Response to review comments by K.W. Nicholls 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, K. W. Nicholls,

Thank you for reading our manuscript so thoroughly and for all your comments on the language and conciseness – your input will help us improve our manuscript substantially. We highly appreciate the effort put into the attached PDF document. 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 comments in the PDF; however, we have worked through the PDF 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 wind-derived anomalies of surface stress caused by storm events over the southern Weddell Sea, upstream of the Filchner continental shelf. They then investigate the impact of those periods of high surface-stress on the Antarctic Slope Current (ASC) near the Filchner sill, and on the flow both of warmer waters onto the continental shelf, and the southward flow of warm waters already on the shelf toward Filchner Ice Front.

This work is a continuation of observational and idealized numerical studies by many of the same authors. Here the mooring time series has been significantly extended. Seven moorings, with time series up to four years in length have been used. Obtaining those moorings has been a colossal effort, and they represent a very impressive resource.

As a continuation, the study is in some ways incremental, providing confirmation of key findings from the previous work, but also raising some interesting questions. I would like to see it published in this journal, after some relatively minor revisions.

Overall, the English is good, in that it is entirely understandable. However, the text could be substantially tightened up, perhaps by a co-author? I've submitted a marked-up PDF with many comments and an incomplete list of minor textual suggestions, but very often sentences could be redrafted more concisely. That is perhaps an editorial decision. Some of the comments are more substantive but most are requests for clarifications that can be very easily dealt with.

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

A couple of more significant questions.

1. Section 3.4

This reviewer was a bit confused about what the authors were trying to say in this section, where they describe a shift in July 2019 in the response to storms events: the response on the shelf to storms went from being inconsistent to consistent, while the reverse was the case for the response at the sill. At the same time the flow direction on the shelf migrated from being primarily north-eastward to primarily eastward.

In line 275 they mention the importance of changes in the upstream wind forcing as a possible reason behind the shift as discussed in an earlier study, but later in the paragraph note that the mean surface stress over the Upstream box doesn't seem to change during the shift. In the next paragraph (line 282) there is a comment about the correlation between wind direction and the current direction at M_CS2; the correlation shifts from negative to positive. Where is this wind? Is it over the Upstream box? If so, I don't see how the mean direction of the stress isn't changing, but the correlation between wind direction and current at M_SC2 is switching sign: the current direction is only changing by 45 degrees.

The paragraph starting at line 297 then seems initially to repeat the statements about the Daae et al paper's findings mentioned in the para starting in Line 275.

I think this section needs to be tightened up considerably. Clearly, the authors have an interesting finding, and haven't yet got an explanation that satisfies them. I feel that it could be explained very much more concisely and clearly.

Re.: We agree with your comments regarding this section. We find the changes we describe interesting to note, but we cannot yet explain how the different shifts are connected to each other and what drives them. We think it is important to mention that there appears to be a shift in 2019 since it emphasizes that interannual variability affects how the warm inflow on the continental shelf responds to atmospheric forcing – and this is clearly something we need to understand if we are to predict how the system evolves in the future. However, we will follow your advice and simplify this section substantially.

In the updated version of the manuscript, we plan to move panels b) and c) from Fig. 10 to the appendix, focusing solely on panel a) in section 3.4. We will move the detailed description of circulation and hydrographic changes, as well as the speculation associated with Ronne- and Berkner modes, to the appendix.

We agree that there is repetition between the paragraphs starting on lines 275 and 297. These paragraphs will be merged and simplified to avoid repetition.

Regarding the specific comment about the correlation between stress and ocean current at M_cs2: The correlated time series have daily resolution, high-pass filtered at 30 days (we will specify this in the updated manuscript), so although the mean surface stress is not changing direction, the sign of the correlation might change. We allow for a variable lag, so while the shift in correlation could be associated with a changed lag between the stress and the ocean current's variability, the shift is not purely the result of a changed lag interpreted as a reversed sign of the correlation. We will revisit the possibility of a shifted lag before resubmitting the manuscript. A major difference between the correlation presented in this study and the study by Daae et al., (2018) is that they specifically wanted to filter out storm events and used 15-day low-pass-filtered data to capture variability on monthly time scales. We will make this distinction clearer in the updated manuscript.

1. Certainty in the ocean response

I think the authors have generated a time series of the strength of the westward component of surface stress and used an algorithm to identify storm events. They then calculate the strength in the response of the mooring time series around the time of each storm. To be reassured of the robustness of the identification of the response, would it be helpful to carry out a randomized test: create a set of random times of pseudo-storm events, and carry out the same calculation of the strength of the "response" as measured by the mooring time series. Carry out the same test for a many different sets of pseudo-storm events. Highly variable currents as measured by the moorings will often have peaks that will occasionally correlate with peaks in storm forcing, regardless of whether they are being caused by the storm events. A Monte Carlo-like approach such as this will make clear whether the relationship between ocean response and storm forcing is robust. If this analysis is not possible for some reason, perhaps sample time series from the current data would help give confidence in the relationship.

Re.: We agree that it is important to distinguish "storm response" from background variability, but argue that we have already done so by including a type of significance test described in section 2.3 "Significant storm response" in the paragraph starting on line 153.

We cannot conduct a traditional Monte-Carlo procedure due to the length of the storm events relative to the length of the time series – the overlap between sample periods would be too large to act as randomized tests.

Instead, we estimate the difference between U_max and U_mean, i.e., U_response, during all 10-day-long windows without storms for each mooring. We let the windows be 50% overlapping. We then let the 90th percentile of U_response of all these 10-day-long storm-free windows be the threshold value in cm/s for a significant storm response. Panels c) and d) of Figure A2 are included to help explain this. In panel c), the blue histogram is U_response for all storm-free windows for the mooring M_slope1. The 90th percentile marker indicates the 90th percentile of the blue histogram. The orange histogram shows the distribution of U_response for all the storms. We define all storm responses to the left of the 90th percentile marker (black vertical line) to be significant, while all the storm responses to the right of the marker are insignificant.

From your comment, however, we acknowledge that we have not described this well enough, and we will improve this section. We will ask one or two peers to read the improved version of this section before resubmitting to ensure that the procedure and its purpose are clear. We can bring in a term like "Monte-Carlo like approach" to give the reader the correct association.

In relation to a comment by anonymous reviewer #2 concerning the choice of region for the ocean surface stress, we will conduct an analysis that is also relevant for the evaluation of the significance of the storm response. The following is copied from the response to reviewer #2: *"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 set the significance threshold at the 90^{th} percentile of current increase in periods without storms and thus accept that the current sometimes increase significantly unrelated to storm events. The described procedure, however, will provide an additional indication of how common it is to have a substantial current increase that is unrelated to storm events.

We notice that an x-label is missing in Figure A2c). This should be cm/s. We will add this and make the vertical line and text blue to clarify its association with the blue histogram.