Reviewer 1 (William Hobbs)

The authors analysis is a valuable contribution to the current work on understanding recent Antarctic sea ice extremes, and I applaud the attempt to make sense of extreme low sea ice events as a whole rather than focussing on individual events; I think this is valuable and important. The paper is mostly clearly-written with appropriate references, the figures are of high quality and the analysis *mostly* supports the conclusions. I have uploaded an annotated PDF with specific comments, but I do have some general suggestions/comments

Thanks for your review and valuable feedback, we plan to submit a thoroughly revised manuscript. Below you can find point-by-point replies to your major and minor points (in blue), with proposed changes to the manuscript (in grey). We hope that we have addressed all your concerns adequately.

1) if possible I think some updates to include summer 2023 would be worth the (hopefully not too much) extra effort in terms of impact; I acknowledge that it may not be possible to extend the model simulation though

Thanks for the suggestion, we agree that including the 2023 event will add value to the study. We have modelled data available until February 2023 and, as you anticipated, we cannot extend the simulation further at this stage due to technical reasons. However, the core analysis is based on JFM means so we cannot really include the event in the full study.

We propose to add an update on the 2023 event in Section 4, with a discussion of its characteristics within the context of our main results. For this, we propose to include two new figures in the supplementary material (see Figs. R1.1 and R1.2): SIC anomalies in JF in the model and observations (complementing Figs. 1 and 2) and SLP/wind anomalies in OND (as in Fig. 5). Furthermore, given the lack of recovery since the previous event (2022), we would remark the possible relevant role of preconditioning for this specific event (2023).

This is the proposed text, which would replace the last paragraph of Section 4:

After the 2022 minimum, a new record was established in summer 2023 (J. Liu et al., 2023; Purich et al. 2023), which we have not included in the study due to data unavailability at the time of the analysis. The distribution of SIC anomalies was in line with the previous events, with prominent negative anomalies in the Weddell and Ross seas. However, a substantial lack of sea ice was observed in all sectors and particularly in the Bellingshausen-Amundsen Sea (Fig. R1.1). The atmospheric conditions during the previous spring were again dominated by a positive SAM. In addition to a deepened ASL, similarly to 2022, a cyclonic anomaly appeared over the eastern Weddell sector (Fig. R1.2). This anomalous large-scale circulation led to prevailing westerly winds, in agreement with the previous cases, though an unusual southerly component was present in the Weddell Sea. While these results are consistent with our main findings for the other events, we speculate that preconditioning played a more important role for 2023 than for other years. In fact, sea ice never fully recovered after the 2022 minimum and negative SIE anomalies persisted even after the annual winter peak, which might have favored the occurrence of the subsequent summer minimum.

Liu, J., Zhu, Z., and Chen, D.: Lowest Antarctic Sea Ice Record Broken for the Second Year in a Row. Ocean-Land-Atmos Res. 3;2023:0007, DOI:10.34133/olar.0007, 2023.

Purich, A. and Doddridge, E.W. Record low Antarctic sea ice coverage indicates a new sea ice state. Commun Earth Environ 4, 314, https://doi.org/10.1038/s43247-023-00961-9, 2023.

2) I was a little suprised that the model performs quite poorly in the Weddell sector, I think it would be worth checking the ERA5 surface temperature to see whether the model is the problem or the surface forcing. Either way, I think the authors need to be a bit more rigorous in expaining how this bias might effect the analysis. Currently it's a bit dismissive, stating the model is a good representation, but with figures that don't really support that. I think this could be addressed with some carefully calibrated text though.

Thanks for this comment, we understand your concern and we agree that we should include more details about the model's biases and limitations. The poor performance in the Weddell sector occurs mainly in spring and summer (Fig. R1.3), and in particular the model tends to lack a substantial amount of sea ice in the western part of the basin (Fig. R1.4). This could be due to an underestimation of sea ice thickness in that region (Rousset et al. 2015), to excessive surface melting (see your point below) or to other deficient processes, including, as you mention, issues with the forcing from ERA5. Indeed, Barthélemy et al. 2018 tested the role of atmospheric forcing in a similar configuration of the model and found variations in the bias in the western Weddell Sea depending on the forcing used (see their Fig. 2). However, a further assessment is outside our scope and not possible with the available simulations.

To address your point in the manuscript, we propose to add a detailed description and quantification of the main model's biases in the SIE/SIC seasonal cycle. This is the proposed text, to be added at the end of Section 2.1:

A full description and evaluation of the model can be found in Pelletier et al. (2022; specifically, we use the same configuration as in their PAROCE experiment). Documented issues include systematic biases in the SIE seasonal cycle, which are related to well-known NEMO-LIM features (Vancoppenolle et al., 2012; Rousset et al., 2015). Particularly, the simulation used here reproduces well the observed growth from March to July (Fig. R1.3), but eventually overestimates the extent around the winter peak (1.1 million km^2 more than the observations in September). Very little melting occurs before November, after which a steep decrease follows, with most of the melting happening between December and January. Due to the excessive winter extent and the short melting season, December is also the month with the strongest bias in the mean SIE, with a difference between the model and the observations of about 4.4 million km^2. In summer, in contrast, too little sea ice is left in the model. A lack of 1.9 million km^2, compared to the observations, is typically present in February, while the difference is smaller in January and March (the average January-March bias is -1.5 km^2). The positive winter SIE bias is mostly related to a sea ice excess in the Bellingshausen-Amundsen Sea and eastern Indian/western Pacific Ocean, but the Ross Sea also contributes (Fig. R1.4). In summer, a lack of SIC is observed in almost all sectors. Particularly relevant for this study is the fact that the Ross Sea is virtually ice-free in February, and that a substantial portion of sea ice is also missing in the western Weddell Sea through the whole season. In the eastern Weddell sector, in contrast, the model seems to systematically overestimate the SIC and extent, particularly in January.

Then, we would also return to this point in the discussion (Section 4), when we remark the limitations of our model (from L390):

We have also discussed the model's biased climatology and how it is related to the model's poor performance in capturing the exact distribution of SIC anomalies, particularly in the Weddell Sea. Nevertheless, the main processes explaining the occurrence of minima in the model are consistent with the ones derived from observations and thus both support our conclusions. Our budget analysis relies on the model only and is thus also affected by its biases, such as the overestimated surface melting in the Weddell Sea mentioned in Sect. 3.5. Furthermore, the underestimate of the negative anomalies at the sea ice edge in the Weddell Sea could also impact the budget and alter the role of ice transport. However, the overall results are consistent between the Ross Sea and Weddell sectors and in agreement with previous results, as discussed above, which further endorses our conclusions.

We also propose to re-calibrate the text in Section 3, for instance by softening the opening statement of section 3.1:

Examining the temporal evolution of the summer SIE anomalies (Fig. 1a), a reasonable agreement between the model and the observations is found.

And by adding the following text at the end of the same sub-section:

While the correlation between the modelled and observed time series is satisfactory (0.65), we acknowledge that the model appears to perform worse during the first 10-15 years. Sometimes, the model also simulates strong negative anomalies in years with observed positive ones, such as 1991 and 1999 (Fig. 1a), which is mostly due to model's failures in the Ross Sea.

We also propose to add more details on the interpretation of the lack of negative anomalies in the Weddell Sea in the model's composite map (L189-19, Sect. 3.2):

This difference may be related to the model biases in the summer climatology discussed in Section 2, which result in limited sea ice left in the eastern Ross Sea in JFM (see Fig. S1). While the positive signal in the Weddell Sea that is evident in the observations is also reproduced by the model, only sparse negative anomalies are found in the model in the single years (Fig. 3a-d) and are almost lacking in the composite map (Fig. 3f). Again, this may be related to the model's systematic summer biases. The observed negative anomalies in the western part of the sector are in fact located in regions where the model usually does not have sea ice at all (cf. anomalies in Fig 2 with the model's climatological sea ice edge in Fig. 3, dashed lines). In contrast, the lack of negative anomalies in the eastern part may be due to the model's tendency to overestimate the sea ice presence there, as discussed in Section 2 (see Fig. R. 1.4).

3) This is my most serious concern, (and sorry, also my most negative) - I think the area budget analysis in section 3.5 is fundamentally incorrect, and is giving the wrong answer. There are 2 key reasons:

a) almost all the sea ice melt, even in spring, is basal melt, not surface melt (e.g. Grodon 1981), so atmospheric thermal advection is unlikely to have a big impact. (If the model diagnostics output the separate melt components then the authors can check this for themselves, or even prove me wrong!);

b) in spring, the heat source for that basal melt is solar radiation collected in leads/open water. Hence, because of the albedo feedback, the dynamic and thermodynamic terms are intimately related - move ice out of the way, the suface ocean can warm, and you get more melt. By integrating over very large areas you lose this relationship - the only dynamic contribution mathematically can be movement in or out of that sector, BUT you lose **all** the information about how the melt is modulated by dynamics

As serious as this concern is, I think it could be resolved fairly easily - rather than spatial integrals, just show maps of the tendency terms' anomalies, and I think that co-dependence should be evident. I note that the climatology maps are shown in the supplement (and indeed show an inverse co-dependence between the dynamic/thermal terms), but I think the anomaly maps are key as well.

And if possible from the diagnostics cmpare the surface and basal melt components (this can actually be done correctly as an area integral)

Gordon, A. L., 1981: Seasonality of Southern-Ocean Sea Ice. *J Geophys Res-Oceans*, **86,** 4193-4197, DOI 10.1029/JC086iC05p04193.

Thanks for this insightful comment and suggestions on how to address it. Concerning point a), we agree that the main source for the spring sea ice melt is basal melt and we did not mean to imply otherwise. However, while basal melt is clearly the dominant process, we also acknowledge that the model tends to simulate too much surface melting, particularly in the Weddell Sea, as shown in Fig. R1.5, where the climatology of the two terms are compared . We thus suggest to explicit both points in the manuscript by modifying the text between L309 and 311:

In both sectors, most years present negative values for the two terms, indicating that both type of processes tend to lead to direct sea ice loss. For instance, surface winds may transport sea ice away while also advecting warm air towards a region, which in turn increases the ocean-sea ice heat flux and favors basal melting. Direct surface melting from thermal advection is supposed to play a minor role in spring but it may be overestimated in our model (Fig. R1.5).

Concerning the other points, we believe that there are some aspects that need to be clarified. First, we should discuss the processes accounted for in the dynamic and thermodynamic terms in our framework. As you correctly point out, the two terms are intrinsically related and the way they modulate each other is not straightforward to interpret. For instance, the albedo feedback that you mention is in principle initiated by the dynamic movement of ice, but in this framework, it would result in an increased thermodynamic term. On the other hand, at the local scale the two terms are comparable and often linked simply because as more sea ice is moved away from a point, less sea ice remains for melting. This is a limitation of our diagnostics that we propose to clarify with the following text, to be placed in Section 3.5 (L306):

Note that the dynamic and thermodynamic terms are not mutually independent as they influence one another both directly and indirectly and it is not possible to strictly separate them. For instance, an anomalous transport of sea ice away from a given point, thus driven by dynamics, would be compensated by opposite anomalies in the thermodynamic term as less sea ice becomes available for melting. In turn, leads created by sea ice transport, effectively a dynamic process, induce ocean warming and melting associated with the albedo-temperature feedback (e.g. Goosse et al., 2023), *which is the dominant mechanism in spring. However, this melting is accounted for in the thermodynamic part in the framework proposed here, and the information about the role of dynamics is not explicitly retained. Hence, the thermodynamic and dynamic terms must be interpreted carefully and particularly the modulation of the thermodynamic component by its dynamic counterpart, which includes direct compensation of the anomalies but also more complex feedback mechanisms.*

Second, how to properly interpret the results from the spatial averages. We agree on your main concern, namely the fact that the local contribution of dynamics is important, but that the transport of ice from one point to another inside the sector is balanced out when integrating over the sector. Indeed, when looking at the maps of the anomalous terms (Fig. R1.6), the dynamic term is often comparable in magnitude to the thermodynamic one, and the two appear to be related in some regions. This confirms that, locally, dynamics play a relevant role and, as you mention, modulate the thermodynamic processes. However, the overall maps of Fig. R1.6 are rather heterogeneous and it is hard to draw conclusions for them. For this reason, and also due to the large number of panels they encompass, we decided to exclude them from the original manuscript. Instead, we carried out the analysis using the spatial averages and we still believe that it is valid and provides useful information, when interpreted within the right framework. Namely, our conclusions on the predominant role of thermodynamics apply to the larger, sectorial scale, while we acknowledge that dynamics play a relevant role at the local scale.

We propose to clarify the rationale of the analysis with the spatial averages and its proper interpretation by modifying the text at L305, which in the new manuscript would be right after the new paragraph cited above:

With this in mind, it is not surprising to see that, locally, both terms contribute to the anomalous sea ice loss during the months preceding a summer minimum (Fig. R1.6). In fact, the negative tendency anomalies in the inner Ross and Weddell Seas arise from the combined influence of dynamic and thermodynamic processes, which have comparable strength at the local scale as they sustain one another.

While it is evident that both terms play a role, a clear interpretation and quantification of their contributions is not straightforward from the spatial patterns, as they are quite heterogeneous (Fig. R1.6). For this reason, we have averaged the NDJ anomalies of the dynamic and thermodynamic terms over areas in the Weddell and Ross sectors with negative JFM SIC anomalies (< -0.1), similarly to Fig. 6.

And then, at the end of the same section (3.5), we would add:

This analysis of the spatially-averaged budget allows us to quantify the contributions of these processes at the large, regional scale and suggest a more prominent role of thermodynamics. However, we stress that the movement of ice away and from adjacent points inside the sector is largely balanced in such spatial averages. Hence, the resulting (anomalous) dynamic term accounts for the net ice transport in or out the region, but does not consider the local contributions that, as seen above, are comparable to the thermodynamic processes and can in fact modulate them.

Finally, we would also remark this in the discussion (Section 4, L371):

The regional differences concerning the predominant winds could suggest distinct contributions from dynamics and thermodynamics in the Ross and Weddell Seas; however, our results are similar for the two regions. We find that, locally, both terms are important and affect sea ice both directly, via mechanical transport and thermal melting, and indirectly, through feedback mechanisms. At the regional scale, we observe that the exceptional sea ice loss in both sectors is generally dominated by thermodynamic processes, though dynamics also play a role, but a minor one.

Concerning your comments on the annotated manuscript, we will take them into account in the revision. Just few comments on the major points:

- In the official OSI-SAF documentation, it is reported that data are missing in 1988 from 01/01 to 12/01, which does not affect significantly the JFM mean. We have also checked that the SIE time series and the event selection are almost identical if we exclude 1988 (except that 1988 is itself a regional minimum for the Weddell Sea).
- The autocorrelation in the JFM time series of the total SIE is 0.40 in the observations and 0.35 in the model. We believe it is reasonable to assume that our statistical test based on the bootstrapping is indicative of the significance of the results.

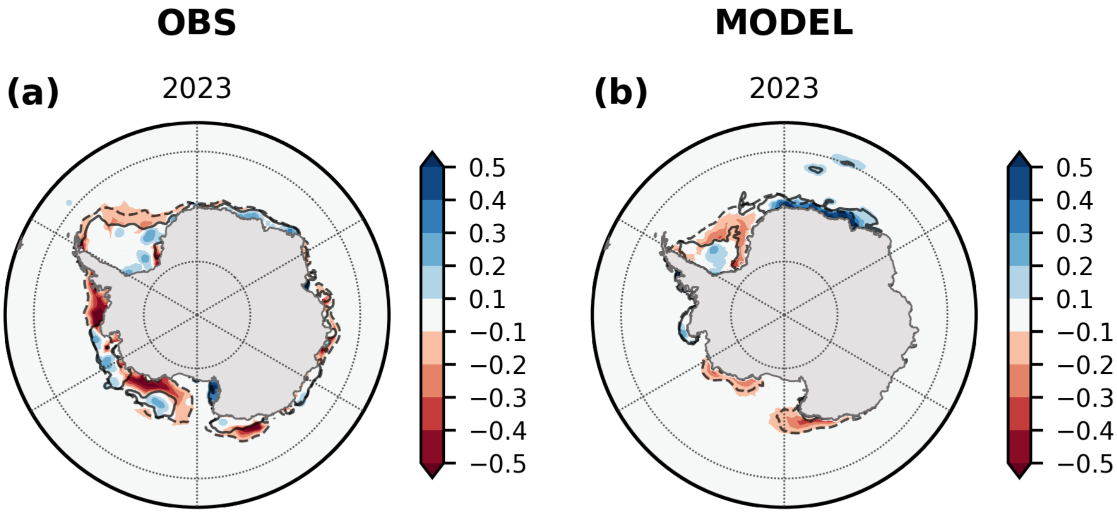

Figure R1.1: Shading: observed (left) and modelled (right) SIC anomalies in JF 2023 (March is excluded due to data unavailability). Contours: sea ice edge (SIC=0.15) in 2023 (solid line) and in the climatology (dashed line). This figure will be added to the supplementary material.

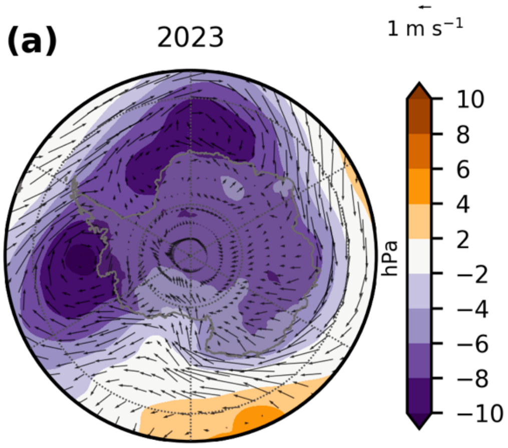

Figure R1.2: SLP (shading) and 10-m wind (arrows) anomalies in OND 2022, before the 2023 summer minimum. This figure will be added to the supplementary material.

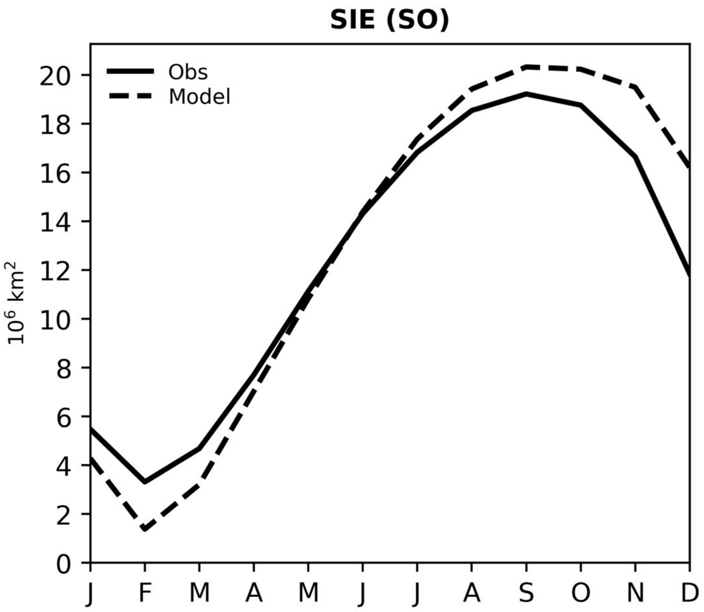

Figure R1.3: Seasonal cycle of the total Antarctic SIE in the observations (solid line) and in the model (dashed line). This figure will be added to the supplementary material.

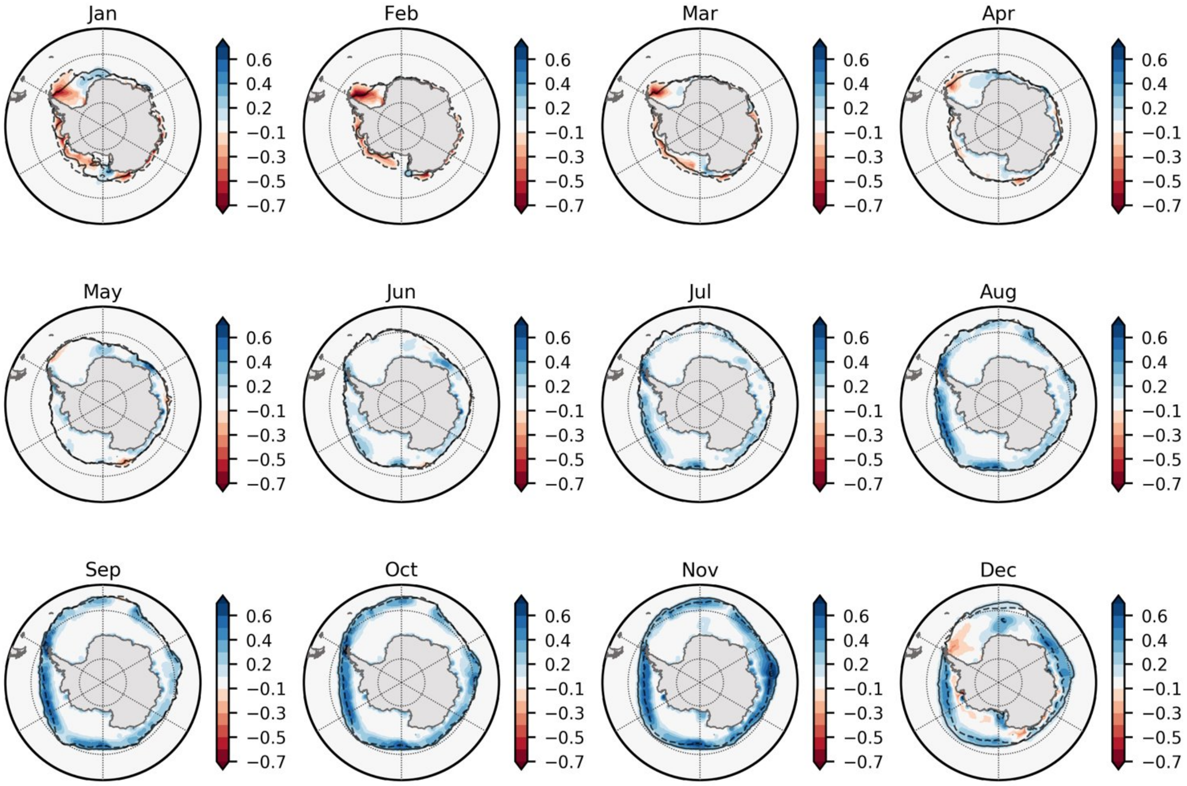

Figure R1.4: Differences in the monthly SIC climatology between the model and the observations. This figure will be added to the supplementary material.

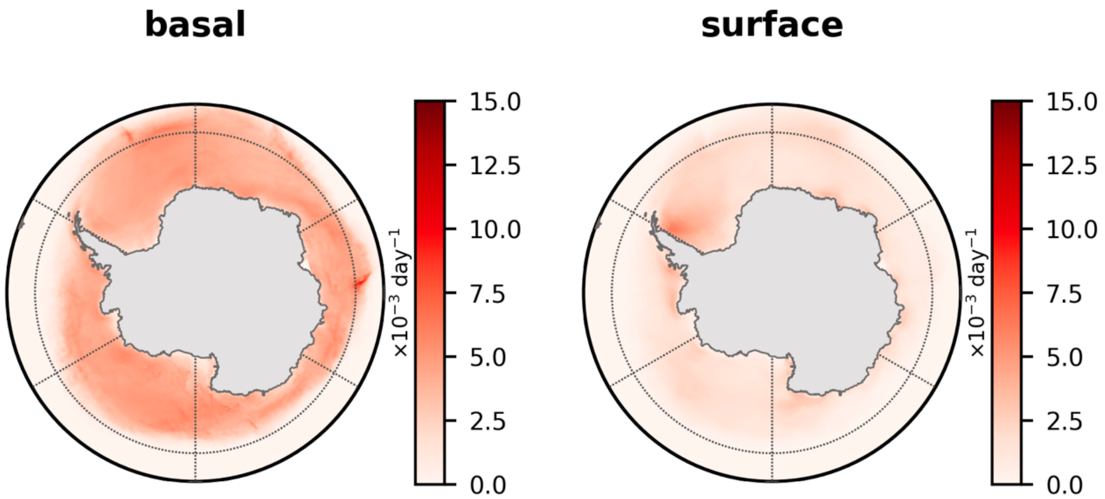

Figure R1.5: NDJ climatologies of the basal (left) and surface (right) melt components in the model. This figure will be added to the supplementary material.

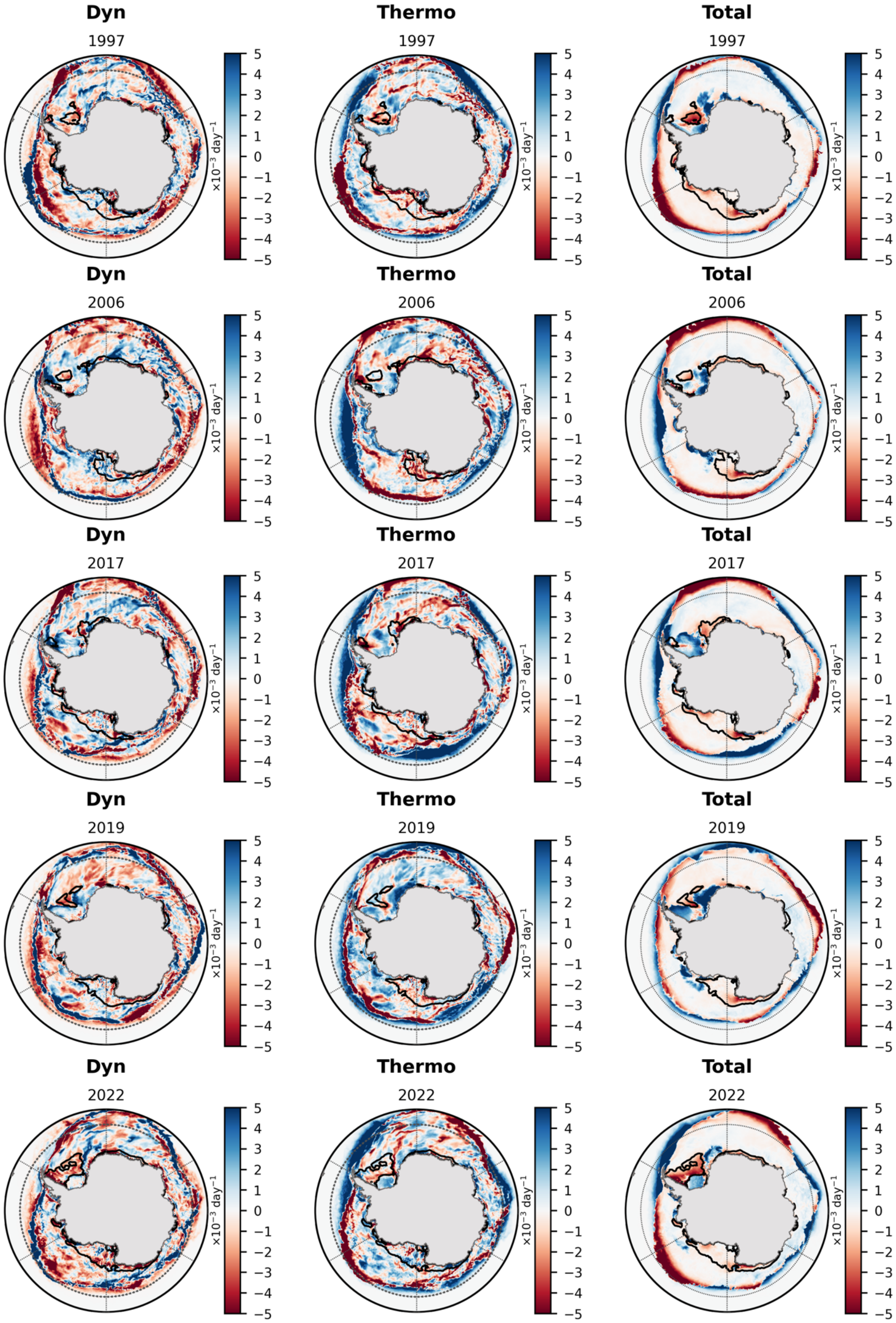

Figure R1.6: Shading: NDJ anomalies of the dynamic (left), thermodynamic (middle) and tendency (right) terms in the years with total SIE minima. Contours: areas with anomalous SIC = 0.1 in the corresponding year in JFM. This figure will be added to the supplementary material.