Response to Review Comments
for

"The response of sea ice and high salinity shelf water in the Ross Ice Shelf Polynya to cyclonic atmosphere circulations"

Xiaoqiao Wang, Zhaoru Zhang, Michael S. Dinniman, Petteri Uotila, Xichen Li, Meng Zhou

Note: Reviewers' comments are in italic font; authors' response comments are in normal font. Revisions in the revised manuscript are highlighted by blue color.

Reviewer comments:

Anonymous Referee #1:

Overview Comments:

This paper is a novel work discussing the formation and exportation of HSSW with the influence of cyclones. The cyclones leading to the extreme winds event can enhance the formation and outflowing of HSSW, while as the authors point out that the previous studies always focus on the influence of seasonal scale winds such as katabatic winds. This paper enriches our understanding in this area. It is very interesting that cyclones contribute to outward barotropic currents by changing sea surface height and thus affect HSSW exportation.

However, some discussions fall short in view of the possible implications, which are listed below.

We thank the reviewer for the considerable efforts in reviewing this manuscript. The useful and helpful comments helped to clarify and significantly improve the manuscript. Please see our response to each comment as follows.

1. In this paper, three cases (SYNO1, SYNO2 and MESO) are shown. However, only SYNO1 and MESO are analyzed in detailed. The authors may consider SYNO2 to be similar to SYNO1, and the analysis of SYNO1 could be also applied to SYNO2. However, there are some differences between SYNO2 and SYNO1, and the reasons for such differences have not been fully discussed. For example, there is a weak correlation between the meridional winds and the HSSW exportation across S1 in SYNO2, while in SYNO1 and even MESO, the negative correlation is significant. Thus, we are not sure that the conclusions got from SYNO1 and MESO can be also applied to SYNO2. If the authors want to use the case of SYNO2 to show that the conclusion in this paper can be generalized, it is necessary to discuss the differences between SYNO1 and SYNO2, otherwise people may doubt the applicability of the conclusion.

Sorry for the lack of discussion on this part. For the SYNO2 event, the main reason for the weak correlation between HSSW export and winds should be related to the cyclone center, which is located on the eastern side of the Ross Sea close to the Amundsen Sea (Fig. S1 in the supplementary), leading to lower wind speeds in the RISP region than the other two events (SYNO1 and MESO). As can be seen from Table 1 in the revised manuscript, in the RISP region the mean wind speed for the SYNO2 event is only about 5.3 m s^{-1}, but for SYNO1 and MESO, the mean wind speed is greater than 7 m s^{-1}. In addition to the lower wind speed, there may be other factors (such as under ice shelf circulations) that play a significant role in regulating HSSW export, which makes the role of wind speed weakened. The related discussion has been added "The reason for the weaker correlations between HSSW export and wind speed weover SYNO2 might be related to the lower wind speed in RISP than over SYNO1 event (5.3 m s^{-1} for SYNO2, 7.0 m s^{-1} for SYNO1, shown in Table 1), resulting from the faraway cyclone center located in the Amundsen Sea. Additionally, other factors (such as ice shelf circulations) could regulate the HSSW exports significantly." (Lines 393–397 in the revised manuscript).

2. The authors set three sections (S1, S2, S3) to describe the exportation of HSSW. However, this paper mainly focuses on the HSSW exporting across S1 and S3 and ignores the S2. I don't think the phenomenon on S2 is trivial, although the correlation between the meridional winds and the HSSW across S2 is not as significant as the cases of S1 and S3, which is not consistent with expectations. But it will also be interesting, if the authors can explain why the correlation is weak.

By examining the spatial pattern of horizontal currents near S2 (Figs. 15 and S7 in the revised manuscript), there is a strong northward flow across S2 at deeper layers (300 m and 500 m) near 175°E (revised Figs. 15e, h, k and f, i, l). This flow originated around 79°S which is located at the RIS (revised Figs. S7e, h, k and f, i, l), so it could be an interaction between basal melting water and the HSSW. Some previous studies have already demonstrated significant effects of ice shelf water on HSSW formation and export (Herraiz-Borreguero et al., 2016; Williams et al., 2016). Accordingly, the weaker correlation between HSSW export and wind speed for S2 might be associated with these ice shelf circulations. This possible explanation has been added as "By examining the ocean currents near S2, a northward flow originated around 79°S which is located at the RIS (revised Figs. S7e, h, k and f, i, l) was observed, and the weaker correlation between HSSW export and wind speed might be associated with local ice shelf circulations." (Lines 477–479 in the revised manuscript).

References:

Herraiz-Borreguero, L., J. A. Church, I. Allison, B. Pen~a-Molino, R. Coleman, M. Tomczak, and M. Craven: Basal melt, seasonal water mass transformation, ocean current variability, and deep convection processes along the Amery Ice Shelf calving front, East Antarctica, J. Geophys. Res. Oceans, 121, 4946–4965, doi:10.1002/2016JC011858, 2016.

Williams, G. D., Herraiz-Borreguero, L., Roquet, F., Tamura, T., Ohshima, K. I., Fukamachi, Y., Fraser, A. D., Gao, L., Chen, H., McMahon, C. R., Harcourt, R., and Hindell, M.: The suppression of Antarctic bottom water formation by melting ice shelves in Prydz Bay, Nat. Commun., 7, https://doi.org/10.1038/ncomms12577, 2016.

Specific Comments:

Following, there is a number of specific comments, many of which can be considered as minor.

3. Line 39-41 ('In general, ... synoptic-scale atmosphere forcing'): This sentence is similar with that of Line 36. May be them can be merged.

Following the reviewer's suggestion, the original sentence "In general, the near-surface wind fields over the Antarctic continent are forced by both katabatic flow and meso- or synoptic-scale atmospheric forcing." has been merge with "These coastal polynyas are mechanically driven by offshore katabatic and synoptic winds" in Line 36, and finally revised to "These coastal polynyas are mechanically driven by offshore katabatic and synoptic winds (Bromwich et al., 1998; Massom et al., 1998; Morales Maqueda et al., 2004), which are regarded as the dominant near-surface wind fields over the Antarctic continent." (Lines 36–38 in the revised manuscript).

4. Line 63: the 'recent studies' need some Refs.

The related references have been added here and the original sentence has been revised to “Recent studies have… based on the observations (Dale et al., 2017; Cheng et al., 2019; Ding et al., 2020; Thompson et al., 2020; Wenta and Cassano., 2020).” (Lines 62–65 in the revised version).

5. Line 52: ‘sea ice production (SIP) rate’ is correct.

“sea ice production rate (SIP)” has been replaced by “sea ice production (SIP) rate”.

6. Line 66-70: is the Thompson et al., (2020) shown to indicate there is a strong response of coastal polynya SIP to the extreme winds event? If so, I think you should tell the readers the magnitude of the mean SIP.

Sorry for the unclear writing. The value of 110 cm d^{-1} in the original sentence (Line 70) is the magnitude of mean SIP found by Thompson et al. (2020). Then this sentence “Thompson et al. (2020) demonstrated using in-situ observations, available from the Polynyas and Ice Production and seasonal Evolution in the Ross Sea (PIPERS) program which conducted an autumn ship campaign in 2017 and two spring airborne campaigns in 2016 and 2017, that the estimated frazil ice production could increase up to 110 cm d^{-1} during the strongest wind events (Ackley et al., 2020).” has been revised to “Thompson et al. (2020) demonstrated that the estimated frazil ice production could increase up to 110 cm d^{-1} during the strongest wind events using in-situ observations, available from the Polynyas and Ice Production and seasonal Evolution in the Ross Sea (PIPERS) program, which conducted an autumn ship campaign in 2017 and two spring airborne campaigns in 2016 and 2017 (Ackley et al., 2020)” to avoid such confusion (Lines 66–70 in the revised version).

7. Line 71: ‘Terra Nova Bay (TNB) polynya’ may be a clearer statement.

“Terra Nova Bay polynya (TNB)” has been changed to “Terra Nova Bay (TNB) polynya”.

8. Line 121: why does only climatological precipitation data come from AMPS, while the rest meteorological factors come from ERA-Interim? The AMPS data covers 2008-2022, which ends much later than the study period (1999-2014). Moreover, the temporal resolution of precipitation, sea level pressure and humidity are different from that of winds and air temperature. Maybe its influence is not significant, or there are some other reasons, but it needs to be claimed.

The main reason for using climatological precipitation from AMPS is related to the performance of coastal precipitation around Antarctica, which is significantly affected by the atmospheric model resolution (van Lipzig et al., 2004). The AMPS product has high-resolution atmospheric forecast fields for Antarctica computed from a mesoscale meteorological model. Therefore, a monthly climatology of precipitation from AMPS was used in our model instead of the ERA-Interim precipitation. Meanwhile, this regional Ross Sea model covered from 1999–2014 was developed in 2015 and we planned on 15-year simulations at that time, so the ERA-Interim product becomes the best choice at that moment. The high temporal resolution for winds and air temperature used in our model is related to their importance in simulating sea ice and currents over the Southern Ocean. For all the other atmospheric forcing, the temporal resolution does not have significant impacts on the model outputs. In addition, Wu et al. (2020) further proposed that the influence of atmospheric variability in the Southern Ocean mainly results from wind fluctuations.

The related details have been added as “Coastal precipitation from reanalysis products for Antarctica is significantly affected by atmospheric model resolution (van Lipzig et al., 2004). Therefore, monthly climatological precipitation used in this model is derived from the Antarctic Mesoscale Prediction System (AMPS), a high-resolution atmospheric model over the Antarctic (Powers et al., 2003; Bromwich et al., 2005), instead of the ERA-Interim product. Furthermore, due to the overestimation of mean clouds over the Southern Ocean from ERA-Interim, monthly cloud fraction climatology data comes from the International Satellite Cloud Climatology Project stage D2…" (Lines 125–131 in the revised version) and "The high temporal resolution for winds and air temperature is related to their importance for simulating sea ice and currents in the Southern Ocean (Wu et al., 2020)." (Lines 123–125 in the revised version).

References:

van Lipzig, N. P. M., King, J. C., Lachlan-Cope, T. A., and van den Broeke, M. R.: Precipitation, sublimation, and snow drift in the Antarctic Peninsula region from a regional atmospheric model, 109, 1–16, https://doi.org/10.1029/2004JD004701, 2004.

Wu, Y., Wang, Z., Liu, C., and Lin, X.: Impacts of High-Frequency Atmospheric Forcing on Southern Ocean Circulation and Antarctic Sea Ice, 37, 515–531, https://doi.org/10.1007/s00376-020-9203-x, 2020.

9. Line 167: how to calculate the SIP in RISP? The mean SIP in the RISP or the in the RISP Polynya region?

Following Nihashi and Ohshima (2015), the calculation of annual cumulative SIP covers the period of March to October, i.e. the ice freezing seasons around Antarctica. Therefore, we calculated the annual cumulative SIP averaged over 2003–2010 (The temporal coverage of satellite-retrieved data) based on this method instead of polynya-mean values for RISP. To illustrate the calculation more clearly, we have added this sentence "Following Nihashi and Ohshima (2015), the calculation of annual cumulative SIP covers the period of March to October, i.e. the ice freezing seasons around Antarctica." (Lines 172–173 in the revised version).

References:

Nihashi, S. and Ohshima, K. I.: Circumpolar Mapping of Antarctic Coastal Polynyas and Landfast Sea Ice: Relationship and Variability, J. Clim., 28, 3650–3670, https://doi.org/10.1175/JCLI-D-14-00369.1, 2015.

10. Line 189: the extent of RISP may be overestimated due to the zero SIP of threshold. The regions covered by thicker sea ice may also produce ice.

To clarify the definition for the extent of RISP, we modified this sentence "The extent of RISP was defined as the area where the multi-year-average annual cumulative SIP is greater than zero based on the Ross Sea model results." as "The extent of RISP was defined as the area where the multi-year-average annual cumulative SIP is greater than zero near the RIS region based on the Ross Sea model results." (Lines 195–196 in the revised version). By examining the spatial distribution of sea ice thickness (SIT) and sea ice concentration (SIC) as shown by Figs. R1 and R2 respectively, the coastal polynya region is characterized by low SIT and SIC values over the SYNO1. These similar features could also be found during SYNO2 and MESO, therefore the extent of RISP we defined before could not be affected by thicker sea ice over the coastal region.

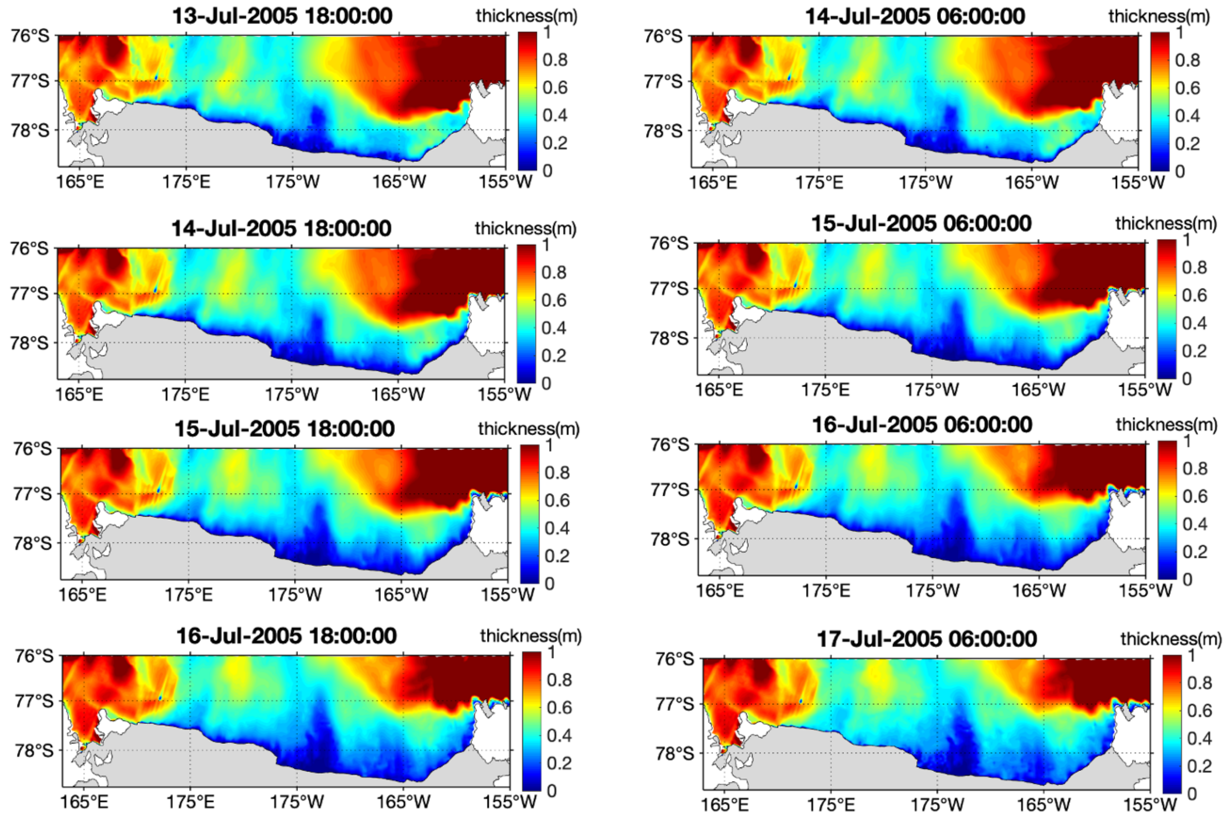

Figure R1. Spatial distributions of 12-hour-average sea ice thickness in the Ross Ice Shelf Polynya over 13–17 July 2005.

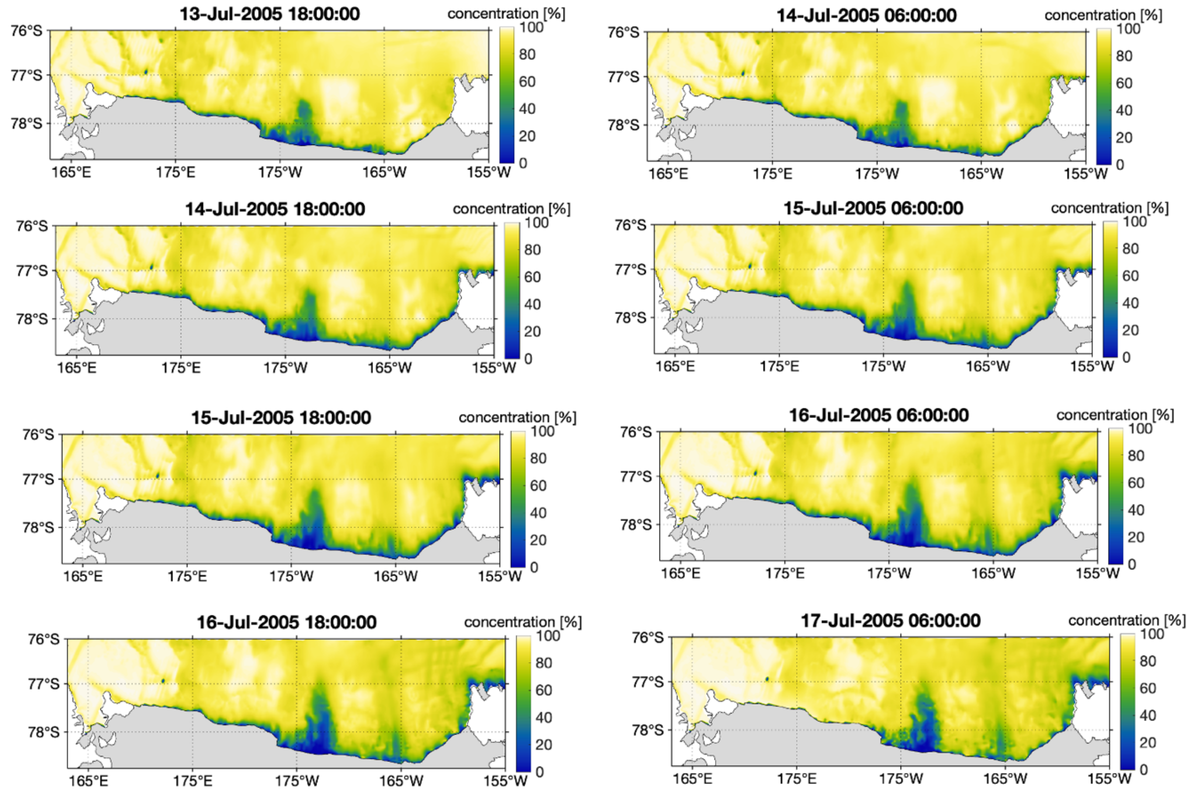

Figure R2. Spatial distributions of 12-hour-average sea ice concentration in the Ross Ice Shelf Polynya over 13–17 July 2005.

11. Line 192: the RISP polygon region comprises the McMurdo polynya. Although this polynya is small and without strong SIP, I think it will be better to limit the western boundary to Ross Island to exclude the McMurdo polynya.

Sorry for the confusion associated with the defined RISP polygon region. As the reviewer suggested, we redefined the boundary of RISP to make a better separation between McMurdo polynya and RISP. The western boundary has been revised close to Ross Island shown in Fig. 2a in the revised manuscript. Furthermore, all related results have been updated based on this newly defined extent.

12. Line 240: maybe claiming the name of case (e.g., SYNO1) here can make the reader clearer.

Following the reviewer's suggestion, the original sentence "For synoptic-scale cyclones, two events were selected for our study, which occurred in July 2005 and September 2014, respectively." has been revised to "For synoptic-scale cyclones, two events were selected for our study, which occurred in July 2005 (SYNO1) and September 2014 (SYNO2)" (Lines 249–250 in the revised version).

13. Line 242: plot the cyclone tracks of the cases on Figure 3. I think you want to show that the cyclones chose here is typical. You can claim it at the end of this paragraph.

The cyclone tracks of three typical cases (SYNO1, SYNO2 and MESO) are added to Fig. 3 as the reviewer suggested, and descriptions of the tracks and cyclone evolutions are added in the revised version (Lines 250–261).

14. Line 258: Can extend the case of SYNO1 forward to when the cyclone did not reach the Ross Sea? It will make the impact of cyclones even more significant.

When selecting SYNO1, we presented the spatial distributions for all related variables from June to September to define the most proper period of SYNO1. Two days before the onset of SYNO1, several low-pressure systems were visible by examining the spatial pattern of sea level pressure as shown in Fig. R3. These cyclones on the north of the Ross Sea gradually merged, which eventually became SYNO1 we have selected. Therefore, if the defined time range is extended forward, other cyclonic processes associated with early development for SYNO1 might be included. But in this study, we only focus on the more mature stage of the cyclone, in other words, when the cyclone has a larger spatial scale, stronger intensity and only one low-pressure center. In addition, the changes in sea ice production in RISP were not significant especially for the western part (Fig. R4), indicating these earlier atmospheric processes could not affect the polynya activities notably. So based on these characteristics, we finally defined 13–17 July as the time range for SYNO1. The main reason has been added as "The SYNO1 occurred from July 13 to July 17 of 2005, when the cyclone was situated in a mature stage, i.e. when the cyclone has a large spatial scale, strong intensity and only one low-pressure center." in the revised version (Lines 272–274).

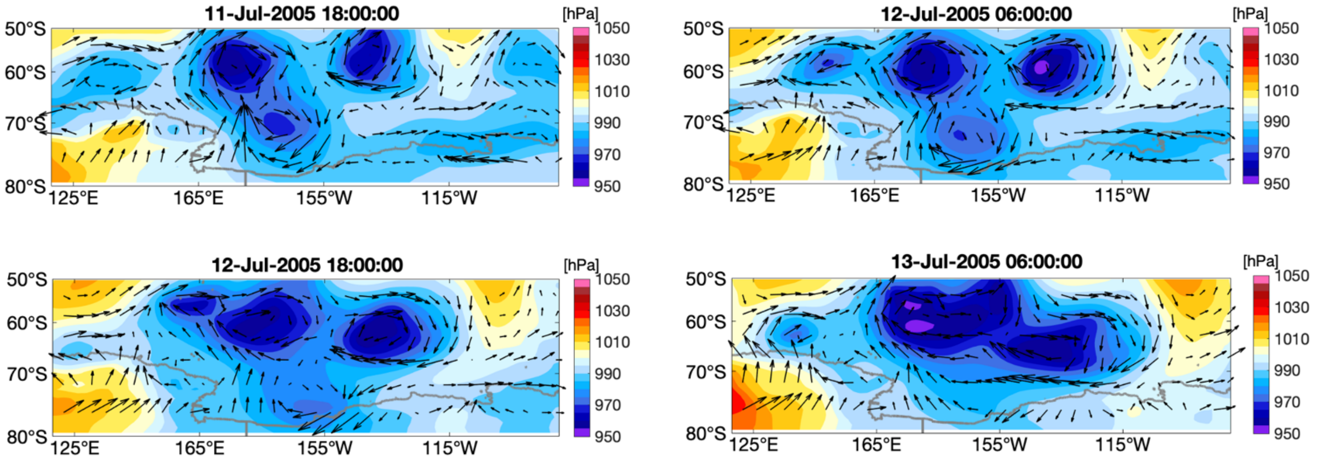

Figure R3. Spatial distributions of 12-hour-average sea level pressure (color shading) and 10-m wind vectors (black arrows) in the Ross Ice Shelf Polynya over 11–13 July 2005.

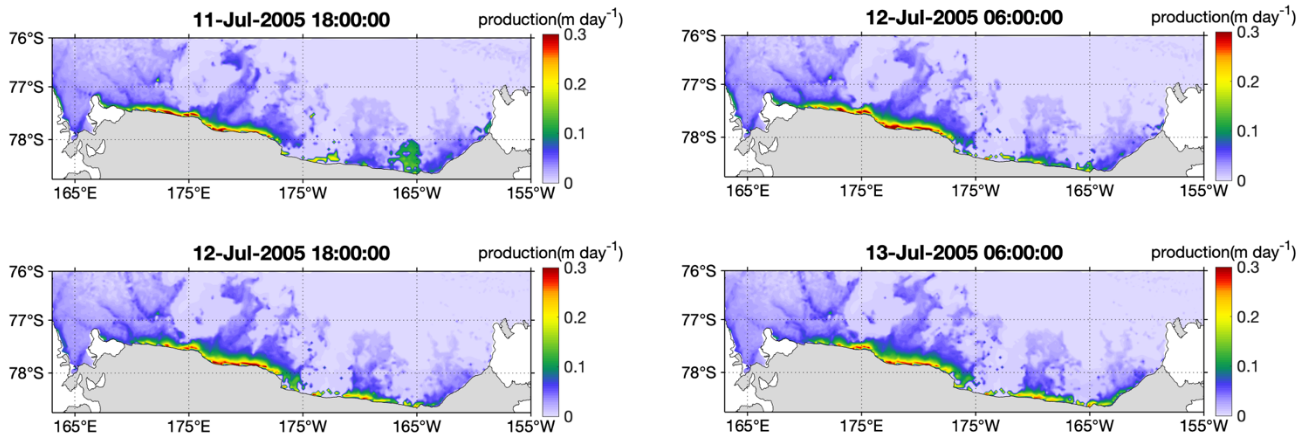

Figure R4. Spatial distributions of 12-hour-average sea ice production in the Ross Ice Shelf Polynya over 11–13 July 2005.

15. Line 301: the Figure 6n shows the HSSW volume, which can reflect the accumulation of density water. However, the cyclones directly affect HSSW generation (i.e., increasing). Thus, the increasing of HSSW may be a more suitable factor, which should be shown here. Furthermore, you said that 'the HSSW volume ... reached the maximum at 06:00 of July 17', but after July 17, HSSW volume still increases. I think here you want to say that the rapid increase of HSSW volume ends at 06:00 of July 17, which can also be illustrated significantly with the plot of increasing rate of HSSW volume.

It is true that this sentence aims to proposed that the increase of HSSW volume ends at 06:00 of July 17. Therefore, it is clearer to show the variation of the increase in HSSW volume as the reviewer suggested. The HSSW volume has been changed to the variability of HSSW volume shown in Figs. 6, 10 and S3. The related content has been revised according to these updated figures as "The HSSW volume increased apparently from 18:00 of July 14 to 06:00 of July 16 (Fig. 6b), when a dramatic increase in SIP occurred in this area accompanied by the intensification of the SYNO1 cyclone. The HSSW volume still kept increasing indicted by the positive values for HSSW volume variability even when SYNO1 weakened from 18:00 of July 16 to 18:00 of July 19 (Fig. 6b), so the HSSW formation could persist around 3 days after the cyclone decayed. For the SYNO1 event, the HSSW volume increased by 0.06·104 km3 compared to the value before the cyclone (Table 1). Meanwhile, the HSSW salinity presented similar features with HSSW volume variability and reached to the maximum at 18:00 of July 16 (Fig. 6c) when the cyclone had intensified for 2 days. The higher-salinity HSSW persisted for 2-3 days after the decay of the cyclone from 18:00 of July 16 to 06:00 of July 19, and then the salinity started decreasing (Figs. 6c).” (Lines 309–318 in the revised version).

In addition, the HSSW is redefined as the water mass with neutral density (γ^n) above 28.27 kg m^{-3}, salinity (S) $>$ 34.62 and potential temperature (θ) $<$ -1.85° C (Orsi and Wiederwohl, 2009; Castagno et al., 2019) in the revised version based on the second reviewer’s suggestion.

References:
Castagno, P., Capozzi, V., DiTullio, G. R., Falco, P., Fusco, G., Rintoul, S. R., Spezie, G., and Budillon, G.: Rebound of shelf water salinity in the Ross Sea, 10, 1–6, https://doi.org/10.1038/s41467-019-13083-8, 2019.
Orsi, A. H. and Wiederwohl, C. L.: A recount of Ross Sea waters, Deep. Res. Part II Top. Stud. Oceanogr., 56, 778–795, https://doi.org/10.1016/j.dsr2.2008.10.033, 2009.

16. Line 309: may be ‘eastward (negative, toward RISP)’ can be clearer.

Sorry for this confusing definition about directions. The definition of along-shelf velocity sign in the original manuscript is indeed not common in the oceanography community, though we wanted to define the direction of HSSW export toward the slope (westward) as positive. As the reviewer suggested, we changed the definition and now the eastward direction is defined positive in the revised manuscript. All related texts and figures (revised Figs. 7 and 11) have been updated using this new definition.

This original sentence “The export of HSSW across S1 has a significant negative correlation with the meridional wind speed (-R=0.70, P=0.012), suggesting that the HSSW had stronger eastward (negative) transport across the meridionally directed transect S1 when the wind speed increased.” has been revised to “The export of HSSW across S1 has a significant positive correlation with the meridional wind speed (R=0.70, P=0.012), suggesting that the HSSW had stronger eastward (positive, toward RISP) transport across the meridionally directed transect S1 when the wind speed increased.” (Lines 331–333 in the revised version).

17. Line 313: the sentence of ‘(R>0.98 and P<0.0001, not shown)’ may indicate that the correlation coefficient is not shown in the figure. But it could be misleading that these two factors are not plotted. So, I think the ‘not shown’ can be deleted. Furthermore, this result would be interesting, if we can estimate that how much of the increase in HSSW exportation is due to the increase in current speed, and how much is due to the increase in HSSW volume (area in the section)?

Following the reviewer’s suggestion, the original sentence “The averaged current velocity on S1 and S3 both have strong positive correlations with HSSW export (R>0.98 and P<0.0001, not shown), suggesting…” has been revised to “The averaged current velocity on S1 and S3 both have strong correlations with HSSW export (R^2>0.98 and P<0.0001), suggesting…” (Lines 335–336 in the revised version).

Time series of HSSW exports across the defined transects (S1 and S3) and averaged current velocity along these transects has been presented in Figs.R5 and R6 for SYNO1 and MESO respectively. Similarly, Figs.R7 and R8 show the time series of HSSW exports and HSSW amount (i.e., HSSW volume) for S1 and S3 over SYNO1 and MESO events. Obviously, the HSSW export significantly correlated with the current velocity for both S1 and S3 (R^2>0.98 and P<0.0001, Figs. R5 and R6). However, there are no consistent correlations between HSSW export and HSSW volume for S1 and S3. The correlation coefficients between HSSW export and volume are above 0.7 (P<0.01) for S1 in SYNO1 and S3 in MESO (Figs. R7a and R8b), but for the other cases the correlations are weaker (Fig. R8a) or even turn to the negative value (Fig. R7b) which might be associated with the persistence of HSSW formation. So generally, the current velocity is the main factor in regulating the HSSW export.

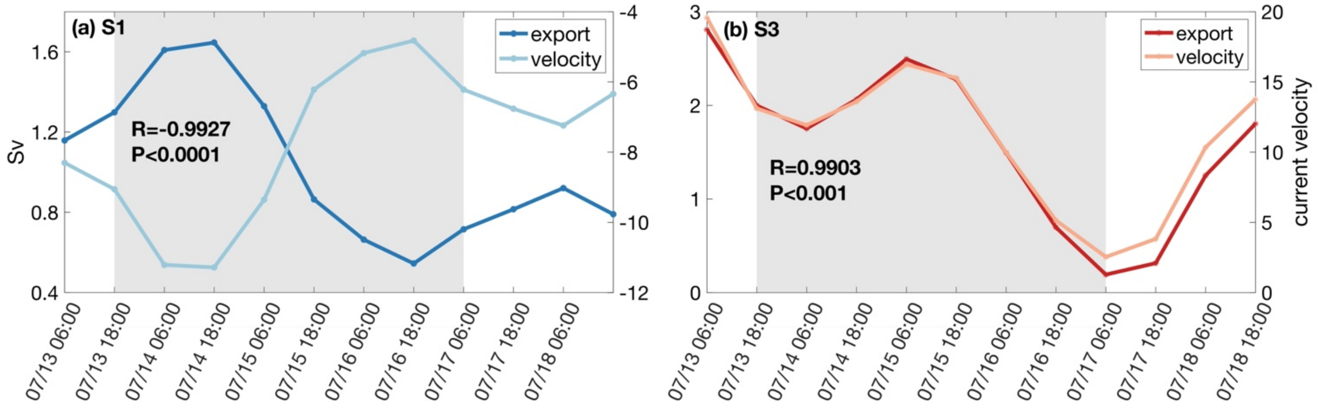

Figure R5. (a) Time series of HSSW exports across the S1 and averaged current velocity along the S1 from 06:00 of July 13 to 18:00 of July 18 2005. The gray shading represents the time of the SYNO1 event. The correlation coefficient R and P-value were calculated between the HSSW export and current velocity. (b) Same as Fig. R5a but for S3.

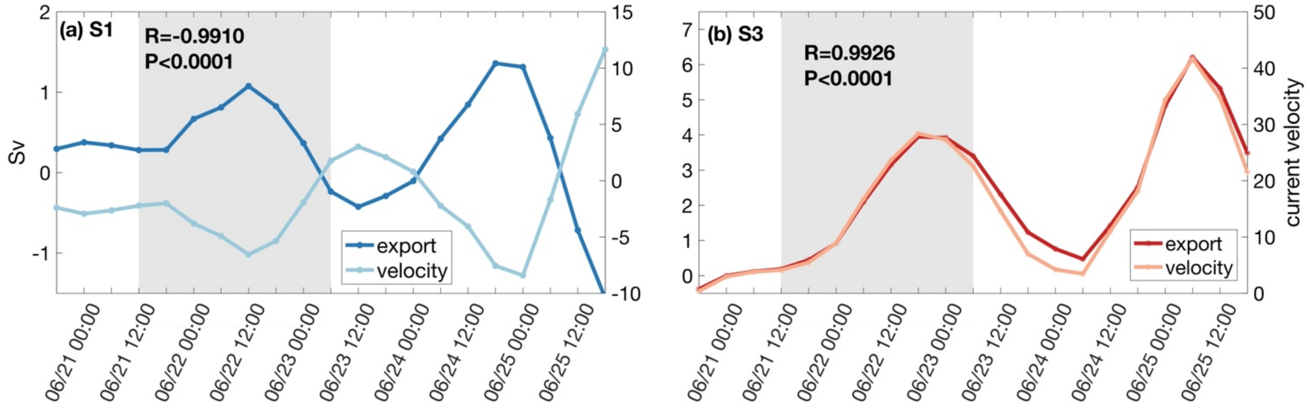

Figure R6. (a) Time series of HSSW exports across the S1 and averaged current velocity along the S1 from 18:00 of June 20 to 18:00 of June 25 2005. The gray shading represents the time of the MESO event. The correlation coefficient R and P-value were calculated between the HSSW export and current velocity. (b) Same as Fig. R6a but for S3.

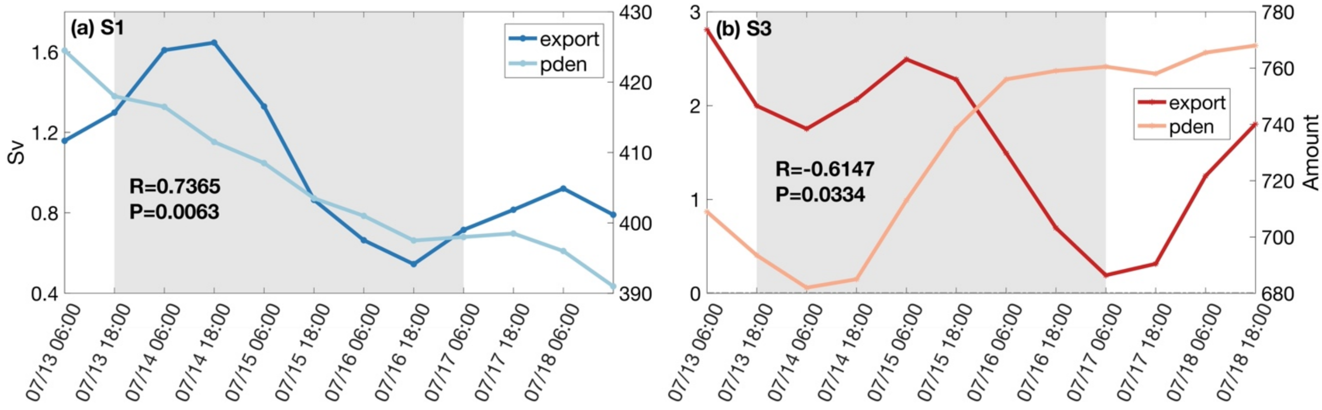

Figure R7. (a) Time series of HSSW exports across the S1 and HSSW amount along the S1 from 18:00 of June 20 to 18:00 of June 25 2005. The gray shading represents the time of the MESO event. The correlation coefficient R and P-value were calculated between the HSSW export and HSSW amount. (b) Same as Fig. R7a but for S3.

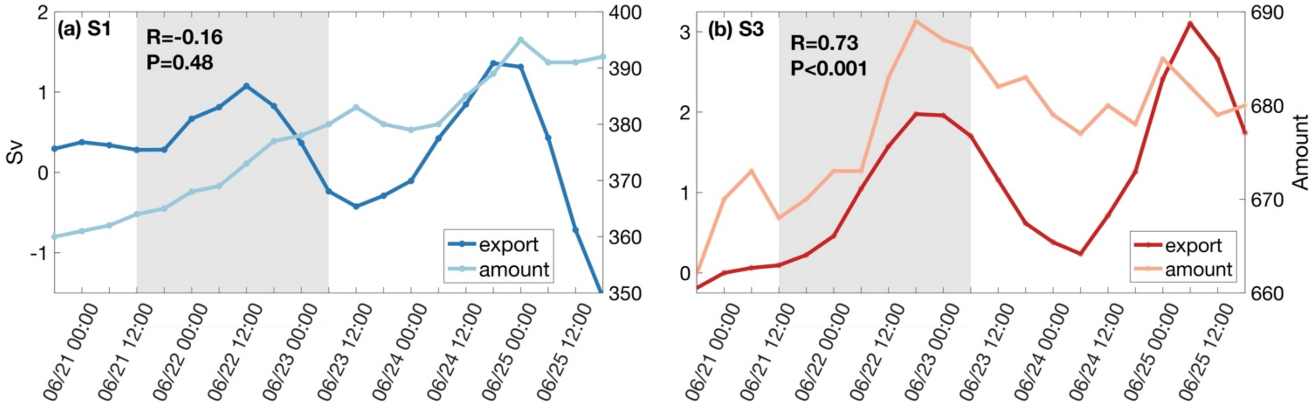

Figure R8. (a) Time series of HSSW exports across the S1 and HSSW amount along the S1 from 18:00 of June 20 to 18:00 of June 25 2005. The gray shading represents the time of the MESO event. The correlation coefficient R and P-value were calculated between the HSSW export and HSSW amount. (b) Same as Fig. R8a but for S3.

18. Line 371: Figure S3 shows that the high salinity water always appears at 163E, far away from the east side of RISP. So, it may be doubtful to attribute the slightly decreasing of HSSW salinity to SIP reduction at east side of RISP. Moreover, the Figure S3 g-j are not mentioned.

The initial idea was that the SIP on the east side and the HSSW on the west side could be linked by the westward coastal current along the RIS (Pillsbury & Jacobs 1985; Jacobs & Giulivi 1998; Smith et al., 2012; Porter et al., 2019), which could be seen over the upper layers in Fig. 15 and Fig. S6 in the revised version (particularly significant in Figs. S6d, e, g, h, f and k). Such current might carry fresher water resulting from the decrease in SIP to the western RISP, which could eventually lead to the decrease of HSSW salinity. However, based on the suggestion from the second reviewer, we changed these T-S diagrams to the time series of HSSW salinity, so this part has been removed.

References:

Pillsbury, R. D., & Jacobs, S. S. (1985). Preliminary observations from long-term current meter moorings near the Ross Ice Shelf, Antarctica. In Oceanology of the Antarctic Continental Shelf, Antarctic Research Series (Vol. 43, pp. 87–107). Washington, D.C: American Geophysical Union. http://doi.org/10.1029/AR043p0087.

Jacobs, S. S., & Giulivi, C. F. (1998). Thermohaline Data and Ocean Circulation on the Ross Sea Continental Shelf. https://doi.org/10.1007/978-88-470-2250-8

Smith JR, W. O., Sedwick, P. N., Arrigo, K. R., Ainley, D. G., & Orsi, A. H. (2012). The Ross Sea in a sea of Change. Oceanography, 25(3), 90–103.

Porter, D. F., Springer, S. R., Padman, L., Fricker, H. A., Tinto, K. J., Riser, S. C., et al. (2019). Evolution of the seasonal surface mixed layer of the Ross Sea, Antarctica, observed with autonomous profiling floats. Journal of Geophysical Research: Oceans. https://doi.org/10.1029/2018JC014683.

19. Line 376: the weak correlation between the HSSW exportation and meridional winds is interesting, which is different from SYNO1 and MESO. I think you should explain it.

Please see our detailed response to Comment #1.

20. Line 405-410: The estimation from Thompson et al. (2020) is obtained at TNB with an extreme katabatic wind. If you want to use it to verity the reliability of your model, maybe the wind speed, air temperature, etc. should be compared, which is important for SIP.

Thanks for pointing out this issue. The maximum wind speed during the MESO event was around 15 m s^{-1} (Figs. 11a and 11b in the revised version) and the 2-m air temperature was below −25°C during the period when the SIP reached its maximum value (indicated by the white boxes in Figs. R9). Therefore, the original sentence "Thompson et al. (2020) proposed that the intensity of ice production could rise up to 1.1 m day^{-1} during the windiest events in TNB, which was calculated from the salt budget using conductivity–temperature–depth (CTD) profiles. Meanwhile, the maximum SIP rate in the RISP during MESO was 1.2 m day^{-1} in our study, which is just slightly different from the value in TNB. Differences in topography and location between the RISP and TNB could lead to such slight differences in atmospheric and hydrological conditions." has been modified to "Thompson et al. (2020) proposed that the intensity of ice production could rise up to 1.1 m day^{-1} during the events when the wind speeds exceeded 20 m s^{-1} in TNB, which was calculated from the salt budget using conductivity–temperature–depth (CTD) profiles. Meanwhile, the maximum SIP rate in the RISP during MESO was 1.2 m day^{-1} in our study when the wind speed was around 15 m s^{-1}, which is just slightly different from the value in TNB. The air temperatures for both TNB and RISP were below −25°C. Differences in topography and location between the RISP and TNB could lead to such slight differences in atmospheric and hydrological conditions." (Lines 428–434 in the revised version).

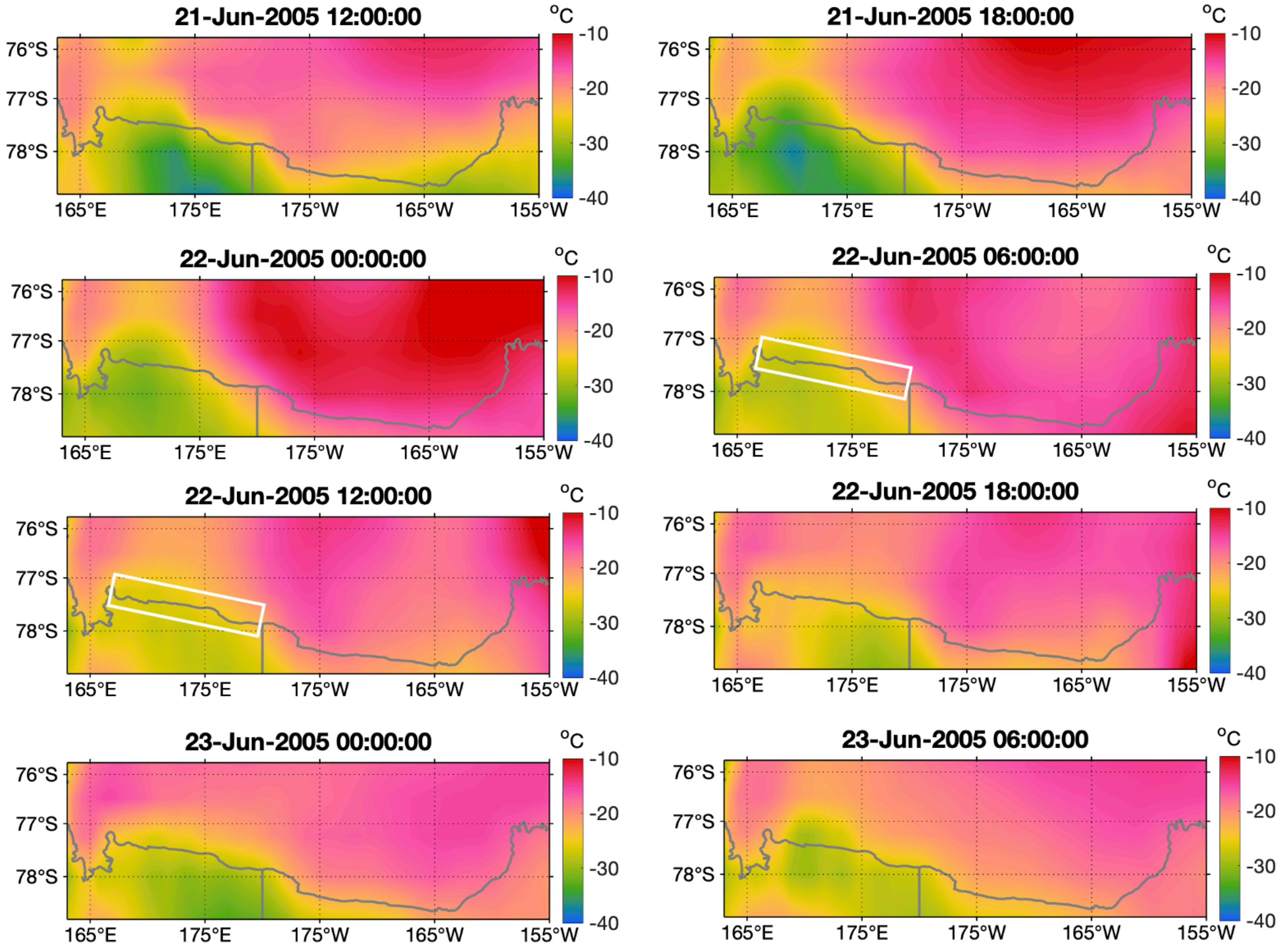

Figure R9. Spatial distributions of 6-hour-average 2-m air temperature in the Ross Ice Shelf Polynya over 21–23 June 2005. The white boxes represent the high sea ice production region during MESO shown in Figs. 9d and 9e.

21. Line 429: The persistence is interesting. At 23-Jun-2005 06:00, the SIP of RISP is weak (Figure 9h), but the decreasing of HSSW salinity starts at 24-Jun-2005 24:00. Can you explain which process provide the salty water? The similar question also can be seen in SYNO1 (Line 297).

The persistence of the higher-salinity HSSW when the SIP was decreasing might be associated with the brine rejection process. A certain time is required from the moment of new ice formation (i.e., SIP greater than zero) until the time when enough salinity is rejected to form the HSSW. Therefore, we speculate that the time required for the whole HSSW formation process may cause a lag in the response of HSSW salinity to SIP, which in turn leads to a continuous increase in salinity during the SIP reduction. Meanwhile, another possible reason might be related to the mixing process between different water masses. The cold and salty HSSW could be mixed with the fresher ambient water and the resulting water mass may also be in a highly-salinity state (i.e., still satisfy the definition of HSSW), which would then also lead to a continuous increase in HSSW salinity. The last possible reason is that the local circulation system might cause HSSW to be trapped in the polynya region for a period of time. To reveal the specific mechanism of this persistence for high-salinity HSSW, we will conduct sensitivity experiments and add online passive tracers to HSSW to identify the time scale required for the entire response process and the specific pathway of HSSW.

22. Line 477: that's interesting. With the Coriolis force, the exportation of HSSW should concentrate on the left side of the trough (e.g., Wang et al., 2013), but here it is on the right side? Can you explain it?

The export of HSSW concentrated on the eastern side of S2 (Figs. 11d, g, j and m in the revised version) originates from the Ross Ice Shelf region, which can be seen by looking at the revised Figs. 15 and S7. The presence of a northward flow across the eastern part of S2 at deeper layers (300 m and 500 m) can be clearly observed near 175°E (revised Figs. 15e, h, k and f, i, l). The flow originates around 79°S which is located at the RIS (revised Figs. S7e, h, k and f, i, l), so the flow could be an interaction between basal melting water and the HSSW. However, for the features revealed in Wang et al. (2013), their model domain as far south as 77°S does not encompass the Ross Ice Shelf, so the influence of ice shelf processes on the HSSW transport cannot be portrayed. Therefore, we speculate that the ice shelf processes are the main reason for such differences. The related information has been added as "For S2, the most eminent feature is that the export of HSSW is mainly concentrated on the eastern side of the section (Figs. 11d, g, j and m), which originates from the RIS region." (Lines 492–494 in the revised manuscript).

In addition, S2 is located at the entrance of the Joides Trough, closer to the RIS, which could be significantly affected by the local ice-shelf circulations. Furthermore, Morrison et al. (2020) also presented a consistent northward flow near the east side of S2 around 175°E (Fig. 4B in Morrison et al., 2020).

References:
Morrison, A. K., McC. Hogg, A., England, M. H., and Spence, P.: Warm Circumpolar Deep Water transport toward Antarctica driven by local dense water export in canyons, 6, 1–10, https://doi.org/10.1126/sciadv.aav2516, 2020.

23. Line 500: Does the barotropic geostrophic current from SSH here be consist with the simulated current field? The significant correlation between the Coriolis term and the pressure gradient term can only indicate geostrophic current, but the barotropic and simulated current need to be evaluated weather it is barotropic.

Following the reviewers' suggestions, we further calculated the barotropic and baroclinic components of the geostrophic currents to make more solid discussions. A As shown in the revised Fig.14, the barotropic flow over S1 is similar to the ocean currents in the upper layer (revised Figs.15a, d, g and j), which presents a southeastward flow across S1. To further identify the dominant flow component on the defined transects (S1, S2 and S3), the vertical sections of cross-transect velocity for barotropic and baroclinic geostrophic flow during SYNO1 and MESO were presented by Figs. S8–S11 in the revised supplementary material. The related features for S1 have been added as "In addition, we further examined the barotropic and baroclinic components for this geostrophic flow along S1 (Figs. S8 and S9). The positive (eastward) velocity in the upper layer in the area bounded by 77.1–76.9°S and 76.7–76.5°S (Figs. 11c, f, i and l) is regulated by the barotropic current (Figs. S8a, d, g and j), while the negative (westward) velocity in the deeper layer (Figs. 11c, f, i and l) is related to the baroclinic component resulting from the density differences across S1 (Figs. S9a, d, g and j)." (Lines 549–554 in the revised manuscript). For S3, the updated findings have been added as "Such barotropic currents could be identified on S3 in Fig. S8. Meanwhile, the baroclinic geostrophic flow also plays an important role in HSSW export across S3 (Figs. S9c, f, i and l). Therefore, the northward flow is regulated by both barotropic and baroclinic components. These features for MESO are consistent with that we found for SYNO1 (Figs. S10 and S11)." (Lines 568–572 in the revised manuscript).

24. Line 504: It may be clearer that plot the geostrophic current or the pressure gradient on Figure 14.

The barotropic geostrophic currents have been superimposed in this figure as Fig. 14 in the revised version.

25. Line 508: Do you mean the Ekman transport contributes to the SSH, which lead to the geostrophic current? Or the Ekman transport dominates the current (but it cannot be described as geostrophic current)?

Sorry for this unclear writing. This sentence aims to illustrate the Ekman transport dominated in the upper layer which contributed to the variation of SSH. The difference of SSH is the main source for the pressure gradient force inducing the surface geostrophic flow. The original sentence "Meanwhile, the relatively strong vertical shear in the upper layer suggests that the Ekman transport could dominate on top of the interior geostrophic current (Figs. 12j and 12t)." has been revised to "Meanwhile, the relatively strong vertical shear in the upper layer suggests that the Ekman transport could dominate on top of the interior geostrophic current (Figs. 12j and 12t), which contributed to the variation of SSH over S1." (Lines 536–538 in the revised version).

26. Line 556: I think that Morrison et al. (2020) meaned that the intruding mCDW (or CDW) is driven by the outflowing of HSSW but not vice versa, while I agree the mCDW (CDW) can influence the HSSW.

We agree with the reviewer's comments, and the original sentence mentioning the role of CDW inflow in HSSW flow has been deleted and revised to "Another factor might be the local circulation system like the outflow of basal melting water and the local gyre over these coastal regions. Herraiz-Borreguero et al. (2016) highlight the role of ice shelf water in controlling the HSSW formation rate and its thermohaline properties in East Antarctica. Formation of HSSW could be hindered by the freshwater input from ice shelves (Williams et al., 2016). Meanwhile, the increased freshwater from ice shelf melting could reduce the transport of CDW onto the continental shelf region (Dinniman et al. 2018), which can further affect the formation of dense shelf water." (Lines 589–595 in the revised version).

27. Line 566: I think that due to the section Mathiot et al. (2012) choosing is influenced by TNB polynya, here claimed the distance from where the HSSW generate (e.g., the distance from S1, S2, S3 to RISP in this paper and the distance from the section in Mathiot et al. (2012) to RISP and TNB polynya) is necessary.

As the reviewer suggested, the findings of Mathiot et al. (2012) and our study do focus on different time scales, so it does not make much sense to make lag time comparisons. Instead, we tried to address the linkage of this study to our study, and reorganized the statements as "Mathiot et al. (2012) documented a 6-month time lag between the HSSW formation in polynyas (TNB and RISP) and the HSSW transport across the topographic sills in the Ross Sea, i.e., the maximum HSSW transport occurred during summer (February/March) while the maximum of polynya activity occurs in winter (August/September). The defined sections across the Drygalski Trough and the Joides Trough in their study were located around 74°S, which is about 330 km further north than the sections we selected. This study provided a baseline for us to estimate the timescale for the cyclone-induced sea ice and HSSW change to influence bottom water properties at the slope.” (Lines 600–608 in the revised version).

Reference

Morrison, A. K., McC. Hogg, A., England, M. H., & Spence, P. (2020). Warm Circumpolar Deep Water transport toward Antarctica driven by local dense water export in canyons. Science Advances, 6(18), 1–10. https://doi.org/10.1126/sciadv.aav2516

Wang, Q., Danilov, S., Hellmer, H., Sidorenko, D., Schroter, J., & Jung, T. (2013). Enhanced cross-shelf exchange by tides in the western Ross Sea. Geophysical Research Letters, 40(21), 5735–5739. https://doi.org/10.1002/2013gl058207