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.

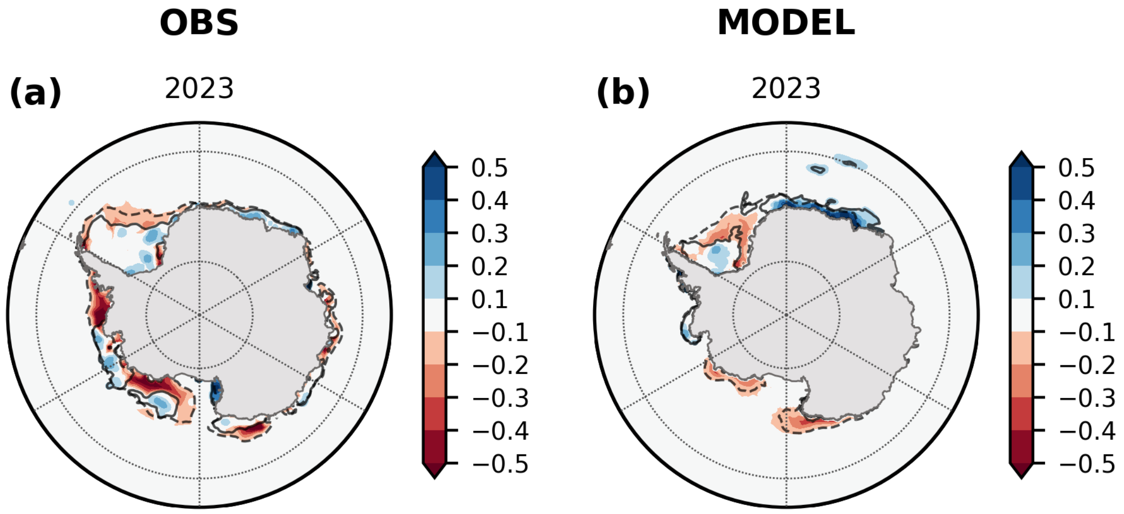

Figure R1.1: Shading: observed (left) and modelled (right) SIC anomalies in JF 2023 (March is excluded due to data unavailability). Contours: sea ice edge (SIC=0.15) in 2023 (solid line) and in the climatology (dashed line). This figure will be added to the supplementary material.

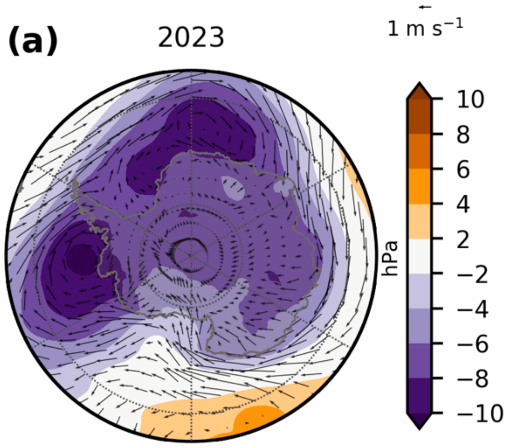

Figure R1.2: SLP (shading) and 10-m wind (arrows) anomalies in OND 2022, before the 2023 summer minimum. This figure will be added to the supplementary material.

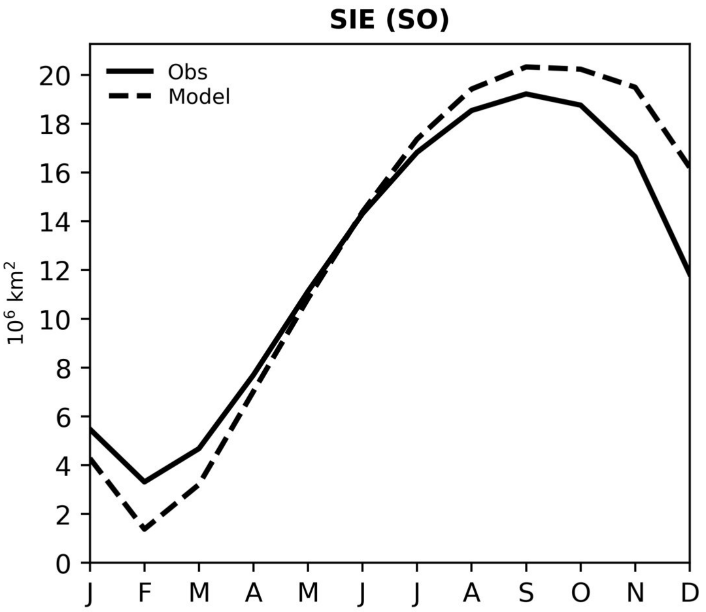

Figure R1.3: Seasonal cycle of the total Antarctic SIE in the observations (solid line) and in the model (dashed line). This figure will be added to the supplementary material.

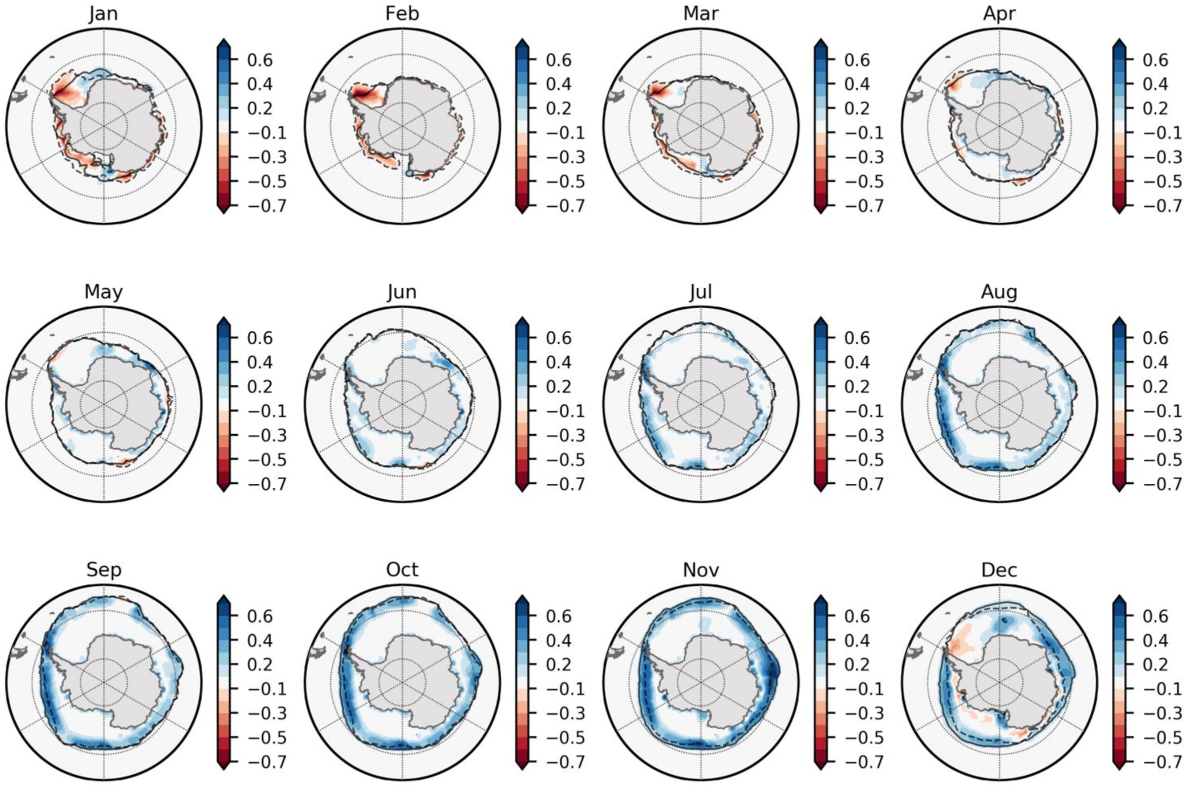

Figure R1.4: Differences in the monthly SIC climatology between the model and the observations. This figure will be added to the supplementary material.

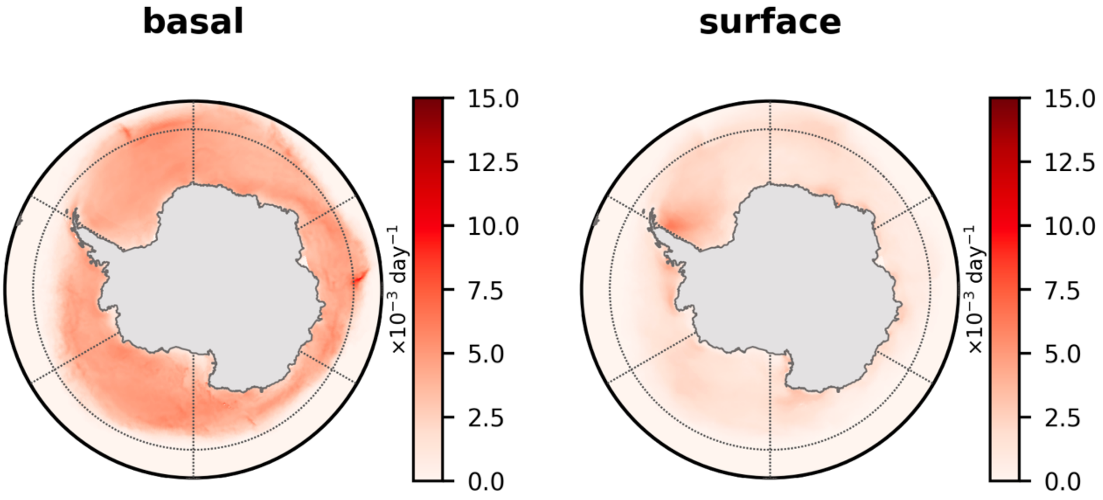

Figure R1.5: NDJ climatologies of the basal (left) and surface (right) melt components in the model. This figure will be added to the supplementary material.

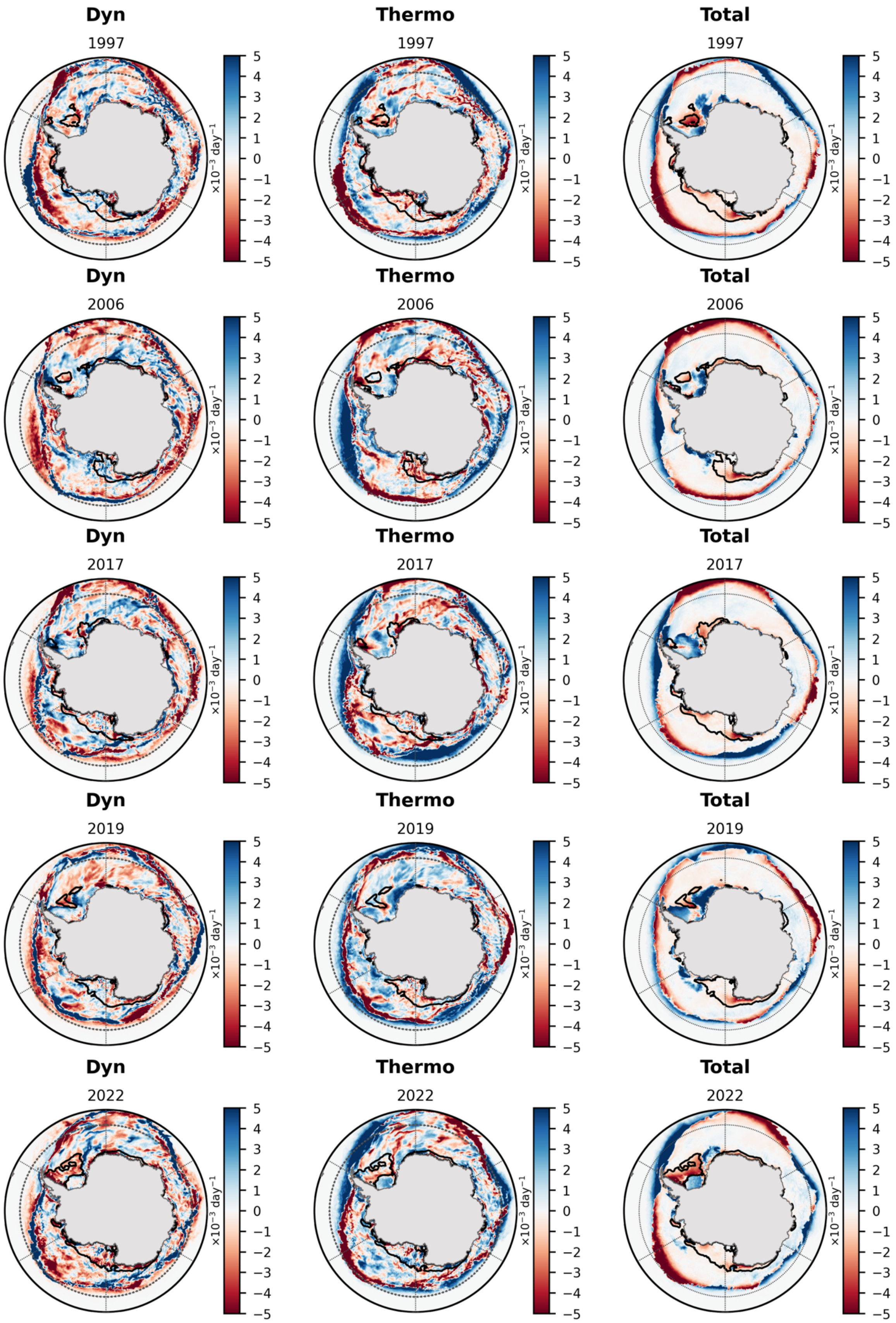

Figure R1.6: Shading: NDJ anomalies of the dynamic (left), thermodynamic (middle) and tendency (right) terms in the years with total SIE minima. Contours: areas with anomalous SIC = 0.1 in the corresponding year in JFM. This figure will be added to the supplementary material.

Reviewer 2

Overview: This paper analyzes multiple summer (JFM) Antarctic sea ice minima to discover if there are similarities between the events, and to better understand the main mechanisms causing (and leading up to) the minima themselves. It finds that there are many regional differences, with the Ross and Weddell Sea sectors being the most important for total sea ice minima. Across the events, there are many different causes, with the study suggestion a similar wind pattern (even with different atmospheric circulation patterns) in both sectors, and a dominance of thermodynamic (melting) effects over dynamic (primarily advection).

The paper is an important study, and when improved, can be a valuable contribution to the field. However, there are some things that can be tightened before the paper should be formally accepted, and as such I'm suggestion a revision that would fall between a major / minor revision (some new analysis, mostly new text).

Thanks for agreeing to review our manuscript and for providing insightful comments that we plan to incorporate in a new version. Please, find point-by-point replies to your major and minor points (in blue) and proposed changes to the manuscript (in grey), which we hope you will find adequate.

Main comments:

1. I think the role of ice advection, including the preconditioning of ice anomalies in the preceeding winter, can be improved substainally. In the comments below, I'm asking the authors to redo section 3.3 with a Hovmöller diagram (with SIE anomalies plotted in longitude and time) to better see any connections with the preceeding winter to the summer minima, and allowing for ice anomalies to move in / out of sectors. I think this will also better guide the interpretation of the dynamic terms, which if I am understanding appropriately, are only calculated at locations of SIC anomalies, and not upstream where the ice anomalies may have originated.

This is a very good point, thanks for your suggestion. Indeed, there may be some memory effect arising from the preceding winter conditions in another sector that is possibly lost in the diagnostics of Fig. 4. We have thus checked this with a Hovmöller diagram, as suggested. Each panel of Figs. R2.1 and R2.2 shows the sea ice area (SIA) anomalies computed for each longitude for the five total minima and their composite, in the observations and in the model. The vertical axis displays time, starting from one year before the minimum at the bottom (e.g. March 1996 in the first panel) up to the event itself (e.g. March 1997) at the top. Focusing on the Weddell and Ross sectors, there is no evidence of winter preconditioning of the summer minima in 1997 and 2006 in neither the observations nor the model (panels a and b of Figs. R2.1 and R2.2). In contrast, there seem to be negative anomalies persisting from at least the previous September until the summer minima for three most recent years (2017, 2017 and 2022). However, there is no clear pattern of longitudinal migration of the anomalies leading to the summer minima, if not within the same sectors. These plots thus confirm our main findings, namely an unclear role of winter preconditioning that has possibly become more relevant in the last years. While the two figures provide additional valuable information, we believe that adding them to the main text would generate confusion, given their heterogenous nature and high number of panels. Hence, we propose to add them to the

Supplementary material and to complement the manuscript with the following text (to be added at L237):

This is further supported by Hovmöller diagrams displaying time-longitude sea ice area anomalies for the five total minima (Figs. R2.1, R2.2), which help to identify the potential propagation of winter anomalies between different sectors. However, there is no consistent zonal migration of negative anomalies from one sector to another leading to the sea ice minima. We also do not observe any clear sign of winter preconditiong in the earlier years (1997, 2006) and in the composites, particularly in the model (panels a,b,f of Figs. R2.1 and R2.2). It is only in some of the most recent cases (2017, 2019 and 2022) that persistent negative anomalies from at least the previous September are present in the Weddell and Ross sectors (panels c,d,e of Figs. R2.1 and R2.2).

2. The ice-ocean model has some rather serious limitations in my view – incorrect sea ice anomalies in general, and a rather poor representation of the patterns of ice loss in the Weddell Sea. The authors mention this originally, and again in the conclusions, but there needs to be more text and insight provided when using the model to understand causes for the SIC anomalies in light of what information the model can actually provide (and how this compares to observations). See some specific mentions in the comments below.

Thanks for this comment, we agree that the model's limitations need to be addressed more thoroughly. We thus propose several additions to the original text.

First, a detailed description and quantification of the main model's biases in the SIE/SIC seasonal cycle, 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 km2 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 km2, 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 km2). 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 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).

Minor comments / line-by-line comments:

Fig. 1- what is striking to me are times when the model produces a minima below 1 sigma, but this is not in the observations (which often show positive SIE, albeit less than +1 sigma, looks like 1991, 1999 as examples from Fig. 1 for total SIE). What causes these extreme minima in the model, and does this limit the usefulness of the model (i.e., is the model producing the right negative sea ice conditions for the wrong reasons in the -1 sigma observations / -0.5 sigma model comparisons?

Thanks for this comment, we do see your point and we hope we have at least partially addressed it with our response above about the general model's limitations. We have checked the SIC anomalies for the two specific cases you mention, 1991 and 1999 (see Fig. R2.3), and in both cases the main disagreement is in the Ross Sea. In 1999, positive anomalies dominate this region in the observations and compensate for the strong negative anomalies in the Weddell Sea, while the models show almost no anomaly in the Ross Sea. In 1991, in contrast, the model simply shows excessive melting in the Weddell and even more in the Ross sector, where little sea ice is left. As we discuss in the proposed text about the model's biases (see above), the model's climatology in both sectors is biased towards excessive summer melting, which may be related to what is observed for these two years. Issues with the atmospheric forcing from ERA5 may also play a role. However, investigating the exact causes for this disagreement is outside our scope. Nonetheless, we believe that when the model and the observations both capture a minimum, it is because the model correctly responds to the forcing and not for the wrong reasons. This is also why we chose a selection criterion for the events that takes into account both the observations and the model.

We propose to briefly comment on this in the revised manuscript, at the end of Section 3.1:

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.

L124-126: There is no Fig. 1f, I think you mean Fig. 2f that shows the sea ice sectors?

Thanks, we will correct this.

Eq 1 (near L140) – Is this a total derivative, or a partial derivative? In atmospheric sciences, at least the dSIC/dt = total derivative (Lagrangian, following the motion, so no advective terms), and (SIC)/t = partial derivative, Eulerian, which is a local tendency that has advective terms, which is what I would expect for the equation referenced?

This is a simple local tendency. Thanks for pointing out at this possible source of confusion. We propose to avoid the problem by removing the formula and simply starting Section 2.3 with:

In general, the temporal evolution (tendency) of SIC at a certain location can be expressed as the sum of a dynamic and thermodynamic term, whose exact definitions can vary.

Fig. 2, why not multiply by 100 and show these anomalies as a percent?

It is customary to show SIC anomalies both as a fraction (e.g. Turner et al. 2022) and as a percentage. Since this is a matter of preference, we would prefer to keep the current figures.

L218-219: I would add another reference to Table 1 here when discussing the overall good agreement with the model and observations – although really the agreement is only good in my view in the Ross sector.

Thanks, we agree but we will likely (re)move this sentence due to the new discussion on the model's bias in the previous sections.

Fig. 4 - I think a Hovmöller style plot would be more informative here – sea ice can move from one sector to the others and this plot fails to show that, and therefore may miss the connection of winter minima in other sectors nearby that can lead to a minima in the Ross (especially) but also the Weddell in summer.

Please see your main point above.

L238 – I suspect you mean center and right columns, but probably a moot point since I'm suggesting a revision of this section / figure. It does seem to suggest though for an extreme minima like we have seen in the last few years, a winter preconditioning seems to be important. From what I recall, the recent events in 2022 and 2023 also had an early peak in maximum extent sometime in August, which is worth mentioning I think.

Yes, thank, we will correct this. In the new (proposed) text, when we discuss Fig. R2.3, we stress more the possible role of winter preconditioning in the most recent years (see above), which is also remarked again in the discussion. Additionally, we propose to add to the new manuscript a brief discussion on the 2023 event, mentioning that we expect an even more prominent role of preconditioning in this case.

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 (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, anomalous 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. 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.

Fig. 5 – I'm wondering the influence of season mean conditions (as shown in Fig. 5) vs. the impact of strong extremes, as in several strong storms that can break up, quickly move and redistribute ice (as shown in Turner et al. (2022)), but may be masked by the use of the seasonal means in Fig. 5. The authors should at least comment on this impact. I suppose extremes could rapidly expand ice (or reduce its retreat), but this seems to be discussed much less in the literature.

We agree that with the seasonal mean approach used in our study we cannot examine the details of small-scale perturbations, such as the storms described by Turner et al. 2022. If their impacts persist and contribute to the SIE minima, however, they should be retained in the JFM means. We propose to comment about this in the text in Section 4 (L369):

We remark that our analysis, which is based on seasonal means, does not explicitly address the possible role of individual storms, which have been suggested to be relevant contributors to, at least, the 2022 minimum (e.g. Turner et al. 2022).

Fig 6 caption – more details are needed here, for example, what are the X markings indicating? Is this for a total SIE <-1, or just a regional one, or something else entirely?

Thanks for pointing this out, we will add more details in the caption of Figs. 6 and 7: indeed, the X markings are for the regional minima.

Fig. 6 – suggest changing 'E' in the figure to 'eastward', since this is for a westerly wind that is moving toward the east. I think the arrows are meant to indicate the wind motion, but expanding on this would be helpful, especially since the text talks about wind direction, not where winds are going (and in that regard, why did you change the orientation of Fig. 6 to represent where the winds are going, not where they are from?)

Thanks for this suggestion, we agree that the current layout may generate confusion. We propose to remove the labels (→ E and ↑ N), which are ambiguous, and instead expand the figure caption:

Figure 6: Average OND 10-m wind direction for all extreme years in the observations (full points) and model (empty points). Circles indicate total minima, while crosses are for regional ones. The values indicate the angle of the wind vectors with respect to the zonal axis: 0° = pure westerly winds, 90° = pure southerly, 180° = pure easterly, 270° = pure northerly. See main text for details.

Section 3.5 / Fig. 7 : does the model have Ekman induced sea ice changes, whereby westerly winds could expand ice through an equatorward ocean movement (from my understanding this was a large contributing factor to the expansion of ice through 2016 via the increased westerlies / positive SAM phases).

Yes, this is included in the model.

Section 3.5 / Fig 7: another question on the interpretation of these results- given the model is prescribed the winds but doesn't often get the right magnitude of sea ice loss (Figs. 2,3 and Table 1), wouldn't the dynamic term be under-represented by the model (it would have a much weaker value than expected since it has the correct winds but incorrect sea ice anomalies)? At the very least, it isn't likely getting the dynamic term right given the winds are prescribed by the sea ice anomalies are wrong. This needs to be mentioned, and some estimate of this bias / error given to better interpret these results.

Concerning the general interpretation of the results, we refer to the response below. On your last point, we would like to stress that the prescribed atmospheric forcing is itself a reanalysis product (ERA5) and it thus also subject to biases, so that incorrect sea ice anomalies (with respect to observations) are not necessarily/fully to be attributed to the model (as also pointed out by Reviewer#1). Additionally, thermodynamics in the model could also be biased and there is no reason in principle to assume that the dynamic term is underrepresented compared to the thermodynamic one. For instance, Barthélemy et al. 2018 evaluated the model's sea ice velocity in a similar configuration to the one used here. They found reasonable agreement with the observations and that differences in the atmospheric forcing are a main source of uncertainty (see their Fig. 4).

L317-318: Wouldn't the dynamic terms be stronger not at the location of SIC anomalies, but upstream, where the ice originated? Also, how do should the magnitudes of either terms be interpreted here given that the model's ice loss / anomaly is often incorrect (especially in the Weddell)? I'm not sure with the model biases and the way this analysis is presented that it is possible to conclusively state the thermodynamic term is often larger than the dynamic. These results should be linked to the Hovmöller diagram I am suggesting.

Thanks for these questions and concerns about the budget analysis. Following these comments and those from the other reviewers, we propose to revise the entire section to clarify the interpretation of the two terms and, in particular, place in the right context our main conclusion of the predominant role of thermodynamics compared to dynamics. (Concerning the location of the anomalies, we refer to discussion above about the Hovmöller diagrams)

First, we propose to clarify what processes are accounted for in the dynamic and thermodynamic terms in our framework. For instance, the albedo feedback (ocean warming due to sea ice being transported away leading to more melting) is could be 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 place 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 warming and melting associated with the albedo-temperature feedback, which is the dominant mechanism in spring (e.g. Goosse et al., 2023). 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. 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.

Second, we need to clarify how to properly interpret the results from our analysis using the spatial averages of the two terms. 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 modulate the thermodynamic processes. However, the overall maps 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:

While it is evident that both terms play a role, a clear interpretation and quantification of their contributions is not straightforward from the anomalous 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 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.*

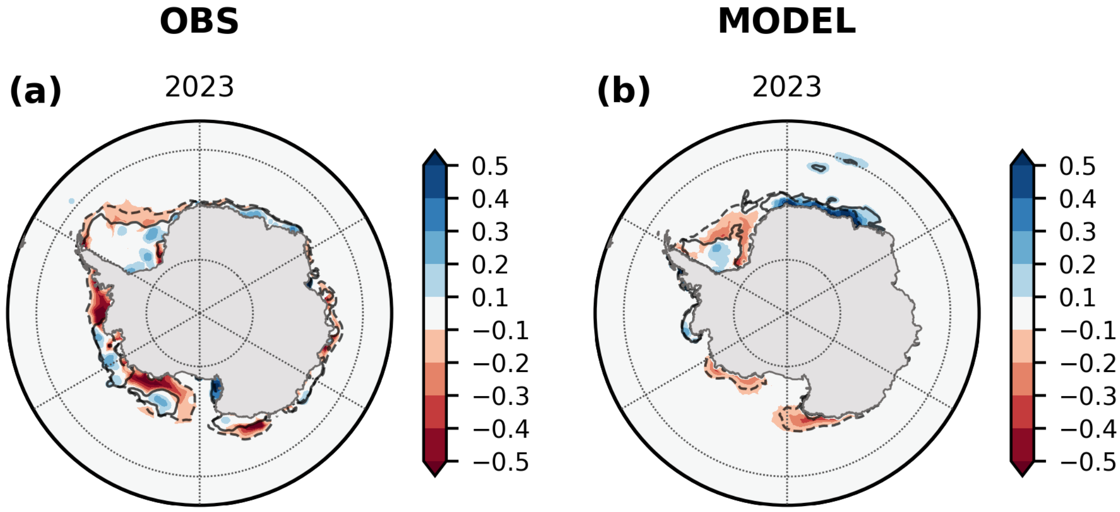

Figure R1.1: Shading: observed (left) and modelled (right) SIC anomalies in JF 2023 (March is excluded due to data unavailability). Contours: sea ice edge (SIC=0.15) in 2023 (solid line) and in the climatology (dashed line). This figure will be added to the supplementary material.

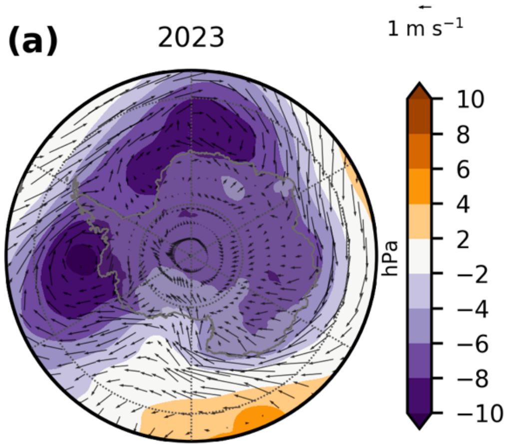

Figure R1.2: SLP (shading) and 10-m wind (arrows) anomalies in OND 2022, before the 2023 summer minimum. This figure will be added to the supplementary material.

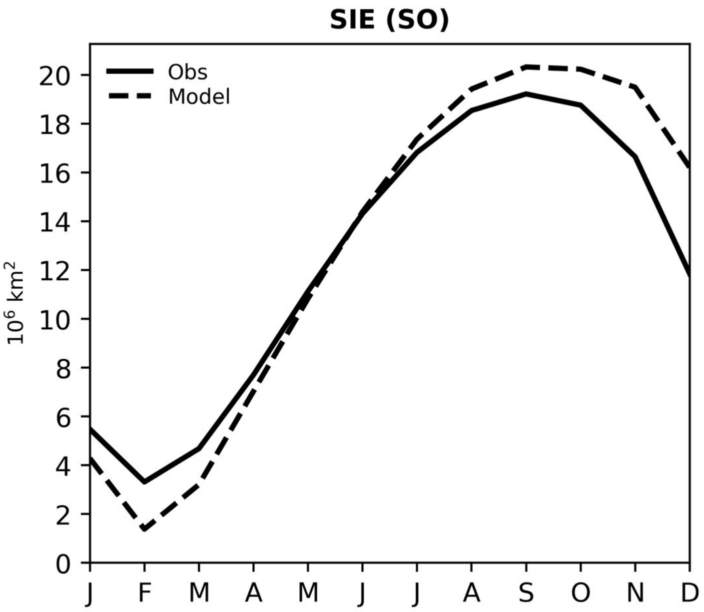

Figure R1.3: Seasonal cycle of the total Antarctic SIE in the observations (solid line) and in the model (dashed line). This figure will be added to the supplementary material.

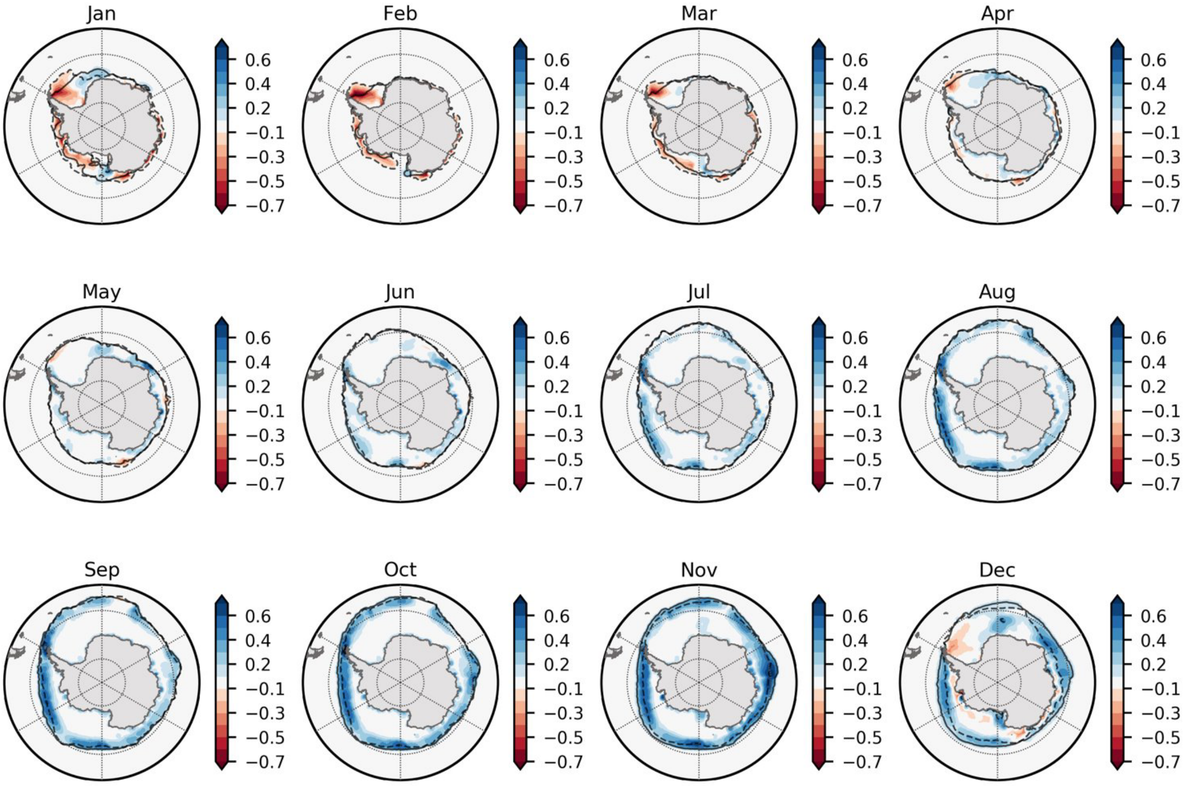

Figure R1.4: Differences in the monthly SIC climatology between the model and the observations. This figure will be added to the supplementary material.

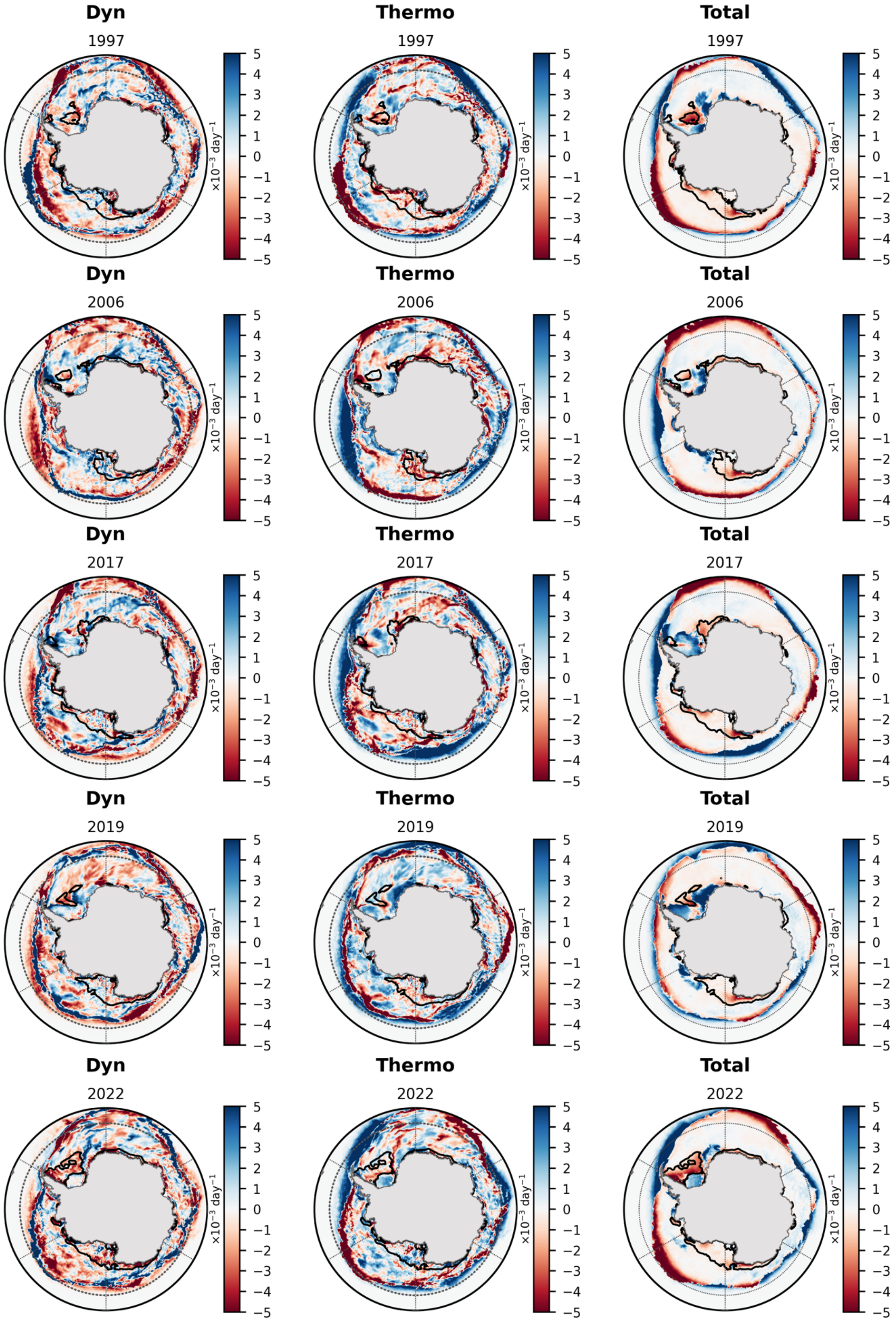

Figure R1.6: Shading: NDJ anomalies of the dynamic (left), thermodynamic (middle) and tendency (right) terms in the years with total SIE minima. Contours: areas with anomalous SIC = 0.1 in the corresponding year in JFM. This figure will be added to the supplementary material.

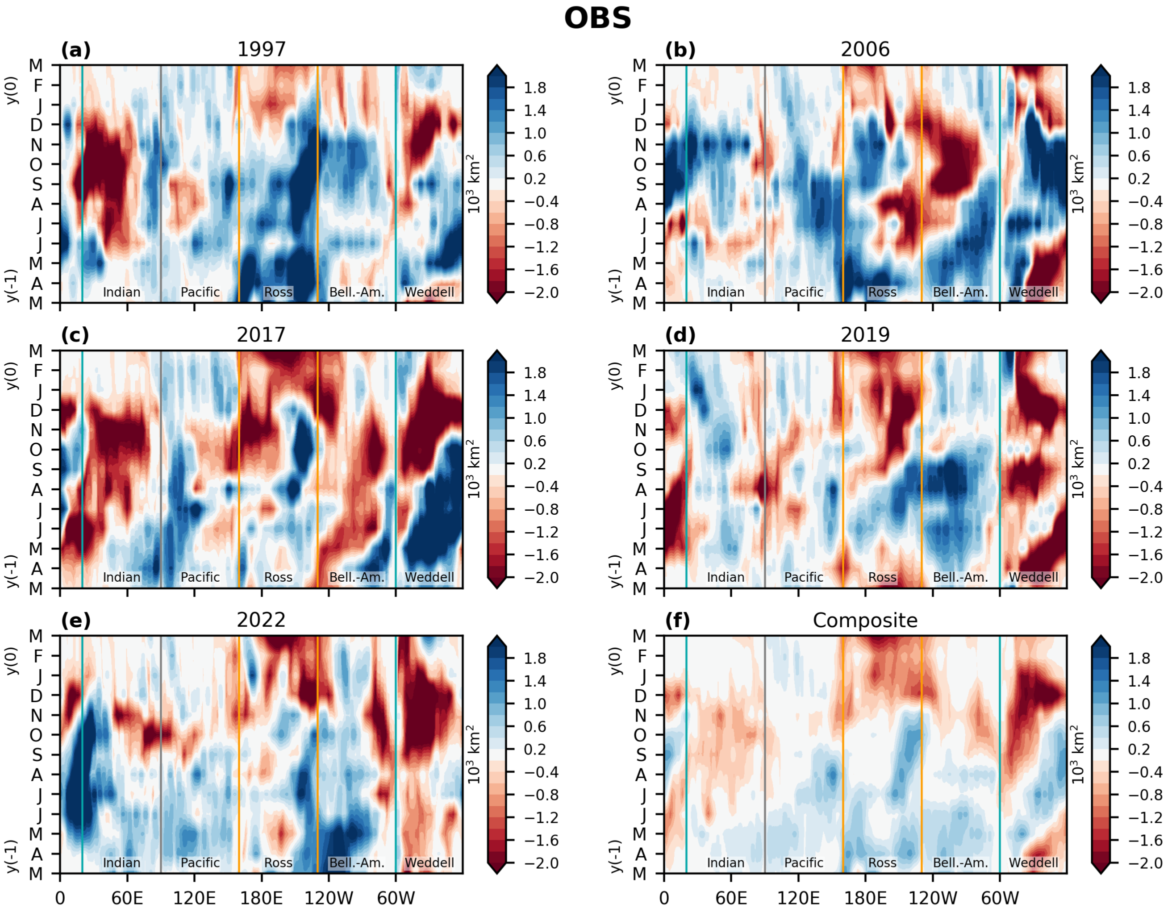

Figure R2.1: Time-longitude sea ice area anomalies for the five total minima and their composite, in the observations. The vertical axis displays time, starting from one year before the minimum at the bottom (e.g. March 1996 in the first panel) up to the event itself (e.g. March 1997) at the top. This figure will be added to the supplementary material.

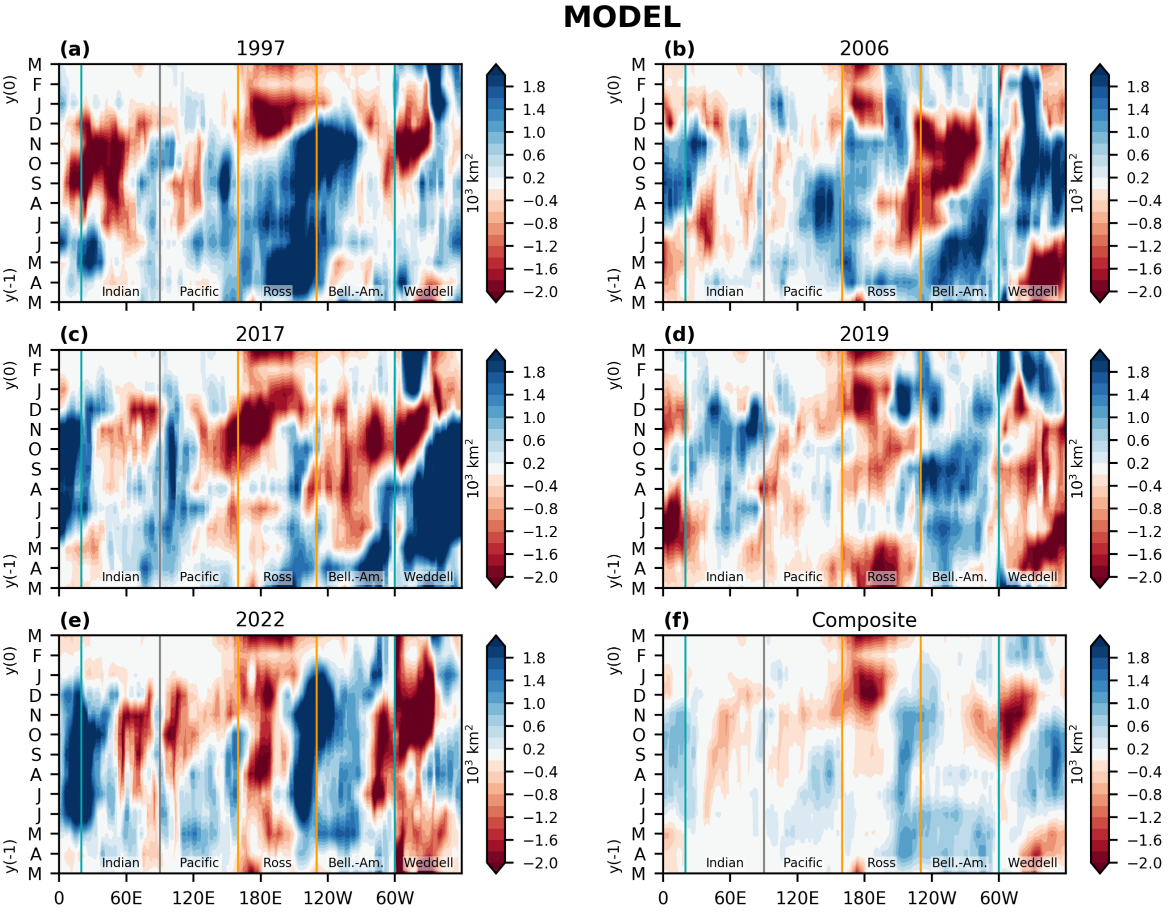

Figure R2.2: Time-longitude sea ice area anomalies for the five total minima and their composite, in the model. The vertical axis displays time, starting from one year before the minimum at the bottom (e.g. March 1996 in the first panel) up to the event itself (e.g. March 1997) at the top. This figure will be added to the supplementary material.

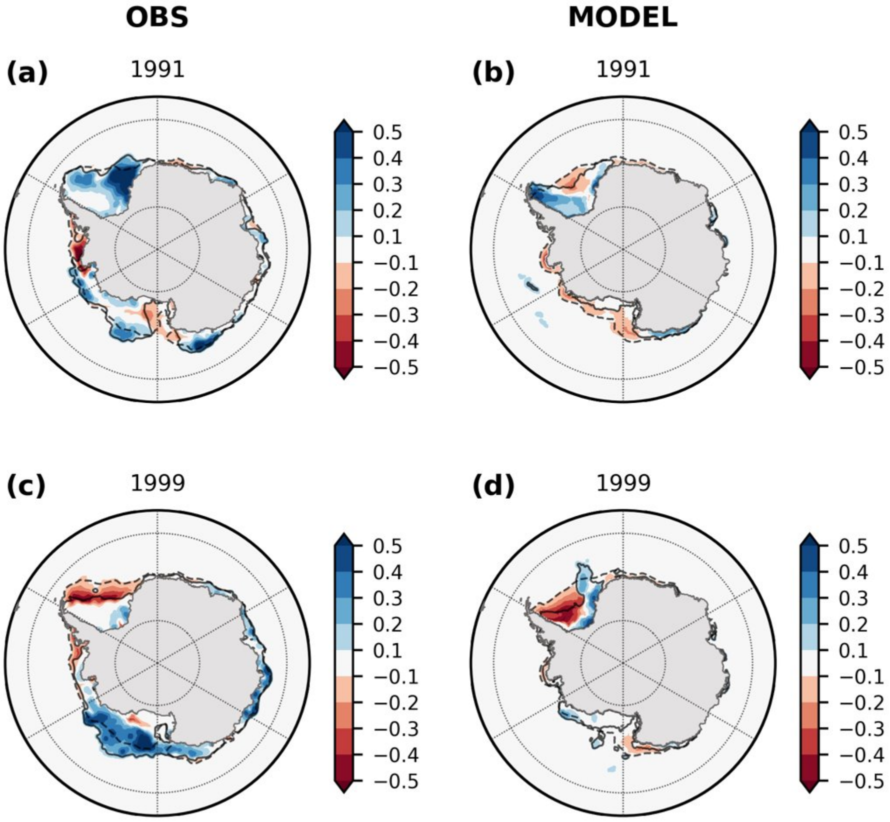

Figure R2.3: Shading: observed (left) and modelled (right) SIC anomalies in JFM 1991 (top) and 1999 (bottom). Contours: sea ice edge (SIC=0.15) in the respective years (solid line) and in the climatology (dashed line).

Reviewer 3

This manuscript attempted to summarize sea ice concentration (SIC) minima events for the Antarctic region and find out universal mechanisms that apply to all events. Results from this study provide important information for understanding the occurrence of Antarctic SIC minima events which might happen more frequently in the future in the context of climate change. The manuscript is overall well organized, with comprehensive analyses and clear interpretations of the results. There are some major issues to be addressed for this work to be published in The Cryosphere as follows.

We wish to thank you for review and for your valuable comments, we now plan to prepare a new version of the 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. While this study has made lots of efforts in revealing the control of major climate modes on the SIC minima events, as shown in the results, it is hard to attribute the occurrence of these events to a universal anomalous pattern of any single climate mode or a combination of climate modes. The authors finally attributed these events to north-westly wind anomalies in the Weddell Sea and south-westerly wind anomalies in the Ross Sea. These conclusions are to some extent useful, but probably not so helpful if we want to predict future SIC minima events. In fact, in addition to SAM and ASL, there are other climate modes that can affect the sea ice anomalies in the Ross Sea and Weddell Sea, such as ENSO, PSA, PSA2, zonal wave 3, etc. While there are interactions among these climate modes, I still suggest the authors to further examine the patterns of climate modes other than SAM and ASL and see if a systematic anomalous pattern of these modes or their combinations can be found for the SIC minima events. If such a pattern can be found, the information would be much more useful for the scientific community to understand the future occurrence of SIC minimum events.

Thanks for raising this point. We agree that it would be beneficial for the community to relate SIE minima to some specific and known modes of climate variability, such as ENSO. Indeed, previous case-studies have related single SIE minima to specific ENSO, SAM, ZW3 or even stratospheric polar vortex configurations (e.g. Stuecker et al. 2017, Schlosser et al. 2018, Wang et al. 2019). However, it is not easy to find a common remote driver for these events. For instance, the 2022 event corresponded to La Niña conditions, while the previous one (2019) coincided with an El Niño. As you mention, it is likely "a combination" rather than a single mode that may be, eventually, identified as a systematic driver. This is also what we conclude in our manuscript: the predominant winds that we identify as related to summer SIE minima may arise from the superposition of different modes of variability. While very interesting for prediction applications, trying to identify such systematic forcing would imply a different approach and an entire new analysis that would likely constitute a paper itself. Furthermore, our current limited sample of five main events would not be suitable. We hope that you will understand that this is out of our scope for this study, but we are happy to take this suggestion as inspiration for future work.

Schlosser, E., Haumann, F. A., and Raphael, M. N.: Atmospheric influences on the anomalous 2016 Antarctic sea ice decay, The Cryosphere, 12, 1103–1119, https://doi.org/10.5194/tc-12-1103-2018, 2018.

Stuecker, M. F., C. M. Bitz, and K. C. Armour (2017), Conditions leading to the unprecedented low Antarctic sea ice extent during the 2016 austral spring season, Geophys. Res. Lett., 44, 9008–9019, doi:10.1002/2017GL074691.

Wang, G., Hendon, H.H., Arblaster, J.M. et al. Compounding tropical and stratospheric forcing of the record low Antarctic sea-ice in 2016. Nat Commun 10, 13 (2019). https://doi.org/10.1038/s41467-018-07689-7

2. Lines 140-141: Separating the processes controlling the tendency of SIC into a dynamical term and a thermodynamical term is a simple way. Though the authors mentioned more detailed terms in the texts that are included in the dynamics and thermodynamics (Lines 145-140), it is better to analyze these terms in Section 3.5 (Sea ice budgets), so the readers could know which specific terms are dominant as well as the physical processes behind these terms.

Thanks for this comment. While it is not possible to separate all the physical processes contributing to the dynamic and thermodynamic terms, we agree that a more detailed discussion of their meaning and interpretation is needed. Thus, following this and the other reviewer's comments, we propose to expand Section 3.5.

In particular, we propose to clarify what processes are accounted for in the dynamic and thermodynamic terms in our framework. For instance, the albedo feedback (ocean warming due to sea ice being transported away leading to more melting) could in principle be 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 place in Section 3.5 (L306):

Note that the dynamic and thermodynamic terms and 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 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. 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.

3. As there exist notable differences between the modelled and observed SIC anomaly patterns in the Weddell Sea and the Ross Sea (Figs. 2 and 3), the authors should discuss how the model performance would affect the sea ice budget analysis in the discussion section.

Thanks for this comment. More details on the model's performance and limitations have also been asked by the other reviewers. To address all these concerns, we propose to first add a detailed description and quantification of the main model’s biases in the SIE/SIC seasonal cycle (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 km2 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 the 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 km2, 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 km2). 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 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).*

Specific comments

4. Lines 137-139: It is hard to understand the two criteria for selecting SIC minima events in the Weddell Sea, especially why different thresholds much be chosen for observations and model results for either criterion, and the authors should provide more explanations.

We understand your concern and we hope we can clarify this. This result is a choice made after several tests. One option, for instance, would be to use the same threshold for both the model and the observations, such as 1σ, but this would lead to a selection of only 2 total minima (as currently mentioned in L124). Another idea would be to apply the criteria separately (e.g. below -1σ in the observations, regardless of what the model is doing, and vice versa). This would lead to a selection of different years for the model and the observations, some of which are shared, some are not. In that case, one risk is to select modelled minima that are only related to the model's own variability and do not provide useful information of the physical mechanisms driving "real" SIE minima. These are just some examples. In the end, the selected criterion was, in our view, the best choice in order to have a reasonable sample of physically insightful events.

We propose to clarify this in the discussion (Section 4, L382):

Note that alternative selection criteria for the minima could be used. For instance, Turner et al. (2019) simply considered the lower quartile of sea ice annual minimum extents. The method does not strongly affect the final collection of years in the observations, where the total minima could be identified almost by eye (Fig. 1a), but in our case it is relevant for the comparison with the model. We have tested various criteria, such as different thresholds or the selection of distinct years for the observations and the model, but they typically lead to too small or inconsistent samples, since the model sometimes simulates SIE minima that are not observed, and vice versa. The final selection of events is based on concurrent conditions for both the observed and modelled time series to ensure the analysis of a reasonable number of observed events that are also captured by the model.

5. Lines 145-146: In my mind divergence results from advection, and the two terms should not be treated separately, though I do see such separations in other literatures. I hope this can be clarified here.

We propose to add a short description of what the two terms mean in that same sentence, hoping this helps to clarify:

Typically, the dynamic term encapsulates the effect of ice motion, namely advection (local *import/export of sea ice) and divergence (openings/closures in the pack), while the thermodynamic term represents local ice melting and formation.*

6. Line 280: ENSO is not examined in this study in a straightforward way so this sentence needs to be revised. Meanwhile, though ENSO can have influence on the ASL, I still suggest the authors to examine ENSO separately.

We propose to remove the explicit mention of ENSO from this sentence, which would then read:

Though it is challenging to identify common large-scale circulation anomalies, we have shown that the regional wind conditions in the Ross and Weddell sectors share some similarities across the minima.

7. Lines 283-284: Southwest wind anomalies can actually bring colder air masses from the Antarctic continent to the Ross Sea and increase the ice freezing, rather than causing "thermodynamic melting" mention here. So how to understand the ice melting?

This sentence was placed before the actual budget analysis and was meant as a bridge between the two sections, but it is true that without the context (and particularly the revision of Sec. 3.5 proposed here) it could be confusing. To avoid misunderstandings, we propose to reformulate it:

The exact roles of dynamics and thermodynamics in leading to the summer SIC anomalies are examined in detail in the next section, for both regions.

8. Lines 314-315: Any explanations for southerly wind in 2017 over the Weddell Sea, which is different from the wind patterns in other years?

We agree that these anomalies are interesting, but we do not have an explanation for them, other than what is suggested in the literature. The event of 2017 has been shown to be linked to an exceptionally strong negative phase of the SAM in November-December, preceded by a positive ZW3 pattern from May to August (Stuecker et al. 2017, Schlosser et al. 2018). Influences of tropical forcings from the Pacific and Indian Ocean (Stuecker et al. 2017, Purich and England, 2019; Schlosser et al. 2019, Meehl et al. 2019) and the stratospheric polar vortex (Wang et al. 2019) have been suggested as favouring factors.

Meehl, G.A., Arblaster, J.M., Chung, C.T.Y. et al. Sustained ocean changes contributed to sudden Antarctic sea ice retreat in late 2016. Nat Commun 10, 14 (2019). https://doi.org/10.1038/s41467-018-07865-9

Purich, A., & England, M. H. (2019). Tropical teleconnections to Antarctic sea ice during austral spring 2016 in coupled pacemaker experiments. Geophysical Research Letters, 46, 6848– 6858. https://doi.org/10.1029/2019GL082671

Schlosser, E., Haumann, F. A., and Raphael, M. N.: Atmospheric influences on the anomalous 2016 Antarctic sea ice decay, The Cryosphere, 12, 1103–1119, https://doi.org/10.5194/tc-12-1103-2018, 2018.

Stuecker, M. F., C. M. Bitz, and K. C. Armour (2017), Conditions leading to the unprecedented low Antarctic sea ice extent during the 2016 austral spring season, Geophys. Res. Lett., 44, 9008–9019, doi:10.1002/2017GL074691.

Wang, G., Hendon, H.H., Arblaster, J.M. et al. Compounding tropical and stratospheric forcing of the record low Antarctic sea-ice in 2016. Nat Commun 10, 13 (2019). https://doi.org/10.1038/s41467-018-07689-7

9. The legend or caption of Fig.7 should also explain the cross symbols in the two panels.

Thanks, we will fix this in the revised version of the manuscript.

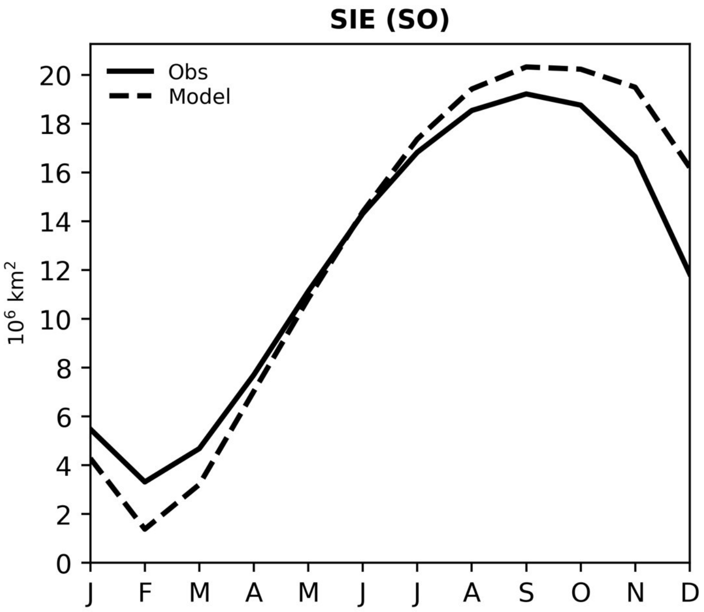

Figure R1.3: Seasonal cycle of the total Antarctic SIE in the observations (solid line) and in the model (dashed line). This figure will be added to the supplementary material.

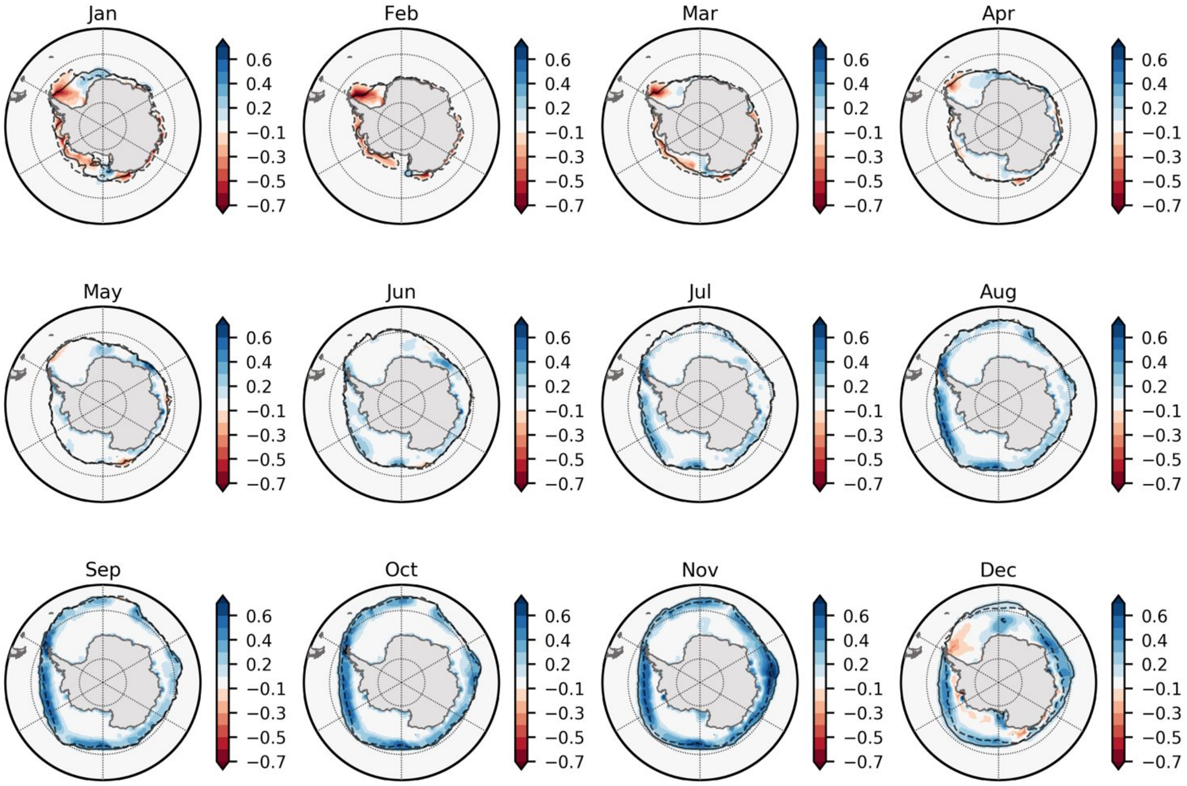

Figure R1.4: Differences in the monthly SIC climatology between the model and the observations. This figure will be added to the supplementary material.

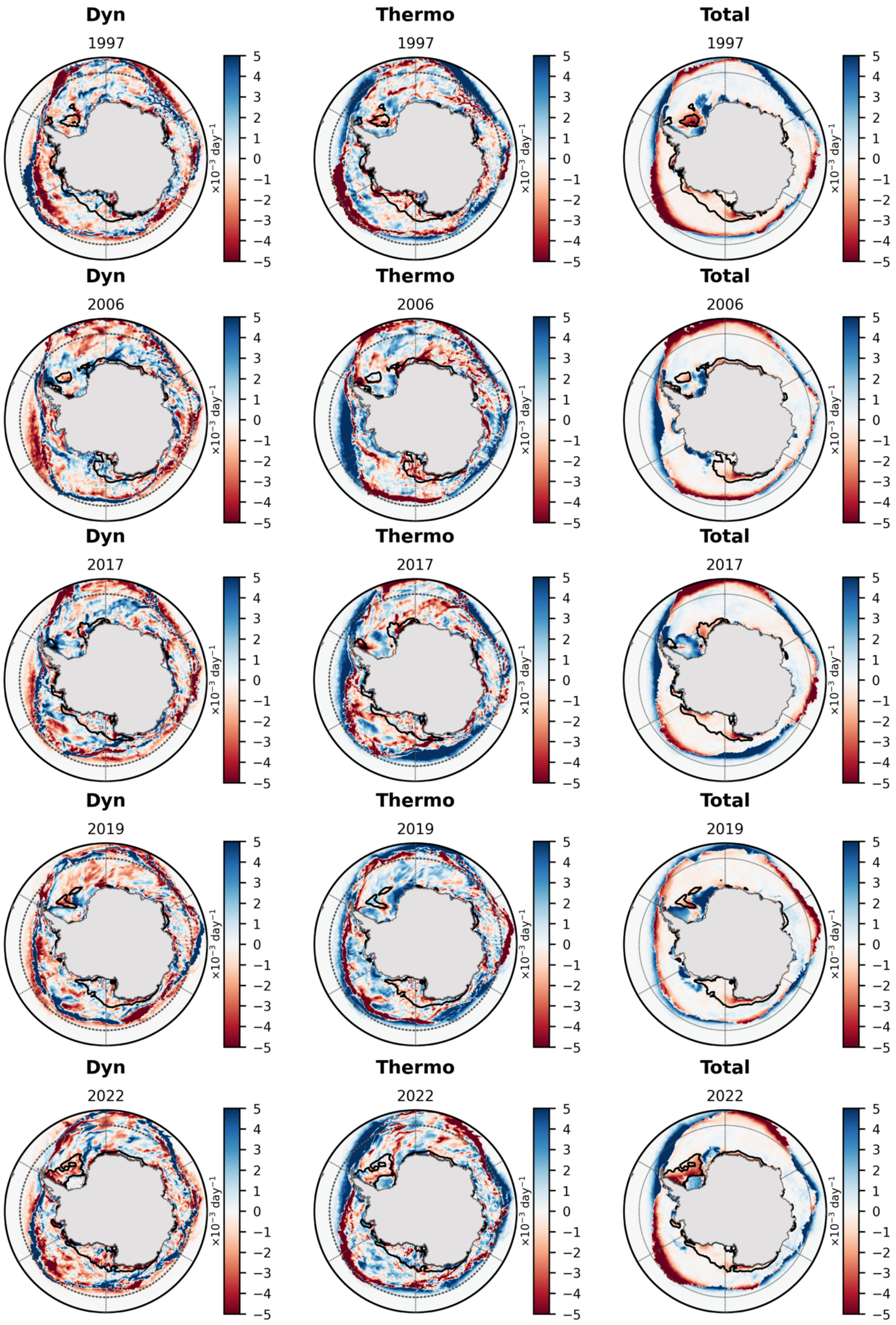

Figure R1.6: Shading: NDJ anomalies of the dynamic (left), thermodynamic (middle) and tendency (right) terms in the years with total SIE minima. Contours: areas with anomalous SIC = 0.1 in the corresponding year in JFM. This figure will be added to the supplementary material.