Reviewer 1

First of all, we would like to thank Ken Mankoff for his time and fast response. We appreciate the feedback received, it has greatly improved the manuscript. Responses to the issues raised (in blue) are written in black, and changes in the manuscript are written in red.

* Issues
** Custom regions

L129-131 states "We made one adjustment by moving the boundary between SE and CE northward to better follow hydrological catchments, as well as to make the regions more comparable in size."
Must you do this? Please don't do this. I assume you are not trying to make your product less useful and usable and add confusion and extra effort for others, but without good justification for adding yet another new and different basin and region product, I cannot let this comment go. See https://x.com/glasheologist/status/1557208390853505024 There are entire community projects (e.g., https://iacs-cryo.github.io/Delineation-WG/deliverable1) and many papers showing that comparing products A and B is hard or impossible or incorrect because they used different boundaries.
We understand your concern regarding the many different basin definitions that are being used in the community. The main reason for doing so is that, for the purpose of this paper, we are 'in-between' applications for land, ocean and ice. This study has the purpose of comparing freshwater sources from the perspective of fjords and climate, as opposed to hydrological basins (on land), ocean basins or glaciological basins. During our research phase, we concluded that using any of the existing basin definitions would not fit our research needs, and therefore a dedicated basin definition was deemed necessary, despite the disadvantage of creating another basin definition as you point out. We followed, as much as possible, existing definitions. We use as a basis the ocean-based basin divides used by Slater et al. (2020), which in turn represents an adjustment of the ice basin divides used by Mouginot et al. (2019). However, the area of CE is very small in Slater et al. (2020), as it was created as a ‘transition’ sector between colder and warmer ocean waters, while not cutting the Kangerlussuaq glacier in half (Fig. 1). This makes NE six times larger than CE, making the regions not comparable anymore. Furthermore, the Mouginot et al. (2019) - CE/SE boundary over the tundra cuts fjords in half, which is unsuitable for our purposes. To compensate for both disadvantages, we chose to follow the 'ice divide' boundary from Mouginot et al. (2019) to differentiate between CE/SE. Over the tundra, we manually adjusted the CE/SE boundary to follow the hydrological basin boundaries over land (see Figure 1 in this document, and Zenodo database, see comment “In addition to using standard regions,”). Figure 6 in the MS confirms that our updated basins have the desired inter-distinguishable hydrological and climatological characteristics, indicating that the chosen regions effectively capture variance across Greenland. We will provide information (see below) to increase reproducibility, and to make the outcomes comparable to other studies. We revised the manuscript as follows:

Because this study considers freshwater fluxes into fjords, which differs from most hydrological, glaciological and oceanic applications, Greenland is divided into seven climatologically distinct regions (Fig 1): North (NO), North-East (NE), North-West (NW), Central-East (CE), Central-West (CW), South-West (SW) and South-East (SE). These are based on the seven land/ice basins from Slater et al. (2020) that in turn are based on the Mouginot et al. (2019) ice divides. We made one adjustment by moving the boundary between SE and CE northward, following the Mouginot et al. (2019) ice divide, to follow tundra hydrological catchments, as well as to make the regions more comparable in size.

In addition to using standard regions, I would strongly encourage you to provide data (even if not results in the paper) at fjord scale. Or in map (spatial) form at GIS scale. Your work is only possible because of all of the data products you were able to ingest and process. You should strive to make your products even more usable than what you ingested - raise the bar if possible. While some of those products were specifically data products (and published in ESSD), many others were science-focused papers such as this manuscript but that also shared usable data. Data shared at fjord scale can be easily combined up to region or GIS scale. Data shared spatially at GIS scale (at a reasonably high resolution, e.g < 25 km grid cell) can be processed down to region or fjord resolution. The only way to have data less useful than providing it at regional scale is to provide a single value at GIS scale.
I know Mankoff products are available at fjord scale, Karlsson is GIS wide spatial format (can be accumulated at fjord scale), and as can your RCM inputs. I'm not sure if any of your inputs are provided at only regional scale.
In summary, use standard regions, and provide data everywhere in spatial format, or at glacier scale in vector format.
Thank you for your suggestion. We have not made individual fjord delineations for this study, only a fjord mask and basin delineations. We will provide the fjord mask and used basin definitions and freshwater data on 1 km scale as you suggested. This will allow others to use their own basin delineation and improve reproducibility. We will include that CARRA data are available from https://cds.climate.copernicus.eu/datasets/reanalysis-carra-single-levels.
The (downscaled) RACMO and MAR data presented in this paper were previously published in Noël et al. (2019) and Fettweis et al. (2020). The 1 km RACMO and MAR data used in this manuscript, as well as the region and fjord masks, are available from https://doi.org/10.5281/zenodo.14551168, respectively.

** MoA
Sect. 2.3 Annoying to see a new term and have to jump around to learn about it. Add a brief intro to MoA here? Anyway, 4.2 just refers straight back to 2.3. I still don't know what MoA is beyond a mathematical definition. You're introducing a new term and concept here (I think? I've never seen it and you have no citation for it). Add some text? What's the point of MoA? What do we learn from it? What are its weaknesses and limitations? What's a reasonable range for it? Min, max, average, median? How does it change spatially or temporally and why? Some of these are addressed in the text, but very few.
I've now spent some time thinking about MoA and trying to understand it, looking at Fig. 6, etc.

MoA = (melt + rain) / (snowfall − sublimation)

low MoA = low melt or high snowfall

high MoA = high melt or low snowfall

This seems to mostly be an atmospheric phenomenon? But perhaps controlled by elevation too? What about providing a spatial map of average MoA. Does something interesting pop out? I assume nothing interesting in the interior where MoA goes to 0 because of no melt. I'd expect regions with high winter snowfall to also have high summer melt (e.g., Southern Greenland) and regions with low accumulation to also have low melt (N. Greenland). The ablation area is where it gets interesting. Is it just a proxy for width of ablation zone? Something else?

I also assume it's only useful as an annual average, not monthly. How it changes in time is also unlikely to be interesting, as we know there's an increase in melt that is larger than the increases in snowfall. But maybe I'm missing something.

I think you need to introduce MoA as a stand-alone product before you start correlating it with external things like Discharge (Fig. 6a) at the beginning of Sect. 4.2.
We acknowledge that the introduction of MoA was somewhat ad hoc, and we regret introducing a term that is not well-known without providing appropriate explanation. We appreciate the reviewer drawing our attention to this issue, which we have resolved by revising the text as follows:
The MoA ratio has been used previously in firn studies to determine the climatic conditions under which melt would generate runoff in the accumulation zone. Previous work has identified a theoretical MoA ratio threshold between 0.6 and 0.7, indicating the onset of runoff (Pfeffer et al., 1991; Braithwaite et al., 1994). More recently, MoA has been used to predict when melt ponding starts on Antarctic ice shelves (van Wessem et al., 2023). In this study, MoA is used as a climatological indicator over the ice sheet, which we hypothesise is highly relevant for the partitioning between solid ice discharge and liquid water runoff into fjords. MoA does not directly depend on runoff, but on melt as well as snowfall; the relative regional sizes of ablation and accumulation areas and the potential for meltwater buffering (through snowfall) also become important.

L255: To our knowledge, no studies have identified a strong link between MoA and freshwater input fractions in Greenland fjords. In contrast, freshwater input fractions into fjords poorly correlate with temperature, melt or snowfall. This novel result will facilitate the interpretation of e.g. future changes in the distribution of freshwater fluxes in terms of climate change.

What is the point of 6a? I think it's a complicated way of saying that when there is high melt, the proportion of discharge goes down.
See also answer RC2: L251-52 / Figure 6(c-d):
“We clarify in the revised text as follows: Furthermore, the regional fractions to total freshwater

input of both GrIS and tundra runoff decrease with increasing fraction of solid ice discharge (Figs. 6c,d). Yet, no such relation is found for fraction of GIC runoff or precipitation. There are more sources than solid ice discharge and meltwater runoff alone, such as ice cap runoff, tundra runoff and precipitation. This means that the relationship between two sources is not a priori linear, and we find that there is such a relation between all pairs of source fractions."

*** Minor comments**

L24: How does something enter a fjord above a grounding line?
Thanks for pointing out this confusing sentence. We will change the sentence to:
(…) whereas in glacial fjords, runoff can enter at the surface and/or subglacially,

Paragraph starting L42: Seems like the text here should make reference to Mankoff (2020) http://doi.org/10.5194/essd-12-2811-2020 which uses your RCM (and MAR) to distribute both ice sheet and tundra runoff at stream resolution. L51 in particular, tundra runoff is not excluded by Mankoff (2020) http://doi.org/10.5194/essd-12-2811-2020. That paper does not "combine different datasets" (L50), but is trivial to combine with other Mankoff products to get solid discharge RCM runoff terms. I also note that the Mankoff discharge product provides an estimate of discharge depth for every stream outlet, addressing the surface vs subsurface discharge issues raised here.
We agree with this observation, and we have changed the reference as follows:
To this end, recent spatially resolved studies estimate freshwater fluxes from marine-terminating glaciers or stream outlets (Mankoff et al. 2020a; Slater et al., 2022; Karlsson et al., 2023).

L143: There is no explicit uncertainty section in Sect. 2, but it sounds like there should be? Is it 1 or 2 sigma?
In this case the uncertainty is the uncertainty as described per data source in section 2. We have changed the wording below:
(...), where the errors in this section represent the uncertainty per source as described in the methods.

L164: Direct inputs in January? Seems unlikely w/ ice cover? I realize you address this at the end of the document, but I think it should be addressed more explicitly throughout.
See answer on comment Sect. 4.6, and we added the following sentence to the method section:
Precipitation is defined as precipitation onto the fjord area, neglecting the potential temporary interference of sea ice that may be present, as discussed in Sect. 4.6.

L164: Basal amount is mostly steady state. What is the goal of reporting this small amount on this month? What is the significance of this sentence?
It is true basal melt is small with low temporal variability. The sentence is included because all terms are discussed, and we want to be consistent by discussing every term.

L240: Mankoff 2020 (freshwater) shows RCMs and obs disagree by >> 100%, not 30 - 50 %.
We changed the sentence as follows:
(...), with differences of up to 30-50 % between point measurements and regional climate models for larger regions, or even more than 100 % for smaller catchments (Mankoff et al., 2020).

Table A1 and elsewhere: Do you know things to 0.1 %?
Thank you for your suggestion. We removed decimals in Table A1 and elsewhere where relative numbers are reported, except for values smaller than 1, where we will keep one decimal.

What version of Mankoff (2019) data did you use? Maybe change Mankoff (2019) to Mankoff (2020) http://doi.org/10.5194/essd-12-1367-2020
We used version v100, from November 2024. We changed the citation to:
Mankoff, K. D., Solgaard, A., Colgan, W., Ahlstrøm, A. P., Abbas Khan, S., and Fausto, R. S.: Greenland Ice Sheet solid ice discharge from 1986 through March 2020 [v100], Earth System Science Data, 12, https://doi.org/10.5194/essd-12-1367-2020, 2020b

Your Zenodo dataset contains folders and files like ".DS_Store" and "__MACOSX/data/temp/._MoA_plot_input_mean_per_region_1990-2023.csv". Consider adding the DOI/URL of "all versions" that Zenodo provides and in the text mentioning which version you used. This way if you update the data in the future, readers can find the version in the paper or the latest version.
Thanks for pointing this out, and apologies. The dataset is more ‘clean’ now and the files created by Mac OS in the zipping process are removed, as well as introducing a version for every new ‘update’ of Zenodo.

"Flux": This is a technical term with specific units of mass or length^3 (volume) per unit time per unit area. You are never using per unit area, and should probably never use the word flux (except perhaps when referring to ice discharge flux gates). I believe the correct term is either "mass flow rate" if Gt/yr or "volume flow rate" if km^3/yr.
Thanks for pointing this out. In this study, we use the word "flux" to refer to freshwater entering fjords as mass per time unit. While we'll remove "flux" whenever feasible, the term has been used in recent literature to describe mass flow rate or volume flow rate (e.g. Bamber et al., 2018; Karlsson et al., 2023). In some cases, "flux" is more appropriate than alternative terminology, which is why we've included this specific definition at the beginning of our methods section:
Although the formal definition of flux is a volume per unit time per unit area, in this study, the term freshwater flux is used to indicate freshwater entering fjords as mass per time unit, similar to Bamber et al., (2018) and Karlsson et al., (2023).

What is sublimation? Is it the net term (deposition + condensation - evaporation - true sublimation), or is it true sublimation?
Sublimation is surface sublimation + sublimation of drifting snow - deposition. RACMO does not model phase changes between liquid and gas over glaciated surfaces like the ice sheet, so condensation and evaporation are zero.

Sect 4.6 It's great that this product addresses freshwater input to fjord surfaces. But I encourage you to be more up-front about the limitations of this new approach, the primary limitation being sea ice. It's the last section before conclusions, but is more important than that. Going back to highly precise uncertainty (fractions of a percent!), what exactly is your uncertainty measuring? Some arbitrary mathematical function of the RCM, or is it telling us something useful about what we do and do not know about freshwater fluxes at regional and temporal scale? If the former, that's easy but not terribly useful. If the latter, that may be more useful for downstream users, but your uncertainty in winter months must then increase because of the neglected sea ice processes. That is, perhaps you can address sea ice in this work in some manner, without adding a full model of sea ice growth, winter RCM accumulation onto the sea ice, and then sea ice advect & decay which allows the accumulated mass to then enter the fjord. That's out of scope for this paper, but I hope some treatment may not be out of scope.
We agree that the effect of sea ice deserves more up-front treatment in the manuscript. We extend Sect. 4.6 as follows:
The effect of sea ice on the fjord's freshwater budget varies per region and season. Most sea ice in Arctic fjords is landfast and thus will melt in the same fjord where it originally formed, temporarily storing freshwater rather than being a separate source (Cottier et al., 2010). Large regional and temporal variability exists in sea ice presence, and timing of formation and melt. In SE Greenland, Arctic sea ice can arrive from offshore, importing freshwater, while fjords in the SW can remain largely ice-free throughout the year (Moon et al., 2024; Stuart-Lee et al., 2021). The timing of seasonal sea ice break-up differs regionally: in SE Greenland, it occurs between May and August, while NO fjords can be ice-covered throughout most of the year (Moon et al., 2024; Johnson et al., 2011; Zahn et al., 2024).

We hypothesize that the uncertainty resulting from neglected sea ice processes will be larger with increasing latitude, more in the east than in the west and in winter months. Exploring the impact of sea ice on the freshwater budget is a potential avenue for future research.

Table 1: MAR "source" is bold but nothing else on that line is bold.
We have changed this accordingly.

Fig 2: y-axis units might be "Mass flow rate" or "volume flow rate" not "loss"?
See comment “Flux”; we changed the y-axis unit to Freshwater flux [Gt yr-1].

Fig 3: y-axis units are not Flux but Mass flow rate.
See comment “Fig. 2”.

Fig 6a: Add units to y-axes. Replace symbols with letters (i.e "NO, "NW" in the plot, no legend needed).
Regarding the y-axes units, we'd like to clarify that the values represent fractions, which are dimensionless quantities. This information is already provided in the figure caption, but we'll ensure it's more clearly stated to avoid confusion.

Table A3, A4: If MoA is interesting and you keep it, should it be a column in these tables (region average MoA)?
Thanks for the suggestion. We think MoA is mostly interesting in predicting/explaining the other variables and is therefore less interesting as a standalone variable. For this reason, we think that the numbers that can be read from the figures are sufficient for the interpretation.

Competing interests: This is weirdly phrased. Why not state who has which specific competing interests?
Agreed, we changed it to:
BN and MB are members of the editorial board of The Cryosphere.

Ack: Cite LanguageTool (URL if no scientific paper)
Added

Cite software used.
Added to code/data availability
Data analysis and figure plotting was done using Python 3.12 and the map was made using QGIS (Van Rossum and L. Drake, 2009; QGIS Development Team, 2025).

Cite all data products DOIs, not just scientific papers. Mention versions of data products.
Changed this.

When you say "From 1990 to 2023" I don't know if this includes 2023 or not. I recommend "A through B" if you went to the end of B.
Changed this throughout the document.

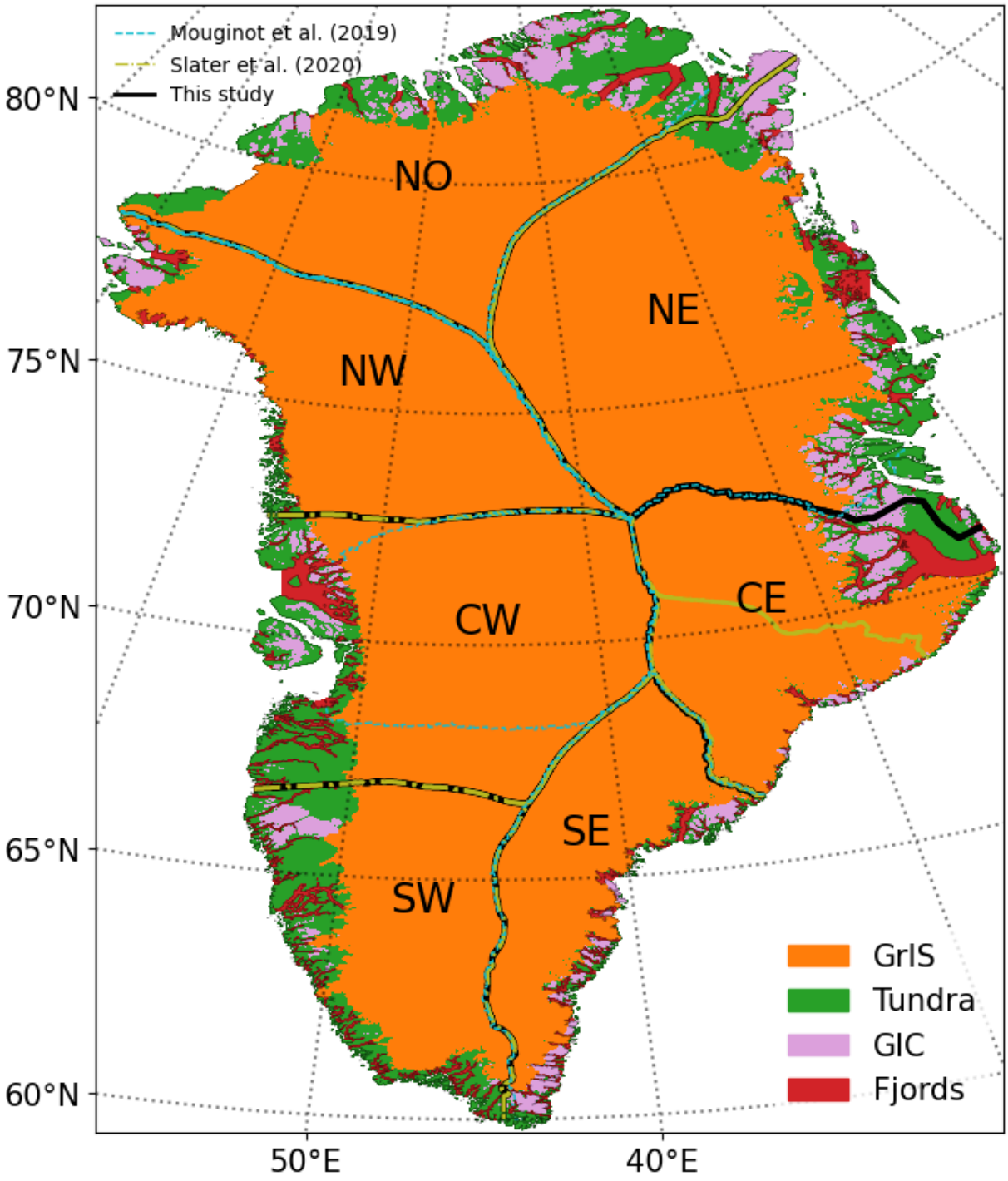

Figure 1: Masks of different surface types at 1x1 km2 resolution representing: the contiguous ice sheet (GrIS, orange), tundra (green), glaciers and ice caps (GIC, violet) and fjords (red). Solid black lines delineate the seven climatological regions used in this study, dashed olive green lines delineate the Slater et al. (2020) basins and dashed cyan blue lines delineate the Mouginot et al. (2019) basins (see legend in the upper left corner).

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