Dear Dr. Li,

Following your decision dated February 15, 2024, we have prepared a new version of our manuscript "Atmospheric drivers of Antarctic sea ice extent summer minima". We have modified the manuscript according to the reviewers' comments and addressed point-by-point their concerns in the attached document. We would like to thank you for managing our submission and the reviewers for their useful comments; we hope that you will find this new version of the manuscript adequate for publication in The Cryosphere.

We would like to stress that this study, first submitted on March 7, 2023, has been now revised by a total of four experts, plus your own assessment.
In addition to the three reviews from the first iteration, we have received a full new assessment by another referee for the revised version. All reviewers considered this study valuable and scientifically sound after some clarifications. Given the already long and detailed revision process and that we have implemented all the important suggestions, we hope that it will be possible to reach soon a final decision for the latest version of the manuscript.

Finally, we would like to highlight that we consider this study as a "research article" and not as a "review article", as it is currently indicated. We kindly ask to adjust the manuscript type to the "research article" as initially specified.

Thanks and best regards,

Bianca Mezzina, on behalf of all co-authors

Report 1

1. I think the authors should include the sea ice concentration budget equation, and think more carefully about the interpretation of the divergence term. I talked with my colleagues working on sea ice dynamics, and we don't think "divergence (openings/closures in the pack)" is a good explanation. What is the force for the openings/closures?

To address this concern, we have fully revised the first part of Sect. 2.3 on the SIC budget, expanding the description and the meaning of all the terms. Specifically, we have included the definitions of advection and divergence based on the sea ice velocity (**u**) and clarified that they are mostly driven by the surface winds. We have also added a general equation for the model's budget diagnostics. However, we think that including more details of their computation in the model would not be beneficial as they are often trivial and do not help with the physical interpretation of the terms. We also recall that these diagnostics have been used and discussed in previous studies, such as Barthélemy et al. 2018, as indicated in the text.

*L150-167: The SIC evolves over time in response to a variety of processes, which can be categorized as thermodynamic or dynamic. New ice formation in open waters and ice melt are thermodynamic processes increasing and decreasing the SIC, respectively. Vertical processes in the sea ice such as bottom growth, bottom melt, surface melt and snow ice formation are also thermodynamic contributors to SIC changes, directly or through their impact on the sea ice thickness. The dynamic processes that influence SIC are essentially related to the sea ice velocity (**u**), which is in turn driven by ocean drag and, mostly, wind stress. These processes include contributions from sea ice advection ($\boldsymbol{u} \cdot \nabla SIC$) and divergence ($SIC\ \nabla \cdot \boldsymbol{u}$). The advection represents the local import (export) of sea ice, effectively increasing (decreasing) the local SIC, while the divergence encapsulates how the sea ice motion leads to openings (closures) in the pack, thus leading to a local decrease (increase) in SIC. Additionally, other mechanisms like mechanical redistributions (e.g. ridging, rafting) also act as dynamic drivers of SIC variations. In our model, at each time step and grid point, the simulated total change in SIC (tendency) is split into thermodynamic and dynamic contributions:*

$$\frac{dSIC}{dt} = \frac{dSIC}{dt}_{thermo} + \frac{dSIC}{dt}_{dyn}$$

The first term represents the change in SIC due to thermodynamics, while the second term includes the contribution of all dynamic processes accounted for in the model (e.g. Barthélemy et al. 2018). In Sect 3.5., we use these diagnostics to evaluate the contribution of dynamic and thermodynamic processes to the summer SIE minima.

2. The authors did not respond to my 2nd comment in Comment 6 "Meanwhile, though ENSO can have influence on the ASL, I still suggest the authors to examine ENSO separately." I understand making such analysis may be difficult and involve substantial work, but at least some discussions should be provided.

We regret to learn that you did not find our reply satisfactory, since we had already motivated our choice to not discuss explicitly ENSO in the response to your Comment 1. We had agreed that it would be beneficial for the community to relate SIE minima to some known modes of climate variability such as ENSO, but we also highlighted that there is not a single, common remote driver for these events as they are likely related to the superposition of several modes. We had also stressed that our current limited sample of five main events is not suitable to identify precisely the contribution of the different modes. We had then revised a sentence in the manuscript that was mentioning ENSO, as requested in Comment 6.

To further address your concern, we have now prepared a figure to examine the link between ENSO and the SIE summer minima. Figure R1 depicts the time series of the observed JFM SIE anomalies (as in Fig. 1a in the manuscript) together with the DFJ Niño3.4 index (N3.4), computed as the area-averaged SST anomalies from HadISST over the Niño3.4 region (5°S–5°N, 170°–120°W). Note that the N3.4 for a given year refers to the winter between that year and the previous one (e.g. the strong positive N3.4 in 1998 refers to the famous 1997-98 El Niño).

The two time series are uncorrelated over this period (correlation ~ -0.01). Additionally, there is no clear link between El Niño/La Niña events and the SIE minima. For instance, one could define *weak El Niño* (*weak la Niña*) events if the N3.4 is above (below) 0.5 (-0.5) σ, and *El Niño* (*la Niña*) years if N3.4 is above (below) 1 (-1) σ. The thresholds are indicated in Fig. R1 in light and dark grey. Then, in our sample of total SIE minima, there are two years where the summer minima follow a weak La Niña (2006, 2022), two that follow neutral conditions (1997, 2017) and one that follows a weak El Niño (2019). Thus, a preferred ENSO phase related to the summer SIE minima does not emerge. We have briefly commented about this in the manuscript, but we think that no further discussion is possible or needed as no role of ENSO can be identified in the summer sea ice minima (as defined in our study) observed over the past 40 years. It is nevertheless important to stress that this lack of correlation between the ENSO phase and sea ice applies to circumpolar conditions, but that ENSO and other modes variability (e.g., SAM) can have regional signatures on the sea ice.

L425: Even focusing on a single mode such as ENSO, no link emerges, since the summer minima follow different ENSO phases (El Niño in 2019, neutral conditions in 1997 and 2019, La Niña in 2006 and 2022).

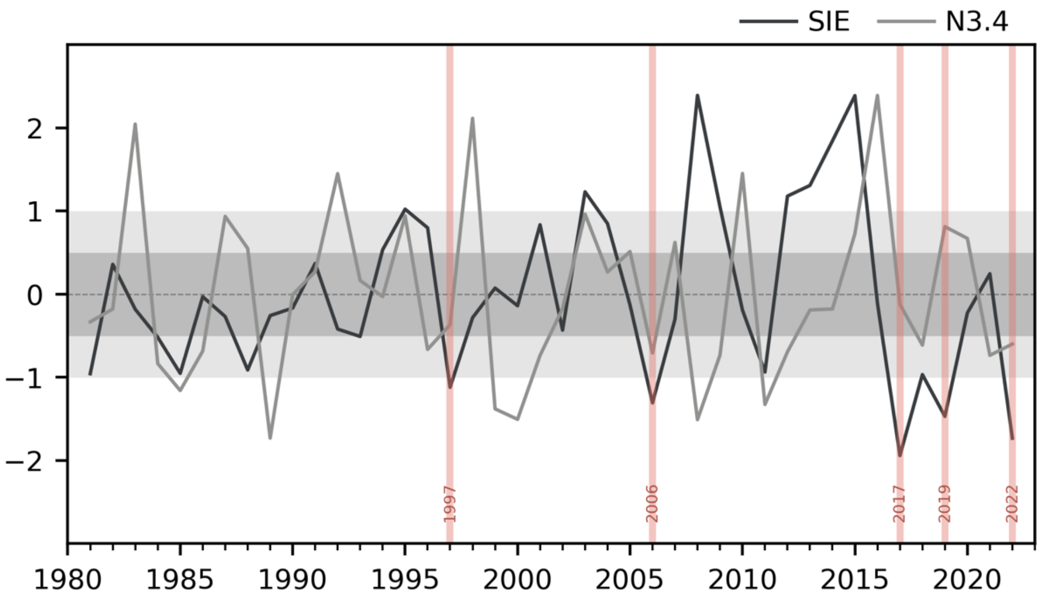

Figure R1. Black line: standardized SIE anomalies in JFM computed over the total SO domain in the observations. Grey line: DJF Niño3.4 index from reanalyses. Dark and light grey shadings indicate the ±0.5 and ±1σ, respectively. Years with a minimum SIE are marked in red.

Report 2

It is an interesting and important topic for exploring the potential drivers for the minimum extent of the Antarctic summer sea ice. The authors try to answer which factors, including pre-winter sea ice extent, spring wind, and dynamics and thermodynamics, attribute to the minimum extent of summer sea ice for the whole Antarctic and two key sea sectors, i.e., the Weddel and Ross seas, respectively. They use the ERA-5 reanalysis data as atmospheric forcing data, satellite data as sea ice extent/concentration observations, and an ocean-sea ice model for sea ice concentration budget estimation. The structure of the manuscript is well organized and easy to follow. But my general concern lies in the robust statistical or theoretical analysis of the main results. Thus, I suggest a major revision.

Major:

The title is too ambitious with the general statement "atmospheric drivers" for the Antarctic sea ice extent summer minima. If this is a review article with a robust analysis of all the potential atmospheric factors based on existing studies, I think the title is just fine. However, the purpose of this study is first to explore the patterns of the minima itself and then to investigate several factors (pre-winter sea ice extent, spring wind, and dynamic and thermodynamic processes) that may correlate with or lead to the summer minima.

We regret to read that you consider our title, "Atmospheric drivers of Antarctic sea ice extent summer minima", too ambitious. We believe it is a suitable title for a study that tries to elucidate how the previous and concurrent atmospheric conditions are related to SIE extreme lows. We perform additional analyses, such as the impact of the sea ice wintertime preconditioning and the contribution of dynamic/thermodynamic processes, but they are all directly or indirectly related to atmospheric conditions. Just including 'Drivers of Antarctic sea ice' would indeed be too ambitious and not specific enough. Being more precise in the title, specifying the processes analyzed, would make the title too long and too heavy. We thus think that the current title is a good compromise that effectively summarizes the main results of the paper and propose to keep it.

Within the abstract, I'm convinced by the first one-third findings of this work (L16-20), such as the contribution of the two sea sectors, the Weddell and Ross seas, to the whole Antarctic minima.

However, I think the main problem of this study is that the other key findings (L20-29) are mainly qualitative guesses from plots and lack robust support with either theoretical or statistical evidence. Here are some examples:

We are sorry to read this. We disagree that our key findings are only qualitative guesses and we think that through the manuscript robust evidence for the main conclusions is provided. We have been probably too careful in the wording of our conclusions, in particular in the abstract, understating our results and potentially giving the reader the feeling that our conclusions are not based on quantitative analyses. However, only a few parts describe our results in a qualitative manner, such as the spatial distribution of SIC and SLP anomalies. This is standard in many studies and this generally helps the reader to better visualize the changes and the contribution of different processes. In contrast, we would like to highlight that we compute quantitative estimates whenever possible and actually draw our conclusions from there. Examples are the contributions of the different sectors to the SIE minima indicated in Table 1, the average wind directions shown in Fig. 6, and the average dynamic and thermodynamic contributions to the sea ice budget in NDJ displayed in Fig. 7. The text of the manuscript has been modified to reflect more accurately the role of those quantitative analyses in our conclusions.

- There are too many uncertain words used throughout the manuscript, such as “seem”, “appear”, “may”, “potential”, etc. For example, the word “seem” appears 23 times in total. This kind of words(“seem/appear”) shows 3 times in the abstract. The authors need to reduce the uncertainty of this work with solid support from data, theoretical analysis, or references.

In most cases, these words just reflect a personal writing style rather than a real uncertainty in the results. Please also note that the word “seem” only appears 13 times in the original manuscript, including the abstract. However, we acknowledge that in some sentences the use of this kind of word could lead to the false impression that our conclusions are uncertain. We have thus modified the manuscript accordingly.

First, we have removed these words in those cases in which the statements are supported by robust evidence or when we simply describe a fact, figure or numerical result without further interpretation:

L128: In the eastern Weddell sector, in contrast, the model systematically overestimates the SIC and extent, particularly in January.
L189: [...] we acknowledge that the model performs worse during the first 10-15 years.
L231: The model also fails in reproducing well the strength of the anomalies, particularly for the 2017 and 2022 events.
L2521: However, the two sectors behave independently, sometimes being even anti-correlated, with regional minima corresponding to regional high SIE in the other sector.
L307: Mixed conditions also emerge for the SAM, with some years characterized by more westerly winds (1997, 2019, 2022) and others in which the westerly winds are weaker.
L429: Instead, the large-scale circulation is more related to the specific events, with anomalous SLP centers of action in distinct locations depending on the case.

Then, we have rephrased some sentences to better differentiate between robust results - in which case we have avoided these kind of expression - and speculations:

L271: The observations thus suggest a role of winter preconditioning at least during the most recent years, but the model is not able to reproduce this behavior, since clear negative anomalies during all six months are seen in 2022 only (Fig. 3d).
L291: Overall, our results indicate a marginal role of a reduced winter extent in preconditioning a summer minimum.
L304: Anomalous SLP centers are present in the Bellingshausen-Amundsen sector during most years, which is consistent with a role of the ASL.
L345: We have shown that the exceptional sea ice melting during the minima is related to anomalous surface winds that share some common regional features throughout most of the events.
L379: Some notable exceptions are however present: in 2022, dynamics leads in both sectors, and in 2011 and 2017 in the Weddell region.

L399: Hence, our results indicate that the variability of the Ross Sea leads the minima, even though the synergy with the Weddell Sea was crucial in the latest cases (2017, 2019, 2022).

Finally, we consider that the use of speculative language is unavoidable in some parts, such as when we discuss the model's issues (e.g. "the lack of negative anomalies in the eastern part *may* be due to the model's tendency to overestimate the sea ice presence there,", Page 8), attempt to interpret physical mechanisms or express pure speculations (e.g. "These five years all fall in the range of the last two thirds of the examined period, which *could* be partly linked to the increased variability over the recent years", Page 7).

The abstract has also been modified with these same criteria.

- There is also a rather long statement on the limitations of the regional ocean-sea ice model NEMO-LIM (L116-129) used in this work. The authors need to strengthen the theoretical introduction and advantages of the model to state why they chose it. If there is no fatal effect on the results of this study, the disadvantages should be summarized clearly into two or three sentences.

This long statement on the limitation of the model is largely resulting from previous iteration with other reviewers. As visible in the public discussion, both Reviewer 1 (W. Hobbs) and the anonymous Reviewer 2 asked for a detailed discussion of the model's limitations. We thus added the statement you refer to, as well as other parts, and we believe that they improved the rigor of the manuscript. On the other hand, we stress that the adequacy of the model's simulation for the objective of the paper is already explained in the Introduction and justified extensively throughout Section 3.

- Some results shown here are too hurried or cursory. The process can be explained or described with more details supported with sufficient evidence. For example, I'm not convinced with a statement L396-397 "this contribution SEEMS to be minor in most years before 2017, indicating that those minima are mainly driven by atmospheric conditions". We cannot draw a solid conclusion just from some uncertain clues. I think the model itself may be a good entry point to explain the summer minima theoretically, despite its nonfatal biases.

We would first like to stress that the example you are referring to is an extract from the "Discussion and conclusions" section, in which we are only summarizing the main findings. In the full analysis (Section 3.3) we describe in detail the results from the model and the observations that finally lead to such a statement. Given that we find contrasting results between the observations and the model and between the large and regional scale, we believe it is reasonable to express our conclusion in a balanced way. However, we think we have provided all the possible details and evidence to justify this statement and that the analysis is rigorous and not at all hurried.
We would also like to highlight that this specific example has been extracted from the most uncertain part of the paper, for which we clearly state in the Discussion that "we have not found a clear and consistent role" and "more data from the next years are needed" (L412 and 415). The rest of the results are sound, and their robustness is not affected by this less conclusive part. In fact, we believe that including this section, despite the unclear conclusion, is an added value to the paper since our meticulous analysis sheds light on the complexity of the topic and highlights explicitly where uncertainty remains. We have reworded this part as follows:

In fact, this contribution is minor in most years before 2017, indicating that those minima are mainly driven by atmospheric conditions. However, a more prominent role emerges for the last events (2017, 2019, 2022). Substantial uncertainty remains and more data from the next years are needed to understand if it is accidental or if new patterns are emerging.

Minor:

L107. The authors didn't clearly state the necessity of comparing the observations and the model.

We briefly commented on why it is useful to complement the observations with a model few lines above, in the previous section:

L92 and 97: This study is based on observations and reanalysis data but also examines results from an ocean-sea ice model driven by observed atmospheric fields. […] Using a model not only allows us to derive more robust conclusions, but also to further investigate the processes at play thanks to output variables that are not available for the observations.

On the other hand, validating the model's results with the observations is a necessary and well-established practice.

L154. The "grid-point area" is not clear. How do you define the grid? The same question is shown in L300 "grid points".

This computation is performed directly on the model's grid. We have clarified this in the new manuscript's version, where this section on the SIC budget (Sect. 2.3) has been fully revised.

L169-170. An incomplete sentence: "Comparison between observations (solid line) and model (dashed line)"

Done.

L262. "all six months" should be "all seven months".

Fixed, thanks.

Please examine the figure references in the whole manuscript to make sure they are correct. I list some incorrect references (not exhausted): L262 "Fig. 3d" should be "Fig. 4d", L263 "Figs. 8, S9" should be "Figs. S8 and S9", L296 "Figs. 10 and S11" should be "Figs. S10 and S11", L314 it seems Fig. 4f instead of Fig. 5f shows the wind effect, L359 "Figs. 3c and 4c" should be "Figs. 2c and 3c", etc.

Thanks for spotting this issue. We have revised the figure numbering throughout the entire manuscript.

L277. 'SIE' should be "Standardized SIE anomalies". Introduction to d-f lacks.

We have added references to panels d-f.

L320 The arrows and hatching in the maps are hard to identify, especially in e and f.

We have modified Figure 5 to make the arrows and hatching more evident.

L324 “Circles indicate total minima, while crosses are for regional ones” is not clear. Adding “years with” before “total minima” and changing “regional ones” to “years with regional minima” may be better. The same issue is with the caption of Figure 7.

Done for both Figure 6 and Figure 7.

I suggest moving some figures in the supplementary file into the main context, such as the Fig. S15 to support the authors’ statement “the relative contribution of dynamic (e.g. ice transport) and thermodynamic (e.g. local melting) processes to the summer minima” (L26-27).

We understand this comment, however we think that it would be tricky to move Fig. S15, which is a massive figure with 15 panels, to the main manuscript. It would require adding an in-depth discussion to the main text that goes beyond our scope and that may confuse the reader. We thus prefer to leave it in the Supplementary for those interested in the details.

L428 “other processes” is unclear.

Some examples of these processes are described in the previous paragraph and in Sect. 2.3. In this last sentence, our intention is to highlight once again the role of the winds, but we think that listing again all the other processes would be redundant.