Responses to Reviewer 2
Anatomy of Arctic and Antarctic sea ice lows in an ocean–sea ice model

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General Comments

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The authors in this study use the NEMO-SI3 model to investigate extreme lows in sea ice extent in both hemispheres over the historical period by using sea ice mass budget terms, both hemispherically and regionally. These modeled mass budget terms are particularly valuable as they are not available in observational products but they provide insight into the processes driving the low extent years. The authors find that basal processes (melt and growth) become increasingly dominant in the mass budget in both hemispheres. Additionally, they find that during case study low extent events the importance of thermodynamics and dynamics can vary, as can the importance of difference processes by season or hemisphere due to the local ice state and drivers. Overall, this study is well designed and provides insight into how sea ice evolution is changing. I have some major concerns, detailed below, about the analysis that I would recommend being addressed in a major revision before publication.

Response:
We wish to thank the reviewer for their constructive remarks and believe that addressing them will improve the manuscript.

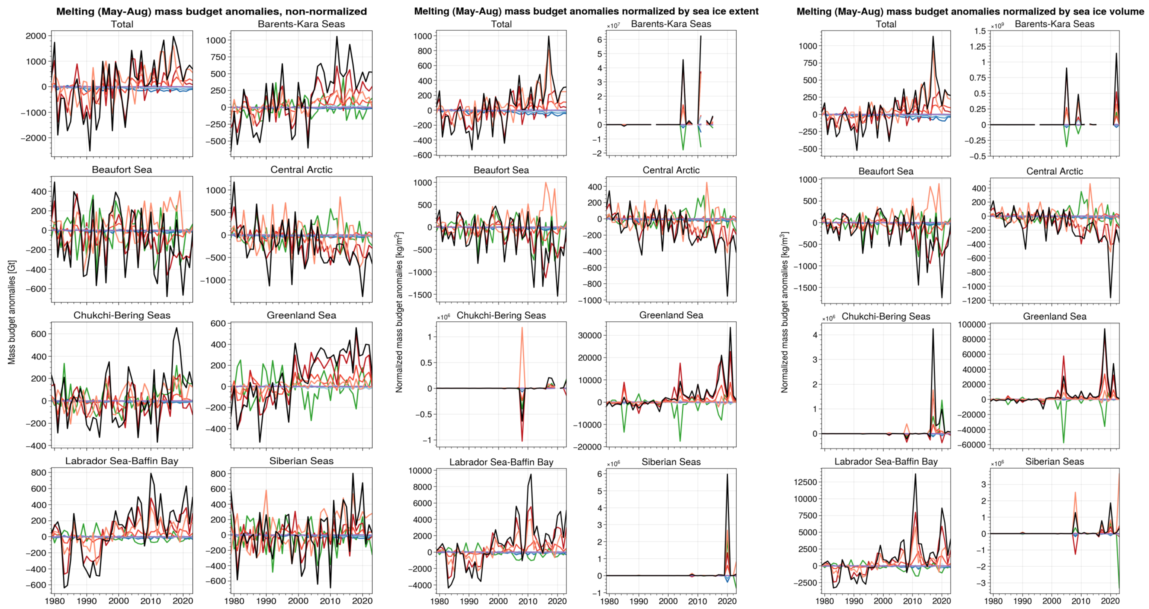
General Comment: Mass budget analysis

The authors present their mass budget analysis in section 4 as absolute mass loss anomalies, which itself is dependent on mean state of the sea ice. As a result, I found that the conclusions and graphs were confusing and a bit misleading about the results and more care needs to be taken with how this is presented. In years with extreme low extent anomalies the anomalously high positive ice mass budgets were sometimes explained as "preconditioning" based on the mean state. I recommend showing the anomalous mass budget terms normalized by the total ice area each year to better understand how the budget terms compare to other years. Additionally, it would be beneficial to show how the dominant budget terms change over time to support your assertions about basal melt change (e.g. Line 480-485) since Table 1 just gives a single linear trend but doesn't show the budget terms over time. It would also be helpful to show a timeseries of the fractional contributions of each mass budget term, like this figure (right) from Bonan et al. 2021 (https://doi.org/10.1088/1748-9326/abe0ec), to show how basal terms become more dominant. Again, you'd want to normalize this so that it isn't just reflecting a mean state change of less melt due to less volume.

Response:

We agree that positive melting anomalies are particularly difficult to interpret (see also comments from Reviewer 3). We have attempted to normalise the anomalies, either by the sea ice extent (SIE, Fig. R1b), the sea ice area (similar to sea ice extent) or the sea ice volume (SIV, Fig. R1c) in August, as it is the final state of our period of integration. Yet this raises some issues in sectors where the SIE or SIV are close to 0 at the end of the melt season, without solving the problem of positive melting anomalies in other sectors (cf. Fig. R1a). This normalization actually tends to introduce the SIE or SIV trend into the signal, exacerbating the anomalies, either positive or negative. Tests with normalization by SIE or SIV calculated at other months of the year (e.g. Apr, May, September) were not conclusive either. We therefore refrain from adding this normalization in our analysis, but will mention in the methods that we conducted inconclusive tests to normalise the fluxes.

We appreciate the suggestion to show the long-term evolution of the mass budget terms (Figure R2). We provide stacked plots for both hemispheres, for melting terms and growing terms. We normalize the terms by the total sea ice mass loss or gain calculated from a year starting in September for Arctic and February for Antarctic, to show the proportion due to each term. While we believe this is an interesting way of showing the respective proportion of the different terms and the interannual variability, we find that the trends are too small to be clearly visible on the figure, hence necessitate to be written on the graph and leading to a less legible overall presentation. Moreover, this requires a lot of panels and is not as concise and precise a way to indicate the trends as the table. We therefore lean towards keeping the table in the main text and inserting those plots in the supplementary information.



(a) Non-normalized (b) Normalized by SIE (Aug) (c) Normalized by SIV (Aug)

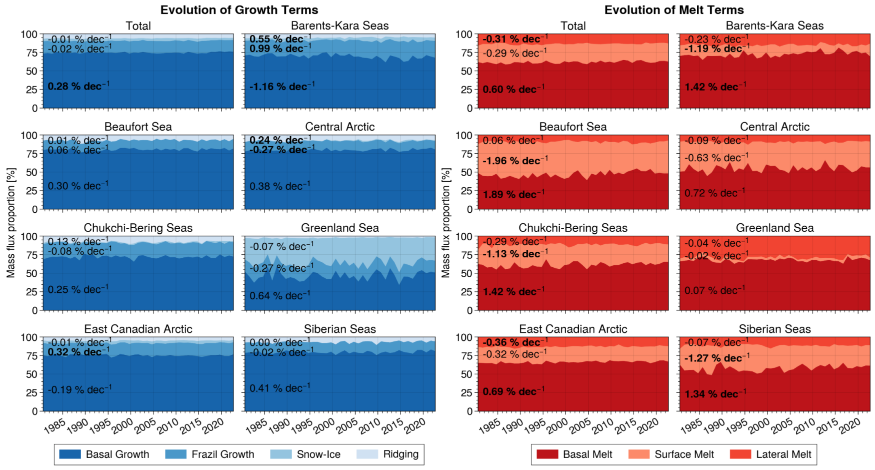
Figure R1: Time series evolution of mass budget terms in Arctic during the melt season (May to August): a) non-normalized terms (as used in the original manuscript), b) normalized by the minimum (August) sea ice extent (SIE) and c) normalized by the minimum (August) sea ice volume (SIV). In sectors and years when the SIE or SIV are close to 0, the normalization leads to very large values, preventing any comparison with other years. In other sectors, the normalization introduces a trend in the signal, due to the decreasing SIE and SIV over the total period.

General Comment: Ocean Heat Content

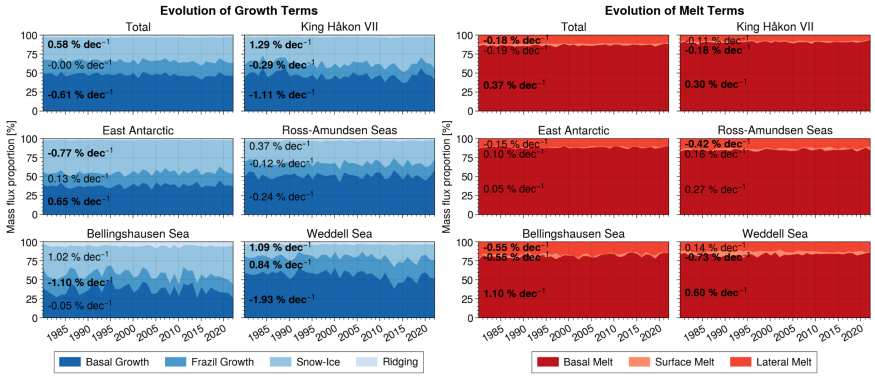
OHC is mentioned several times (Line 353, 358, 455, 454, 466) but is not clearly shown in figures and needs more analysis to support your assertions. For the Arctic cyclone in 2012, you should have a figure like Fig. 6 that shows the OHC aspect of this story and the impact on basal melt. For the Antarctic, you should do a spatial correlation of OHC and ice anomalies to quantify the relationship described at Line 445. Since you're analyzing winter months, there may be important implication of OHC anomalies that are not just from surface heating or the albedo effect. Additionally, for the panels in Figure 6, you say you are integrating OHC south of 50S. In many of the sectors this means you're including anomalies that are not actually in contact with the sea ice since they're well north of the seasonal sea ice zone. This makes your analysis at Lines 454-456 and 466-467 of the anomalously low OHC in the Ross-Amundsen sectors confusing since much of the anomalously low areas are not in contact with sea ice. I recommend you mask Figure 6 to show only that year's sea ice zone. Additionally, you may want to calculate a regional mean OHC anomaly only over points that are within the SIZ to better understand how the sea ice will be affected by OHC year to year.

Response:

We appreciate the reviewer's comment, which is also supported by Reviewer 3's suggestion. We believe that properly addressing both reviewers' comments would require to delve into a long investigation regarding causality between OHC and basal melt. Another in-depth study using advanced statistical methods, namely the Liang-Kleeman information flow,



(a) Arctic



(b) Antarctic

Figure R2: Long-term evolution of mass budget terms, expressed as a proportion of total sea ice mass gain for growing terms (blue shades, left columns) and of total sea ice mass loss for melting terms (red shades, right columns), for the Arctic (a) and the Antarctic (b). Percentages indicate the long-term trend for the different terms, in bold font when significant (p-value < 0.05), expressed in % per decade (as in Table 1 of the original manuscript). Melting residuals are not plotted as they represent less than 1 % of total mass loss and are therefore not visible anyway. Trends for growing residual (porous ridging) are not indicated to avoid overcharging the figures.

is precisely in preparation, led by one of the co-authors, to convincingly disentangle the causality between OHC and basal melt. Diving into this would therefore be redundant with the study in preparation, would take too much space and effort relative to the rest of this manuscript, and would also be out of the scope of this study which focuses on the sea ice mass balance.

We therefore suggest to simply remove most of the analysis around the ocean heat content, including Fig. 6 and the paragraphs l.436-476. We will remove mentions of OHC when mentioning the lack of impact of the 2012 storm (l.349-359) and will shorten the part in the Discussion section (l. 551-561) to a couple of sentences simply mentioning the fact that the model reproduces the documented OHC increase in both the Beaufort Gyre and the Southern Ocean, and will simply refer the reader to the literature analysing the link between subsurface ocean heat content and transport and the sea ice lows.

We hope this addresses the reviewer's concern.

General Comment: Antarctic transport

Antarctic transport: The discussion from Lines 229-241 on Antarctic transport was confusing. You mention the "circumpolar current", which probably refers to the ACC that is on the northern edge of the sea ice zone. What about transport from other currents like coastal currents which are in the reverse direction? I think that the transport piece itself is complex and needs to be more carefully assessed and discussed.

Response:
We agree that the transport is difficult to analyse properly. We will correct this analysis by clarifying that we indeed talk about the ACC, and by also mentioning the westward coastal currents. Their effects on ice mass budget would be similar, because ice inflow coming from an eastern sector would be compensated by an ice outflow towards a western sector, except in regions of re-circulation (e.g. Weddell Sea). We will discuss this aspect in the manuscript. We fully agree that the transport piece is complex, and we did our best to address it properly when it becomes one of the dominant processes.

General Comment: Clarity

Clarity: Much of the paper consists of VERY long paragraphs that are difficult to follow for a reader and often contain several points (e.g. Lines 132-162, 368-393, 402-435, section 5). Please break these paragraphs up so that they're clearer and more honed to the points you are making. A few places I found particularly challenging to follow were Section 4 and the discussion conclusions. Section 4 is framed as case studies of different hemispheres and years. I think it could be more powerful to frame these in terms of processes. For example, for section 4.1 a title like "the impact of ice volume on extremes" will help underscore the conclusion is that sea ice extent alone does not tell the full story and volume is necessary. Section 4.2 seems to be comparing hemispheres, but depends on how free drift and dynamics vs. thermodynamics impacts the extremes. Section 4.3 is better in that it clarifies seasonal differences are the focus. You may want to compare seasons for the Arctic too to confirm your results and see if the processes driving each hemisphere differ similarly by season. The discussion and conclusions sections are wordy with long paragraphs that make it difficult to separate the primary conclusions from the discussion. Perhaps make conclusions its own, small final section and better hone the paragraphs in this entire section.

Response:
We agree with the reviewer that many of our paragraphs are too long. We will do our best to split the paragraphs into shorter paragraphs and to clarify our writing.
Following the reviewer's suggestion, we will modify Section 4 by framing it in terms of processes, modifying the titles to match the main results of each subsection and reformulating the conclusions of each subsection. We might keep the mention of the comparisons as subtitles of each subsection, as we believe this could also help the reader to anticipate the content of the subsection. We refrain from adding a comparison to an Arctic winter, as those winter conditions are strongly constrained by geography; winter 2006 could be an interesting case study but the ice loss is limited to the Labrador and Greenland Seas, which would require to include a discussion on deep convection and OHC, which we excluded following the reviewer's suggestion. Winter 2025 would be great to analyse, but our model outputs do not (yet) extend to 2025.
We will also implement the reviewer's suggestion to split Section 5 into two sections by

merging the second and third paragraphs into a Discussion section and the first and fourth paragraphs into a Conclusion section. We will split the Discussion section into two subsections, the first one addressing discussion of the processes (equivalent to the current third paragraph of Section 5) and the second addressing limitations of the model and methods (equivalent to the current second paragraph of Section 5).

Specific Comments

Comment 1

Figure 1: Since a big part of this paper is showing that sea ice volume or mass is critical for understanding extreme ice extent loss events (e.g. line 166, 322, 326, etc.), you should include panels with the timeseries of volume anomalies in this figure. These are already Figures S1a and S4a, but they should be included as main figure panels as well. Additionally, it could be beneficial to compare to a hindcast that uses data assimilation (e.g. PIOMAS for the Arctic, SOSE for the Antarctic?) to see how well your model compares to some estimates of these variables.

Response:
We will add two panels to Fig. 1 (see updated version Fig. R3) showing the sea ice volume anomalies from the model and from PIOMAS and GIOMAS (SOSE does not seem to extend prior to 2008). We will comment those two panels in the main text in parallel of the comparison of the sea ice extent (l. 146-159).

Comment 2

Figure 1: Panels a and c are averages over some years – you should list the years they are averaged over in the figure caption and show the standard deviation to indicate variability.

Response:
We will make these modifications (see Fig. R3 for standard deviation).

Comment 2

Figure 3: This is a great figure! The map legend doesn't show white on white for the contour, so this should be fixed.

Response:
Thank you. This mistake will be corrected by adding a gray background to the legend (Figure R4).

Comment 2

Figures 3/4/5/6: These figures are nice, but the graphs overlain on the maps makes them hard to read, especially the center Antarctic one. Additionally, there are no y axis labels on the regional graphs. For figures 4/5/6, the bar charts axes limits aren't consistent, so it is hard to compare the panels with one another. Consider have a white, opaque background or reorganize the figures so that the graphs are separated from the maps and easier to compare, and please be sure to label axes.

Response:
We will improve those figures by adding a white background behind each inset panel (Figs. R4, R5 & R6). We refrain from adding a y-axis label to all regional panels, as it is the same for all and would therefore be redundant while cluttering the figures. We will clarify that the y-axis label and units are the same for all panels in the figure caption.

Figures: new versions

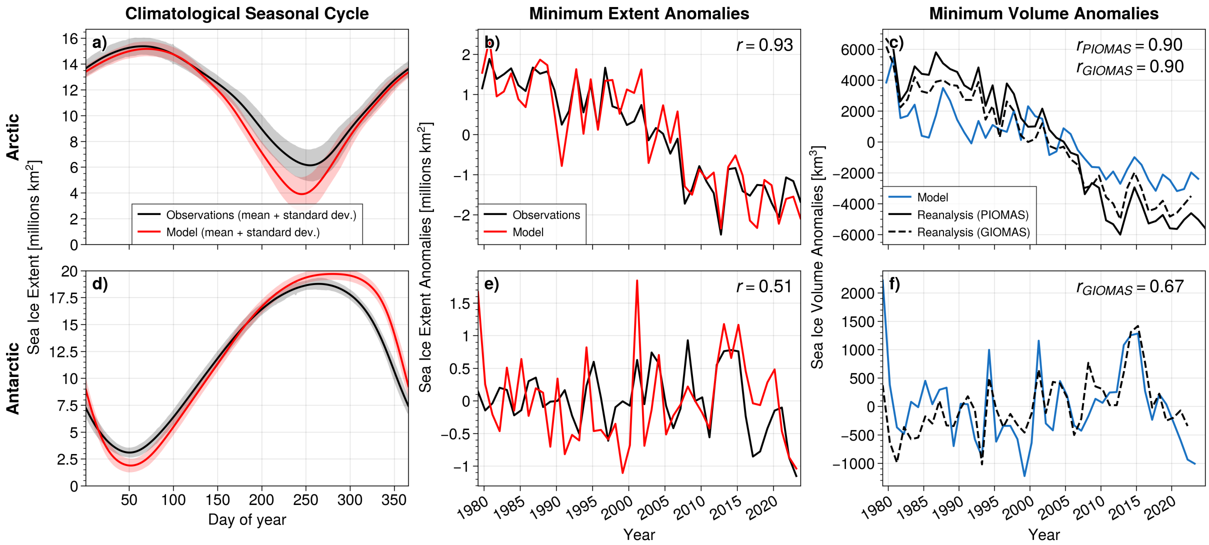


Figure R3: Figure 1, new version. Modified caption:
Comparison of observations-based products (black) versus model sea ice extent (red) and volume (blue). Observations-based products are the satellite-based NOAA/NSIDC Climate Data Record (CDR) for sea ice extent and the PIOMAS and GIOMAS reanalyses products for sea ice volume. Mean seasonal cycle of sea ice extent for (a) Arctic and (d) Antarctic. Shading indicates one standard deviation. Minimum sea ice extent anomalies relative to the mean seasonal cycle for (b) September in Arctic and (e) February in Antarctic. Minimum sea ice volume anomalies relative to the mean seasonal cycle for (c) September in Arctic and (f) February in Antarctic; correlations between observations and model are given in the top-right corner of each panel for the minima comparisons (panels b, c, e and f). Note that the time values include the month, meaning that the point for e.g. September 2000 is closer to the x-tick value 2001 than 2000. For more robust comparisons, all mean seasonal cycles used in this figure are calculated over the whole available satellite period (1979-2023).

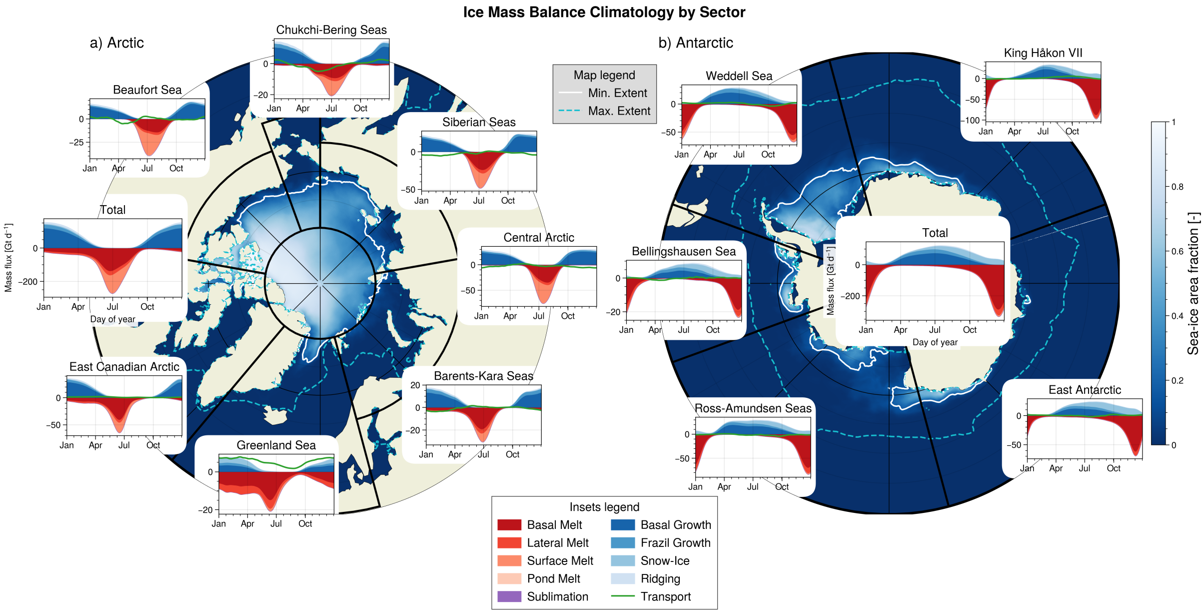


Figure R4: Figure 3, new version.

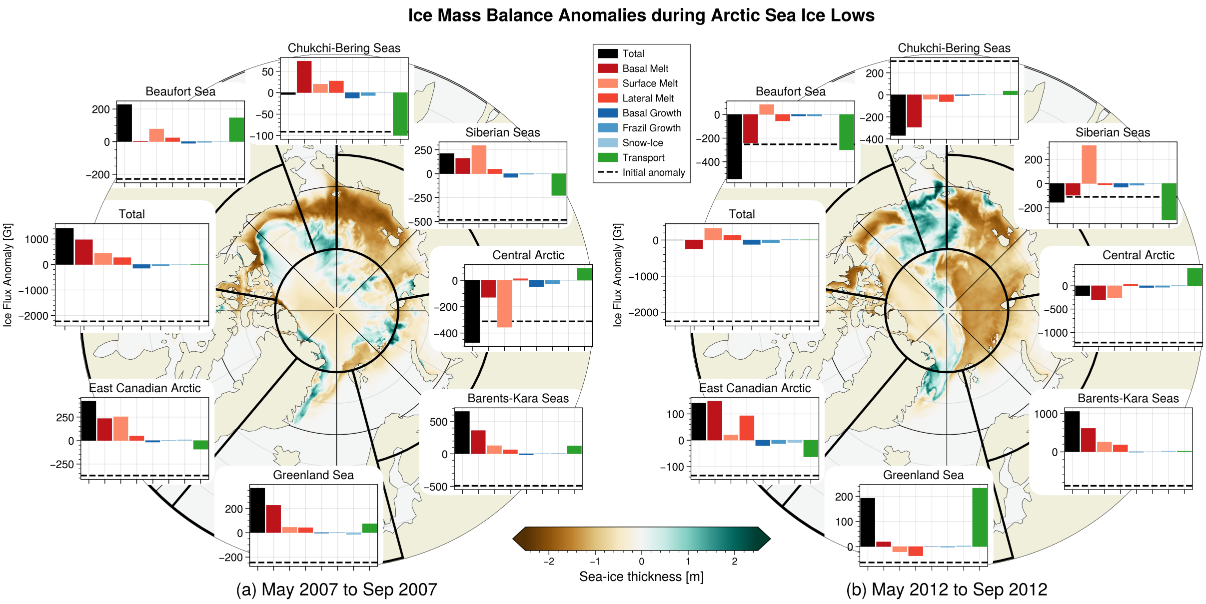


Figure R5: Figure 4, new version.

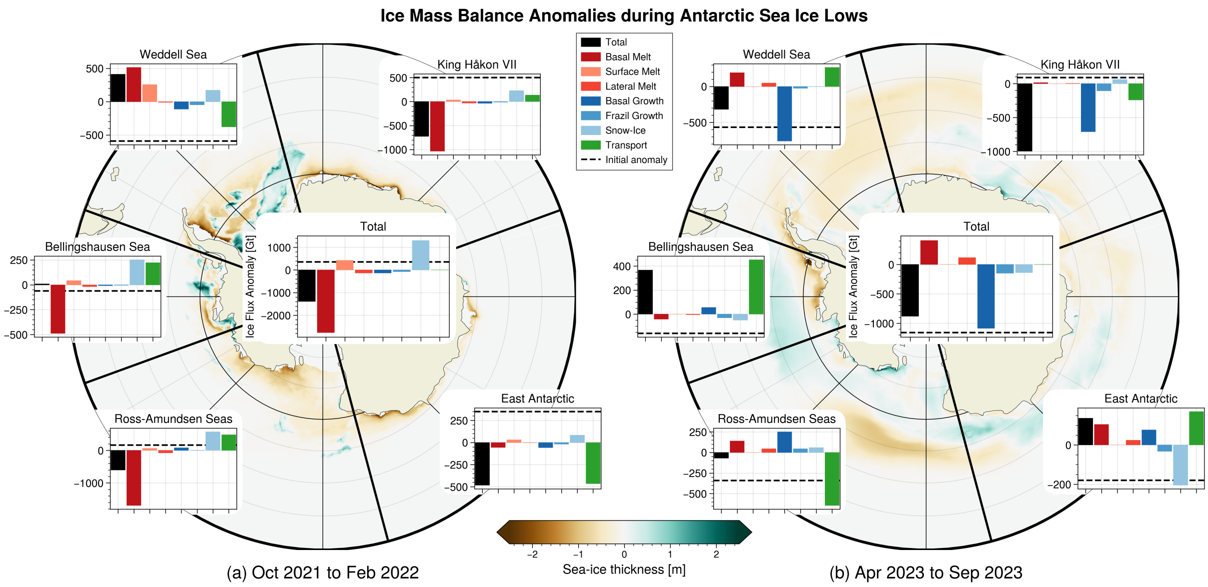


Figure R6: Figure 5, new version.