Editor:

The authors have submitted their revision. Please help to further evaluate the new version of the manuscript. In particular, please help to check whether the "heat buildup mechanism" for the initiation of deep convection has been well established in the revised manuscript? Also please help to check whether the issue about the ensemble spread has been well addressed in the revised manuscript.
Many thanks!

Response: We thank the editor so much for the kind decision letter together with two reviewers' constructive comments. Those are very helpful for improving our original manuscript by re-examining the heat build-up mechanism we proposed and also discussing the over-confidence issue on our model prediction. We have carefully addressed all the concerns raised by the reviewers in the revised manuscript and provided our point-by-point response to the reviewers in a separate sheet. We would greatly appreciate if the editor could kindly consider the revised manuscript for possible publication in this journal.

Reviewer 1:

The authors investigated the multidecadal variability and predictability of Antarctic sea ice using the GFDL climate model. In particular, they highlighted the role of deep convection and surface wind in the multidecadal variability of Antarctic sea ice. Using a number of model simulations, they also suggest that the multidecadal variability of Antarctic sea ice can be predicted 6-10 years in advance.

This is definitely an interesting paper and contains some useful information. My major concerns are with their interpretation of how the deep convection was initiated around the 1980s and with the structure of the paper writing.

Response: We thank the reviewer so much for providing the constructive comments on our original manuscript. Those are very helpful for improving the manuscript by updating the deep convection index in the density coordinate and discussing further roles of the surface salinity increase in the deepening of the mixed layer during the 1980s. We have provided our point-by-point responses (in Italic font) to the reviewer's comments below and one supplement file including the updated figures.

Major Comments/Suggestions

[Section 3] This section is quite massive and messy – I tried very hard not to get lost. If it was me, I would try to make it more concise to focus on the main points. I would also include some subtitles (subsubsections) to guide the readers.

Response: We have trimmed the sentences to make them clearer and separated Section 3 into five subsections as below:

3.1 Antarctic Sea Ice Multidecadal Variability Simulated in SPEAR_LO Model

3.2 Physical Processes on the Simulated Antarctic Sea Ice Multidecadal Variability

3.3 Ensemble Spreads of the Simulated Antarctic Sea Ice Multidecadal Variability

3.4 Skillful Prediction of Antarctic Sea Ice Multidecadal Variability

3.5 Potential Sources of Antarctic Sea Ice Multidecadal Predictability

[Lines 361-417] I disagree with the heat buildup mechanism for the initiation of deep convection. Based on Figure 6, the upper ocean (200 m) stratification, which is essential for heat exchanges to the surface mixed layer, is dominated by salinity changes, not by temperature changes. The heat distribution in the 1960s does help destabilize the water column, but it is still overwhelmed by the salinity stratification (Figure 5e,f). To me, both the temperature/salinity redistribution are more like responding to the surface wind changes. In the late 1970s, the negative wind stress curl/stronger westerly wind favored the formation of polynyas and enhanced vertical mixing, bringing more heat to the surface. The heat buildup in the 1960s amplifies the vertical heat flux, not necessarily the vertical mixing itself. This also explains why a positive wind stress curl around 2005 does not cause significant sea ice increase.

Response: Following the above and minor comments below, we have calculated the streamfunction in the density coordinate and updated all the figures related to the deep convection (see Figs. 3d, 7d, 11e, 12f in the supplement). The updated figures show that the sea ice decrease in the late 1970s and early 1980s are associated with the deepening of the mixed layer which is followed by the stronger deep convection (Fig 3d).

We have also modified the figure on the ocean heat tendency anomalies in the upper 200 m (Fig. 6a in the supplement) and replaced the figure on the subsurface heat tendency anomalies with the salinity tendency anomalies in the upper 200 m (Fig. 6b in the supplement). The updated figures show that the upper-200 m ocean heat and salinity tendency anomalies become positive in the late 1970s and early 1980s, mostly due to vertical advection associated with deeper mixed layer and the associated deep convection.

The mixed layer starts to gradually deepen in the 1960s when the subsurface ocean stratification anomalies become negative (Fig. 5d) due to the density decrease (Fig. 5b) associated with the anomalous heat buildup in the subsurface ocean (Figs. 4b, 5e). In the 1970s and 1980s, the mixed-layer further deepens owing to the density increase associated with salinity increase in the upper 200 m (Fig. 5f). As the reviewer pointed out, stronger westerlies and negative wind stress curl anomalies in the late 1970s and 1980s can contribute to deeper mixed layer by upwelling of saline water from the subsurface ocean (Fig. 3a). However, the surface wind anomalies cannot explain the gradual deepening of mixed layer in the 1960s because they have opposite signs with weaker westerlies and positive wind stress curl anomalies (Fig. 3a).

Therefore, we have concluded that the subsurface heat buildup helps destabilize the upper ocean in the 1960s, while in the 1970s and 1980s, the surface wind anomalies also play a role in the deepening of the mixed layer and the associated deep convection following the reviewer's comments. We will modify the relevant paragraph by discussing the role of surface wind anomalies.

Minor comments/suggestions

[Line 54] between *the* atmosphere and *the* ocean. Similar issues exist throughout the paper.

Response: We have corrected them throughout the manuscript.

[Line 55] high-salinity dense water. Actually, the water formed on the Antarctic continental shelf is called high-salinity shelf water, which entrains circumpolar deep water as it flows down the continental slope and forms Antarctic Bottom Water.

Response: We have corrected the words.

[Line 56] travels → flows into the bottom of the Southern Ocean

Response: We have modified the sentence.

[Lines 83-85] This is quite out of the context and not the main processes responsible for the sea ice increases in Lecomte et al. (2017). As I understand it, the vertical heat redistribution is more like a consequence of the increased stratification, not the reason for the increasing SIE. I would get rid of this sentence.

Response: We have removed the reference accordingly.

[Lines 121-123] I am confused. In SPEAR_LO_DRF, it is also starting from January 1st, which contradicts this speculation.

Response: We have removed this sentence to avoid confusion.

[Section 2 and hereafter] the HadISST → HadISST.

Response: We have corrected.

[Line 180] The NSIDC sea ice concentration is reported on the polar stereographic grid with a resolution of 25 km, not 0.25 degree.

Response: We have corrected.

[Lines 192-193] .. have a nominal 1o horizontal resolution, which increases to 1/3o in the meridional direction …

Response: We have corrected.

[Lines 225-238] Make it clear that no nudging is used here. The discussion of monthly anomalies is a bit out of the context and confusing. Suggest move it to another paragraph.

Response: We have clarified the discussion and moved it to the following paragraph.

[Lines 247-248] Is the overturning circulation streamfunction calculated in depth coordinates? If so, it may be misleading/not accurate.

Response: We have calculated the streamfunction in the density coordinate and updated all the relevant figures on the deep convection (see Figs. 3d, 7d, 11e, 12f in the supplement).

[Lines 283-295] The mentioned polynyas during 1974-1976 and 2016-2017, I believe, are in observations. How are these polynyas reproduced in SPEAR_LO_DCIS? Are the long-lasting strong deep convection between 1975-1990 related to some unrealistic polynyas in the Weddell Sea? Figure S3 compares the sea ice concentration, not the sea ice concentration decrease, and cannot be used to support the claim that SPEAR_LO_DCIS does not overestimate the SIE decrease.

Response: We have replaced the SIC with the SIC anomalies (see Fig. S4 in the supplement). The SPEAR_LO_DCIS captures the negative SIC anomalies associated with these polynya events in the eastern Weddell Sea during 1974-1976 and 2016-2017, and the simulated amplitudes are weaker than the observed ones. It is difficult to attribute these polynya events during 1974-1976 to the long-lasting sea ice decrease associated with the strong deep convection simulated in the 1980s. We have added this discussion in the relevant paragraph.

[Line 299] in several studies, including the ones using satellite images of …

Response: We have modified.

[Lines 350-352] The lead-lag relationship is ambiguous with the 5-year moving average. Even if this is real, this lead-lag relationship is likely arbitrary due to the definition of deep convection here. Convection (in the upper ocean) that entrains warm/salty water from the subsurface should take place simultaneously with the mixed layer deepening. I don't see any reason for such a lag.

Response: We have removed the reason and modified the sentence as "The stronger deep convection and the associated deepening of the mixed layer tend to entrain more warm water from the subsurface ocean."

[Line 381] The negative stratification anomaly around 1980 is mainly in the upper 200 m, not below 100m.

Response: We have modified.

[Lines 463-474] This paragraph starts by talking about the SPEAR_LO_DRF runs, but then switches to discussion of the persistence prediction based on HadISST1 (I assume). This is quite confusing.

Response: We have modified the sentences to avoid confusion.

[Section 3.2] I am not in the field of climate predictions and got confused by what is done here (SPEAR_LO_DRF). Could you elaborate what the "ensemble-mean SIC anomalies predicted at lead times from 1-5 years to 6-10 years" mean exactly?

Response: This means the ensemble mean (i.e., simple average of 20 members) of the predicted SIC anomalies for the SPEAR_LO_DRF at the prediction lead times from 1-5 yr to 6-10 yr. For example, in case of 1-5 yr lead prediction for the concerned observation period of 2010-2014, we used the predicted anomalies starting from 2005-2009 to 2009-2013, respectively. We have clarified this in the beginning of the second paragraph.

[Section 3.2] Similar issues in Section 3.1. There are a lot of details here and I tried very hard not to get lost. It would be better to make it more concise and focus on the most important results here. Could also add some subtitles to guid the readers.

Response: Please refer to our earlier response.

[Lines 550-555] Is this correct? P-E is positive (more precipitation) around 1980. There is a small fraction of negative values close to 1985.

Response: Yes, P-E in Fig. 11f shows slightly negative anomalies in the early 1980s when the negative SIC anomalies in Fig. 11a reach their peak. We will modify the sentences accordingly.

[Lines 636-649] The discussion of the influence of atmospheric model resolution is useful. Critically, the ocean model resolution may be more important in simulating deep convection and its climatic impacts (e.g., https://os.copernicus.org/preprints/os-2020-41/).

Response: Since the suggest paper is not accepted nor published in the journal, we have discussed the importance of the increase in the ocean resolution by referring to another paper as below:

"Furthermore, the increase in the ocean resolution may help better represent the mean state and variability of the Southern Ocean which involves rich mesoscale eddies (Hallbert et al. 2013) thereby improving the decadal predictions, but this is beyond the scope of this study."

Reviewer 2:

In this study, the authors investigate the mechanisms underlying the Antarctic sea ice variability and predictability on decadal time scales. They find that deep convection during the high sea ice period plays an important role in the sea ice decreasing and the surface wind variability plays a greater role in the sea ice extent variability. When compared with the persistence forecast, the dynamical prediction shows some advantages, and the two proposed mechanisms can be also examined in the reforecast experiments. In general, this study is well-written and logical, especially for the decadal prediction in the Antarctic, which has not received much attention. However, a few issues need to be clarified for publication. Below are my personal comments:

Response: We thank the reviewer so much for providing constructive comments on our original manuscript. Those are very helpful for improving the manuscript by adding discussions on the initial states of Antarctic sea ice and over-confidence of sea ice prediction skills in our model. We have provided our point-by-point responses (in Italic font) to the reviewer's comments below and one supplement file including the updated figures.

General comment:

In this study, the SPEAR_LO_DCIS assimilates SST observations and atmospheric reanalysis into the SPEAR large ensemble simulation experiments at each 20 model years from 101 to 681 to construct the initialization and then integrates from 1958 to 2020. I'm wondering if the Antarctic sea ice in these large ensemble simulations at fixed interval years are in the same phase (for example, lower values than the mean state). If the hypothesis holds true, it may have an impact on the simulation. To avoid this, some procedures have been applied, for example, in Bushuk et al. (2019) '*In order to assess how predictability varies with the initial SIV state, we choose start years based on SIV anomalies, selecting two high volume years, two typical volume years, and two low volume years.*' So I'm curious about the situation in this simulation.

Response: Bushuk et al. (2019) selected different sea ice volume years to see their impacts on the prediction skills in their perfect model experiment. In contrast, our study initialized the SPEAR_LO model from the SPEAR_LO large ensemble simulation (SPEAR_LES; see below) and constrained the model with the observed SST and atmospheric reanalysis over 1958-2020. So, the influence of the initial conditions gets weaker with the integration period. We have calculated time series of Antarctic SIE anomalies from 101 to 700 years for the SPEAR_LES and found that the initial years of the SPEAR_LO_DCIS include 14 high SIE states and 16 low SIE states, respectively (see Fig. S1 in the supplement). Our selection of the initial years do not much affect the simulation of the Antarctic sea ice. We have discussed this issue in Section 2.

For ensemble hindcasts, the ensemble spread can be very large. For example, in Figure 9, there are large differences in ACC of different members. As a result, I think it's better to add some discussions on the ensemble spread, to see if the forecast system is over-confident or needs to be improved in the uncertainty estimation.

Response: To investigate whether our model prediction is over-confident, we have calculated the average of the individual members' ACCs and the signal-to-noise (S/N) ratio in the model following the previous studies by Eade et al. (2014) and Scaife and Smith (2018). The results (see Figs. 9a, b in the supplement) show that the ACC of the ensemble mean is relatively high *compared to the average of the individual ACCs for both the pan-Antarctic and Weddell Sea. The S/N ratio in the model also exceeds the ACC of the ensemble mean. These results indicate that our model prediction is under-dispersive and over-confident and may be due to a small ensemble size, a lack of ensemble spread, and systematic errors in predicted signals (Eade et al. 2014, Scaife and Smith 2018). We have added these results in Section 3 and discussed the over-confident issue in Section 4.*

As we know, obvious differences in the simulation of Southern Ocean can be found in difference models, while the conclusions of this study are drawn from SPREAR_LO. Besides, the first author has extensive experience with other models, such as SINTEX-F2 in Morioka et al. (2022). Thus, please consider clarifying whether the findings in study are model-dependent.

Response: SPEAR_LO_DRF shows similar prediction skills with the SINTEX-F2 model (see Fig. 1f in Morioka et al. 2022), but the prediction skills in the Indian sector is higher (Fig. 8d). SPEAR_LO employs the SST and atmospheric initializations since the 1960s, while SINTEX-F2 uses the SST, sea ice cover and subsurface ocean temperature/salinity initializations since the 1980s (Morioka et al. 2022). The results shown in this study can be model-dependent, but it is difficult to directly compare the prediction skills and discuss the reasons for the differences because of large differences in the model physics, initialization schemes and hindcast periods between the two models. We need dedicated comparison studies in the future using different models with the same initialization schemes and hindcast periods. We have discussed this issue in Section 4.

Specific comment:

L63: Actually the Antarctic sea ice reaches its record-low on 1st Mar 2017 (Turner and Comiso, 2017). Please consider updating.

Response: Recent observation shows that Antarctic sea ice reaches its record low on Feb 2022 (Simpkins 2023), so we have decided to cite this paper and modified the text accordingly.

L77: "Goose" should be "Goosse".

Response: We have corrected.

L77-82 and 89-92: It seems somehow divergent for the role of brine release in the sea ice evolution. In Lines 77-82, the brine release contributes to the sea ice increase. However, in Lines 89-92, the brine release is responsible for the sea ice decrease. Why is the difference? Please consider clarifying this in the manuscript.

Response: Reviewer 1 also pointed out this issue and claimed that the vertical heat redistribution is a consequence of the increased stratification, not the reason for the SIE increase. Therefore, we have dropped the discussion on Lecomte et al. (2017) to avoid the confusion.

L132-133: This study highlights the role of subsurface ocean processes in the low sea ice state. The subsurface ocean is also constrained in Morioka et al. (2022) through assimilating subsurface ocean temperature and salinity, why it still cannot capture the sea ice decrease in the 1980s? In addition, I noticed that nudging and 3DVar are adopted in this study and Morioka et al. (2022) respectively. How do different assimilation methods affect the simulation and prediction of Antarctic sea ice on decadal timescales?

Response: As mentioned above, SPEAR_LO employs the SST and atmospheric initializations since the 1960s, while SINTEX-F2 uses the SST, sea ice cover and subsurface ocean temperature/salinity initializations since the 1980s (Morioka et al. 2022). There are few observations in the Antarctic Seas before 2005 when the Argo floats are implemented, so the impact of the subsurface ocean initialization on the prediction skills are very limited in the 1980s (Morioka et al. 2022). In contrast, this study employs atmospheric and SST initializations in the SPEAR_LO model since the 1960s, so the low-frequency subsurface ocean conditions may partially be improved due to better representation of wind stress and surface heat flux. For example, the prediction skills in the Indian sector are higher for the SPEAR_LO model, although we cannot neglect the differences in the model physics, initialization schemes and periods. We have discussed this issue in Section 4.

L225-227: The hindcasts in this study are also initialized from the every 1st January of 1961-2020, while the introduction states that 'The low prediction skills are likely because most of the models are initialized every 1 January when the sea ice extent is very low and little sea ice information persists in the models.' in L121-123. Do the results from this work also underestimate the predictive skill of Antarctic sea ice?

Response: Reviewer 1 also pointed out this issue. Since the SPEAR_LO_DRF starts from every January 1st of 1961-2020, the low prediction skills may partly be related to the initial month. To avoid this confusion, we have dropped the statement (L121-123) from the main text.

L271-272: I'd question about the 5-yr moving average to extract the low-frequency variability beyond a decade (10 yr). I think it should be a 10-yr moving average.

Response: We have used the 5-yr moving average to extract the low-frequency variability with a period longer than 5 years. We have modified.

L320-324: Is the same reason for the large discrepancies between NOAA SIE and HadISST SIE from 1980-1985?

Response: Since HadISST shows lower SIE in the Amundsen-Bellingshausen Seas between 1980-1985 and higher SIE after 2010 than NOAA/NSIDC, it is difficult to explain these differences by the lack of passive microwave derived SIC in the HadISST. Rather, this may be related to the SIC reconstruction of the HadISST that uses different sea ice datasets (US National Ice Center, NASA, NCEP) before and after the mid-1990s which tend to show higher SIE in the latter period. We have modified the sentence.

L333-334: I see that the ensemble spread of SOM is relatively low from 1975-1980 (Fig. 3b).

Response: We have modified the sentence as "ensemble spreads of the SAM index are relatively large except some periods (early 1960s, late 1970s, and early 1990s)".

L347-348: Please elaborate clearly on the regions where the mean SST anomaly is calculated. Is it for the whole Weddell Sea? Or the ice-covered region only. And how does the SST under the sea ice is represented in the SPEAR model?

Response: We have calculated the SST anomaly and other variables averaged in the whole Weddell Sea (60ºW-0º, south of 55ºS). In the SPEAR_LO_DCIS, we have nudged the SST between 60ºS and 60ºN to ERSSTv5, so most of the SSTs below the sea ice are freely interacted with the overlying atmosphere and sea ice in the model. We have clarified the region of the Weddell Sea in the first paragraph of Subsection 3.2.

L367-370: I'd question the reliability of the positive temperature below 100m in the early 1960s. As the SST is assimilated in the early 1960s, can the subsurface respond to the assimilation in only a few years? Please consider making this clear in the manuscript.

Response: In the SPEAR_LO_DCIS, we have not nudged the SST south of 60ºS to ERSSTv5, but nudged the atmospheric temperature and winds to the JRA-55 reanalysis over the globe since 1958. The subsurface ocean responds to the prescribed atmospheric forcing during the 1960s, but we recognize that it takes some time to develop the temperature anomalies in the subsurface ocean considering the oceanic adjustment time in the polar region. We will discuss this uncertainty in the simulation of subsurface heat buildup during the early 1960s in the main text.

L424-430: How to explain the positive correlation in around 1980 with lead times longer than 6 years (Fig. 7a)?

Response: This reflects the phase change of zonal wind stress anomalies before and after 1975 (Fig. 3a), but the correlation is lower than the negative correlation in around 1980 with lead times shorter than 5 years (Fig. 7a). It is difficult to claim that the ensemble members with weaker westerlies than the ensemble mean simulate more negative SIE anomalies with lead times longer than 6 years, because the weaker westerlies tend to suppress the upwelling of warm water from the subsurface and contribute to sea ice increase. We have discussed this issue in the main text.

L494-495: Have you used the area-weighted mean SIC anomaly? Or just the simple mean calculation?

Response: We have used the area-weighted mean SIC anomalies. We have clarified in the sentence.

L508-509: I'm curious that the predictive skill improves with the increase of the lead years, which is somehow counterintuitive (Figs 9c,d). Why is this?

Response: As the lead times increase, the amplitude of the predicted sea ice anomalies decreases and gets closer to the observation, particularly during the 1980s (Figs. 9c, d). This is in contrast to the prediction skills of the phase that deteriorate with the increase of the lead years. Since the time series of the predicted anomalies for the 1-5 year leads resembles that of the initial sea ice anomalies in the model (Figs. 2a, b), the influence of the Southern Ocean deep convection initialized in the model during the 1980s remains stronger for the earlier lead times, causing larger differences in the observed and predicted amplitudes. We have also calculated the SIC anomalies normalized by standard deviation and found similar results, although the amplitude of the predicted anomalies gets closer to the observation (Fig. S8 in the supplement). We have discussed this issue in the relevant paragraph.

L563-565: I'm lost about how this correlations are calculated. Please consider providing a more detailed explanation on the calculation of the correlation coefficient.

Response: For example, the correlation coefficient in 1980 with lead times of 1-5 years is the one between the SIC anomalies predicted in 1986-1990 and the SST anomalies predicted in 1981-1985. We have provided more detailed explanation.

Figure

Figure 3: For subplot b, why is the orange shading area not symmetric to the red line? Besides, I recommend some improvements over Figure 3, as I find it difficult to capture the mentioned lines in the manuscript (some are the same colors).

Response: We have found a technical problem in calculation of the SAM index and updated Fig. 3 (see Fig. 3 in the supplement). The updated SAM index shows no ensemble spreads because the atmospheric temperature and winds in the SPEAR_LO_DCIS were constrained by the JRA-55 reanalysis. We have also updated the line colors in Fig. 3 as well as Figs. 2 and 6 (see the supplement) to make them clearer for the people with color blindness.

Figure 11: "slat" should be "salt", and please consider adding some explanation of the years in the x-axis.

Response: We have corrected and added "initial/predicted years" in the x-axis and "lead years" in the y-axis with detailed explanation for Figs. 10-12a (see the supplement).

Refernece:

Bushuk, M., R. Msadek, M. Winton, G. Vecchi, X. Yang, A. Rosati, and R. Gudgel, 2019: Regional Arctic sea–ice prediction: potential versus operational seasonal forecast skill. *Clim. Dyn.*, **52**, 2721-2743.

Morioka, Y., D. Iovino, A. Cipollone, S. Masina, and S. K. Behera, 2022: Decadal Sea Ice Prediction in the West Antarctic Seas with Ocean and Sea Ice Initializations. *Commun. Earth Environ.*, **3**, 189.

Turner, J., and J. Comiso, 2017: Solve Antarctica's sea-ice puzzle. *Nature*, **547**, 275-277.