Responses to Reviewers' Comments for Manuscript
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Greenland Monthly Accumulation Maps (1960-2022): A Statistical Semi-Empirical Bias-Adjustment Model

Addressed Comments for Publication to

The Cryosphere

by

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General comments

Lindsey-Clark et al. describe a statistical method to bias-correct and present arguably the most accurate estimates to date of mean annual and seasonal 1960 to 2020 Greenland Ice Sheet (GrIS) snow accumulation rates. The work is motivated by GrIS snow accumulation uncertainty, most of which the authors attribute to poor observation sampling and limited underlying model resolution in regional climate models (RCMs) and in reanalyses. The methodology, a principal component analysis (PCA) applied using GrIS accumulation estimates from RCMs, the Copernicus Arctic Regional Reanalysis (CARRA), and observations of SMB components, is compelling and well executed. The figures and tables are labeled and presented clearly and, combined with the conclusions, are of good quality.

Depending on its applicability to the next generation of GrIS precipitation hind-casts and as forcing data for dynamic ice sheet and climate models, the manuscript presents a good level of substantial progress and novelty and could be fairly significant to the community researching ice sheet-climate interactions. The PCA and resulting improvements to observation-based RMSE scores are well suited for GrIS snow accumulation dependent studies and should be interesting to readers of *The Cryosphere*. On the other hand, the authors discuss their own findings in detail but not in the context of a substantially relevant body of literature. A more developed discussion of more recent work would improve the manuscript's fair to poor scientific quality in this regard.

AC: Thank you for your comments and interest in our paper. We have carefully addressed each comment as follows.

Specific comments:

1. The abstract is heavy on background material and light on quantitative results. To improve conciseness, the authors could **cut most of lines 2-8**, most of which could be merged with the introduction or removed altogether. While the authors present over 40 subpanels of maps and plots plus several meaningful statistics throughout the body of the manuscript, few of these results are reported in the abstract. To improve completeness, the authors should consider **summarizing more of their results in the abstract or submit for publication with their manuscript a supplement that repurposes extraneous figures**. I suggest that the authors consider which subset of their numerous results are essential to the main findings (i.e., those reported in the abstract and conclusions) and **streamline the presentation of data by moving figures that can be interpreted more as supporting evidence to a supplement.**

AC: We agree that the background material in the abstract could be condensed, and selected details merged with the introduction. We also agree that the manuscript is quite long and would benefit

from a more focussed presentation and discussion of the main results. We will carefully consider which figures to retain in the manuscript, and move selected figures and discussion, such as the PC correlations analysis, to supplementary material.

We will restructure the manuscript and aim for a more compact presentation of the most important results and findings, moving more exploratory analyses, such as the PC correlations, to supplementary material. We will adapt the abstract to reflect the more compact selection of results, stating explicitly the study's main findings while condensing the background information. The conclusions will be revised to reiterate the main results highlighted in the abstract. Additionally, we will streamline the text by removing unnecessary figure/table commentary to avoid repetition and improve the clarity of the manuscript.

2. To improve the overall presentation quality, parts of the **data section (section 2) could benefit from better organization**, which suffers from a **poor balance of details regarding the gridded data** sets compared with the observations. **Much of section 2.3 could be cut and merged with material in section 5**, which would improve the discussion of other recent related work and better enable readers' understanding of the broader context.

AC: We agree that the description of the gridded datasets would benefit from additional detail. We intend to keep the presentation of SUMup (section 2.3) in the data section, but we will consider whether specific sections, such as the pre-bias-adjustement histograms of model-SUMup residuals, would be better placed in the results section or moved to supplementary material.

We will expand the descriptions of model datasets to provide necessary additional detail. We will carefully evaluate which parts of section 2.3 could be either cut down, relocated to the results section, or moved to supplementary material.

3. In my view, the work's significance and its application to GrIS precipitation estimates depends on it being reproducible and/or the accessibility of the resulting data sets for use with impact studies (e.g., forcing data for firn, ice sheet, and sea level rise modeling and projections). To improve the work's potential impact, I recommend that the authors **clarify details of their input data and methods where possible.** For example, there are **lacking details regarding the RCMs and CARRA data sets (section 2.1)** and of **the applied regularization process (lines 291-296)**. **HIRHAM is mentioned several times but no citation appears until section 2.1.1**. **Regarding the RCM and CARRA data sets, there are so few details provided that an unfamiliar reader must refer to other sources to learn what the acronyms represent** and to understand basic components of the modeling systems, let alone their more technical nuances and strengths and weaknesses regarding

estimating GrIS precipitation rates. While I understand that the authors need not fully describe previously published data sets, the level of details regarding these “main characters” is severely out of balance compared to that provided for the observational data (section 2.3). And while the detail of the methodological description is sufficient, due to the size of data sets and complexity of algorithms involved, full traceability of results would almost certainly require that the **authors submit for publication with their manuscript archived source codes and/or software containers** that have been tested on commonly used computing platforms and operating systems.

AC: We agree that the potential impact and reproducibility of the study would be improved by providing clearer descriptions of the input datasets and methodological steps. While we are unsure whether we can redistribute raw model output, we will review the relevant terms to confirm what is permissible. Either way, we will provide a minimal working code package that can be run using, for example, the SUMup data including our applied dating uncertainties, as well as the EOFs and PCs derived from the raw model data.

We will expand the descriptions of model datasets to provide necessary additional detail, clarifying all acronyms at first use and adding an earlier citation for HIHRAM. Additionally, we will revise the description of the regularisation procedure to include additional detail such as the grouping used in cross-validation and the training/validation functions. In the description of the regularisation process, we will include additional details such as the grouping used for the cross-validation and the training and validation functions. We will supply a minimal working code to enable users to repeat the analysis with relevant input data.

4. The authors’ discussion of other work **lacks consideration of underlying physical processes**, **quantification of modeling errors and uncertainty**, and **limitations of data assimilation methods used by reanalysis systems**. Beyond their own findings, discussion of mean annual and seasonality of biases and trend and EOF analysis includes only four studies published within the last seven years **(van der Schot et al., 2024; Box et al., 2023; Gan et al., 2023, Matsumura et al., 2021)**. **Underlying data assimilation challenges inherited by reanalyses, sources of model biases inherited by both reanalyses and RCMs, and North Atlantic teleconnections are closely related areas of research but are not discussed in the manuscript**. For example, the authors mention that **GrIS surface temperature variability and its related impact on precipitation is driven by El Niño events but do not elaborate on the "...teleconnections with North Atlantic atmospheric conditions…." (line numbers 620-635) nor develop the current body of research in this area.** To improve scientific quality, I suggest the authors **refine the discussion of their results with respect to the context of more recent relevant work,** particularly regarding **driving processes**, **uncertainty quantification and speculations of error sources in fit-for-purpose models and reanalyses** (including those considered and not considered in their study).

AC: We agree that a more complete treatment of uncertainty would strengthen the scientific quality of the manuscript. As the uncertainty covariance of the SUMup dataset is unknown, we will investigate approaches to quantify uncertainty in our results. Specifically, we may use the spread in the cross-validation fits for the optimum regularisation parameter to derive uncertainty estimates for key reported statistics, including those highlighted in the abstract. However, for some derived metrics, it may be more appropriate to characterise uncertainty using the spread across the four bias-adjusted model outputs, and we will consider this where relevant.

We agree that speculative statements regarding driving processes or teleconnections should be supported by appropriate citations. To improve clarity and focus, we will restructure the discussion to emphasise the key findings of the study. As part of this restructuring, we will consider removing the section on PC correlations and teleconnections (including lines 620-635) along with other discussion on driving processes, which are exploratory and not essential to the main objectives of the paper.

The primary scope of this paper is to present our methodology and provide improved, data-informed accumulation estimates. A detailed investigation of the physical mechanisms driving biases, data assimilation limitations, or North Atlantic teleconnections–though closely related areas–is beyond the intended scope. Nevertheless, where we retain results that touch on related topics, we will ensure that they are properly contextualised with reference to the relevant recent literature, such as van der Schot et al. (2024), Box et al. (2023), Gan et al. (2023) and Matsumura et al. (2021).

We will introduce uncertainty estimates for key statistics. We will restructure the discussion to focus on the core findings and carefully consider whether to remove discussion of the PC correlations, teleconnections, and other speculative interpretations of driving processes, or place them in supplementary material. We will revise any remaining discussion of physical drivers to ensure it is supported by appropriate recent literature and consistent with the scope of the study.

Technical corrections:

1. Line 15: Please define the acronyms HIRHAM5, RACMO, and CARRA in the abstract.

AC: Thank you for this comment.

In the revised manuscript we will define the acronyms for all models in the abstract.

2. Line 29: Further articulate the SMB definition(s), considering if the terms "specific SMB" and/or "climatic mass balance" are more appropriate for use with this manuscript.

AC: Thank you for suggesting this clarification. We will review the use of 'SMB', 'specific SMB' and 'climatic mass balance' to ensure that our definitions are consistent with glaciological conventions and with the characteristics of the data used in this study.

We will consider carefully that our definitions are correct and sufficiently specific for the introduction and whether further clarification in the observation–model comparison sections is needed to express how these definitions relate to the model output and observational records used in this study.

3. Lines 103-105: The purpose of the work, which is to improve GrIS SMB estimates and downstream modeling of sea level rise, is not stated accurately or is misrepresented.

AC: Thank you for highlighting this. We agree that this key aim of the study is currently not highlighted in the introduction.

In the revised manuscript, we will ensure that this section of the introduction clearly situates the method and bias-corrected accumulation fields within the context of improving GrIS SMB estimates and downstream modelling of sea-level rise.