Reply to reviewer #2

We are very thankful for the anonymous reviewer's constructive review of our manuscript. Please find here our answers to their main and specific comments.

Following reviewer #1 comments (section 1.1), we performed major changes to our algorithm spatial interpolation method. When calculating the mean annual mass-balance anomalies, our previous inverse-distance weighting (IDW) methodology lacked a way to capture the varying errors with distance to the measurements used for interpolation. We decided to replace the IDW spatial interpolation (for which there is no integrated error propagation) by a kriging spatial interpolation (that includes error propagation natively). We invite reviewer #2 to read our answers to reviewer #1 for more details on this. Some of the comments in this review are not relevant anymore due to these changes. We state them accordingly.

The document is color coded as follows:
Black: reviewer general comment
Green: answers to reviewer
Blue: extracts of the revised manuscript

In this study the authors provide mass changes for a large number of world glaciers from 1976 to 2023 by combining data sets based on in situ and space-based measurements. This new product, to be distributed by Copernicus services, will be of invaluable value for climate applications, including sea level rise and land hydrology. It will be definitely of high interest for the scientific community.
While I greatly appreciate the efforts made by the authors in combining different datasets, in performing appropriate calibration and in providing product uncertainties, I find that the manuscript requires major improvements in terms of presentation. The paper is very difficult to read. First of all, it lacks a number of general information about the data used to be understandable to non-experts. Some sections are quite technical and poorly explained. A large number of variables are not defined, and some important information is just provided in tables without explanation in the main text.
I recommend to the authors to consider my comments below and provide in a revised version a text clear enough to be appreciated by both experts and non-experts. As it stands, it is not the case.

We revised the manuscript to make it more accessible to non-experts. Important variables are now clearly defined and explained in the main text (already in the abstract and introduction), technical sections are better explained and, hopefully, will be appreciated by both experts and non-experts.

General comments:

- The abstract and introduction are too vague and lack useful information. It is unclear in both the abstract and the introduction which data sets are considered and combined. How have they been obtained? The data section refers to the data sources given by their acronyms, but no information is provided on the methods to obtain the data. What is the proportion of in situ data and remote sensing data? Are the latter only based on ASTER DEMs as described in Hugonnet et al. (2021)? What is the spatio-temporal coverage of each data set? It is insufficient to say (as written in the abstract and introduction) that geodetic and glaciological data are used. It is unclear whether in addition to the Hugonnet et al's data, other remote sensing data are considered.

We agree the abstract and the introduction were not clear enough in showing this specific information. Most of this is communicated later in the input datasets section and Fig. 1 in the data and methods section. More details are now provided in the abstract and introduction, more specifically:

1. The input data sets used with their proper references, where users can find all the information on the methods to obtain the data.
2. The proportion of in situ and remote sensing data used, their spatio-temporal coverage and a summary of their technical details (sensor sources) and references.

We think that the level of detail provided in the revised abstract and introduction, and in the data section is well adapted to the scope of our work. We provide all the necessary references for users to get more information on input datasets, methods, etc. if needed.

- While the paper mentions the percentage of glaciers considered by Hugonnet et al (96%) and the total number of glaciers is never mentioned, the percentage of glaciers considered from in situ measurements.

This information was available from Fig. 1 but as the reviewer correctly states, not in the abstract, introduction or in the manuscript text. This is now corrected.

I suggest to rewrite the abstract and introduction to clarify these issues and provide the reader the missing information. The figures are in general too busy. The figure captions need to be extended and provide the definition of the parameters appearing in the figure.

The abstract and introduction have been re-written accordingly. We edited the figures so that they provide useful and intuitive information properly defined in the captions.

Line-by-line comments

- Abstract, line 14: the term 'remote sensing' is too generic. Mention how elevation measurements are obtained (in addition to streo images), laser altimetry and space gravimetry can be also used (the latter given rirectly mass changes).

The abstract has been fully re-written. We mention in the revised introduction how glacier elevation change measurements from DEM differencing are calculated with multiple citations that explain in detail the different sensors from which they can be obtained and how they are calculated. It is out of the scope of this paper to describe fully all the existing methods to measure glacier changes (and even more out of scope to describe them in the abstract). We refer the readers (and reviewer) to a recent review in Reports on Progress in Physics (Berthier et al., 2023).

L55-59: DEM differencing was initially applied to individual glaciers with DEMs derived from maps (Joerg and Zemp, 2014) and aerial photographs (Finsterwalder, 1954; Thibert et al., 2008; Papasodoro et al., 2015; Belart et al., 2019), but has now evolved to include data from airborne Lidar (Echelmeyer et al., 1996; Abermann et al., 2010) spaceborne altimetry (Jakob and Gourmelen, 2023; Menounos et al., 2024) and satellite derived DEMs from multiple sensors (Toutin, 2001; Berthier et al., 2023).

We use glacier-wide (therefore gravimetry estimates are excluded) elevation changes available in the Fluctuation of Glaciers database. All the details of the observations used are available from the citations provided in **section 2.1.2 Glacier elevation and mass change observations:**

L123-127: For more details on the specific input data, auxiliary data, retrieval algorithms and uncertainty estimation of the independent FoG glacier elevation and mass change observations please refer to WGMS (2024). More details on the glaciological method can be found in Østrem and Brugman (1991), Kaser et al. (2003) and Zemp et al. (2013, 2015). For the geodetic method and its error sources see WMO (2023) and about measuring glacier mass changes from space, see Berthier et al. (2023).

- Abstract, line 15: '… resolved information'. What is this information?

This is not relevant for the revised manuscript. The abstract has been fully re-written

- Abstract, line 16: how many world's glaciers?

This number is given in the introduction, we do not think it is needed in the abstract.

Abstract, lines 19-20: sentence unclear. What do you mean by 'leave-one-out cross validation'. This is a technical term. Should it appear in the abstract?

The abstract has been fully re-written. Cross validation is a new and very important aspect of this work. Due to the lack of independent measurements available to compare and validate our glacier-wide mass change assessment (because all the available measurements are used), performing a cross validation is the only way to validate our results and their uncertainties. We believe it is important to note this in the abstract since it highlights the huge efforts put into validation and uncertainty assessment of our dataset. These exercises are explained in more detail in their respective sections **5.2 Leave-one-out cross validation** and **5.3 Leave-block-out cross validation**

- Abstract: Estimated global glacier mass change and inferred sea level rise should appear in the abstract

Agreed. This has been added to the revised abstract

- Introduction, line 54: What is FoG? Please define (in fact it is defined latter, line 74…)

The introduction has been fully re-written. FoG is correctly defined at its first appearance.

- Introduction, line 74: Explain what is an 'hydrological' year.

Hydrological year is explained in the methods, we added a citation to the methods when hydrological year is mentioned in the introduction.

L144-148: For simplicity, throughout this work hydrological years are represented as the last year of the hydrological cycle (e.g. 1976) starting on the 1st October to 30th September in the Northern Hemisphere, and from 1st April of the previous year (e.g. 1975) to 31st March of the year (e.g. 1976) in the Southern Hemisphere. For the Low Latitudes region, we assume the hydrological year to be equal to the calendar year from 1st January to 31st December.

- Introduction, line 78: 'our methodology performs well…'. What is this methodology?

The introduction has been fully re-written. Our methodology is fully explained in the methods section 2.2.

- Introduction, line 82: ‘leave-one-out cross validation’. Explain what this consists of.

The leave-one-out cross validation (and the added leave-block-out cross validation) is explained in more detail in the respective sections **5.2 Leave-one-out cross validation** and **5.3 Leave-block-out cross validation**

- - Section 2.1.1, line 91: ‘to spatially locate glaciers’. Give some information on their size, number, distribution.

The number of glaciers, their total area (and the area region-by-region) is available in the manuscript in **section 2.1.1 Glacier inventories**. The most relevant information for the scope of our work is illustrated in Fig. 1.

- Section 2.1.1, lines 96-97. Explain what GLIMS consists of.

GLIMS is an initiative from the early 2000s to improve glacier inventories using satellite data (in particular from ASTER). This clarification has been added to the revised manuscript L101.

Section 2.1.1., line 108. Fog already defined

Corrected

- Section 2.1.1, line 144. ‘96% of all world’s glaciers’. See comment above.

This information is given before in the revised text.

- Section 2.1.1, line 118: ‘more details on the glaciological methods…’. This sentence should appear earlier. Moreover, it would be useful for non experts to briefly explain what it consists of.

We think it is appropriate to put these references in the data section, this level of detail is too high for an introduction. Describing the glaciological method in detail is out of the scope of this work. Still, it is properly described and referenced in the revised introduction with its strengths and limitations.

- Section 2.2, line 135. What is WGMS-id?

This is the FoG database glacier identifier. We added this clarification in the data section:

L114: Individual glaciers with available observations are identified in the FoG database with a WGMS-Id.

- Section 2.2, line 140. ‘low latitudes…’

Glacier region names in RGI have capital letters.

- Figure 2. in the figure caption give the definitition of the parameters beta g, Y, beta cal,g, etc.

We prefer not to have equations in the captions. These parameters are properly defined in the text, with reference to the figure. We added in the figure’s caption that "notations are defined in the text".

- Section 2.2.1, line 160. Give the definition of the variables.

Defined

- Section 2.2.1, line 168: What means a ‘threshold of 8 years’?

This means that a glacier needs to have at least 8 years of glaciological in-situ observations within the 10-year-reference period to calculate their annual mass-balance anomaly. We clarified this in the text L173-181

- Section 2.2.1, lines 171 to 174: Sentence unclear.

This has been rephrased.

- Section 2.2.1: What means a ‘threshold of 1000 km’?

This is no longer relevant with the change to the kriging method (see answers to reviewer #1)

- Section 2.2.1, line 180: Give the definition of the variables.

This is no longer relevant with the change to the kriging method (see answers to reviewer #1)

- Section 2.2.1, lines 191 to 193: Give the definition of the variables.

This is no longer relevant with the change to the kriging method (see answers to reviewer #1)

- Table 2: Define the acronyms CAN, SjM, ALA, SCA, CEU, SAN…

The RGI 19 first order glacier regions can be identified by a region number (1,2,3…19) , a region code (ALA, WNa, CAN…ANT) and a region full name. Most of the figures have the number and long name, sometimes using the region codes make figures easier to visualize (Fig. 7, Fig. 8.1, Fig 8.2, Fig 9). All three equivalences are available in Table 5. We changed to full names in table 2 to make it clearer.

- Section 2.2.2, line 219, 224, 229: what means ‘k-calibrated’

Clarified in L242-243

- Section 2.2.3, line 234. What is PoR?

PoR = Period of record, defined in L230. This needs to be defined as an acronym since it is used in the equations. We changed the appearances in the text to “period of record”.

- Section 2.2.4. This section is very technical. I would suggest to move the maths to an annex and explain in the main text the followed approach. As in the previous section, many variables are not defined…

We agree this section is technical, however, we need to properly describe the methodology of our dataset for publication in a data journal like ESSD. To our knowledge there is no supplement document or annex for this journal. A non-expert can always skip this section of the article if judged too technical. Advice from the editor is welcome here.

- Section 2.2.4, line 295-296. Give a reference for C3S (https://climate.copernicus.eu/)

Added

- Table 3: The content of Table 3 should be summarized in the main text.

We disagree, we intentionally decided to put this information on a table to make comparison simpler. If this is added as text, it is likely to be hard to read and understand.

- Table 4: same comment as above.

As above, we disagree, we intentionally decided to put this information on a table to describe the datasets in a simpler way. A paragraph with all this information would be in any case very hard to read and understand.

- Section 4. The 'Results' section should appear before the section on products

We believe this section is well located after the methods section, we describe the products and where they are located. Then we proceed to analyze the results. We are also open to changing the order if the editor agrees with the reviewer.

- Section 4, line 331: how much sea level rise?

This section has been rewritten and now include the sea level rise

- Figure 3: too busy. The panel on the global mass change should appear separately.

We disagree, we think it should be part of the same figure. We modified it to make it look more similar to the regional panels.

- Table 4, line 366: Should be Table 5.

Corrected

- Section 5.2.1, line 406: Why Ba? Why such a notation for a mass change?

Defined in EQ 1 (L167) and:

L449-450: For each selected glacier, we compare the original 'reference' mass balance time series (reference Ba) as available from the FoG database, with its leave-one-out calibrated mass change time series (Leave-one-out Ba).

- Figure 7: define the acronyms appearing in the figure.

Acronym equivalences are defined in Table 5. We prefer to use acronyms in these plots.

- Table 5, line 470: Should be Table 6. The content of the table should be summarized by a few sentences in the main text.

Corrected. The most relevant part of this information is available in the discussion text of **section 5.4, Improvements with respect to earlier assessments**. We chose the Table format to add extra relevant information in a simple format.

- Figure 8: the 'global' panel should be a separate figure. There is no discussion on the interannual variability? What is its origin? Some quasi periodicity is apparent? Could you provide a spectrum of the detrended time series?

Interannual variability differences are discussed in:

L589-595: Most regions display increased interannual variabilities when compared to both previous studies. The Gaussian regression used to fit the DEM time series in Hugonnet et al. (2021) has a smoothing effect to the point where annual variability is no longer detected (Fig.10). Similarly in Zemp et al. (2019), the variance decomposition model (Eckert et al., 2011; Krzywinski and Altman, 2014) employed to extract the temporal mass change variability for each region has shown

to contribute to a slight smoothing of the annual amplitude signal (Zemp et al., 2020). Our approach allows us to better represent the interannual variability at the individual glacier level, supported by the Leave-one-out cross validation exercise and the effect is inherited to the regional and global level.

We think it is beyond the scope of this data paper to make a deeper analysis of the variability (and periodicities in the signal).

- Section 5.3.1, line 560. What means 'Hexagon corona'? Is it the name of a glacier? Where?

Clarified in the revised manuscript:

L633 "unlocking historical United States spy satellite archives (e.g. KH-9 Hexagon and Corona declassified satellite imagery)"

- Section 5.3.1, line 576: Instead of just quoting 'gravimetry', mention GRACE results and give references.

Added GRACE + references to (e.g. Blazquez et al., 2018; Chen et al., 2022)

- Conclusions, lines 619-620. A reminder of the data used and methodology would be useful. A few sentences on the novelty of the study and the main results should also be added.

The conclusion has been fully rewritten

- Conclusions, line 640. Mention to ECVs and GCOS is too vague. Explain.

The conclusion has been fully rewritten, EVS and GCOS are not cited anymore.