

Reviewer 2

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This study makes an interesting point about the sensitivity of sea ice to the prescribed atmospheric reanalysis that is used to force sea ice and ocean models. To the extent that integrations forced in this way are still appearing in the literature, this result is valuable.

We thank the reviewer for their comments and feedback. These comments have been invaluable in improving the manuscript. We have adjusted figures (colour bars, and the additional of directional wind fields) as well as the manuscript text in line with their feedback. We are glad the reviewer finds the results valuable for simulations using atmospheric forcing, which we would argue are, and are likely to remain, a common and useful methodology for analysing sea ice processes. We address each of the specific comments in turn below, with the original reviewer comment in black text, our response comment in blue text, and edited manuscript text as appropriate in green.

General Comments

It is inescapable that the feedbacks at the surface are incorrect when atmospheric forcing is prescribed. It is good to see this mentioned in a few places, but I'm left wondering why ignore your own words of caution and carry on doing more uncoupled runs. Relaxing the winds or temperature in a fully coupled model is a better option. It would be good to write this paper in a way that informs relaxation efforts too since the target of relaxation is generally reanalyses.

it would be more valuable if the study could say which field or fields are the culprit for driving the sea ice differences that they found. The evaluation of differences between the reanalyses in this study is helpful, but doesn't go far enough. Plus the figures are quite hard to interpret when the range of the colorbar is so large.

Sorting out which fields matter in an uncoupled framework is easy to do in simulations forced by hybrid versions of the reanalysis. I'd be interested to see at least the following additional simulations. First, a pair of integrations with JRA-55-do for all fields but with ERA5 winds and vice versa. Second, a pair of integrations with JRA-55-do for all fields but with ERA5 air temperatures and vice versa. The integrations done in this study are coarse resolution and only 14 years long, so four more simulations is relatively little computing.

In any case, prescribed atmosphere integrations are problematic because the prescribed air temperature strongly controls the sea ice edge in summer (less so in winter). However, the experiments I suggested with hybrid forcing could at least rule out whether the air temperature differences in the reanalysis are driving differences in the simulations.

I did enjoy reading the Intro and Discussion and applaud the effort to summarize the findings about 2023 and their reliance on reanalysis. It does read a bit like a review, and might be better framed as such if the authors can't do more simulations to strengthen their own findings.

On the use of Atmosphere forced simulations

We use atmosphere forced ocean-sea ice simulations in this study since they are a widely used methodology for understanding particular sea ice events (we reference several such studies in

the discussion section), and the motivation for this study is to understand how the choice of atmospheric reanalysis can impact the inferred mechanism of sea ice loss in such studies (we have modified the title to better reflect this intent). In the introduction we mention ‘These reanalyses are widely used to force sea ice and ocean models, offering a computationally efficient alternative to fully coupled simulations’ however it is worth expanding on this point.

Our study focusses on simulations of the 2023 Antarctic sea ice minimum, by atmospheric reanalysis forced simulations, rather than fully coupled models. Atmosphere forced simulations are computationally cheaper than fully coupled climate simulations, and can compensate for sparse in situ observations in the Antarctic. Using forced simulations over fully coupled models can also aid reduction of the carbon impact of climate science by using less resource intensive methods. In addition to this, fully coupled simulations require dedicated/specialist setup. Sea ice modellers are not atmospheric model specialists (and vice versa). Atmosphere-forced simulations are also often used for studies focused on sea ice model development and / or understanding how improvements in the representation of particular processes can impact sea ice evolution (Flocco et al., 2012; Roach et al., 2018; Li et al., 2025). In such studies, the use of a forced atmosphere can be beneficial to allow multiple simulations (e.g. sensitivity studies), and also makes it easier to understand particular changes in sea ice without the additional internal variability introduced with a coupled atmosphere. In the context of the continued utility of atmosphere forced simulations, it is therefore important to analyse the impact of reanalysis choice as we have done here.

We agree that the approach of relaxing winds or temperature in a fully coupled model could partially solve the feedbacks limitation, however as we mention above, this has its own drawbacks, and would not guarantee a more realistic sea ice simulation. As such, because our study focusses on the implications of reanalysis choice for forced simulations, and because coupled relaxation experiments would be a substantial undertaking, in terms of setup and resources, we consider them outside of the scope of the paper. However, we have updated the text to mention this in the suggested future work section of the discussion, as it could form the basis for a future complementary study – this text is given below:

As well as this, our study focusses on the impact of atmospheric reanalysis when used in as forcing for a coupled ocean sea-ice model, as this is a popular methodology. To extend this, it could be beneficial to investigate the impact of reanalysis choice when used as the target of relaxation for winds or temperature in a fully coupled model setup.

On the proposed ‘hybrid simulations’

We also thank the reviewer for the suggestion of proposed hybrid simulations. We have considered this suggestion carefully, however we do not think it would deliver the clean attribution results the reviewer is hoping for, for a few reasons. Internally inconsistent forcing would not necessarily truly isolate the role of whichever variable is switched. A wind field from one reanalysis paired with a temperature field from another does not correspond to any physically realised atmospheric state. For example, near surface air temperature is strongly coupled with surface downwelling longwave radiation, thus an air temperature field imported from a different reanalysis to the one that supplies the radiation fields creates an implicit radiative inconsistency. It also risks reintroducing the very feedback issue the hybrid

simulations are proposed to address, as additional ‘feedbacks’ would be introduced, as the new sea ice state forced by the switched variable in the hybrid simulation may now also respond differently to other variables in the simulation. One example of this is thinner ice caused by the switched variable, could then lead to a different fraction of SW downwelling radiation absorbed. As such, a simple variable switch does not isolate the impacts of just one variable as intended and would undermine the clean attribution desired from the hybrid experiment.

The aim of the paper is to show the impact of reanalysis on the kind of forced simulations that are common in the literature; as hybrid simulations are not usual, it is more directly relevant within the scope of this paper to examine the differences between reanalyses and the sea ice state they produce. However, the idea is not without merit and could form the basis of a future complementary study that would be more pertinent to coupled setups with relaxation.

Thus, we argue that our current analysis is sufficient without the suggested additional simulations, and consistent with the defined aims of the study. Our discussion section discusses the prior body of literature on the Antarctic sea ice minimum of 2023 in the context of our simulation results, and as such should not be framed as a review paper. Please see response to RC1, point 5, for an updated discussion section with improved readability that should address these concerns.

In addition, we agree with the suggestion to improve the colour bar scales, and the Figures will be revised as such.

Specific Comments

1. Figure 1. I suggest that you show sea ice area and not extent since the grids are grossly different between the coarse-resolution model and the sea ice products. The excuse to avoid area because satellites confuse open water and melt ponds is not relevant in the Antarctic. The seasonal cycles could be very different in area, and you may come to different conclusions.

We compute the observed SIE (Sea ice extent) after interpolating the SIC (Sea ice concentration) to the model grid. SIE is the correct metric to compare models and observations as the uncertainty associated with satellite measurements of sea ice concentration carries through more strongly onto SIA (Sea ice area) than SIE. The reviewer is correct to say that melt ponds are less relevant to the Antarctic than the Arctic, however this uncertainty in satellite derived SIC is not simply due to melt ponds- thinner ice in summer is also less distinguishable from open water, plus lower summer SIA then becomes more comparable with the uncertainty (Wernecke et al, 2024). We update the main text with these additional factors for completeness. Also, we show map plots of SIC (Figure 2, Figure 3), which effectively convey the information desired from SIA, such that our conclusions are not overly reliant on SIE.

2. Figure 5. The statement "October relative to the 2010– climatology" in the caption is pretty awkward. I'm guessing what is meant is the mean of 14 Octobers minus October 2010. I think this is a nice way of computing anomalies and presenting differences so the reader can gauge how the difference compares to recent trends.

We have corrected the Figure caption to: **relative to the 2010-2023 mean.**

3. Figure 5 The evidence of the sea ice edge in surface wind speed and downwelling shortwave is odd. Please explain. Also, I think it would be valuable to show the wind direction differences since the direction has such an important impact on sea ice transport and ocean upwelling.

This arises because the reanalysis products include their own representation of sea ice (e.g., from assimilated observations) which affects the surface energy and momentum balances around the location of that ice edge in the atmospheric model. We have added the following text and citation to explain this in section 3.4.

The marginal ice zone causes wind speed differences that mean that differences in the position of the ice edge between the reanalyses cause evidence of the ice edge to show up in the wind speed difference plot. Horizontal temperature gradient in the boundary layer, caused by oceanic heat flux seaward of the ice edge, induces an increase in wind speed seaward of the ice edge (Overland et al, 1983).

Thank you for the suggestion on wind direction - it is true that the wind direction is important, so we will add an additional row to the figure showing wind direction differences.

4. Figures 6 Please change the range of the colorbars independently for Figs 5 & 6, especially for 2m air temperature in Feb. If this field differs by even a few tenths of a degree, I'd expect it to make a big difference to the sea ice. With a 20 deg C range, I can't tell whether the difference is substantial or not.

We thank the reviewer for this suggestion, we originally kept the colour bar scales consistent across the Figures, but we agree it makes sense to emphasise smaller temperature differences in Feb, so we will adjust the Figure accordingly.

5. Line 272 caught me by surprise. EOF2 is also a typical difference pattern, just less typical than EOF1 by the ratio of their explained variances. So I don't think 2023 is atypical. In fact, it occurred quite strongly in the last three years of the integration.

This is a good point, our wording here can be improved. What we intended to convey was that EOF2 is the pattern that exhibits less explained variance compared to EOF1, across the whole time period, which makes it more notable for it be stronger than EOF1 in 2023. The reviewer is also correct that it occurs strongly in the last three years of the integration, so it cannot be that 2023 is fully atypical. What we intended to convey was that 2023 is atypical compared to the overall period of study (2010-2023), and that we also note that the entire 2021 - 2023 period is atypical in this regard. We adjust the manuscript text as below:

EOF1 represents the more climatologically typical difference pattern, as it has a larger explained variance than EOF2. This makes it notable that in 2023, the EOF2 signal is much stronger than EOF1. This could suggest that the 2023 differences were unusual compared with the 2010-2023 period as a whole and may reflect a distinct atmospheric configuration or physical mechanism.

6. Line 286 What pronounced peak? I'm seeing 3 years in a row, which I'd call a pronounced plateau.

Reworded text: Lines 268–269: PC1 is weakly negative during February 2023. In contrast PC2 remains strongly positive in 2021, 2022 and 2023.

7. Line 346 Why have you used the term "represented"? Is it intentional to indicate that strong winds may not have been present in nature? In any case, this claim surprised me since other parts of the paper emphasized the large differences between the reanalyses winds.

We thank the reviewer for raising this point. To avoid confusion, we therefore change the wording to 'present in'. We do not find large differences between reanalysis winds, as stated in line 360, the winds are consistent between both.

8. Line 346-7 says "strong wind anomalies in October and February 2023 were consistently represented between ERA5 and JRA-55-do, lending confidence to conclusions that emphasise wind-driven dynamical mechanisms." But this is a weak statement since it is merely correlation, and no experiment has been shown that this is key to driving anomalies in your simulations. It could equally be due to the trends in surface air temperatures. A version of this was repeated in #6 of the conclusions, which I don't think is justified.

We thank the reviewer for raising this. We did not intend to conclude that wind forcing is the primary driver, instead that similar wind patterns across the two reanalyses strengthen studies using ERA5 to identify a key role for wind forcing in the 2023 sea ice minimum. To clarify this point to reduce confusion we have adjusted the text as follows:

The presence of both the strong wind anomalies, as well as the agreement between the two analyses, lends confidence to conclusions from studies using only one reanalysis that emphasise wind driven dynamical mechanisms. We do not show however that wind anomalies are necessarily the key, causal driving factor of the 2023 Antarctic sea ice minimum.

9. Do the authors think that ERA5 causes greater errors in the integration forced with ERA5? The integration with JRA-55-do could have compensating errors, such that the model errors are somewhat masked, making that integration seem more successful due to the forcing. However, a different model may have the reverse result. To be clear, I think the point that that the reanalysis causes differences is not in question. I only question drawing conclusions about which is "better". Conclusion #1 is already worded conservatively. But readers may still come to the conclusion that JRA-55-do is better, unless you tell them they should not. I suggest including some words of caution in Conclusion #1

We thank the reviewer for this point. We adjust the concluding paragraphs to include some words of caution as suggested:

Although our JRA-55-do forced simulation more closely replicates the 2023 minimum, this does not necessarily mean that JRA-55-do is therefore more generally the more effective atmospheric reanalysis for driving sea ice - ocean models.

We also add the following text to the discussion to highlight the possibility of compensating errors between the model and forcing dataset:

Our JRA-55-do forced simulation more closely replicates the sea ice state during the 2023 minimum, but it is possible that there could be a compensation of a model error that masks

errors with the JRA-55-do forcing data, not detected in our results, but potentially noticeable with a different sea ice or ocean model.

10. Line 460 Conclusion #4. While the downwelling longwave differences are big, this study hasn't shown (yet) that the sea ice is most sensitive to this difference in this field. I see the word "suggesting" used, but I still think this statement is too bold based on the results presented. To draw such a conclusion, this forcing should be isolated in additional experiments where only the downwelling longwave differs.

Thank you for this comment – we attempt to not be too strong in our conclusions here, as without additional simulation work then downwelling longwave flux cannot be conclusively confirmed as a primary mechanism. However, the strength of the disparity between the reanalyses does ‘suggest’ it could have a role in differences in the sea ice state. Figs. 5–6 are showing what the possible driving factors of the differences are, in a qualitative sense, and we simply are pointing out that longwave radiation is the largest of those. It is not about showing which atmospheric field that the sea ice state is most sensitive to in the model, it's about showing which are the strongest differences between forcing fields that are most associated with the strongest differences in the simulations. Please also see earlier response to the hybrid experiment suggestion.

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