Responses to Reviewer 3

Anatomy of Arctic and Antarctic sea ice lows in an ocean–sea ice model

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General Comments

Manuscript Synopsis

This manuscript, using an atmosphere forced ocean-ice historical reconstruction attempts to analyse sea ice flux mass contributions for exceptional sea ice events relative to climatology. The paper uses the common methodology to compare and contrast sea ice events in the Arctic and Antarctic, as well as comparing/contrasting melt and freeze-up season events. The paper could provide a useful measure examining exceptional sea ice events, but suffers considerably by confusing and incomplete graphics as outlined in my major comments below. In particular, the pre-existing sea ice anomaly plays a huge role in allotment of mass balance fluxes into anomalous fluxes, particularly for melt events, where there is a definitive upper bound on sea ice removal (you cannot remove more ice than what already exists). The authors discuss this "pre-conditioning" (their term) in the text, but the lack of a graphical representation of this term in their budget can lead to confusing interpretation of the results, particularly by a reader seeking quick visual summarization of the results.

Response:
We thank the reviewer for their thoughtful and thorough revision. We address their comments below and believe implementing them will significantly improve the manuscript.

Major Comment 1

Fig. 4 & 5. I found the presentation of these figures very hard to follow, as the discussion relies heavily on an additional term (preconditioning or initial mass anomaly) that requires careful reading of the manuscript to draw out. As the figures stand now, it is very easy to convince oneself that positive flux anomalies mean a decrease in sea ice mass, when in actual fact they mean the opposite (increase in sea ice mass), but in a vast proportion of the sea ice thickness anomalies, this incorrect assumption does seem to visually confirm – and in some cases (Labrador Sea / Baffin Bay) seems to be wholly nonsensical. To make graphical interpretation much simpler:

Response:
We thank the reviewer for their comment. This major comment is at odds with comments from the other reviewers, which makes it particularly difficult to address. Please note that the other reviewers praised those two figures and don't seem confused by the sign

convention. We have attempted to find a middle ground. We provide more details below when addressing the detailed comments from the reviewer.

> The sea ice thickness anomaly must be over the same period (May to August) as the flux anomalies (and not just August).

Response:
We do not think implementing this suggestion would be appropriate. Showing the mean anomaly over the whole melt season would prevent any direct comparison between the map and the mass flux anomalies in the insets. Indeed, we are here calculating a mass budget, integrated over the melt season (and expressed as anomalies). Following this method, the ice mass budget explains the *difference* of ice mass between the beginning and the end of the melt season, not the *mean* state during the melt season:

$$\int_{May}^{Aug} \frac{\partial M}{\partial t} = M_{Aug} - M_{May} = \int_{May}^{Aug} \Sigma F_{mass} \tag{1}$$

In the current background map of Fig. 4, we plot a proxy (the thickness instead of the mass, as is standard in the literature) of the anomaly of M_{Aug} while the anomaly of M_{May} corresponds to what we call the preconditioning. We could potentially replace the thickness anomaly by the difference in state between May and August, but consider this would make the plot less easily comparable to other studies or data sources while providing a limited improvement in the interpretation of the results. As suggested below by the reviewer, we instead provide the preconditioning by sector in the inset panels.

> This still does not lead to a visual flux closure as it does not account for the initial sea ice mass anomaly (pre-conditioning in the author's terminology), which in many, if not all cases is the main offsetting factor. Therefore an additional "preconditioning" pseudo-flux should be added to the bar chart representing the initial mass anomaly (technically it should be for 30 April, but an average over April should be close enough if more convenient/smoother). I initially though you would need to convert this into a flux – but if I understand correctly the fluxes are already time integrated into mass gain over the 4 months? [You would likely not wish to add this pseudo-flux to the total, just leave it separate.]

Response:
We appreciate this suggestion and will include the mass anomaly on the first day of the period of interest (May 1st for Arctic, October 1st for Antarctic Summer 2022 and April 1st for Antarctic Winter 2023) as a dashed black line in the insets of Fig. 4 & 5, instead of in the Supplementary information (see Figs. R3 & R3). We will also clarify in the figure caption that "the net mass anomaly for each sector at the end of the period of interest can be estimated by summing the preconditioning anomaly (dashed line) and the total mass flux (black bar)." We hope this will improve the interpretation of the figure. We believe it now makes our initial analysis clearer, by showing that the total positive flux anomaly in sub-Arctic seas is of similar amplitude as the negative preconditioning term, except in the Greenland Sea sector in 2012 where advection plays a important role, as already properly mentioned in

the initial manuscript. We will correct our analysis of the Beaufort Sea in 2007, as this new format seems to indicate that preconditioning could play a role, though by changing the dynamical term rather than the thermodynamical ones. We also see a good agreement for Weddell Sea in 2022, validating our initial assessment.

> Only then will it the figure visually balance the fluxes in the sector with the mass loss/gain contours.

Response:
We agree that adding the preconditioning now leads to a closure of the budget.

> It will also visually confirm large segments of the text which discuss that the apparent flux anomaly is actually due to "pre-conditioning" (i.e. the initial mass anomaly), with the increase in anomalous ice mass fluxes (i.e. mass growth) being largely offset by the initial anomaly. In other words, there is an increase in anomalous ice growth largely due to there being less ice than climatology to melt!

Response:
This was indeed our initial interpretation, and we believe the reviewer's suggestion to add preconditioning in the insets now makes it clear.

> Ultimately, the usage of anomalous fluxes seems to be less than informative, the size of the flux ultimately being hugely dependent on the underlying sea ice volume. A better strategy (with no guarantee of success) might be to use normalized (either by total ice volume, or ice volume change, the latter assuming a definitive melt/freeze sign by sector) fractional flux anomalies. For instance does the fraction of basal sea ice melt increase or decrease from climatology in the exceptional years? Note: The fractional flux could be greater than 1, or less than zero. Sign conventions, for lack of better terminology, might be messy. I do not suggest pivoting to such an analysis now, I would view this manuscript as a learning process in best practices in this regard.

Response:
This is also a suggestion from Reviewer 2, that we have explored (see response to Reviewer 2, Figs. R1 & R2). The normalization introduces significant challenges that make the interpretation of the results even more confusing and complicated than the current methodology, to our opinion. Expressing the mass fluxes as a proportion of the total annual ice loss or growth (Fig R2) is a reasonable approach to investigate interannual variability. Yet, a major caveat of this approach is that the transport term cannot be included, as it can be either positive or negative and therefore cannot be expressed as a percentage of total mass loss or gain.
We like the reviewer's view that this manuscript can participate to the learning process of how to best investigate budget analyses. It is worth noting that this is not specific to ice mass budgets, and is a shared issue with other topics, such as ocean heat budget analyses (e.g. for marine heatwaves).

Examples of confusing aspects:

- (a) Erroneous statement of Major Comment 3.
- (b) Statement concluding Subsection 4.1 (Minor Comment 1)
- (c) Using text explanations to highlight effect of pre-existing mass anomaly (preconditioning) without additional graphical assistance (ll. 311, 314, 338, 360, 363, 383, 394, 489–505).
- (d) Large sea ice growth flux anomaly in Baffin and Hudson Bays/Labrador Sea sector with only a small manifestation of sea ice loss in the Canadian Archipelago.

Response:
See Major Comment 3 and Minor Comment 1 for our response. For the text explanations of preconditioning (point c), we will now be able to refer to Fig. 4 & 5 explicitly and hope this makes the text clearer. We do not understand the reviewer's point d, as there is no growth anomaly. Did the reviewer mean a positive melt anomaly? If so, first, the absolute value of those anomalies is relatively small compared to other sectors, and this means a lack of melt that is clearly explained by the pre-conditioning term, as initially suggested.

Major Comment 2

Ocean Heat Content: I am not entirely convinced of all the claims made in the manuscript with regards to increased heat content leading to increased basal melt. (a) The stated alignment of the increased heat content (Figure 6; red; numerically positive) and decreased sea ice volume (Figure 5b; red, numerically negative) do not line up as well as suggested as demonstrated in the enclosed animated gif which purposes to overlay the two (I see a lot of alternating red/blue). Caveat: As with my comments with regards to Figures 4 & 5, the heat content (Figure 6; April to September) does not align in time perfectly with the sea ice volume (Figure 5b; September) either. (b) The choice of the 100-200m heat content is a little confusing, and not justified. The winter time mixed layer depths [Uotila et al., 2019] range from 100 to 300m, which means an increase or decrease in the mixed layer could have opposing tendencies in the top 100m and 100-200m – if the mixed layer increases one might expect the upper layer to warm while the lower layer cools (increased surface mixing with the warmer below mixed layer waters), with the opposite cooler surface, warmer 100-200m if the mixed layer decreases (isolates the surface). (c) The previous point is very well illustrated in Figure 2 of the reference Zhang et al. [2022]. Indeed, the zero lag in that paper would seem to require an accompanying negative anomaly in 100-200m heat content. The mechanism also requires a long period lagged relationship that I see no evidence of here. (d) I might speculate that the heat contents are sea ice driven: Lower sea ice creation implies lower brine rejection and increased stratification (isolation) of the surface waters, increasing the 100-200m heat content. This would explain the relative uptick in Antarctic September heat content relative to April heat content seen at the end of the pan-Antarctic time series in Figure 6 – but it is also difficult to see if this is an isolated event, or a common occurrence. (e) As the authors state, the increased/decreased basal melt/growth may be driven by the atmospheric forcing, especially in low sea ice thickness states as the downward heat fluxes directly heat the ocean surface layers. (f) I do not advocate that my speculations, or any of the alternative explanations are more or less likely than the mechanisms suggested by the authors. I do suggest there is a lack of current evidence in the manuscript for any conclusions connecting the heat content to the loss of sea ice volume. (Seasonal) Lead/lag relationships may be critical.

Response:
We appreciate the reviewer's comment, which is also supported by Reviewer 2's comments. We agree with the overall statement that we do not provide a strong proof that OHT leads to sea ice mass decrease, though we tried to avoid making such a claim in the initial manuscript and attempted to rather suggest that out model is in line with other studies. In any case, 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, is precisely in preparation 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.

Major Comment 3

Erroneous statement: l. 328. The manuscript states there is an increase in basal melt in the Chukchi Sea in July 2007. Supplementary figure S2m-o shows a blue (positive) basal melt anomaly in the Chukchi Sea. But positive flux anomalies are defined as anomalous gain of ice mass. Therefore this is not an increase in basal melt, but a decrease in basal melt. If I am incorrect, please correct me, but this does demonstrate my confusion generated by the figures. I suspect this positive basal melt anomaly is completely due to "preconditioning," – i.e. there is a anomalous lack of sea ice to melt.

- Similarly, the Chukchi and Bering Seas sector shows a net positive basal melt flux (so again decreased basal melt) in the bar charts of Figure 4a.
- ll. 329. If I am not confused, and the basal sea ice melt is actually decreased, the connection to ocean heat transport may no longer be appropriate, however, you should have stated (the perhaps obvious, nevertheless still useful) that there are observations of increased northward heat transport. I briefly contemplated the authors meant there was an observed southward transport of heat to match the flux anomaly.
- The statement "this increase (in sea ice mass) is not sufficient to compensate the export of ice" is correct – but again only added to my level of confusion.
- Please check your characterization of your flux sign convention elsewhere in the manuscript. Having noticed this, I cannot convince myself there may be other instances where I have matched my interpretation of the sign convention in the graphics to match the text commentary (i.e. I can be easily confused into agreement).

Response:
We believe there was here a lack of clarity from our part on the exact location of the anomaly, rather than an erroneous interpretation. We were referring to the small red anomaly visible in July (and August, though less clear) just offshore/north of the Chukchi Sea, over Barrow Canyon. Barrow Canyon is a known hotspot for advection of Pacific water into the Central Arctic. We interpret this negative basal melt anomaly (also slightly visible in the surface melt as a lack of anomaly) as increased warm inflow, matching the observation-based documented inflow (Woodgate et al. 2010). This was a clear mistake from us to not mention the exact location of this negative anomaly, and we will correct it in the revision. We hope this answers Major Comment 3.

Minor Comments

Comment 1

ll 362-363: "It (2012) is therefore a low not only in sea ice extent, but also in volume, in contrast with summer 2007." This comment cannot be made here without specifying you are excluding the seasonally ice covered Labrador Sea / Baffin Bay and Greenland Sea and Barents-Kara Seas sectors as previously mentioned in the text. Readers just reading the section concluding remarks (it does happen) will immediately refer to figure 4 and both conclude you have this backward – 2012 has no change in volume, and 2007 has a low in sea ice volume (i.e. invert your sign convention). But if you also include the pre-conditioning flux this will also be rendered visually correct.

Response:
While we might not fully understand the reviewer's comment, we do not think that excluding the sub-Arctic Seas (Labrador Sea/Baffin bay, Greenland Sea and Barents-Kara Seas) is necessary for our statement to be true. First, Fig. S1.a, which shows the SIV including the subarctic seas, indicates a minimum in September 2012, justifying our statement. Second, those seas are almost ice free in a climatological sense at the end of the melt season. Therefore, including or excluding them does not change significantly the estimate of the August sea ice volume nor extent. We think that including the preconditioning in Fig. 4 clarify this, as anticipated by the reviewer.

Comment 2

ll. 81-82. There are considerably more examples and research concerning climatic implications of changes to sea ice [e.g. Screen, 2013] – I would normally provide a more extensive list, but I am stressed for time here (no conflicts in solitary suggestion).

Response:
We thank the reviewer for the suggested reference that we will incorporate, along with others (including Honda et al. 2009, Strey et al. 2010). Note that many of the references investigating the climatic impacts of sea ice loss tend to focus on long-term (multi-year) loss, rather than a single-year minimum.

Comment 3

ll. 110-111. The tri-polar grid is designed to remain eddy-permitting in the Arctic (grid cells of order 12km). I should probably know this, but even so, others readers might not. Does the eORCA025 grid remain eddy-permitting throughout the Antarctic domain?

Response:
Yes, it remains eddy-permitting in the Antarctic domain, with an effective resolution around 7 km in the Ross and Weddell Seas, which is slightly lower than the Rossby deformation radius in those regions.

Comment 4

l. 117. Is it standard to have equal numbers of sea ice and snow layers (2+2)? I obviously do not know, but I seem to recall the multi-layer thermodynamics sea ice models I have dealt with have more sea ice layers than snow layers. Is there a rationale for this?

Response:
This is the default setup in NEMOv4.2.2 and we kept it that way. Other configurations with more sea ice layers than snow layers exist (e.g 2+1; 10+5 as the new default in NEMOv5), but we do not know of any definitive rule about this.

Comment 5

l. 151: entire time series. I assume this is your entire analysis time period (1979-2023), but perhaps it is worth repeating here? And the last two decades are presumably 2004-2023?

Response:
We will clarify that: "[...] when calculated over 1979-2023, but increases in the Antarctic when considering only 2004-2023."

Comment 6

Figure 1b/d: From the looks of the plot, I assume the numerical minimum for both the Arctic and the Antarctic has a time value assigned by exact time in year (year + month + day), or in other words, the Arctic sea ice minimum for 2009 is more closely aligned (2009.75, or slightly to the left) of the 2010 grid line than the 2010 (2010.75) minimum. The same applies to the Antarctic, but in this case it is more closely aligned with the correct calendar year. If so (or if not) this should be communicated in the caption. The same question can apply to Figure 6 – with not as much consequence – are the annual mean (+0.5) aligned in time with April (+0.25) and September (+0.75), or are they offset by 0.25? Similarly figures S1 and S4.

Response:
This is indeed the case, meaning the September year X anomaly is indeed closer to year X+1. We will clarify this in the caption: "Note that the time values include the month, meaning that the point for e.g. September 2000 is closer to the tick value 2001 than 2000."

Comment 7

l. 171. Units are slightly confusing, suggest reordering somewhat (in kg for sector analysis, and kg m-2 for individual grid points). However, it is also important this information be added to the figure captions – at least in the first instance of occurrence in a figure. (Figure 3, 4, 5 for sector flux values; Already included for grid values (S2, S3, S5, S6).

Response:
We will implement this suggestion. Note that in the figures, we actually converted the kg to Gt to avoid exponents and to make it easier to compare to other studies.

Comment 8

l. 192. Keen et al. [2021] is an extension of Keen and Blockley [2018] to a multi-model analysis. I would think that (no conflicting interest) the original budget analysis would be a more appropriate citation. Keen et al. [2021] would remain applicable for placing this manuscript's results within the CMIP context (ll. 224, 226, 289, 290). Note the DOI is correct (https://doi.org/10.5194/tc-15-951-2021, but the link does not work properly (across two lines, with the automatic line numbering interfering?) for Keen et al. [2021].

Response:
This is a good suggestion, we will incorporate it. We expect the broken doi to be fixed at the proofreading stage.

Comment 9

The sector Labrador Sea and Baffin Bay also includes Hudson Bay, which is likely an equal contributor to the sea ice mass changes over the May-August period. While Labrador Sea / Baffin and Hudson Bays is likely too lengthy for labelling purposes, I favour the more accurate East Canada Arctic moniker (Eastern Canadian Arctic is more grammatically correct, but longer).

Response:
We agree with the reviewer and will rename this sector to "East Canadian Arctic". The name "Labrador Sea-Baffin Bay" is the one used in Koenigk et al. (2016) from which we took the region definitions: we will clarify in the text that we have modified the nomenclature for accuracy purposes.

References

Honda, M., Inoue, J., and Yamane, S.: Influence of low Arctic sea-ice minima on anomalously cold Eurasian winters, Geophysical Research Letters, 36, `https://doi.org/10.1029/2008GL037079`, 2009.

Strey, S. T., Chapman, W. L., and Walsh, J. E.: The 2007 sea ice minimum: Impacts on the Northern Hemisphere atmosphere in late autumn and early winter, Journal of Geophysical Research: Atmospheres, 115, `https://doi.org/10.1029/2009JD013294`, 2010.

Figures: new versions

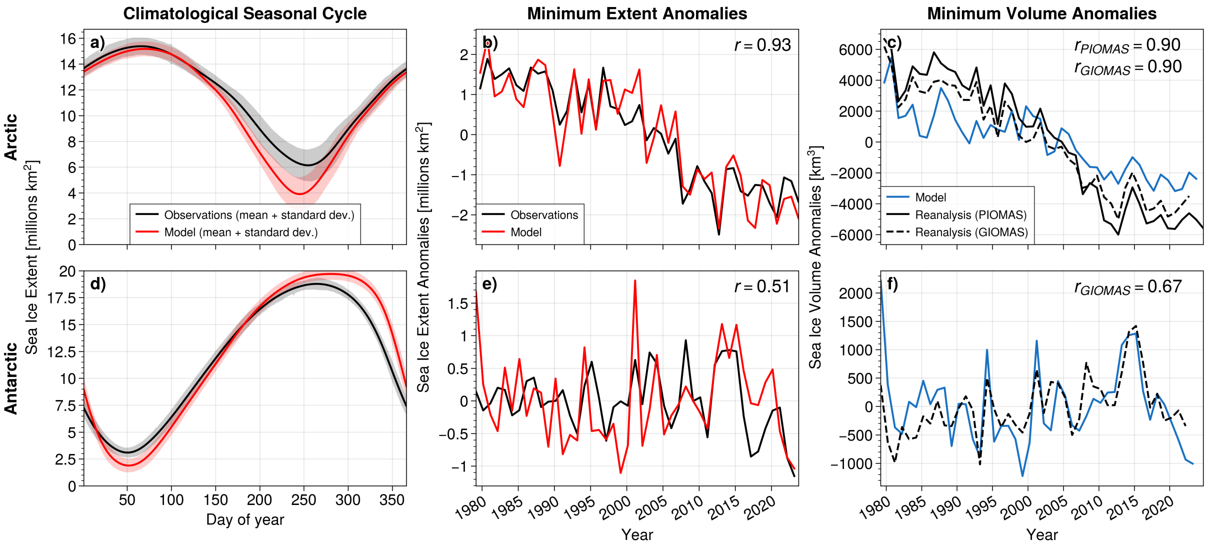

Figure R1: Figure 1, new version. Modified caption:
Comparison of observations-based products (black) versus model sea ice extent (red) and volume (blue). Observations-based products are the satellite-based NOAA/NSIDC Climate Data Record (CDR) for sea ice extent and the PIOMAS and GIOMAS reanalyses products 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).

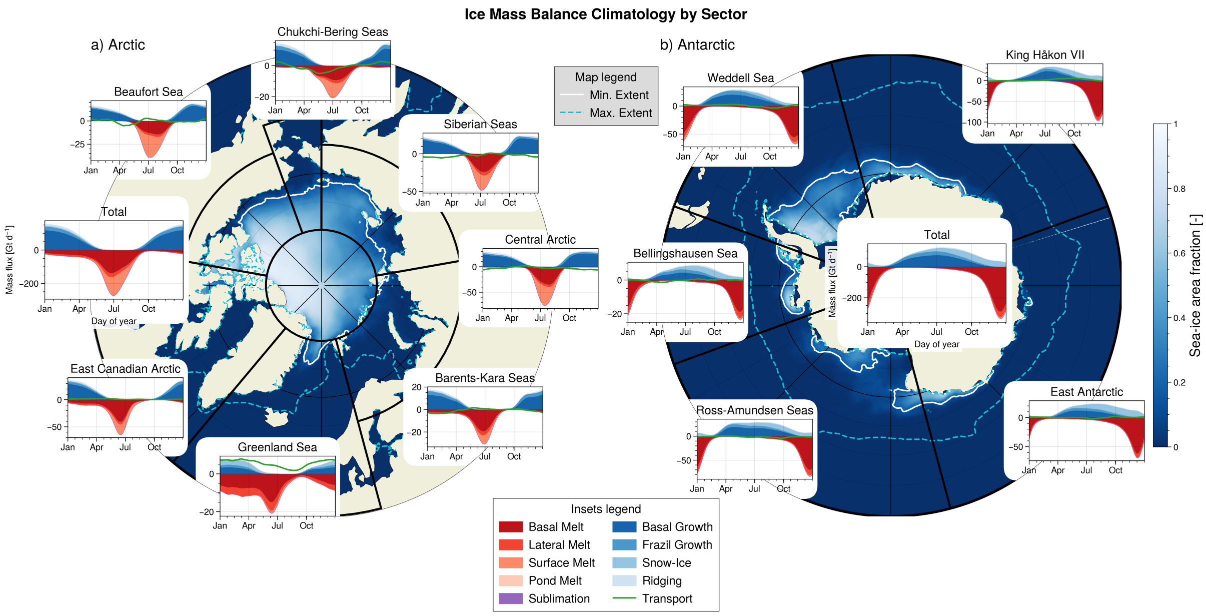

Figure R2: Figure 3, new version.

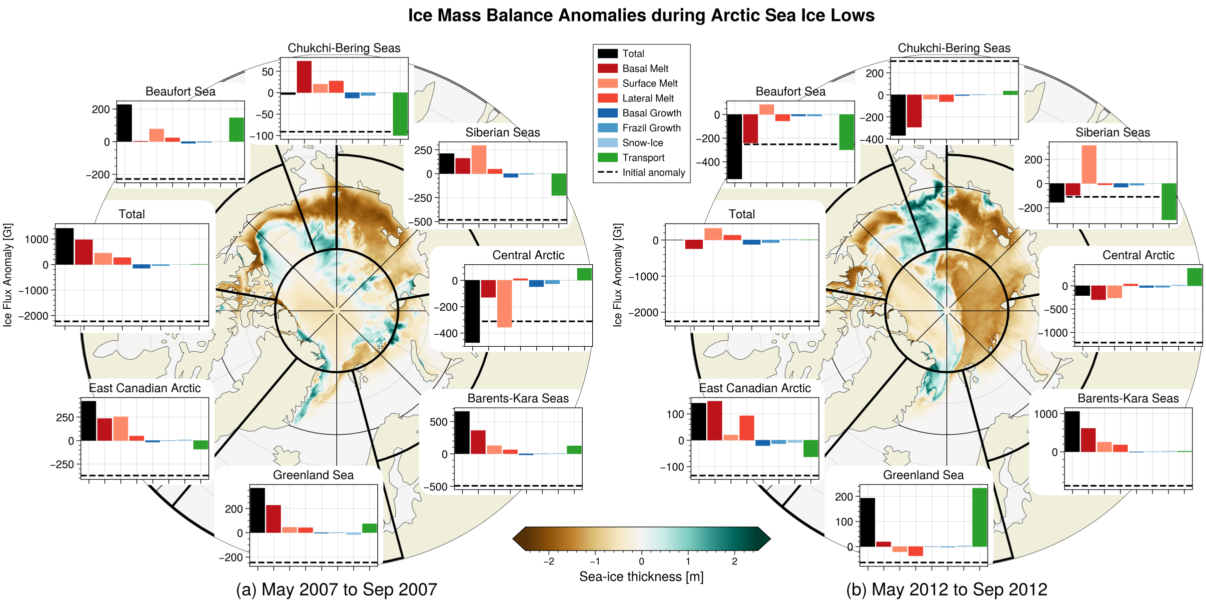

Figure R3: Figure 4, new version.

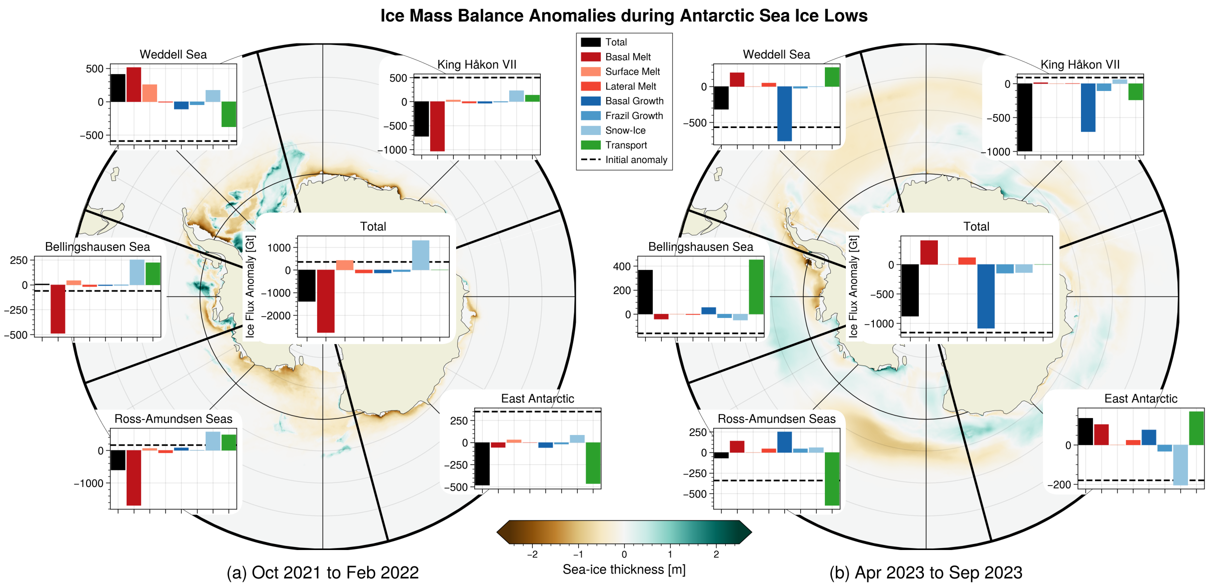

Figure R4: Figure 5, new version.