Response to review comments by anonymous referee #2 on “The effect of storms on the Antarctic Slope Current and the warm inflow onto the southeastern Weddell Sea continental shelf” by Vår Dundas, Kjersti Daae, Elin Darelius, Markus Janout, Jean-Baptiste Sallée, and Svein Østerhus.

Dear reviewer,

Thank you for reading our manuscript so thoroughly and for your input. Your major comments will help us clarify several aspects and help us improve our manuscript substantially. Below, we address your major suggestions. Your submitted comments are in black text, and our responses are in green. At this stage, we do not include responses to minor comments; however, we have worked through these comments as well, and agree with most of the comments. Upon resubmission, we will include all of these comments as well. We apologize for the errors in some of the figure references; these will be corrected before resubmission.

Sincerely,
Vår Dundas, on behalf of all co-authors

The authors present an observational analysis investigating the effect of storm events on warm inflow at the Filchner Trough. I am not that familiar with the existing literature on this topic in this particular region, so can not comment on the novelty of the work. However, I found the results interesting, the manuscript well-written and figures nicely presented. I recommend publication after my comments are addressed.

Thank you for your effort in reviewing our study and for the overall positive assessment.

Major comments:

1. The authors appear to treat ERA5 as ‘truth’ and do not discuss how inaccuracies in the wind stress they use from ERA5 may impact their findings. How good is ERA5 in this region? Have any past studies validated it against nearby in situ weather station data (if any exists)? Is it possible that inaccuracy in the representation of wind stress in ERA5 could be the

explanation for the complicated ocean response. e.g. Imagine ERA5 overestimates the wind stress for some storms but not others, could that explain why there is no response to some storms?

Re.: Thank you for pointing this out. Caton Harrison et al. (2022) conducted a detailed comparison of coastal easterlies in three reanalysis products with satellite and in situ observations and concluded that ERA5 has the overall best performance. However, ERA5 underestimates coastal wind and wind speeds exceeding 20 m s−1 in this region (Caton Harrison et al., 2022). It is therefore possible that the strongest wind events during our study period are underestimated in magnitude by ERA5.

We will highlight this issue in sections 2.2 “Atmospheric and sea ice data” in the resubmitted version of the manuscript.

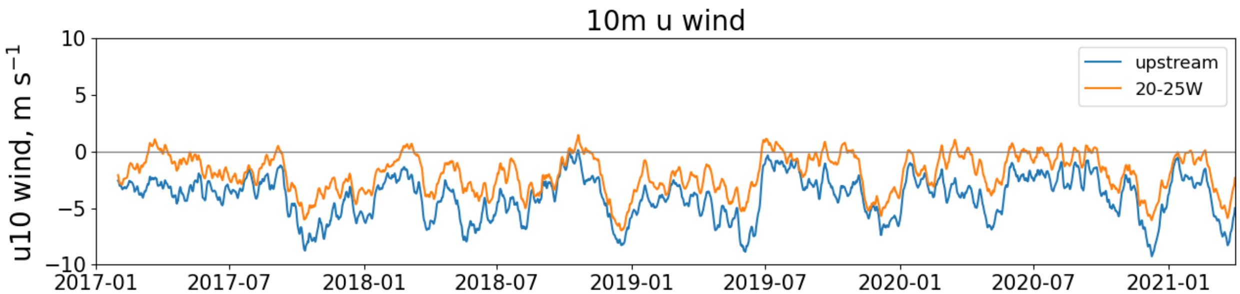
2. I am confused by the specific choice of the ‘Upstream box’. It would be helpful to discuss further the sensitivity of the storm response to the location of the ‘Upstream box’. The authors argue that it is not sensitive to location based on Fig A1. However that figure shows perhaps a correlation of only ~0.5 between the upstream box and the region closer to the trough inflow. Intuitively I would have thought that moving the box say 20 degrees west (to 20-25W) would result in a stronger connection to the inflow. If there is a physical justification why the chosen upstream box has the strongest connection to trough inflow, it would help future studies to explain further why this particular region is dynamically important.

   Re.: Thank you for highlighting this aspect. We base the choice of the Upstream box mainly on:

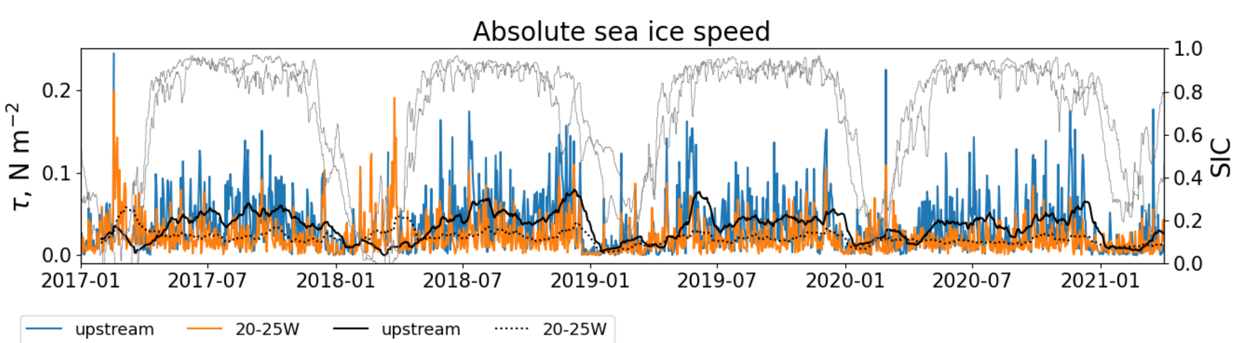
   - previous results by e.g., Daae et al,. (2018) and Lauber et al., (2023) who find that upstream atmospheric conditions are of particular importance for the local current speed along the Weddell Sea continental shelf break,
   - The wind pattern closer to the mooring sites is more chaotic, i.e.., more variation in wind direction, which makes it harder to connect to oceanic responses, and
   - that we do not want to choose a substantially larger box as this would smooth the time series of the ocean surface stress and potentially average out the strongest stress events. Therefore, we cannot, e.g., use the bounding applied by Daae et al., (2018).

Before settling on the Upstream box, we did parts of the initial analysis with the wind field over a more local box, and concluded that while the overall stress over this region is weaker, the main variability and statistics stayed similar. The atmospheric patterns are generally large relative to our study region, and thus we expect similar wind stress forcing along the coast east of Filchner Trough. The sea ice cover might, however, vary considerably.

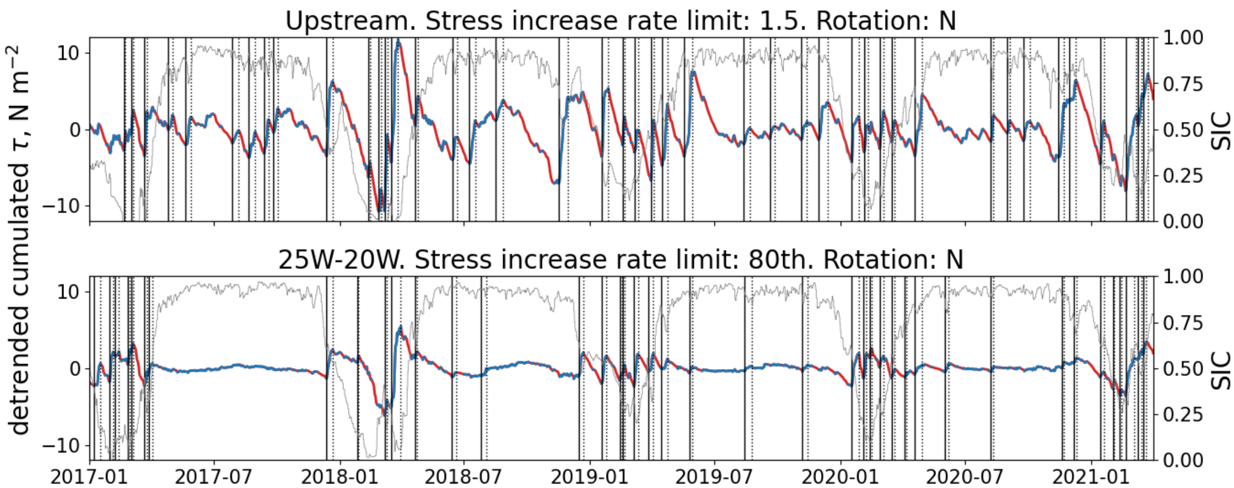
We have run through the final version of the storm-identification algorithm with the region you suggested from 25W-20W and 74S-72S. The 10m u-component of the wind has very similar variability on both monthly and sub-weekly time scales (figure below, 30-day rolling averages). The main difference is that the 25W-20W box has lower wind speeds. The v-component fluctuates around zero in both boxes.



Similarly, the sea ice movement has a similar variability in both regions, but the movement is generally less in the 25W-20W box (figure below). Notice the gaps in the time series when the SIC is zero. The grey lines in the background are SIC for both regions, and the black lines are the 30-day rolling averages of the sea ice movement.



Running the “storm”-identification algorithm yields these storm events:



The grey lines in the background are SIC. To identify the storms in the 25W-20W box, we have lowered the criteria for how much the cumulative stress needs to increase because the absolute values are overall lower. The limits for both regions are at approximately the 80^{th} percentile of the detrended cumulative stress.

The main difference between the two box selections is that there are fewer events during winter for the 25W-20W box. This difference between the winter seasons is as expected, since sea ice movement is consistently lower during winter in the 25W-20W box. The higher the sea ice concentration, the heavier the sea ice movement is weighted in the equation for the total ocean surface stress following Dotto et al., (2018).

This is another reason that we chose to use the Upstream box in our analysis: We want to capture the potential influence of strong wind forcing also during winter, when the more local surface stress changes are weak due to higher sea ice cover and lower sea ice movement. By working with the surface stress over the Upstream box we do not rule out the possibility that the fast barotropic oceanic response to the surface stress forcing can propagate a substantial distance along the coastline and continental shelf break and still impact the circulation in the Filchner Trough region.

Aside from during winter, there is a clear overlap in events. During the summer months, most of the disagreements between the two boxes arise due to the thresholds of stress increase rate: the stress increase might be evident in both time series upon inspection, but only selected as a storm for one of the boxes. This reflects that although we have made an algorithm that does a fair job at identifying periods of substantial stress increase, there is still some subjectivity involved.

5/18 significant storm response events at the along-slope moorings (M_slope1 and M_slope2) are not captured when using the ocean surface stress over the 20W-25W box. These are all during the winter season. There might, however, also be significant storm responses during events that are only identified within the 25W-20W box.

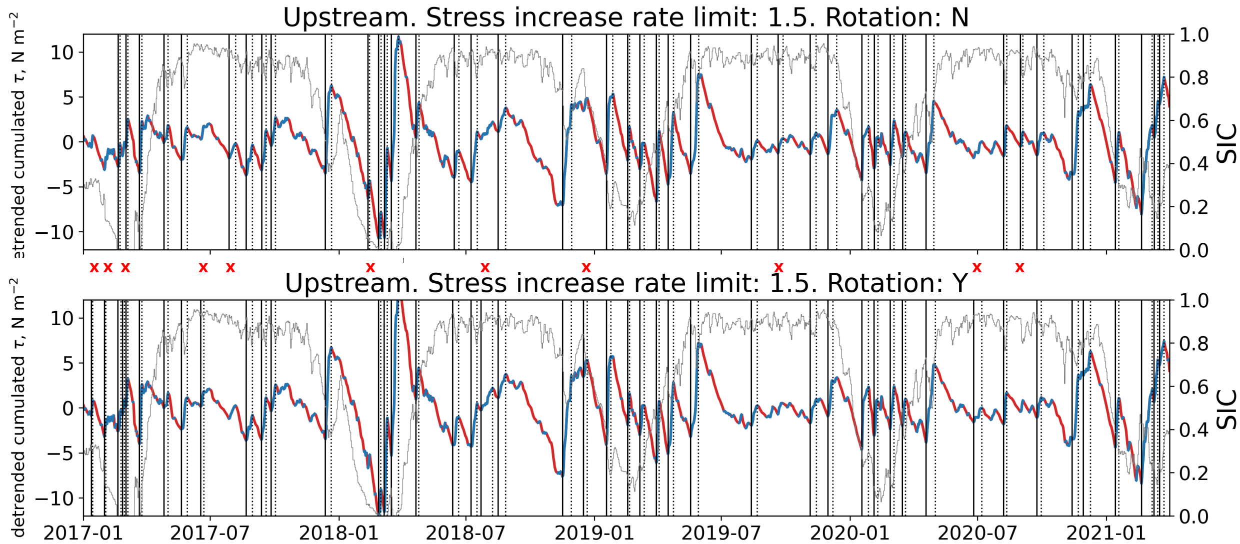
Before resubmission, we will check this and investigate the sensitivity of the resulting storm response to the choice of one box or the other. As an additional and independent procedure, we will also use the time series of the current at the slope moorings to identify periods or events when the current increase is large enough to be classified as a significant storm response. By comparing the timing of these events to the identified storms over the Upstream box and the 25W-20W box, we will get an indication of whether some storm response events can be explained by a stress increase over the more nearby 25W-20W box but not the upstream box and we can quantify the fraction of the events that are associated with a storm. We will consider whether we should change which box we use for the analysis throughout the manuscript.

3. In relation to the last point, does the direction of the wind stress have an impact on the storm response? Based on the mechanism described in the introduction, I would have thought that along-slope wind stress would be more important than westward wind stress (which is what I think has been used).

   Re.: Thank you for pointing this out. While rotating the ocean surface stress with the bathymetry does not make a large impact on the storm identification, we agree that it makes more physical sense to rotate the ocean surface stress before our analysis. We will therefore conduct a rotation of roughly 30 degrees in our updated version of the manuscript and update text and figures accordingly. While we will do this update, I will add some explanation of why we do not expect this to impact the main results much below.

   When working on why the storms sometimes give enhanced current and other times not, the direction of the ocean surface stress both before and during the event was one of the aspects we considered. We did, however, find that this did not appear to explain the difference in storm response.

When running the storm-identification algorithm with and without a 30-degree counterclockwise rotation of the coordinate system, which roughly corresponds to the angle of the bathymetry in the upstream box, there is some change in the identified storms (figure below). Black solid and dashed lines indicate the start and end of storm events, and the storms that are identified using one method but not the other are indicated by a red "x".

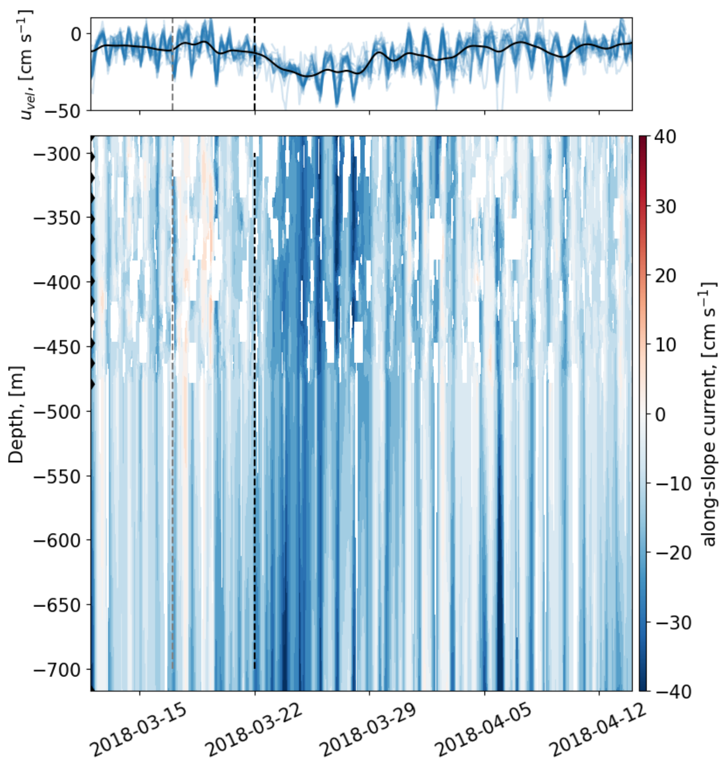


The offset between the number of identified storms and the number of storms that yield a response in the current is substantially larger than the difference in identified storms when applying a rotation to the stress, as opposed to not. Two of the mismatching events occur before the deployment period of the slope moorings, three events are only identified without rotation, while six events are only identified with rotation.

4. Out of curiosity, how barotropic is the response in the moorings? From what I can tell, only depth-averaged flow is used in the analysis. Also, is there any response in temperature or salinity after storms? This point does not need to be included in the manuscript if the authors do not wish to. I am just asking in case there is something interesting to say there that could add to what is currently in the manuscript.

Re.: This question is a bit tricky to address, as the resolution of velocity measurements varies from mooring to mooring, and thus we cannot state a common statement for all locations. The best vertical resolution is at the moorings Mslope1 and Mslope2, and these indicate that the response is relatively barotropic, as expected from the proposed coastal Ekman transport mechanism. There is, on average, a clear baroclinic component of the current at these two slope moorings with a bottom-intensified velocity field (Darelius et al., 2024), but the increase in the westward current following the storm is (relatively) depth independent.

Below is an example of the storm response with depth at M_slope2 during the storm event in March 2018 used in the case study. The upper panel shows the time series at each depth in pale blue, while the depth-average is shown in black. The color in the lower panel indicates the along-slope velocity in cm/s. The dashed vertical lines indicate the start of the storm (grey) and the maximum stress during the storm (black). We will consider whether to include a figure similar to the one below in the appendix to illustrate the response of the ocean currents with depth.



We comment on the response in the temperature field on lines 202-206:

"The thermocline over the slope at Mslope2, represented by the -1.7◦C isotherm, is only weakly pushed down (on average 30m during the storms with a significant

response at Mslope1 and Mslope2, not shown). This is substantially less than the high-frequency fluctuations caused by shelf waves and tides (which are on the order of 100-200m, Semper and Darelius, 2017; Jensen et al., 2013) and thus, depression of the thermocline caused by the storms do not substantially impede the access of warm water onto the continental shelf."

Since the impact on the thermocline is relatively weak and we do not detect a strong seasonal dependency, we have not commented on this further in the manuscript.