revised manuscript section 2.1.

The revised section 2.1 now reads:

2.1 Mapping of GGP terminus

Glacier images were manually selected using Landsat-5, Landsat-7, Landsat-8, and Sentinel-2 satellites (Table 1). Landsat-5 carried the Thematic Mapper, collecting data across seven bands. Landsat-7 used the Enhanced Thematic Mapper Plus, and Landsat-8 further advanced the series with two instruments, using the Operational Land Imager and the Thermal Infrared Sensor (Crawford et al., 2023). All Landsat products used in this study have spatial resolutions of 30 meters. Sentinel-2 satellite carries the Multi-Spectral Instrument, acquiring images within 13 different spectral bands at spatial resolutions ranging from 10 m to 60 m (Dursch et al., 2012). All images were processed and exported from the Google Earth Engine (GEE) platform, which is an open-source and extensive up-to-date archive of images. All the selected images are from September, ranging from 1989 to 2024. The month of September was chosen since it is typically the end of the melting season, and it corresponded to when all of the fieldwork campaigns were conducted. The 13 cloud free images here are posted as RGB composites (red, green, blue – Fig. S1).

In addition, an outline of GGP from 1973 was completed to extend the time series. This was achieved by manually digitizing an archived map constructed by the Tajikhydromet (Vladimirova et al., 1976; see data availability). The map contains one ground control point with known coordinates, as well as other known features in the landscape for reference.

The ice front variation of GGP for the 1973 image and the time interval 1989-2024 (Table 1) was determined based on the methodology in Moon and Joughin (2008). First, an opened box was delineated, covering the glacier and taking the base of the opened box as a reference line (Fig. S2). Then, each ice front is manually digitized from the satellite images. The mean retreat or advance of the glacier is determined by the area change divided by the width of the box. This methodology is more suitable for accounting for uneven changes at the ice front, rather than using only a single reference line (Moon and Joughin, 2008).

Fig S1: What is the source of the background image and the acquisition date? The lines are hard to see, consider different colours or thicker lines to improve visibility. The 2023 and 2024 GPS markers are hard to distinguish, consider different colours. I believe this figure could be improved in terms of color blind friendliness.

The image source is Bing Maps (© Microsoft) and imagery data © 2025 Maxar. This figure has been updated with different and thicker lines for visibility, along with different markers and then the image source in the figure caption.

The updated figure with lines and markers appears as:

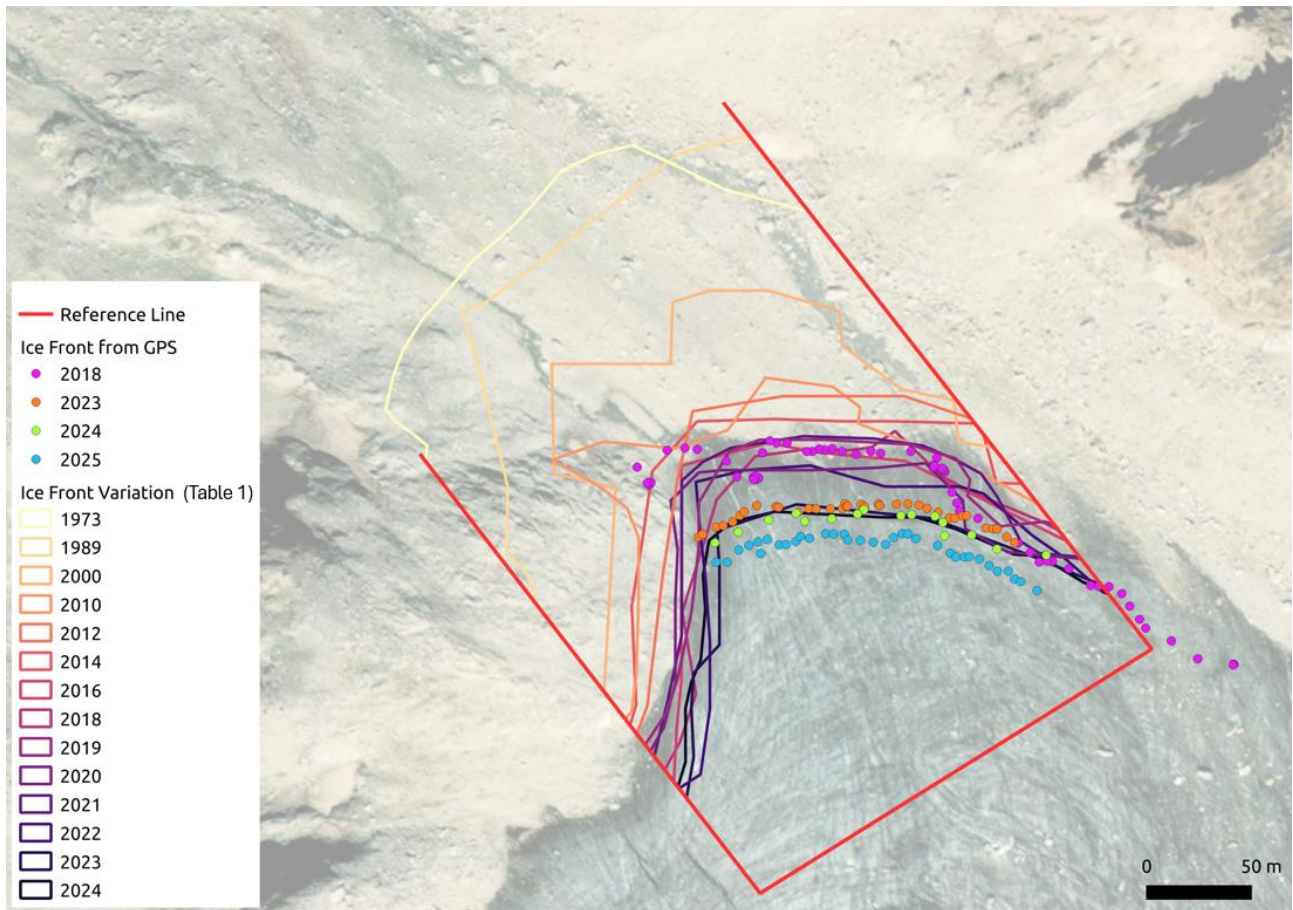


Figure S2. Front variation of GGP terminus from 1973 (Vladimirova et al., 1976) to 2024, and the GPS surveys conducted during expeditions. Image source Bing Maps (© Microsoft) and imagery data © 2025 Maxar.

L94 *This methodology better assesses the ice front variation* Better than what? Are you comparing it to something specific?

Since the ice front is not a straight line, but rather, variates spatially we adopted this methodology. As the authors of the in-text reference suggest, this methodology accounts for these front irregularities. Thus, we are not comparing it with something specific but are instead using a method that takes this into account.

In text we have revised the sentence to:

This methodology is more suitable for accounting for uneven changes at the ice front, rather than using only a single reference line (Moon and Joughin, 2008).

General remark: For what time of year was the terminus position delineated? How were the satellite images selected? (manual selection of snow and cloud free satellite scenes, averaging over multiple images, something else?) I think this sections warrants a bit more detail on the mapping process including image selection.

The terminus was delineated in the month of September each year for consistency, which corresponds to the timing of the field campaigns. This detail and the other remarks are answered in the comment above (regarding section 2.1). In the revised manuscript this information is now described in section 2.1.

Section 2.2 L97 semi-professional grade quadcopters Semi-professional is vague, I suggest just stating the make and model.

The model and make is now stated in the updated table (containing UAV information) in the revised section 2.2, while the wording 'semi-professional' has been removed in the revised text section of 2.2. See comment below (regarding 3.2) where updated table and text of 2.2 is shown.

L98 ...were used... Should be "was used" in the current sentence structure

Fixed.

L102 *the drone was able to fly for about 20 min at about 4 km altitude* Clarify altitude above sea level here (assuming that is what you mean, rather than flight level above ground)

Yes, we are referring to altitude above sea level. Fixed in the revised section 2.2.

L104 *was used utilizing* Consider going through the manuscript to simplify phrasing (this could be something like "was used with")

Fixed.

L109 *such as large boulders or part of rock spread around the glacier* PartS of rock? Are parts of rock different from large boulders?

This was poor wording on our part. What we meant was large boulders and parts of bedrock outcrops around the glacier.

The revised sentence now reads:

To mitigate this incompleteness, Google Earth was used to create GCPs artificially by using significant natural check points such as large boulders or parts of bedrock outcrops that are spread around the glacier.

L110 *During 2023 and 2024 the drilled ablation stakes positions were marked with red crosses of about 2 m arm length, but not all of them were found on acquired images.* How was this marking done in the field? Did you attach additional red poles/some other material to the ablation stakes? Please

explain. Consider mentioning in a discussion section why this did not work as anticipated (crosses too small to see, otherwise disappeared?)

We marked red crosses on the ice surface right next to the ablation stakes with red spray paint. Unfortunately, we could not see any of them with the UAV imagery because the crosses were either too small or not distinguishable enough. We were surprised and unsure this did not work, and so it is difficult to pinpoint why it did not work. We have, nonetheless, stated that markings were not visible and that caused limitations in the UAV work.

L113 and to generate the digital surface models (DSM) and orthomosaic overlays of GeoTIFF and Google Maps tiles How/why is the DSM and orthomosaic generation related to google Maps tiles? Unclear. Please state the spatial resolution of the orthomosaics and DSMs somewhere.

This was unclear wording from our part. The reference to Google Maps tiles was not intended to imply that DSM or orthomosaic generation depends on Google Maps. The DSMs and orthomosaics were generated as georeferenced raster products in GeoTIFF format, while the Google Maps tile format was produced only as an additional visualization format to facilitate easy browsing and use by potentially interested readers.

We have revised the sentence to make this clearer and have added the spatial resolution of the photogrammetric products. Based on the Pix4D quality reports, the average ground sampling distances were 4.25 cm in 2018, 3.87 cm in 2019, 3.69 cm in 2023, and 4.02 cm in 2024. These values are stated in the text of the revised manuscript (as well as in Table 2).

The revised manuscript text now reads:

The UAV imagery was processed to generate georeferenced orthomosaics and digital surface models (DSMs) in GeoTIFF format. In addition, Google Maps tile layers were produced only as a user-friendly visualization format to facilitate online browsing of the datasets. The average ground sampling distances of the photogrammetric products were 4.25 cm in 2018, 3.87 cm in 2019, 3.69 cm in 2023, and 4.02 cm in 2024.

Section 2.3 L122 *The approximate accuracy of the measurement is within 5 to 10 m under normal conditions* Consider specifying positional accuracy, assuming that is what you refer to here. How did you determine the accuracy? (e.g., information provided by GPS manufacturer?) The sentence is missing a period.

We are referring to positional accuracy here, and the accuracy is based on the information given by the GPS manufacturer. In the revised manuscript, this information has been added, as well as a period at the end of the sentence.

The revised sentence now reads:

The approximate positional accuracy of the GPS measurement is within 5 to 10 m under normal conditions, as provided by the GPS manufacturer.

Section 2.5 L128: *Glacier ablation was measured with the conventional glaciological method (e.g., Cogley et al., 2011), with stakes being drilled into the ice throughout the glacier ablation zone, which are traditionally transformed into ice ablation by multiplying the height change by an assumed ice density of... Consider simplifying the sentence by breaking it into two parts. Start a new sentence after “ablation zone” (e.g., “Measurements of height change are transformed into ice ablation by...”).*

Fixed. This sentence is now broken down into two parts based on the suggested simplification.

L130: *Stakes with a height of 4 m* Were the stakes inserted fully into the holes or did you leave them partially sticking out? (as is sometimes done if accumulation may occur) Refer to Fig 3 in this section to indicate the location of the stakes. In Fig 3 it seems like the stake network was expanded in 2025. Any particular reason for this? From the figure it is not clear to me if the markers represent the position of all stakes as measured in a given year, or if they represent the position of the stakes drilled in that year. As in, do the blue markers indicate the position of the stakes drilled in prior years AND new ones drilled in 2025, or only the latter?

Stakes were typically inserted fully into the drilled holes as we were not expecting accumulation to occur. If they were left sticking out, this height was noted in the ablation stakes table. The stake network was not expanded in 2025, but rather, had another visit by a smaller team which then had the stakes measured. These details are indicated in the ablation stakes table. In figure 3 the color represents the year the stake was visited, but then also the stakes that were drilled that year.

Section 2.6 L136 *this location had to be shifted* Why did it have to be shifted? L143 *All parameters are measured with a 10 s time resolution* Do you really have 10 second resolution? Should this be 10 minutes? Fig. 2: Add date and name of photographer if possible.

As explained later in the text (section 3.6), the shift in location was due to exceptional snow built-up at the first location. Based on the general comment given above on the structure of the manuscript (the differentiating between methods, results, and the interpretations of these results), we have added the explanation for the AWS location shift in the newly revised section 2.6. The 10 second resolution comes from the instrument sensors, which is then averaged 10 mins by the AWS data logger. This data, which was downloaded was then further averaged (or summed depending on the measurement parameter) to hourly data for this manuscript. The date and name of the photographer have been added to the revised figure caption text.

Section 2.6 now reads:

2.6 Meteorological measurements

An AWS (Vaisala MAWS 201) was installed in 2018 adjacent to the glacier front (38.95541N, 68.27000E, 3390 m asl) for long-term continuous observations of the meteorological parameters. Due to high snow accumulation during the first winter at this location, with both the solar panels

and some of the sensors being covered with snow, resulting in no power available for the AWS, the position was changed in September of 2019. The second location is about 2 km from the glacier terminus (38.96491N, 68.26330E, 3275 m asl), and the reported meteorological data is therefore from two different locations (see Fig. 1b). The AWS measures temperature (T), relative humidity (RH), precipitation (P), pressure (p), wind speed (WS), wind direction (WD), solar global irradiance (GI), and snow depth (SD). The AWS operates with a 37 W solar panel, three 7 Ah, 12 V batteries. The measured parameters together with sensors used are described in Table 1, while the AWS is visualized in Fig. 2. The precipitation gauge is not heated; thus, the precipitation record is limited to recording liquid. It may, therefore, possibly include melt from accumulated snow also. The AWS data logger reports data in 10 min averages that have been manually downloaded during field expeditions. The data reported here are hourly averaged values (unless otherwise indicated). The data points flagged as invalid by the sensors themselves were omitted from the data set. The AWS data contain two main gaps. The first consists of the period when the AWS was buried in snow at the first location (losing data at 2018.12.31 but then resumed automatically on 2019.06.19). The second major data gap took place between 2021.09.13 (starting only a few days after completion of the 2021 expedition) and the 2023 expedition, when the main power switch to the AWS was found to be turned to the OFF-position due to an unknown reason. Measurements were resumed on 2023.9.19. An additional data gap is present in the wind sensor data (WS and WD) during periods 2019.9.13–2019.11.06 and 2020.10.08–2021.09.31 due to bad data quality (data gaps and noisy measurements). The SD data during the period 2019.06.26–2019.09.03 were omitted due to an unknown technical issue that caused unrealistic values. The reported P data does not consider any wind undercatch corrections and is the cumulative precipitation for each hour, while the reported SD and maximum WS are the maximum values for each hour. The WD data is the circular mean.

Sec 2.7 L 153 *“transported back to Helsinki in a melted state but light-protected state to the laboratory in Finland ..”* Consider rephrasing for readability, remove extra period at the end of the sentence.

Fixed.

The wording now reads:

The snow samples were collected in 20 mL glass vials and were packed in a light-protected state for transport to Helsinki. During transport it was not possible to have the samples remain frozen.

L166 (*Waters*) Is this a reference with missing year? Unclear

This does not refer to any reference, but instead, the manufacturer of the chromatograph. We have added the trademark symbol TM to indicate this in the revised manuscript.

Sec 2.8 L168 *The prevailing surface reflectance at the glacier, in connection with snow sampling, was determined with a handheld spectroradiometer,* What do you mean by prevailing? What does “in

connection with snow sampling” mean? Snow sampling was carried out in 2018 and 2019 as per sec 2.7. Sepctroradiometric measurements were carried our in 2019 and 2023...?

In 2019, we took synchronized snow sampling and snow reflectance measurements, and by that we mean that we first took surface reflectance measurements and then sampled the snow from that specific site. It is correct that in 2023 we did not take snow samples, while in 2018 we did not measure any surface reflectance, and so for these years we do not have both measurements. By prevailing we are referring to the dominating surface conditions that we observed at the glacier during the field campaign.

L173 *The measurements were conducted by first taking a white reference measurement (Spectralon Labsphere Inc.), followed by measurements of the glacier surface, and finally followed by another white reference measurement. This allowed minimizing the effects of instrument response to changes of the field brightening conditions. Please clarify if you took multiple measurements at one location (then presumably averaging over multiple spectra for one point) or were moving between different measurement points at similar times of the day —> What time of day? Does “minimizing the effects of instrument response” imply you did extra processing to account for this? If so, what were the processing steps? Refer to Fig 3 in this section to indicate measurement locations.*

Our operating procedure was to first take a white reference measurement, then taking three spectras for the targeted surface, followed by another immediate white reference measurement. The white references before and after acted as quality control to see if there was any drift in the instrument response during those brief moments when the measurements were conducted. The three target spectras are then averaged. The measurements were usually conducted around the local noon when the light conditions are more optimal (the exact times are indicated in the results file). With this, we are not implying any ‘extra’ processing was done to our data.

In the revised manuscript, we refer to Fig.3 with the measurement locations and have added the sentence:

For the targeted glacier surfaces a minimum of three spectras were sampled, and these are averaged.

Sec 2.9 Where was the atmospheric aerosol sampling carried out? Multiple measurement points or only one, why those locations? Can this be added to Fig. 3? Fig 3: State the acquisition date of the image (at least the year). Check color-blind suitability of marker colours.

These measurements were measured at multiple locations, with a majority of them at the designated base camp, but also some while at the glacier (for the more mobile absorption measurements). For these measurements we believe that the fine scale resolution is not needed as the aerosol should be well-mixed throughout the basin, and so reporting by the glacier is sufficient. In the results file, however, the coordinates of each measurement are included and so the fine resolution data is available if it is desired. The year of the imagery data (2025) is stated in the figure caption.

Sec 3.1 *“Landsat-5 carried the Thematic Mapper I, collecting data across seven bands, including visible, near-infrared, mid-infrared, and thermal infrared spectrum. Landsat-7 used the Enhanced Thematic Mapper Plus (ETM+), including a high-resolution panchromatic band....”* The description of all the satellite sensors would be better suited for the methods section and could be shortened. As far as I understand, you visually delineated the terminus in true color composites. Is the list of spectral bands given here relevant for your approach and if so, how? Table 3: As above, how were those images selected? You might consider making a figure similar to Fig S3 showing the images listed in the table.

The description text is indeed better suited for the methods section and could be shortened. The spectral bands were not relevant for our study, and thus, has been removed in the revised manuscript. As mentioned above in response to the other remote sensing comment, the images were manually selected, cloud free, around the time of year when field campaigns were organized. This information is now supplied in the revised manuscript text. We have also expanded Fig. S3 (which in the revised manuscript has changed to Fig. S1) to include all satellite images for this study (i.e. now includes also 1989-2016 images).

For changes in the revised manuscript text, please see comment above (Section 2.1), where the new methods text is included.

Fig S2: How was the 1973 outline “reconstructed”? Digitised from a map? Please explain, I suggest adding this to the methods subsection explaining the terminus mapping.

This outline was indeed manually digitized from an archived map based on work by the Tajikhydromet (in Vladimirova et al., 1976). The map contains one ground control point with known coordinates, as well as other known features in the landscape for reference. These details have now been added to the revised manuscript, in section 2.1 (see above), as suggested. The naming of section 2.1 has also been revised to ‘Mapping of the GGP terminus’, which is more suitable for this section since it now contains this 1973 data point (instead of the previous section title ‘Remote sensing of GGP terminus’).

L 212 on the order of 200 m over for this 50-year period “over for” → phrasing (choose either over or for)

Fixed, ‘over’ has been chosen in the revised manuscript.

L213 areal decrease area decrease

Fixed.

L214 *The yearly appearance of GGP in the month of September during the 2018 to 2024 expeditions years displays the annual variation in snow conditions* It would be good to comment here on whether

these images are considered representative of minimum summer snow cover extent. Is the snow cover in 2018-2020 mainly left over from the previous winter, or were there smaller summer snow falls that may affect the surface conditions of the glacier during image acquisition?

Based on our experience, we consider this period to be representative of minimum snow cover and the snow cover in 2018-2020 is mainly left over for the previous winter. During our work, we have typically not observed any summer snow falls.

This information has now been added to section 2.1 as follows:

All the selected images are from September, ranging from 1989 to 2024. The month of September was chosen since it is typically the end of the melting season, and it corresponded to when all of the fieldwork campaigns were conducted.

Similarly, for section 3.1, this information has been added as follows:

The yearly appearance of GGP in the month of September, which can generally be considered the month (or sometimes August, see sec. 3.4 below) with minimum snow cover in the area, is highlighted in Fig. S1.

L215 *From this imagery it is especially visible how the snow conditions have diminished at an exceptional rate during the last years* I can see what you mean but this is rather qualitative and you have not previously introduced snow cover change as a focus of the monitoring. Consider moving this to a more general discussion section (or expanding this part to a quantitative approach).

This statement is certainly qualitative, and we have no intention of expanding this to anything quantitative since it is beyond the scope of this work. We believe that it is rather interesting from the imagery, however, that we can observe such a distinct difference in snow cover annually and that this has been the case for the indicated years. In the revised manuscript, we have changed the wording of this statement, removing 'diminished at an exceptional rate', with replacing it with 'from this satellite imagery, it can be observed that the snow cover at GGP has been less extensive in recent years'.

Furthermore, we have added this sentence to the conclusion/summary section of the revised manuscript:

The overall results of the remote sensing displays the retreat in glacier front, as well as how the snow have diminished during the last years (2021–2024).

Sec 3.2

L227 *MTP* Abbreviation not explained Much of the information in this section could be placed in a table. This might make it easier for the readers to compare different years and keep an overview. Table 4: error values in 2023 appear considerable larger than in the other years. Can you comment on reasons for this?

MTP stands for moving target problem and is now explained. It is true that that much of the information could be placed in a table. The existing UAV table 4 was expanded in the revised manuscript to include this information.

For the larger error values reported for 2023 it is most likely related to the limited ground-control configuration available for that survey. According to the Pix4D quality report, the 2023 dataset was georeferenced using only two 3D ground control points and only one independent check point was available for validation. Although the residuals at the two ground control points were small, the single check point showed substantially larger deviations, particularly in the vertical component. Therefore, the 2023 error estimates should be interpreted with caution, as they are based on a very limited number of independent validation points and are sensitive to the position and quality of that single check point. We have added a comment to the manuscript explaining that the comparatively large 2023 errors are mainly attributed to the limited number and spatial distribution of ground control points/check points available for that year. Future surveys should include a larger number of well-distributed GCPs and independent check points to better constrain the photogrammetric model and provide a more robust accuracy assessment.

The added text in the revised manuscript now reads:

The comparatively larger error values in 2023 are likely related to the limited ground-control configuration for that survey. Only two 3D GCPs and one independent check point were available, making the accuracy assessment sensitive to the residuals of a single validation point. Therefore, the 2023 errors should be interpreted with caution and mainly reflect the limited number and distribution of GCPs/check points rather than the overall image matching quality.

And the expanded Table 4 now reads:

Table 4. Summary of the project assembly from the Pix4D software, as well as localization accuracy per ground check points (GCPs), control points (CPs), moving target problem (MTP), ground sampling distance (GSD), and mean errors in the three coordinate directions and average projection error in pixels.

	2018	2019	2023	2024
Drone type	DJI Phantom 4 Pro V2	DJI Phantom 4 Pro drone	DJI Phantom 4 Pro drone	DJI Phantom 4 Pro drone
Numbers of images (disabled)	1478 (147)	615 (150)	515 (13)	407 (0)
Number of GCPs	7	5	2	5
Number of CPs	4	3	1	2
MTP	-	11	3	-
GSD [cm]	4.25	3.87	3.69	4.02
Area [km ²]	0.925	0.767	0.286	0.334

Error X [m]	1.52	1.71	-0.07	0.73
Error Y [m]	2.63	0.99	-6.15	1.42
Error Z [m]	5.86	4.52	-22.91	0.81
Average projection error [pixel]	2.42	0.63	0.85	0.52

L239 The DSM and orthomosaics (Fig. 5) indicate on high-resolution the glacier surface characteristics for each respective expedition years, and the dynamics occurring on year-to-year scale. What do you mean by dynamics here? Are you referring to glacier movement (dynamics) or something else? If the intention is to highlight a specific aspect of the imagery, please explain clearly what it is and where we can see it in the figure. Fig. 5: acquisition date of the background image? Maybe add a border around the UAV images to better distinguish them from the background. There is red-ish colouring in some of the orthomosaics - did you observe algae that cause this or is it due to something else? Is it possible to compute elevation change maps from your DSMs?

The intention with the dynamics wording was to indicate for readers that the DSM and orthomosaics can be used to study processes at a higher resolution scale than the previous section discussed in the text (remote sensing). With this statement we are referring to glacier movement indirectly, but then are not intending to do anything else with it. In the revised manuscript we have modified the text by removing 'dynamics' to 'changes'. The redish color was observed in situ, and that snow was collected for the snow chemistry characterization, along with surface reflectance measurement. Our strong belief is that this is glacier snow algae is indeed. It is not possible to compute elevation change maps because of overlap issues. Or rather, it is possible, but it would be useless to do since elevation is not accurate enough.

Sec 3.3 Fig. 6: This is an okay visualisation but since you have the UAV imagery you could consider making a multi-panel figure with the orthomosaics of all years to visually show the change and how the GPS points align with the imagery.

Fixed. For the years where we have GPS data, we have included it on the previous orthomosaics figures.

The updated orthomosaics now appears as:

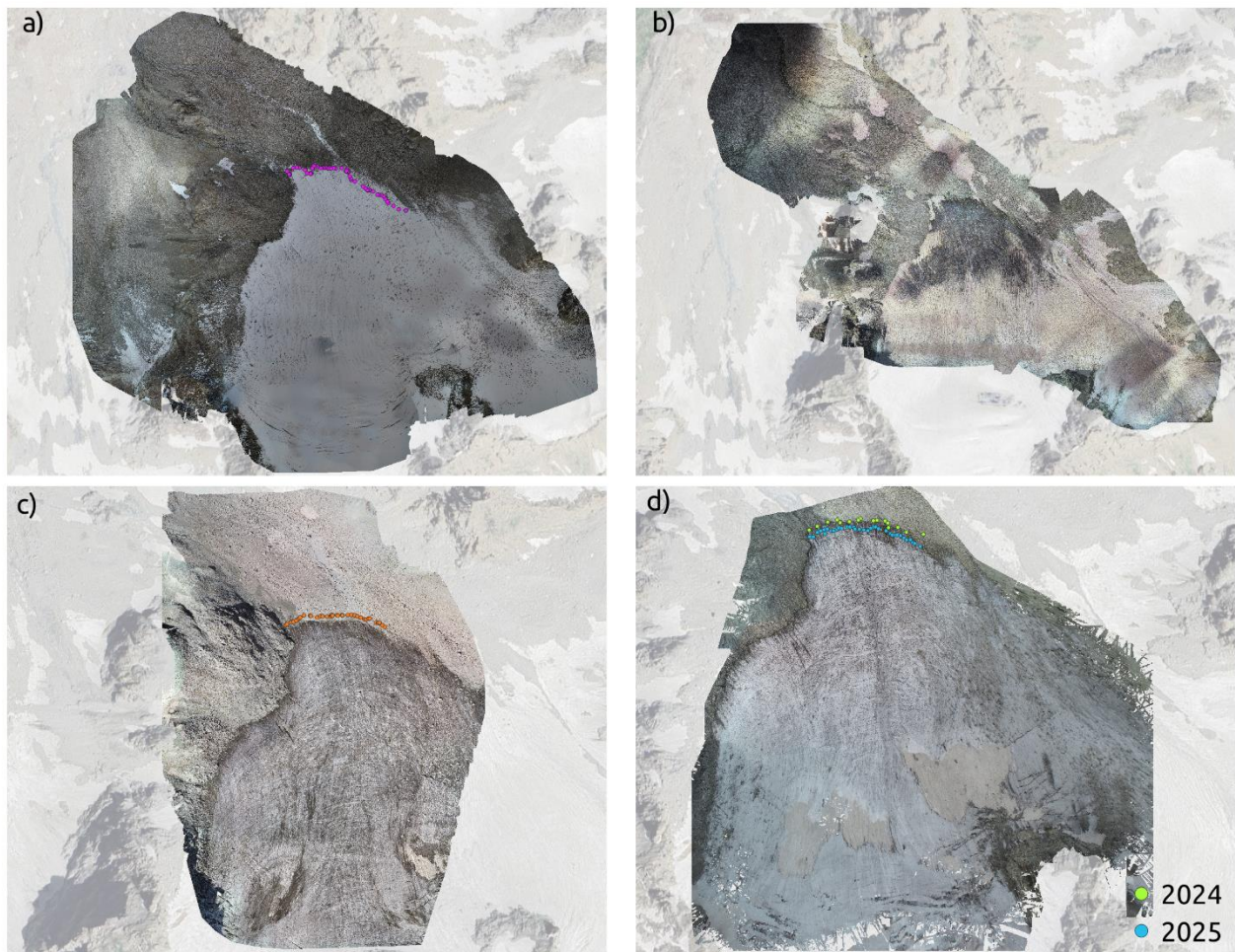


Figure 5. Orthomosaic overlays of GGP generated by Pix4D software for each year: a) 2018, b) 2019, c) 2023 and d) 2024. Ice front locations collected with GPS-surveys from different expedition years. Background image available through OpenStreetMaps.

Sec 3.4 L257 *At the same time, from the images it is visible that more snow had ablated in 2023*
 Phrasing - what does “at the same time” mean here? Maybe something like “Furthermore” would be better, or omit this entirely and start the sentence with “From the images”.

Fixed, we now start the sentence with ‘From the images’.

Sec 3.5 266 *was laterally from -4 m to 11.5 m* Laterally as in sideways? Also applies to “lateral flow” a few lines down - to my understanding lateral implies a sideways movement. I assume you mean movement of stakes with the direction of the ice flow? Consider rephrasing. The usage of the stakes to track movement was not introduced in the methods section. Rather, the methods indicate that the stakes were used to measure ablation, which is not mentioned here. I think this should be harmonised to present a consistent overview of measurements and results (i.e., add mention of stake movement tracking to the methods and add some information on ablation results here).

The primary objective with the stakes is indeed to measure ablation, but since the positioning of the stakes were also made with a GPS, it is correct that movement can be tracked and this was not indicated in our manuscript. In our observations, we observed an unexpected movement of the stakes 'sideways'.

In the revised manuscripts methods section (2.5) we have added: 'The position parameters of the stakes were recorded with a GPS (same as terminus mapping) to observe any possible movement of the stakes'.

3.6

L270 *10 min average* Above it says 10 sec - see earlier comment

As indicated, this corresponds to the comment above where this has been addressed.

L271 double period

Fixed.

L280 and following: Some of this should be in methods/is repeated from methods (like station position, reason for changing the position, ...) I recommend restructuring to clearly separate the content presented in the methods and results sections. Wind direction: I assume this is a circular mean? Please confirm.

This has been addressed in the comment above. We can confirm that it is a circular mean for the wind direction.

Sec 3.7 Check repetitions from methods and clearly separate methods and results/dataset description.

This has been addressed in the revised manuscript.

L345 *and this layer can possibly be interpreted as the previous year's surface layer with the higher concentration being due to melt amplification (Doherty et al., 2013)* This is a bit speculative and may be better suited for a discussion section.

It is true that this is a speculation. Based on the comment above, which concerns the restructuring of the manuscript, and the fact that we do not see the argument for a separate discussion section, we have not moved this text to a different section. We have, however, separated this speculation from the results in the revised section 3.7 with a new paragraph, as well as replaced 'interpreted' with indicate.

In the revised manuscript, section 3.7 now reads:

The highest snow impurity content was measured in the surface of pit 1 in 2019, with elevated carbonaceous particles and trace elements, which were dominated by aluminum and iron at 18.1 $\mu\text{g ml}^{-1}$ and 12.0 $\mu\text{g ml}^{-1}$, respectively.

A secondary elevated impurity layer was observed at the bottom of the pit, and this layer can indicate the previous year's surface layer with the higher concentration being due to melt amplification, commonly observed in glacier snow pits (Doherty et al., 2013). Between these layers, the impurity concentrations remained relatively low, about 10% of those in the surface layer. A similar pattern for snow impurities was observed in pit 2.

3.8 As above, I recommend restructuring to clearly separate the methodological content from results/ data description and interpretation/discussion. You link the reflectance data to the chemical composition (L360) but this remains vague and we can't see it from the figures ("*The black and red impurity spots in the midsection had even lower reflectance (Fig. 10b), which had coinciding chemical compositions, with elevated ammonium amounts*") Fig 10: please add somewhere how many measurements were used for the averages. I suggest adding grid lines to the plots. In panel b, were the dashed lines selected to show particular impurities? Are these single measurements or also averaged over multiple spectra? Are these dashed lines included in the average and std (solid lines)? (Fig 10, panel b: Might the pronounced dip in the curves in this panel be related to the chlorophyll absorption band at ~680nm?)

Yes, the dashed lines in panel b were selected to show that the black and red impurities had much lower reflectance than the brownish impurities. Thus, they are not included in the average reflectance. As explained above in the comment related to the procedure of these measurements, all of the targets had three spectras taken, which are then averaged for the results. This is now explained in the methods section (2.8). Yes, the dip in the curves is very likely related to chlorophyll absorption, which is now stated in the revised manuscript.

3.9 See above comments on clearly separating methods, results, and interpretations. In my opinion, information on timing and duration of the measurements would be better suited to the methods section, whereas comments on the origin of particles would be suitable for a discussion section, where the described data are briefly interpreted.

We agree that the information on timing and duration is better suited for the methods section. This has been addressed in the revised manuscript by incorporating the information from the first two paragraphs in the old 3.9 section into the new revised section 2.9.

Regarding the comment on the origin of particles, we again want to emphasize, based on the comment above on the restructuring of the manuscript, that we do not see the argument for a separate discussion section for this. Since this is very basic information on the aerosol particles, and not necessarily any speculation, we believe it can remain in this result section. As with the other results sections, we have, however, separated this information on the origin of the particles into an

independent paragraph from the measured results to mark that this information is different than the observed aerosol properties.

In the revised manuscript section 3.9 now reads:

Black carbon concentrations ranged from 200 ng m⁻³ (25th percentile) up to 930 ng m⁻³ (75th percentile) during individual expeditions with average campaign concentrations varying between 400 ng m⁻³ and 550 ng m⁻³ (Fig. 11). The observed particle number concentration was 1850 ± 810 cm⁻³, while the particulate mass concentrations for PM₁, PM_{2.5} and PM₁₀ were 5.6 ± 0.3 µg m⁻³, 7.5 ± 1.1 µg m⁻³ and 14.6 ± 6.4 µg m⁻³, respectively.

The black carbon concentrations were relatively high for a remote location, similar to those encountered in European background locations (e.g, Savadkoohi et al., 2023). The average absorption Ångström exponent derived from the wavelength dependence of the absorption coefficient was 1.45 during the 2023 campaign, indicating based on the “Aethalometer model” (Sandradevi et al., 2008) that 58% of the absorption was caused by fossil fuel burning sources, and 42% by biomass burning sources. Although the particle number- and particulate mass concentrations were measured only for a brief period in 2018, and the values may not be representative of the prevailing atmospheric conditions, they are reported here as these are the very first data of its kind from the region. The observed particle number concentration is similar to levels measured in European rural areas (Rose et al., 2021). The PM_{2.5} concentration was 50% of the WHO’s 24-hour guideline value of 15 µg m⁻³ and 32% of the 24-hour PM₁₀ guideline value of 45 µg m⁻³.

Anonymous Referee #2

The manuscripts present a comprehensive dataset for at GGP glacier in Tajikistan, spanning several disciplines from classical glaciology to snow chemistry. While data collection spans a relatively short period, it is valuable because of the scarcity of data in the area. With revisions regarding mainly the methods description but also figure and table updates I can recommend this data description paper for publication.

Suggested revisions and clarifications

Introduction: I am missing an overview of similar data available in the area – maybe a map of something. You state that measurements at this elevation are sparse – does that mean that there are none published?

As far as we are aware there are no other published measurements from the area, and this we have tried to highlight in the third paragraph of the introduction by briefly outlining the published measurements that exist and are ongoing in Central Asia. As we state in the last sentence of the third paragraph, it is truly that case that cryospheric and atmospheric are acutely scarce in the region, emphasizing the uniqueness of our reported measurements here.

Line 60: It would be nice to make this data available also or point to a source?

The Tajikhydromet have their own institutional policy under which terms they share their old records. The source of the data is indicated in our data availability section.

Line 91: It would be nice to see the box outlined on one of the maps.

Fixed. This box is included in Fig. S2 and we now reference this box in the revised section 2.1.

Paragraph around line 114-117: Maybe this would be easier to read in a table?

It is certainly true that this text is difficult to read as it is. We are, however, unsure of how to make this into a table, but have instead placed this text in the supplement.

Section 2.3: Do you use the same GPS for stake positions? In line 130 you mention the stake positions were recorded with GPS, but with which system and with which accuracy?

Yes, we did use the same GPS for the stake positioning and the accuracy for this GPS is stated in this section. Based on the comment given above by the other referee regarding accuracy, we have clarified that the accuracy is based on the information given by the GPS manufacturer (in section 2.3), while in section we stated that it is the same GPS used for these measurements.

Line 162-163 needs a revision for readability “[...] in a melted state but light protected state [...]” and it ends with ..

Fixed.

The wording now reads:

The snow samples were collected in 20 mL glass vials and were packed in a light-protected state for transport to Helsinki. During transport it was not possible to have the samples remain frozen.

Section 2.5: From Figure 3 it looks like new stakes were drilled every year, and that is not really clear in this section. Could you add some information, for example was the stakes redrilled every year and at what position, the original position or other places? State here also what the measurement strategy was, like did you only measure the stakes at the end of the melt season? When you say stake height is it then from the ice surface to the top of the stake? And was it deliberate to not put any in the accumulation zone? Maybe it was not logistically possible?

It was indeed deliberate not to put any stakes in the accumulation zone of the glacier as it was difficult to access it with stakes and the additional necessary gear. Our aim was to measure the stakes at the end of the melt season since we typically only visited GGP once a year. The one exception was in 2025, when the site was visited earlier in the year also and the stakes were measured. The stake height is from the ice surface to the top of the stake. All this information is described in the ablation stakes table.

Section 2.6: Is there any calibration routine suggested by the producer of the AWS and was this done?

In principle there is a calibration routine. Vaisala (manufacturer of AWS) recommends that you calibrate the sensors every year for optimal performance of the sensors. For our work and the remoteness of this location—this is not practical nor possible to do in the field. Two of the sensors were, however, changed during this work. The sensor which measures T, p, RH was changed to a calibrated one in 2024.09.18; while the wind sensor (including WS and WD) was changed in 2023.09.06. This information has now been added to section 2.6.

166: I don't know what an Ion chromatograph is, so maybe that's why I don't know what you mean with “(Waters)” maybe this could be elaborated a bit.

Waters is the manufacturer of the chromatograph instrument that we used to measure different ions. We have added the trademark symbol TM to indicate that Water is the instrument company this in the revised manuscript.

171-172: I don't really understand what is meant by "[...] to be representative of the specific years' expedition [...]". I think it becomes clear later in the text but could be elaborated a bit here.

We are referring to the general surface conditions that we observed at the glacier during the field campaign years taking surface reflectance measurements.

In the revised manuscript, the section now reads:

The surface reflectance of GGP was measured with a handheld spectroradiometer, ASD FieldSpec HandHeld 2 (Malvern Panalytical Ltd; spectral range 325–1075 nm) during the 2019 and 2023 campaigns. Different areas from the glacier surface that appeared to be representative of the specific years' expedition were selected for measurements. In practice, this meant that in the upper section of the glacier (next to pit 1) during 2019, three different surface types were measured, while in the midsection of GGP, three additional measurements focused on impurity-rich areas commonly found across the snow surface (which are visualized in Fig. 9). During the 2023 campaign, the snow on the glacier surface was mostly ablated. Eighteen reflectance measurements were carried out laterally across the glacier within the 3600–3680 m a.s.l. elevation range.

Table 2: In the *) lines: I think it should be 2019.8.31 – 9.3 (not 3.9)

Fixed.

Figure 3: I don't understand why there are two different image sources Bing and Maxar?

The image source is Bing Maps (© Microsoft), while the imagery data is © 2025 Maxar.

215: I think the word "diminishing" suggest a trend, but there are too few years of observation to say anything about highly variable snow cover.

This is certainly true. The idea with this statement was to put our results into context, but the wording ended up being poorly chosen. In the revised manuscript, we have removed 'diminished at an exceptional rate', with replacing it with 'from this satellite imagery, it can be observed that the snow cover at GGP has been less extensive in recent year'.

Figure 4: The y-axis should state that it is a retreat from the first observation, this should also be made clear in the figure text.

This is a good point and now has been changed in the revised text. The y-axis now reads:

Ice front retreat from 1973 observation (m)

while the figure text says:

Retreat of GGP terminus from 1973 outline (Vladimirova et al., 1976) to variations determined 1989-2024.

221-236: Consider putting this information into a table?

It is true that that much of the information could be placed in a table. Based on the comment above from referee #1 (regarding sec. 3.2), where the same was asked for, we have updated the existing UAV in the revised manuscript to include this information.

Table 4: please describe in details in the caption what is GPC and CP

Fixed.

The table caption now reads:

Summary of localization accuracy per ground check points (GCPs) and check points (CP) and mean errors in the three coordinate directions and average projection error in pixels.

Figure 5: As far as I am aware Open Street Map is vector maps only and they do not offer any satellite images, so I wonder what is the source of the background image?

This is correct, but the background imagery is available via OpenStreetMap and we believe they are the ones that need to be accredited here. In the figure caption we are now mentioning that the background images are available through OpenStreetMaps.

266: If you used the same GPS as was used to outline the terminus area, you stated that there is an accuracy of 5 to 10 m, then these changes are so close to the accuracy limit that maybe it is not correct to conclude anything? Maybe there are some arguments to be stated to prove me wrong.

We do not have any arguments to prove the referee wrong here. We do state that the accuracy is 5 to 10 meters, and that the movement is up to 11.5 meters, and so technically we are 'above' the uncertainty limit with this statement. Also, it is good to note that we are not aiming at concluding with this statement, but rather to report on the measurements that we observed. In the revised manuscript we have slightly modified the wording around these statements, and it now reads:

Near the bottom of the glacier, the movement was smaller (and thus, within the uncertainty of the GPS receiver), showing that there is very little lateral flow by glacier, rather only vertical melting.

Line 267: bottom -> terminus?

Fixed.

298-299: Maybe add a few words about the expected temperature increase due to normal lapse rates, but for sure also keep the point about the local temperature being affected by other things than just elevation.

This is a good point which now is added in the revised manuscript. In the revised section 3.6, where location shift results are discussed, now reads:

When the station was moved to the new location, the altitude of the AWS decreased about 115 m. Due to the lower altitude, and the expected lapse rate, the temperature increased. This was observed in a shift of about 2 °C. Surrounding landforms and surfaces (e.g., longer lasting snow cover) may also potentially affect the T.