

## Response to reviewer comments

--- Our responses are indicated in blue color ---  
Line numbers refer to the revised manuscript.

Anonymous Referee #1, 21 Apr 2026

### **Overall impression**

I think this is an interesting manuscript built around a nice new dataset. The 115-year Ba/Ca record in Disko Bay presented here is a good contribution, and I think this paper should be published on that merit. I get the impression the record was collected carefully and I very much appreciate the extra effort to place it alongside climate and ice-mass products. I gotta say upfront that my background is on the ice-sheet/climate modelling side, not on proxy development or biological climate records. So, I am not even going to try and out-specialist the authors on the proxy part itself, but I think a non-specialist reader like me should still be able to clearly see why the title, abstract, and conclusions are justified, and at the moment I am not there yet. I had a look at some of the references to try to understand the methods better, and most of the points below come from that experience. So here's the disclaimer (and request): if any of my points/questions/etc. have obvious answers within the field, I would simply ask that those answers are visible in the text for the non-specialist reader. As said, I think the record is worth publishing, but the manuscript currently pushes the interpretation quite a bit harder than the text can support. I'll go for "major revisions", but not because the study is weak; I just think the framing is trying to do too much.

### **General comments**

#### **On statistics**

I am not 100% there regarding the statistics. The text seems to give two different answers to the same question. For example, the caption in figure 2 says the filtered Ba/Ca–MAR correlation is significant at the 99% level (the number that catches the eye), but at the very end the table A1 reports the same filtered comparison with autocorrelation accounted for and a p value of 0.08. The other filtered comparisons in Table A1 also seem to miss significance thresholds once effective sample size is reduced. I may be off regarding which test the authors want the reader to treat as the relevant one, but as it is written I struggle to reconcile caption and table. If table A1 uses the appropriate treatment for 15-point-smoothed series then is the 99% significance quoted in fig. 2 really the number the main text should be leaning on, or? If it truly is, then I'm a bit confused on the role table A1 is supposed to play here. So, if I'm not that off, please pick the appropriate test and apply it consistently, and then make the abstract, short summary, figure captions, and conclusions in line with that pick. My worry is not really that the relationship evaporates, but that the manuscript might be sounding much more statistically grounded than it actually is.

==> We thank the reviewer for bringing this point to our attention. The text in the figure caption did not consider the reduced effective sample size, which was reported in Table A1. We have now changed the text in the figure caption (Figure 2) to address this. If the reduced effective sample size is considered, the significance level is 90% and a statement was added in line 196 to align the caption with the value shown in Table A1: "*Correlations significant at*

*90% level (15pt Gaussian filtered records,  $R = -0.54$ ; 1903-2012; considering reduced effective sample size after filtering)."*

### **On nonlinearity**

“Nonlinear” makes the paper sound quite strong, but I do not think it’s fully delivering here. From what I can see there is a change point in the smoothed series followed by a steeper increase, plus a plot with the highest years clustered at the end. Interesting, for sure, but none of that truly establishes a nonlinear response in the strong sense implied by the title. I imagine a linear trend with persistence / autocorrelation could produce a pattern like this as well, or? How can I truly distinguish between nonlinear and monotonic increase? I think the manuscript’s got to choose here: either test the claim directly and show why nonlinear is the right description, or tone down to something like “accelerated” or “stepwise” or similar.  
=> *In order to address the reviewer comment, we have changed the title to "Accelerated increase of Greenland Ice Sheet runoff into Disko Bay"*

Same with the way the paper positions its emergence result. Defining emergence relative to 1932-2000 is fine if that is what the study supports, but it is different from emergence above a pre-industrial baseline. Text should be much clearer about that, especially when referncing that earlier Greenland work to avoid sounding more general than it really is.

=> *Since we do not have annual resolution observation-based data from the preindustrial era and data gaps are large even in the early 20th century (see text line 96ff), we cannot compare to this time period. The reference period was chosen based on the following reasoning, cited in line 285: "The starting point 1932 represents the earliest year for which at least five samples are averaged in the Disko Bay algal master chronology. The end year (AD 2000) was selected as a time before the onset of major GrIS mass change."*

We show the longest record so far from annually resolved surface ocean proxy data from Disko Bay. Our record shows decadal-scale variability before 2000 (see Figure 2 a). In line 321 we report that *"No significant trend is detected in Ba/Ca before the change point (0.01 STD/year)."* The year 2000 was chosen as the end of the reference period as a trend change point was identified in algal Ba/Ca at around 2001, while in western GrIS mass change was identified in 1998.

By the way, both the emergence date and the change-point date come from a 15-year Gaussian-smoothed series, so it’d be nice to see a bit more caution in how precisely specific dates are presented in-text.

=> *We have shown the confidence interval in Figure 2, please see figure caption "Orange box denotes 90% CI for change points". In addition, we now have calculated the error for the emergence point and added a statement in line 345: "Time of climate emergence is detected in the algal proxy in 2007 ( $\pm 2$  years error, assuming a 90% confidence level for smoothed Ba/Ca master record),..."*

### **On the proxy**

The proxy discussion moves (in my read) from “Ba/Ca goes up” to “runoff goes up”, then to causality too fast. No issues with the saying that Ba/Ca contains useful runoff information, of course but the manuscript sometimes treats Ba/Ca as a clean one-variable runoff-o-meter, and I am not so sure about that. I’d be OK if the discussion read as “this is an integrated environmental signal, that seems sensitive to runoff, and is consistent with glacier/ocean change patterns”. The current framing promises a clean proxy (and a clean causal

interpretation) that is, to my taste, not actually demonstrated.

==> Starting in line 108, we reference a number of studies that have successfully used Ba/Ca ratios from organisms that produce calcium carbonate skeletons as a proxy for reconstructing past variability in the input of terrestrial material, runoff and sediment flux in a number of environments. We have previously demonstrated that coralline algal Ba/Ca from the same species can be used as a sensitive proxy for glacier-derived runoff in a similar Arctic setting (Svalbard, Hetzinger et al. 2021). See line 106: *"Seasonally resolved coralline algal Ba/Ca ratios have previously served to reconstruct changes in past glacier-derived meltwater input in high-Arctic Svalbard (Hetzinger et al., 2021), successfully tracking changes in regional glacier mass balance and runoff."*

As requested, we now include an additional cautioning sentence in the discussion, line 310: *"Coralline algal Ba/Ca variability is an integrated environmental signal, that is sensitive to runoff, and is consistent with glacier/ocean change patterns."*

I also think that the manuscript needs to be more careful with external comparisons. MAR SMB, GRACE total mass change, historical/photograph mass-loss estimates, solid ice discharge are surely related but distinct components of the glacier mass-balance system.

==> Available data from actual observations of runoff for the 20th century are very sparse, and thus we can only use satellite-based estimates (short, starting in 1982) of glacial discharge and model data for comparison. We mention this in line 96ff: *"Available reconstructions of glacial discharge rely mostly on short periods of satellite observations and model data (Karlsson et al., 2023), or historical photographs (Khan et al., 2020), which are insufficient to describe potential long term changes occurring in shallow marine ecosystems. Limited temporal resolution and often discontinuous measurement time series cannot provide records of adequate length needed for assessing interannual- to decadal-scale variability and trends. Thus, long-term continuous proxy reconstructions from the surface ocean can provide additional understanding of glacier–ocean interactions..."*

Since we are limited to using either short (satellite-based observations) or model-based data for comparison to the proxy record, we chose to include further datasets that give us related information on changes in the glacier-mass balance system, and possibly, showing similar trends at the end of the 20th century.

To clarify this, we added a statement referring to the fact that actual observations of runoff in Disko Bay are very sparse and generally short, or based on model data, see line 115: *"Using a proxy-based approach to reconstruct past runoff variability is a big step forward as it provides for the first time continuous annual resolution data from Disko Bay surface ocean, where direct observations of runoff for the 20th century are very sparse, limiting us to either using short satellite-based estimates of glacial discharge or model data for comparison."*

Here, SMB is the most direct comparison target for a runoff proxy; the others are related, not equivalent quantities. The text flies over those differences and gives me the impression that all of them are agreeing validations of the same runoff interpretation. I do not think that is precise, so please be much more explicit about what those external products are truly supporting.

==> As noted by the reviewer, SMB probably provides the most direct variable in order to assess past runoff variability. This is the primary reason why we have chosen modelled SMB data from MAR for comparison to the long master proxy record (Fig. 2) as a starting point, and additionally the much shorter observations from GRACE (Fig. 3). Only the GRACE data are based on actual observations, however, but the record is quite short (available only since 1982). As described in the Introduction, multidecadal-scale length continuous observations from Disko Bay and the GrIS in at least annual resolution are scarce. See line 80ff (*"However,*

*due to a lack of long-term multidecadal-length continuous oceanographic measurements in Disko Bay, effects of increased GrIS melting on the surface ocean are not well understood and widely undocumented. Satellite observations indicate that warming of sea surface temperatures (SSTs) in Disko Bay (central-western Greenland) is exceptional and much faster than in other regions, reaching a warming of >1°C/decade since 1982 (Figure 1).") and line 86: "Although the GrIS's marine-terminating glaciers transport and subsequently deposit vast quantities of sediment into the ocean, there are no long-term datasets monitoring sediment transport from the marine margins, either by iceberg-rafting, or from subglacial plumes (Andresen et al., 2024)."*

Hence, it is highly necessary to use other available variables that have been used to estimate past changes in the GrIS mass balance system as well, even if they are "only related".

Nevertheless, these products and reconstructions are very relevant in providing important information to study and compare our proxy to, for example, the change in trends that we investigate here. In Figure 3 we compare our high resolution proxy record to the annual resolution calculation of Jakobshavn Isbrae mass loss, which is based on historical photographs (Khan et al. 2000), that delivers rare direct information from the glacier feeding into Disko Bay, affecting our study site.

### **On light/sea-ice conditions**

Related to the the Ba/Ca => runoff link. Here I had to read around, so take this as a question rather than a direct challenge. Checking the references (some of it by authors overlapping with this one) I get that summer light availability seems to have a significant control on the data. From what I saw, Disko Bay has lost a large fraction of its seasonal sea-ice cover across more or less the same emerging interval. So, my question: can changing light/sea-ice conditions at the site modulate either Ba/Ca, the age-model used to build the record, etc., in a significant way post-2000 rise? I don't see the manuscript addressing this, and as a non-specialist reader I can't tell from the text whether this possibility is negligible or not (or already ruled out in the specialist literature?). If yes, then I would just ask that the relevant references are part of the discussion here. Else, some short site-specific discussion would help. More generally, I would really like the chronology structure and robustness to be a bit more on the transparent side in the main text.

==> During sample quality control only samples with clear seasonal cycles in Mg/Ca ratios (used for building the agemodel) are selected, allowing direct comparison between seasonal geochemistry and annual growth increments providing sufficient age control. Changing light availability has no influence on the agemodel, as it is based on clear seasonal cycles in Mg/Ca ratios. To better illustrate this for the non-specialist reader, we now show an additional figure (new Figure A2) displaying the high-resolution microscope scan (photomosaic) of the upper part of a coralline sample. Mg/Ca ratios are superimposed on the image showing clear annual cycles based on Laser ablation analysis and additional age markers. Reproducibility of coralline algal Mg/Ca ratios, the basis for developing the agemodels, that are used for all other element/Ca ratios (including Ba/Ca), has already been tested successfully in Hetzinger et al. 2018 for the same coralline algal species. We have added a statement in line 175. Here, we use even more individual specimens for developing our master chronology than in earlier studies.

The post-1932 part of the record is doing different work from the earlier single-specimen extension, so I do not think the current "115-year multi-specimen" framing says that transparently enough.

==> Although the record extends back 115 years in total, not all samples reach back that far. Length of chronologies, overlaps and details are explained following line 175. In a

multispecimen master chronology not all individual records have the same length. Hence, trend analysis has to be done for the common time period ( $n > 5$  starting in 1932). However, our record is a significant improvement to earlier coralline algal studies, and many other high-resolution paleoproxy studies, which often are based on geochemical records from one single specimen. We have added a sentence noting again that the multispecimen record is starting in 1932 and that statistical analyses are based on this record (also see line 202, Figure caption), and line 343: "*We use 1932 as starting point in the analysis, as this represents the earliest year for which at least five samples are averaged in the Disko Bay multispecimen algal master chronology.*"

A small metadata table and a simple robustness check showing that the post-2000 rise is shared across specimens would be a nice addition, I think.

==> Supplementary Figure A2 shows all Ba/Ca chronologies for individual samples (two transects per sample averaged, nine individual samples, annual resolution), as well as the Ba/Ca intersample average (i.e. master chronology). The post-2000 rise, and especially the extreme years 2012 and 2018 are seen across individual specimens.

==> As requested, we now have analyzed the trends for each individual sample by estimating the slopes before and after the year 2000. Our results show: Post-2000 (after 2000), all samples show a significantly increasing trend at 95% confidence level. Pre-2000 (before 2000), 5/9 samples show a significantly increasing trend. 3/9 samples (samples 10-12-13, 10-22-11, and 16-1-19) show an insignificant trend, and one sample (10-22-29) shows a significantly decreasing trend. 8 of 9 samples show a significant acceleration of the trend post-2000, relative to pre-2000. Although one sample (10-12-13) shows an acceleration in the increasing trend, the trend is not significant at 95% confidence level.

These new statistics are summarized in **new data Table A2**. A statement was added to line 322: "*Trends were also analyzed for each sample individually by estimating the slopes before and after the year 2000. Post-2000 (after 2000), all samples show a significant increasing trend at 95% confidence level (details in Table A2).*"

## On AMOC

Sticky point right here. The AMOC paragraph felt like it belonged to a different paper, for sure. Greenland freshwater flux clearly matters to the North Atlantic, and I understand the motivation to add this type of lines, but this manuscript is not presenting so far solid evidence on the AMOC topic. Sounds more like motivation than anything, and not something that follows from the data shown here. I would keep it much, much shorter and toned down.

==> We mention the importance of potential freshwater input for large scale North Atlantic ocean circulation (and AMOC) only in the Discussion, as it is indeed an important motivation. However, at no point we mention that we have evidence for a direct relationship of our data to AMOC variability. Since the Disko Bay region, with JI being one of the biggest glaciers of the GrIS, is delivering significant amounts of freshwater to the ocean, we deem it appropriate to discuss this link, i.e. that a drastic increase of freshwater input into the surface ocean over a historically short time span, which is indicated by cited studies, may have large-scale impacts in the near future, as is also shown in a large number of modeling studies. In fact, after referring to the newest findings from modeling studies, we even question whether the findings of our study from Disko Bay can be identified in other Greenland coastal areas. See line 411: "*While recent studies based on an observational campaign suggest a lesser role of the western subpolar gyre off Greenland for the origin of AMOC variability (Menary et al., 2020) as compared to the eastern subpolar gyre, it remains to be seen whether the unprecedented trend change in glacial runoff increase in Disko Bay shown here can also be identified in east*"

*Greenland marine-terminating glaciers."*

## **Summary**

So, in short I think the data set itself is a reason for the paper being published. My friction is that the manuscript is trying to make the strongest possible claim at every chance: title, abstract, conclusions, broader implications, etc., when the material in its current form supports a valuable new proxy record consistent with recent Greenland runoff and (broad) glacier change, rather than a cleanly demonstrated nonlinear response with a well-resolved causal interpretation. To be fully clear here: my request is not more analysis for its own sake. I simply expect the strong claims to either be properly supported, or bring the wording back to what the paper is able to support. It would make for a better paper overall, and an easier one to defend (for you) and to read (for us).

==> As requested by the reviewer, we have toned down our text and we have deleted the "non-linear response" from the title and text and replaced it in the title using "accelerated increase".

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NB: I expect most of my specific comments covered if the tone and precision is fine-tuned following my comments above, including adding references and a few typos here and there. No point in pointing all that out before a revised version that should catch most of them.