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.*

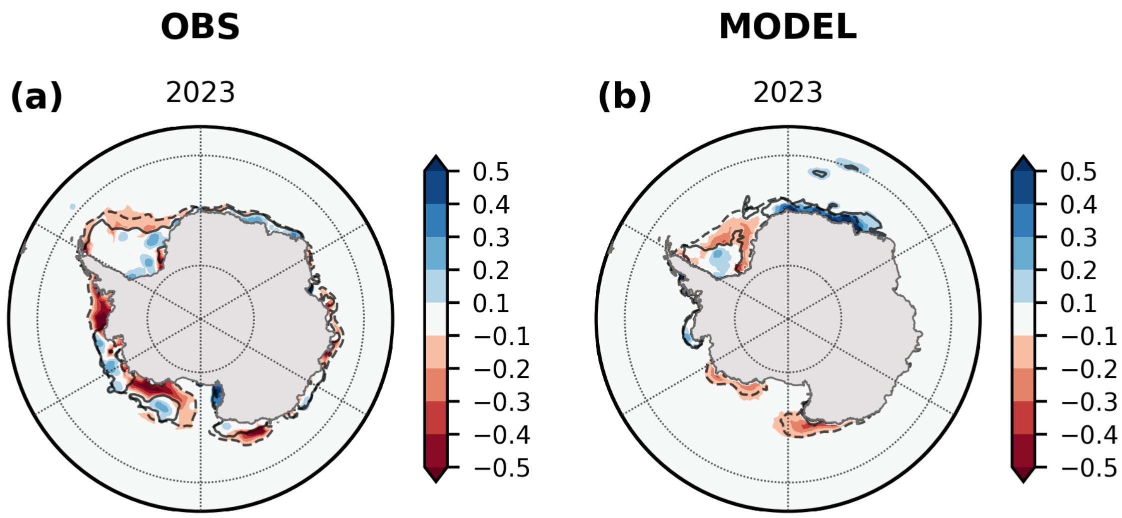

Figure R1.1: Shading: observed (left) and modelled (right) SIC anomalies in JF 2023 (March is excluded due to data unavailability). Contours: sea ice edge (SIC=0.15) in 2023 (solid line) and in the climatology (dashed line). This figure will be added to the supplementary material.

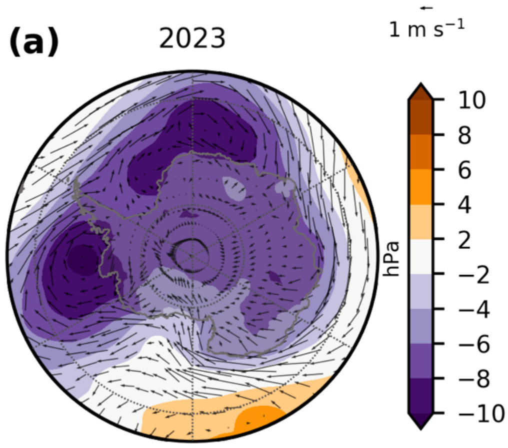

Figure R1.2: SLP (shading) and 10-m wind (arrows) anomalies in OND 2022, before the 2023 summer minimum. This figure will be added to the supplementary material.

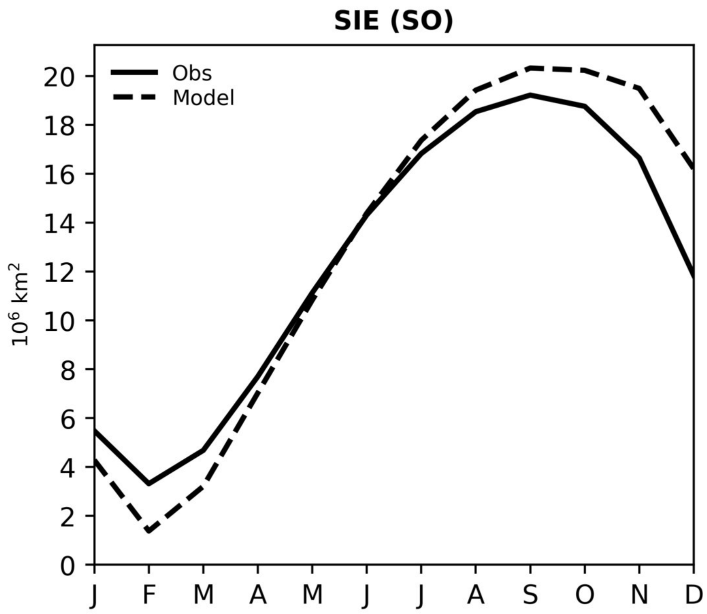

Figure R1.3: Seasonal cycle of the total Antarctic SIE in the observations (solid line) and in the model (dashed line). This figure will be added to the supplementary material.

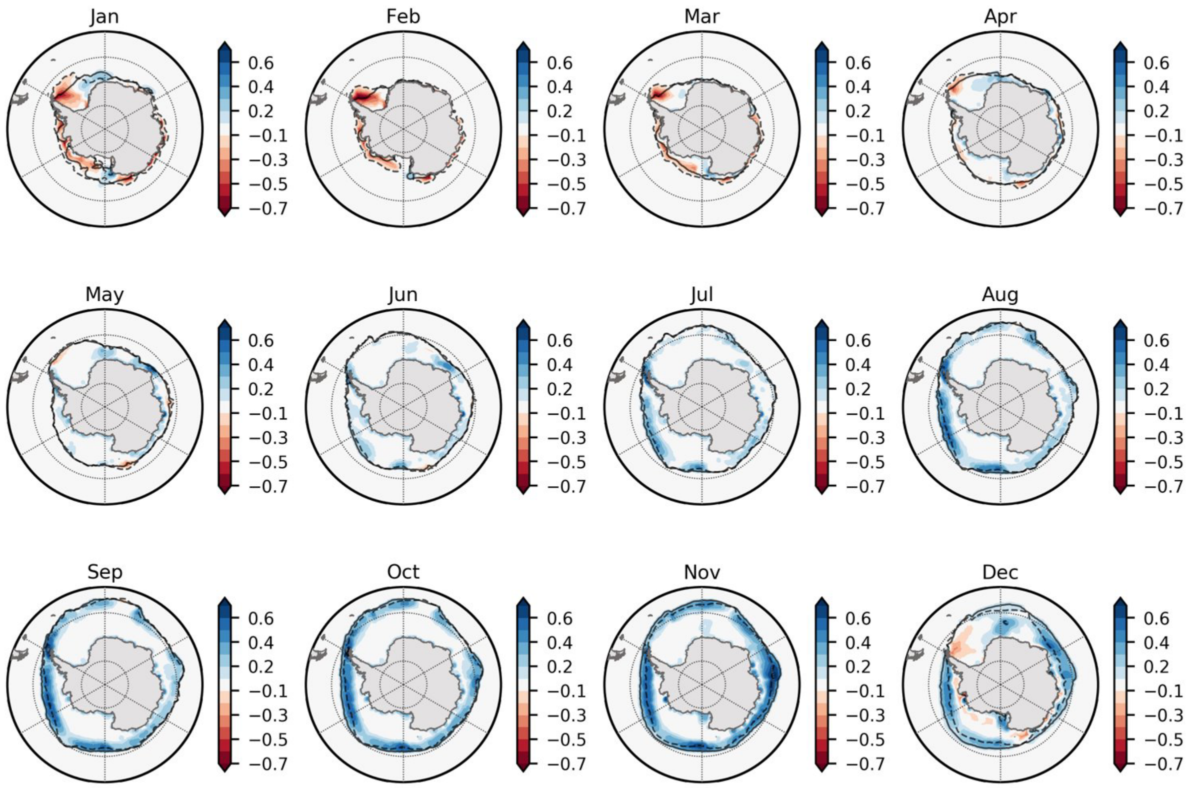

Figure R1.4: Differences in the monthly SIC climatology between the model and the observations. This figure will be added to the supplementary material.

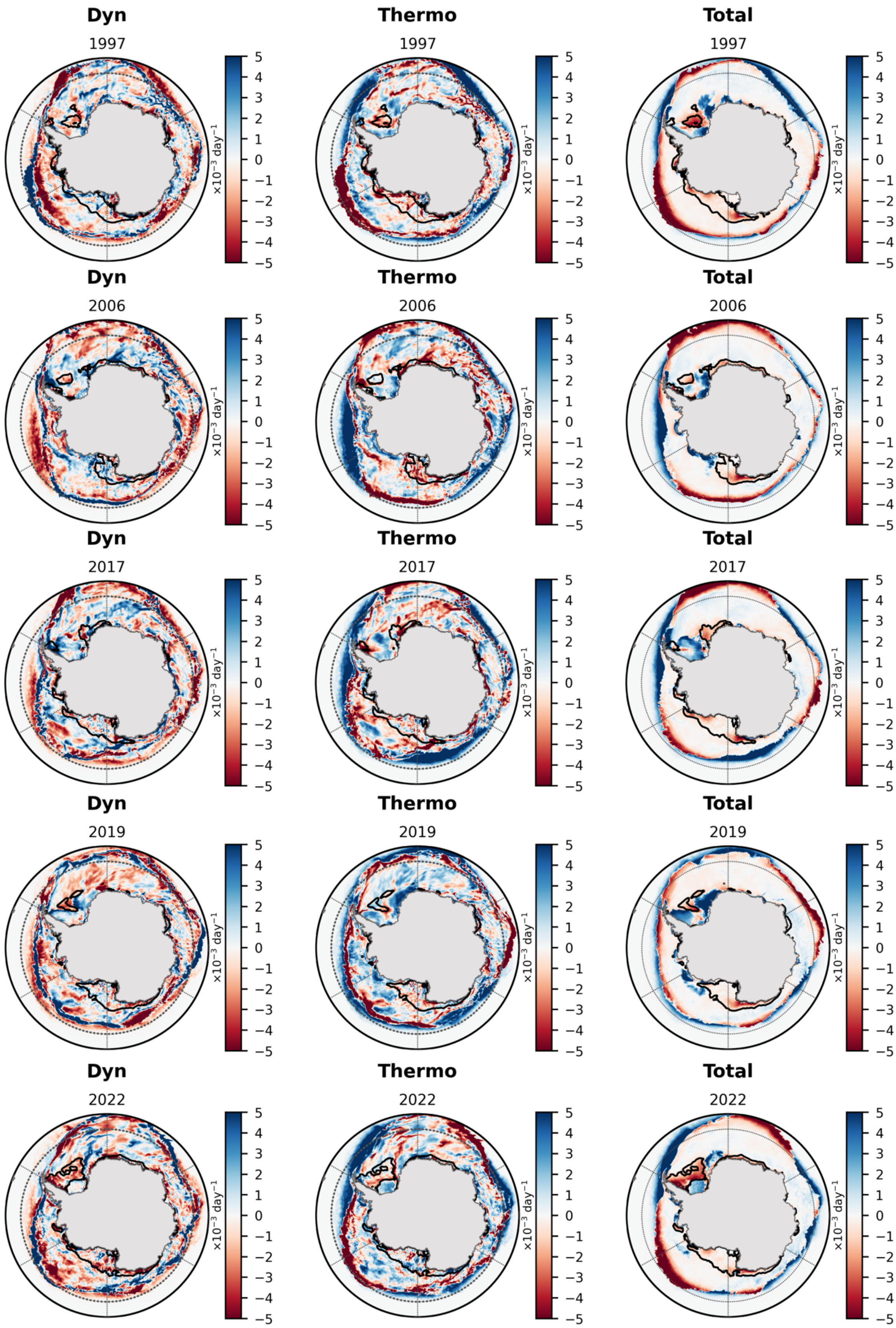

Figure R1.6: Shading: NDJ anomalies of the dynamic (left), thermodynamic (middle) and tendency (right) terms in the years with total SIE minima. Contours: areas with anomalous SIC = 0.1 in the corresponding year in JFM. This figure will be added to the supplementary material.

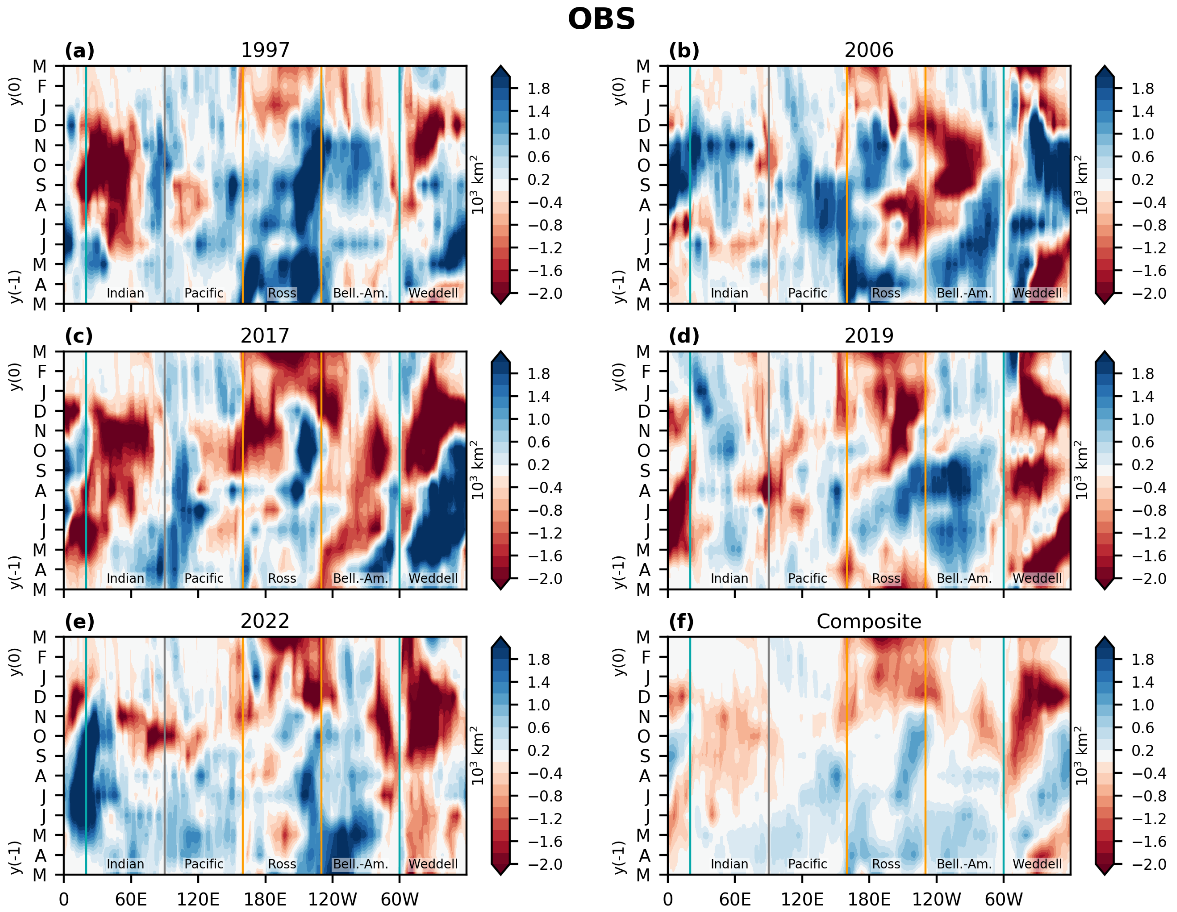

Figure R2.1: Time-longitude sea ice area anomalies for the five total minima and their composite, in the observations. The vertical axis displays time, starting from one year before the minimum at the bottom (e.g. March 1996 in the first panel) up to the event itself (e.g. March 1997) at the top. This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

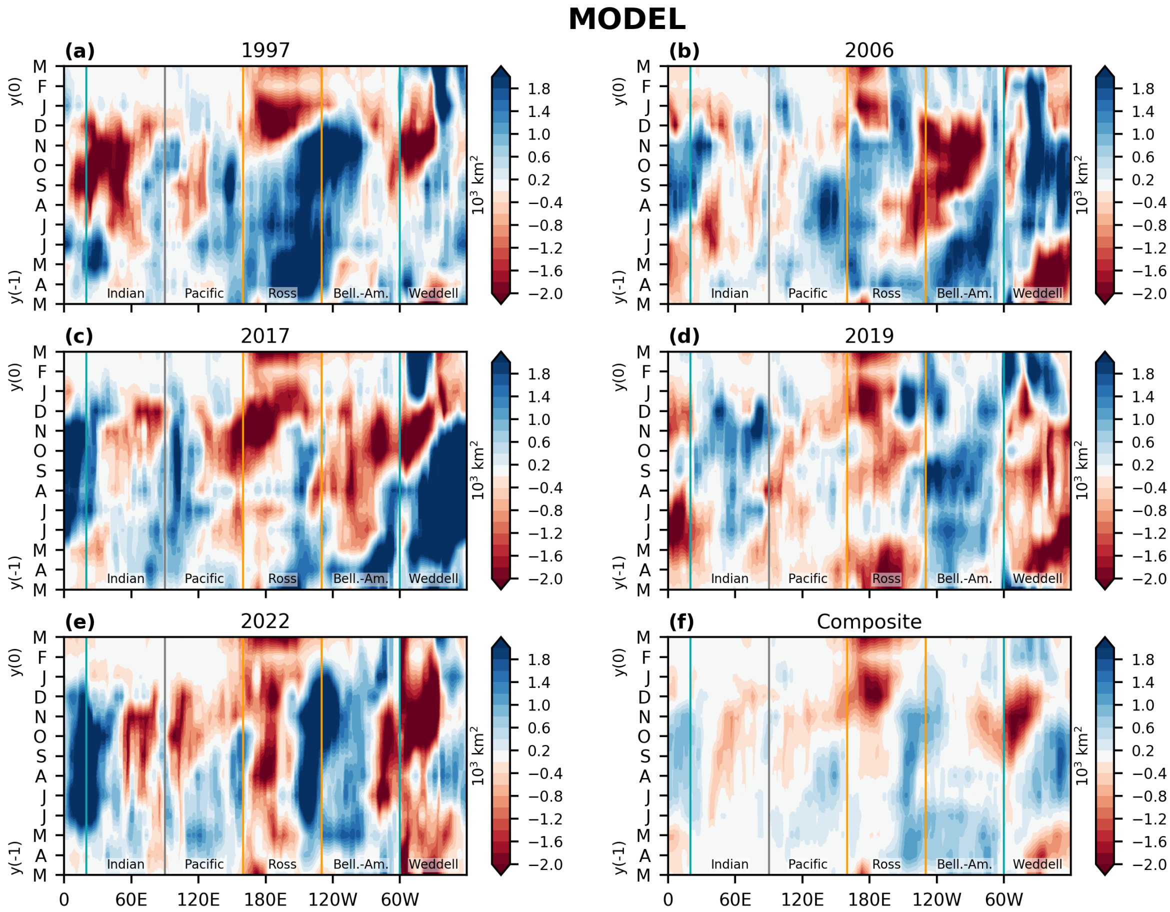

Figure R2.2: Time-longitude sea ice area anomalies for the five total minima and their composite, in the model. The vertical axis displays time, starting from one year before the minimum at the bottom (e.g. March 1996 in the first panel) up to the event itself (e.g. March 1997) at the top. This figure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

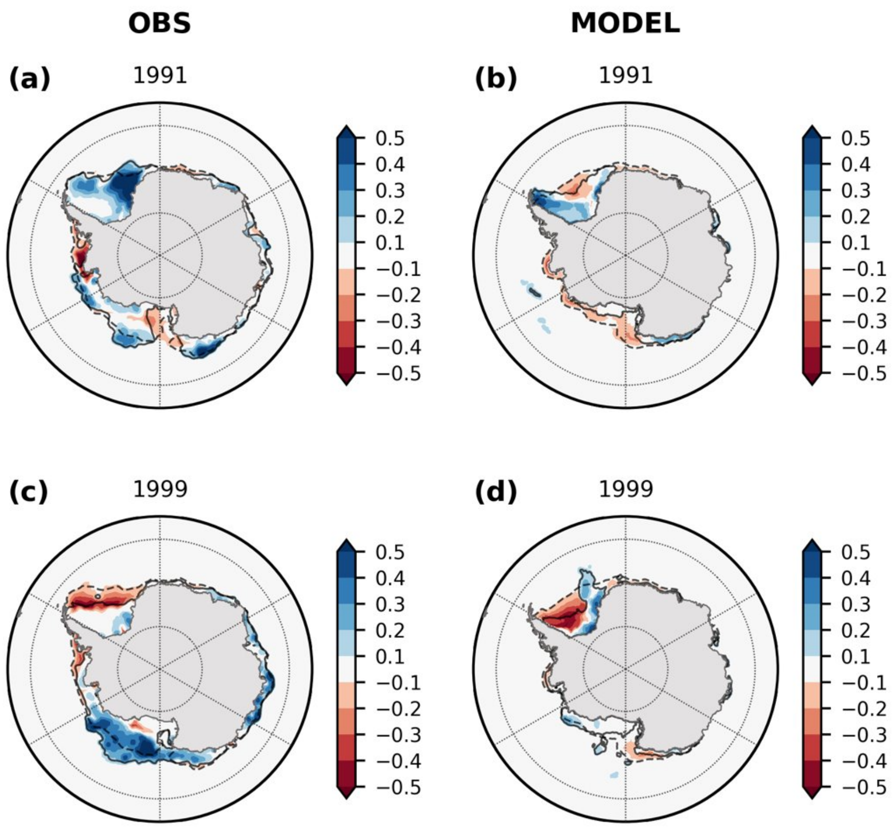

Figure R2.3: Shading: observed (left) and modelled (right) SIC anomalies in JFM 1991 (top) and 1999 (bottom). Contours: sea ice edge (SIC=0.15) in the respective years (solid line) and in the climatology (dashed line).