Responses to Reviewer 1
Anatomy of Arctic and Antarctic sea ice lows in an ocean–sea ice model

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General Comments

General Comments

Richaud et al. present an analysis of record lows in Arctic and Antarctic sea ice extent using mass balance decomposition in an atmosphere-forced ocean–sea ice simulation. Their findings highlight that interactions at the ice–ocean interface are common to the four case studies examined, and that the role of basal melt in particular is increasing with climate change. The main novelty of this study is that different sea ice low events from both hemispheres are analysed in a self-consistent framework, which is distinguished from previous literature examining single cases using a variety of observational data sources and/or model configurations. I agree this approach has merit. While there is not much new insight into the individual cases (which have been studied extensively), the more generalised understanding and comparison of sea ice lows makes up for this. The manuscript is structured logically overall, and the figures are very clear. I like the design of Figs. 4 and 5 in particular, reflecting nicely the use of "anatomy" in the manuscript title.

Response:
We thank the Reviewer for their thoughtful review and believe that their comments and suggestions will help us improve the manuscript.

> I would appreciate some clarification on how anomalies in the mass budget terms are calculated. This may reflect a misunderstanding on my part or a potential issue in the methodology. Lines 196–204 explain how the climatological seasonal cycle is calculated, but it does not seem like the long-term trend is removed from the data. This would cast doubt on the magnitude and sign of the anomalies presented in Fig. 4 (and, to a lesser extent, Fig. 5), and hence the interpretation, depending on the strength of the trends in each term. For example, Fig. 1.b has clearly not been detrended. In that case it doesn't really matter for the purpose of the plot, but if the corresponding time series for the mass budget terms looked like this then everything after roughly the mid-point of the time series would be a negative anomaly. Despite all this, Fig. 4 clearly show anomalies of both sign, and the resulting interpretation is consistent with previous studies, which suggests either (1) the trends are sufficiently small for this to not matter too much or (2) the trends have actually been removed. I would have thought the trends should be removed (and, if only stylistically, also for Fig. 1b, for consistency with the description of a sea ice low on l.38 as "when the sea ice extent becomes significantly lower than the trend line").

Response:
As the reviewer supposed, we do not remove the trend for the mass budget terms, for two main reasons.

1. The first one is physical and is a question of perspective: one can consider that what really matters is the specific short-term event(s) of the year, therefore excluding the long-term trend from the anomalies, as suggested by the reviewer. But one can also consider that the external forcing represented by the trend is part of the causes of sea ice lows, and that it should therefore be included in the anomalies (as we do here). Both perspectives are valuables, have pros and cons, and the choice between both methods should be guided by the overarching research question. In our case, we compare both hemispheres and different seasons, on top of different years, and consider that the differing trajectories in sea ice extent between the Arctic and the Antarctic are an interesting component of the causes of the sea ice lows. We therefore prefer to keep the trend in the signal. Note that this is reminiscent of the considerations around the baseline choice in the marine heatwave literature (Smith et al. 2025).

2. The second reason for keeping the trends in the signal is a technical reason. The trends are highly seasonal: for example, the melting terms are null during the whole growth season, meaning there is no trend for that time of the year, while there can be some trend during the melt season. Removing an annually calculated trend (linear or high order) would then lead to non-null values for the melting terms during winter and for growing terms during summer (Figure R1). This is unphysical and more difficult to interpret and justify. Removing a seasonally-varying trend would only partially solve the problem, as it would still lead to an arguable, positive value for the melt term at the growth-melt transition period if melting starts later than usual. This would add significant complexity not only to the method, but also to the interpretation of the results and of the trends, as each term would require its own seasonality.

For those two reasons, we refrain from removing the trend from the anomalies. We acknowledge that the manuscript did not clearly state this choice and we will correct this

omission by adding the following sentence in the methods:
l. 201: "The trends are not removed from the anomalies, to compare the impact of the differing trajectories of sea ice extent between Arctic and Antarctic. This means that the long-term changes are included in the analysis of sea ice lows."

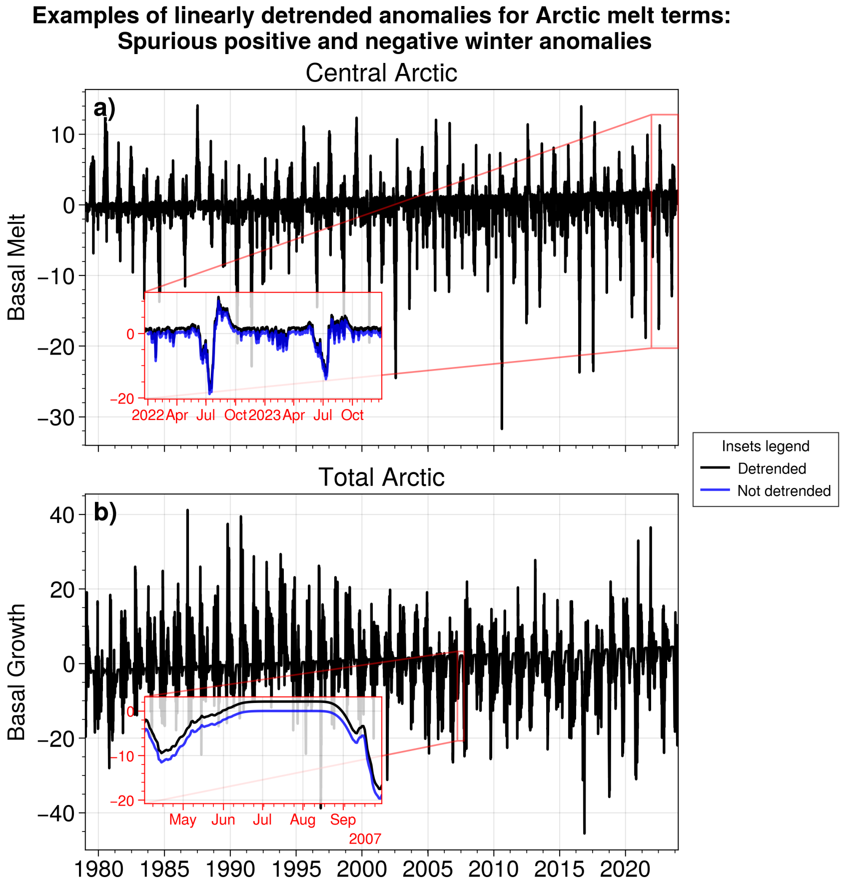

Figure R1: Examples of linearly detrended anomalies, for a) basal melt in the Central Arctic and b) basal growth for the Total Arctic. A spurious positive trend seems to emerge, but is actually due to the artificial creation of a positive trend only during the winter for basal melt and summer for basal growth, seasons during which the anomalies are supposed to be null. Inset panels provide a zoom over shorter periods (2022-2024 for panel a, Apr to Sep 2007 for panel b) to exemplify the impact of removing the trend. The blue lines in the inset panels show the non-detrended anomalies (as used in the manuscript).

> Also, while the writing is clear enough, there is a somewhat common thread of vague statements that should be clarified, expanded upon, or removed (see specific comments). If the authors are able to address these among other (overall minor) concerns, I would be happy to see the study published in The Cryosphere.

Response:
We will simplify the writing and clarify any vague statements.

Specific Comments

Comment 1

L10: “The Antarctic 2022 event was partly driven by a strong interplay between dynamic and thermodynamic processes”: this is vague.

Response:
We will clarify the sentence as follows: “The Antarctic 2022 event was generally driven by dynamic processes transporting sea ice towards sectors where more melt occurred.”

Comment 2

L13–14: “highlights the potential of the ice mass budget decomposition [. . .]”: do you mean in terms of applying such a decomposition to observations specifically (linking to the final paragraph of the discussion)? Not clear what this means otherwise, as mass budget decomposition is a fairly standard technique.

Response:
We agree with the reviewer that the ice mass budget has become a fairly standard technique, but not systematically used nonetheless, and often simplified as a dynamic versus thermodynamic contribution, while the decomposition into basal versus surface terms is less often used. We will clarify this as follows:
l.14 “highlights the potential of the ice mass budget decomposition to disentangle oceanic and atmospheric contributions in the evolution of the sea ice state in a changing climate.”

Comment 3

L37–39: this is not a definition: what is ”significantly lower” or ”noticeable”? I suggest removing ”defined here as”, e.g.: ”[. . .] occurrence of sea ice lows—instances when the sea ice extent becomes [. . .]”. Then the next sentence then follows naturally as-is, without having to give a specific definition of ”sea ice low” at all.

Response:
We will implement this suggestion.

Comment 4

L44: “was quickly ruled out (Schweiger et al., 2008)”: I think it is important to note that this is according to a modelling study.

Response:
We will mention this, l. 43-44: “[...] was quickly ruled out by a modeling study (Schweiger et al. 2008)”

Comment 5

L50–51: again, I think it is important to note that this is according to modelling studies (as in both references here).

Response:
We will specify the data sources for those studies:

l. 49-51: "Studies based on reanalysis data suggested that a strong summer cyclone occurring in August could have played a role (Simmonds and Rudeva, 2012; Lukovich et al., 2021), but modeling investigations argued that the impact of this cyclone on ice loss was minimal (Zhang et al., 2013; Guemas et al., 2013)."

Comment 6

L53–54: I suggest adding the month in brackets, as done earlier in the sentence for spring.

Response:
We will add the month, l. 54: "in austral summer 2017 (February)"

Comment 7

L74–77: (i) I'm not sure it can be both "striking" and "expected"; I would just go with "striking". (ii) I would suggest rephrasing the first part to emphasise that both cyclonic and the opposite anticyclonic conditions are plausible drivers. (iii) Remove "etc.".

Response:
(i) We will remove "though expected". (ii) We will rephrase, l.73: "the diversity of candidate drivers is striking: both anticyclonic (in 2007) or cyclonic (in 2012) atmospheric conditions, [...]". (iii) We will remove it.

Comment 8

L71: Suggest starting a new paragraph with "While come causes [. . .]", as you are now discussing both Arctic and Antarctic sea ice low events whereas the current paragraph is just discussing the latter.

Response:
We will split this long paragraph as suggested.

Comment 9

L100: "It also highlights [. . .]": this sentence summarises findings, whereas the previous sentence explains methodology. Suggest rephrasing this to, e.g., "Our analysis reveals [. . .]".

Response:
We agree, l.100: "Consequently, our analysis highlights the dominant role [...]"

Comment 10

L106: "Section 5 [. . .] provides a perspective [. . .] on the potential for future sea ice lows": there is no such perspective given in section 5. The only part of section 5 relevant to this is the reiteration of the trends in mass budget terms (L481–484), but there is no discussion on the potential of future sea ice lows. In my view, this is a key aspect missing from the discussion section and should be added (e.g., a paragraph, perhaps beginning with the information from L481–484, and linking in the results of section 3).

Response:
This was indeed an involuntary omission that we will correct by adding the following

sentences in the discussion:
"[...] in order to reach a state that is statistically less likely. On this basis, a number of criteria can help us anticipate future sea ice lows. First, the thinning and shrinking of sea ice leads to more potential for preconditioning through thinner ice that can melt away earlier in the season. Thinner ice is also more mobile because of a lower tensile strength, potentially increasing its dynamics (Olason & Notz, 2014; Docquier et al., 2017). Heat conductivity is higher through thin ice and therefore modifies the thermodynamics and the melt onset (Bitz & Roe, 2004). Considering that all investigated sea ice lows are a combination of factors, future sea ice lows are likely to also be triggered by several co-occurring events. Changes in the frequency and intensity of such events is not clear. Arctic cyclones are not expected to become more frequent in the future (Crawford & Serreze, 2017), but both Arctic and Antarctic cyclones have intensified (Zhang et al., 2023; Chemke et al., 2022). Heatwaves, especially marine heatwaves, have increased in frequency and intensity in the Arctic (Huang et al. 2021) and could therefore contribute to more sea ice lows."

Comment 11

L120: "lateral melting through ice floe size parametrisation": suggest citing Lüpkes et al. (2012) for this.

Response:
We will add the reference.

Comment 12

L121: "level-ice melt ponds": suggest adding a citation (Hunke et al., 2013) for this too as it may not be obvious to all readers that "level-ice" refers to a specific parameterisation choice.

Response:
We will add the reference.

Comment 13

L140: I am satisfied with the justification that the bias in the forcing, and hence simulation, can be overlooked by removing the mean/trend and examining anomalies. However, I wonder why the authors decided to use ERA5 as atmospheric forcing despite known issues (the authors themselves cite a study from 2019) with temperature biases in the polar regions specifically. What is the benefit of using ERA5 over another reanalysis, for example? It would be good if the authors could add a bit more here to justify the choice of ERA5, even if it is just on practical grounds such as data availability or suitable resolution.

Response:
We use ERA5 mainly for practical reasons, as it covers the right period at a proper horizontal and temporal resolution; it is also a well-validated, often used reanalysis product for forcing ocean-sea ice models. Finally, it is updated in near-real time, which allows for operational simulation of recent events, for other projects. It is worth noting that other reanalysis products exhibit similar biases in polar regions, for the same reasons, and that ERA5 seems to behave rather better than most other products (Batrak & Müller 2019).

We propose to clarify this by adding, l.122: "[...] extracted from the ECMWF ReAnalysis v5 (ERA5, Hersbach et al., 2020), a well validated, regularly updated forcing set at the 1/4° horizontal resolution "
and by modifying, l. 145: "[...] due to lack of representation of snow over ice in the models used to produce most reanalysis products, including ERA5 (Batrak and Müller, 2019)."

Comment 14

L164–167: I think this important justification for using a model analysis would make more sense at the beginning of section 2.1 or perhaps somewhere towards the end of section 1, because at this stage the model has already been introduced/described.

Response:
We agree, we will move those 4 sentences at the beginning of the last paragraph of section 1; it will actually provide a great transition with the previous paragraph.

Comment 15

L183: do you mean "become clearer in Section 3" (this seems to be discussed there, on L232–233, L282–283).

Response:
Yes, this was a typo, thank you.

Comment 16

Figure 2: I'm not sure this figure adds much and is potentially a little confusing. The arrows point upwards for positive terms which corresponds to increasing ice mass. This works visually for, e.g., basal growth (mass moves from the ocean to ice), but it does not work for the snow–ice term (the arrow is consistently drawn upward for the sign convention, but visually this is like mass moving from ice to snow). The transport term makes sense visually but does not match the upward/downward = positive/negative flux. I realise this is explained in the caption, but overall the diagram itself does not consistently illustrate the sign convention or the physical processes (the latter being the more intuitive use of such a figure). I would suggest removing as the budget decomposition is fairly standard and in any case it is intuitive enough which terms increase or decrease sea ice mass. As for the sign convention, this is stated clearly enough in the main text and helpfully repeated in the captions of Figs. 4–5, so again I do not think the figure is needed.

Response:
We understand the reviewer's point and agree that arrows referring to physical processes is also often used for such figures and can seem more intuitive. However, as this figure provides a visual summary of all the processes we investigate in this paper, and not all readers will be familiar with all the sea-ice mass balance terms, we think that it is useful to keep it. Moreover, comments from the other reviewers make us think that the sign convention is not so obvious, and that this schematic illustration can help clarify it. Could the editor provide a third opinion on whether we should keep this figure or not?

Comment 17

Figure 3: is it possible to arrange this horizontally, as in Fig. 4?

Response:
A new horizontal version will replace the previous one. See Figure R4.

Comment 18

L225: ”slightly higher”: some estimation/quantification should be given (e.g., is it a few percent?)

Response:
On top of a different time period, Keen et al. 2021 restrain their study domain to the high Arctic (similar but not identical to the sum of our Barents-Kara Sea, Siberian seas, Central Arctic, Beaufort Sea and Chukchi sea north of Bering Strait sectors). Therefore, we cannot give a direct comparison. Nonetheless, we propose to provide an estimate of the difference and will rephrase, l. 225: “The absolute magnitudes are 25 to 40 % higher than those of the ensemble mean reported in Keen et al. (2021) for a similar though not identical domain. This could be due to differences in the study domains and periods of interest, and to the overestimated amplitude of the seasonal cycle in ice extent in the model used here.”

Comment 19

L226–227: it might also be related to the absence of atmospheric feedbacks? Anyway, this is a relevant limitation of the methodology which should be mentioned in the discussion with some consideration of potential impacts on the results.

Response:
This is a good point that we will incorporate by adding a sentence, l. 227: “It could also be related to the lack of atmospheric feedbacks in our model, as coupling the ocean and sea ice to the atmosphere (as is the case in the CMIP6 models) should dampen the sea ice response to atmospheric or oceanic perturbations.” Regarding those limitations in general, they are already mentioned in the Discussion (l. 507-530), when we discuss how the biased mean sea ice state could impact the amplitude of the mass fluxes.

Comment 20

L246–247: could you expand on/clarify this comparison? The study cited is examining the sensitivity of a similar model’s mass balance to the discretisation of the ice thickness distribution. The statement that ”some quantitative discrepancies exist” is vague.

Response:
Here again, we will expand this comparison, as suggested by the reviewer. please note that those comparisons have limited values, as the backbone model is the same so the data are not independent, and the methodologies differ on the area and period of interest. Moreover, our sea ice volume climatological seasonal cycle seems closer to the GIOMAS reanalysis than that of Massonnet et al. (2019). We will rephrase this comparison as follow, l. 247: “They are also in good qualitative agreement with the S1.05 experiment from Massonnet et al. (2019), though some quantitative

discrepancies exist. For example, our model exhibits a lower basal melt and frazil ice growth in the summer and fall seasons, likely due to a different domain definition and sea ice volume state in our simulation. Winter and spring values agree very well with the Massonnet et al. (2019) study."

Comment 21

L248–251: This paragraph comparing the Arctic and Antarctic is quite short. If there is not much else to say, I would suggest merging it with the end of the previous (Antarctic) paragraph. Alternatively, it could be expanded by drawing some conclusions, such as expecting to see more sea ice lows in certain regions of the Arctic but more uniformly around Antarctica. Also, why is it interesting to note similarity of the Greenland Sea sector with the Antarctic? Presumably, it means we might expect similar mechanisms for sea ice lows occurring in those sectors.

Response:
This paragraph is indeed short and we will merge it with the previous one. Anticipating the results of section 4 based on the climatology might confuse the reader and we therefore opt to not expand this paragraph. But we agree with the reviewer that this climatology comparison hints toward more heterogeneity in the Arctic sea ice low spatial distribution and believe this is more or less what we see when comparing 2007 and 2012. We also agree on the fact that Greenland Sea exhibits mechanisms similar to the Antarctic. We could expect that with Arctic Atlantification, more similarities might be observed in a near future in other parts of Arctic. But we refrain to mention this in the manuscript, as this might seem too speculative.

Comment 22

L311–313: Figure S1 is plotting May, not March.

Response:
Thank you, this was a typo.

Comment 23

L334: "consistent with observations": I think a reference is warranted here to justify this statement (as is done later for the Antarctic case, L410).

Response:
Surprisingly enough, no peer-reviewed map of the sea ice state in September 2012 could be found. We will include a footnote referring to the following webpage: https://nsidc.org/sea-ice-today/analyses/arctic-sea-ice-extent-settles-record-seasonal-minimum

Comment 24

L350: "Yet, no significant influence of the cyclone is visible in the model outputs (not shown)": this is vague and unsubstantiated, and also slightly confusing given that the next few lines discuss indirect effects on the ocean heat content over the Beaufort sea. Perhaps, in the quoted line, the authors meant "direct", rather than "significant", impacts? Some comment on what impacts on the model outputs were checked but not seen should be added here.

Response:
When mentioning “significant”, we meant an influence that could be first order in determining the September sea ice mass. We have looked at time series of mass flux terms (including surface and basal melt) and while some increased basal melt over the Beaufort and Chukchi Seas and ice import in Central Arctic sectors can be seen in early August (coinciding with the cyclone), the magnitude of those anomalies are within the range of the variability of those terms over the whole season, and no traces of those anomalies can be seen on the equivalent time series for the whole Arctic. We will clarify this in the text by rephrasing, l. 350: “In the model, basal melt in Beaufort Sea and Chukchi Sea and ice import in the Central Arctic both show a small increase in early August, coinciding with the cyclone, but do not result in a visible anomaly in the respective terms integrated over the whole Arctic.”

Comment 25

L375: ”match well with satellite observations of sea ice concentration anomalies”: but the plot is of sea ice thickness? Similar for L409–410.

Response:
Indeed, we skipped a step by comparing the sea ice thickness anomalies with the observed sea ice concentration. While we do expect both to show similar spatial patterns, we did not clarify this in the initial manuscript. We will correct this omission by adding maps of sea ice concentration anomalies with observed sea ice extent contours in the SIs (Figure R2). We will adapt the main text to refer to this figure, as well as to the background sea ice thickness anomalies maps.

Comment 26

L401: in section 4.3, Fig. 5.b is not cross referenced anywhere (in fact, Fig. 5.b is not referred to anyway in the manuscript).

Response:
Thank you, this was a mistake. We will add a reference to it at the end of l. 408.

Comment 27

L425–426: The phrase ”more difficult to analyse and understand” should be removed as it is vague (no reason is given) and the next few sentences seem to provide analysis of this sector anyway. Also, I’m not convinced that the anomalies there are ”small compared to the other sectors”, assuming the units of each inset panel of Fig. 5.b are the same: for example, the total mass flux (black bar) is about 130 Gt, certainly larger in magnitude than that of the Ross–Amundsen sector (less than 100 Gt), and the snow-to-ice formation (pale blue bar) is larger in magnitude than any of the other sectors.

Response:
This is a good point, we will remove this sentence and modify the one after by (l. 426): “In the East Antarctic sector, a small positive mass flux anomaly [...]”.

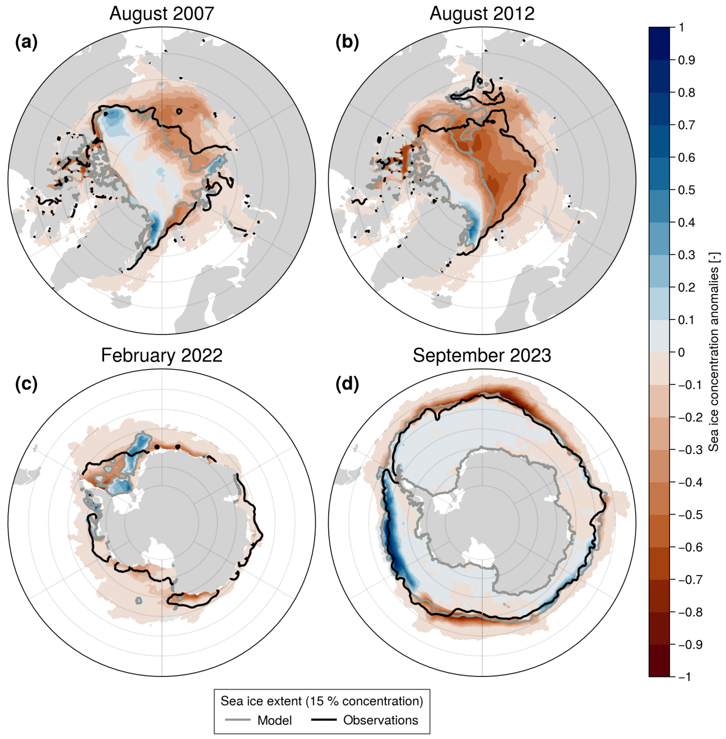

Figure R2: Suggested figure to include in supplementary information, showing the modelled sea ice concentration anomalies (colours) and sea ice extent (grey contour), to compare with observation-based sea ice extent from NSIDC CDR product (black contour), for all four sea ice lows detailed in the manuscript.

Comment 28

L544: “direct”: did you mean “indirect”? (I would have thought the “direct” effect of the atmosphere would be via surface melt/growth, and the “indirect” effect via eventual influence on basal melt/growth as described in the text).

Response:
We consider a change in the heat conductive flux as a direct effect of the atmosphere forcing, in opposition with an increased basal melt due to warmer waters that would have been heated by the atmosphere in leads or in the marginal ice zone, for example. We agree this is slightly subjective, but the basal melt would respond nearly immediately to a change of the temperature profile (and therefore the conductive flux) induced by atmospheric conditions and would not be subject to another medium transferring the heat (unless we consider the ice as an intermediate medium, which could also make sense). We propose to keep the “direct” wording, as the context of this paragraph is to describe the role of the atmosphere, as opposed to the role of the ocean, in the basal heat budget.

Comment 29

L551: important to note that the studies cited here are about the longer timescale (multi-decadal) impact of ocean heat transport on sea ice, not about yearly lows.

Response:
Good point, we will add this note, l.551: “[...] a number of recent studies have shown

the importance of oceanic heat transport to explain ice melt over multi-decadal time scales"

Comment 30

Figures 4 and 5: these are very nice figures, but a couple of small suggestions: a different colour map for the sea ice thickness might be helpful visually, as it currently clashes with the colour choices for the budget terms. Also, in the captions, I suggest noting that the units of each inset are the same (assuming they are?), e.g., "the scales of the y-axes differ among panels, but the units are always Gt)".

Response:
Thank you. We will change the colormap of the thickness anomalies (brown to green), hoping this clarifies the distinction (see new version in Figs R5 & R6). We will also mention in the caption that units and y-axis labels are the same for all inset panels.

References

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Figures: new versions

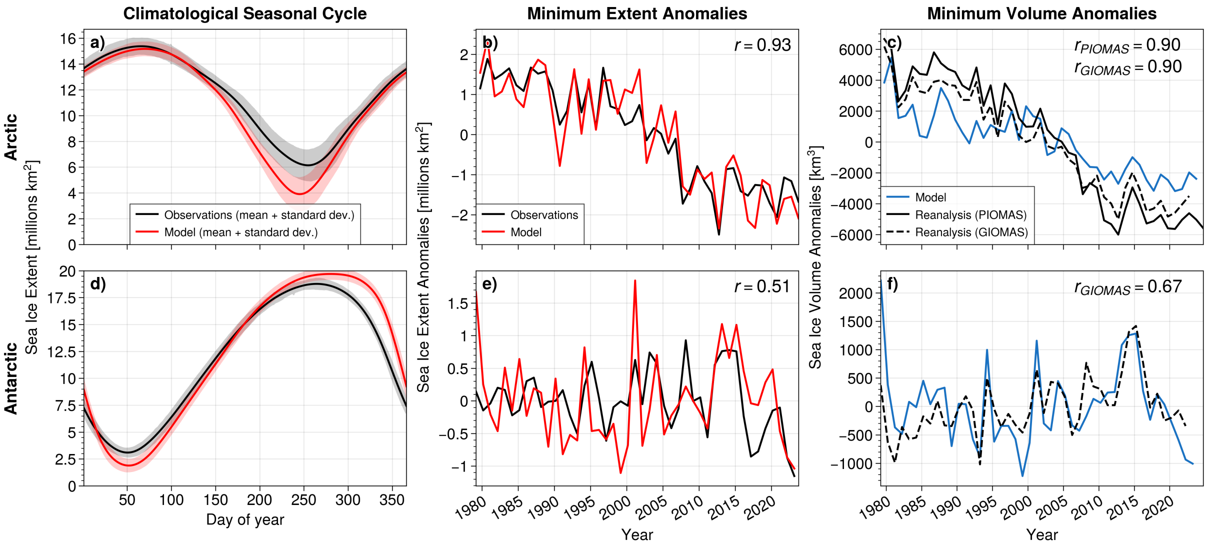

Figure R3: Figure 1, new version. Modified caption:
Comparison of observations-based products (black) versus model sea ice extent (red) and volume (blue). Observations-based products are the satellite-based NOAA/NSIDC Climate Data Record (CDR) for sea ice extent and the PIOMAS and GIOMAS reanalyses products 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).

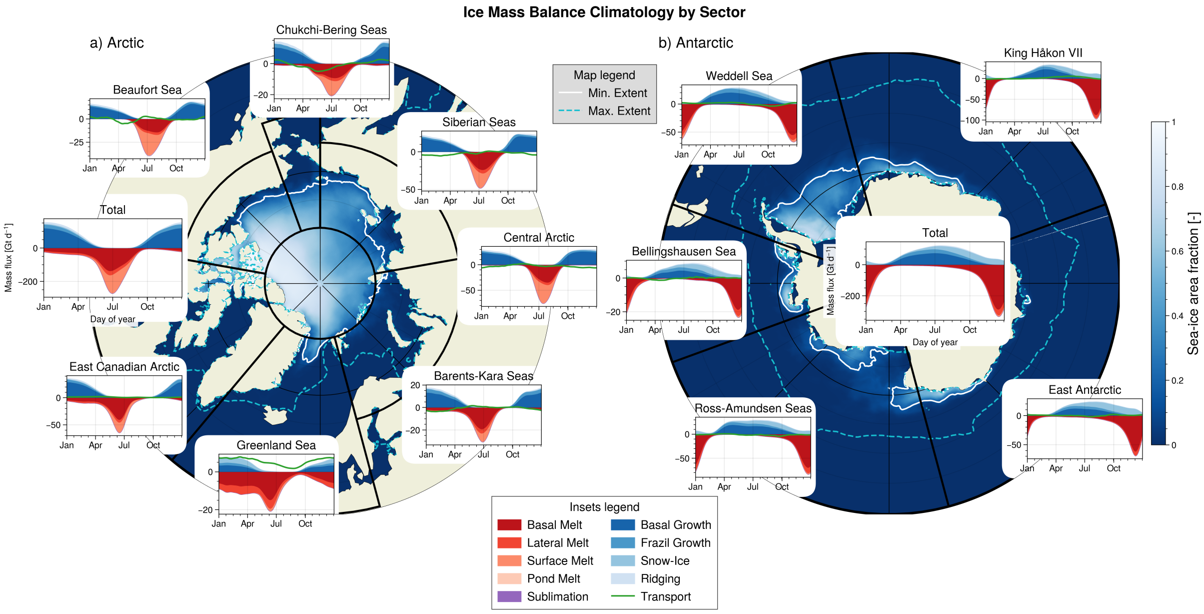

Figure R4: Figure 3, new version.

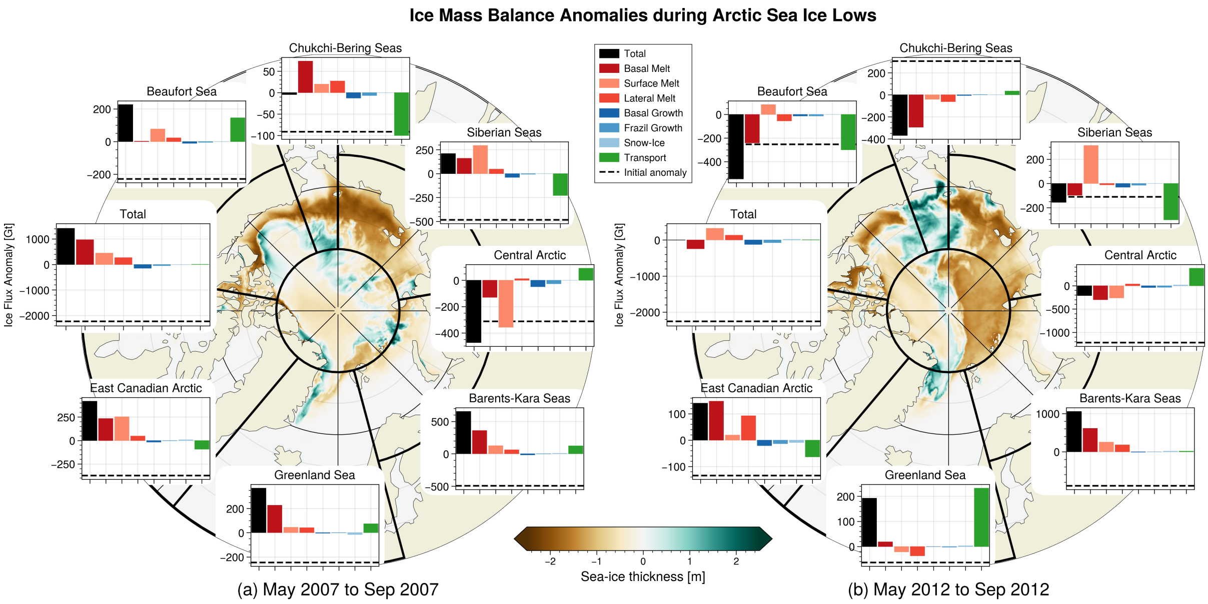

Figure R5: Figure 4, new version.

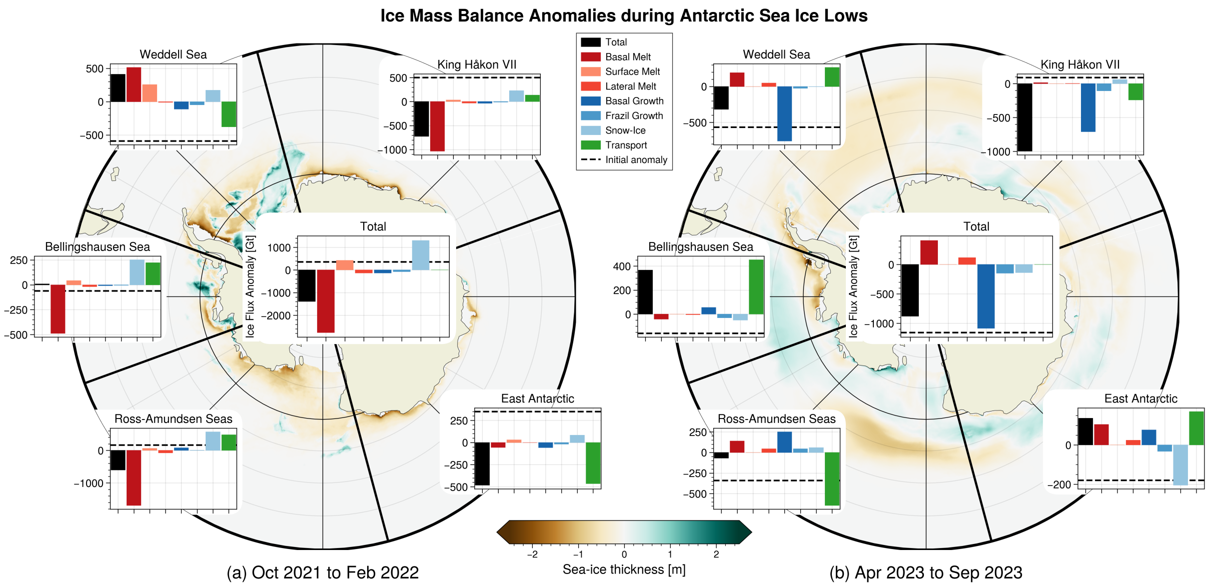

Figure R6: Figure 5, new version.