Review of "On the drivers of regime shifts in the Antarctic marginal seas" by Verena Haid, Ralph Timmermann, Özgür Gürses and Hartmut H. Hellmer

Summary comments

The manuscript presents nine model experiments used to explore a potential regime shift in the "Antarctic marginal seas", the authors find that the Filchner-Ronne ice shelf tips from cold to warm and the Ross ice shelf does not. The authors ran over 70 experiments where nine are presented and they consist of 1 control, 5 idealised perturbations and 3 reversibility experiments. The authors identify that in the Weddell Sea, the key criteria for warm water to cross the continental shelf and fill the ice shelf cavity is the balance between the density at sill depth (L1; Figure 2) and the densest water produced by sea ice formation on the continental shelf (maximum density along the Ronne Ice Shelf). The authors think a similar metric for the Ross Sea is relevant but speculate that it does not tip because of a 'higher salinity threshold' (L274) where the Ross Sea has drifted from its WOA starting point. To arrive at these conclusions the 5 idealised perturbations modify ERAI by applying anomalies from HadCM3 21C-A1B, interestingly, I believe this is the same forcing as used in (Hellmer et al., 2012, 2017) (see Table 1). Moreover, these studies have shown a Weddell Sea tipping point (Comeau et al., 2022; Daae et al., 2020; Hazel & Stewart, 2020; Hellmer et al., 2012, 2017; Naughten et al., 2021) whereas (Bull et al., 2021) did not find one; impressively this study looks to make process-based progress on a problem that has already received a lot of attention. The importance of initial conditions and the metrics used to inform tipping behaviour are similar to previous studies. Here, a novelty is that the Weddell sea tip is reversible, I believe (Hazel & Stewart, 2020; Hellmer et al., 2017) are the only studies to have tested this and they found hysteresis behaviour. In the Amundsen sea (Caillet et al., 2022) tipping behaviour has been reversible. Another novelty is that contrary to other (publication bias?) work, it suggests the Weddell Sea is more robust to tipping than the many papers that are published on this would imply. And that the perturbations that tip the Weddell Sea do not tip the Ross. The parameter space of perturbation forcings would need to be understood in more detail however for these last two points to be satisfactorily resolved.

Unfortunately, in its present form, I found the study hard to follow and difficult to understand the proposed connection between forcing → ocean response. I was often unsure what the mechanism was that the authors were looking to test. I hope the authors will carefully re-read the manuscript and refine the story they wish to tell.

I would recommend the authors heavily revise the manuscript, considering the following major issues. I believe the study could be strengthened in four key areas:

1. By a careful re-framing of the study. Presently, the title implies a focus on "Antarctic marginal seas" but in practice most of the content is about the Weddell Sea. One option is to remove the Ross but in some sense the two systems are similar so it could be an exciting new opportunity to understand why one tips and the other doesn't. (As the authors note, the past studies in this space have been regional -- with the exception of Comeau et al., (2022) but that is not cited.)
2. I appreciate this is a process based study but the authors presently provide no model evaluation that the tool is fit for purpose. For example, the authors indicate that the Ross might not tip because of a model bias which requires further investigation. Relatedly, no bias

correction is done on the projection forcing and I believe that is not standard practice (Jourdain et al., 2022; Naughten et al., 2018, 2021).

3. The perturbations applied are complex and there is no presented assessment of the effective fluxes the ocean experienced from the experiments forcing. An updated Figure 4 could show the effective ocean stress and buoyancy flux experienced across the 5 perturbation experiments. At present, to this reader, it is impossible to determine what our expectations should be for how the ocean and ice shelf cavities should respond because I don't know how the forcing has effectively changed. Some further inspiration for relevant metrics might come from Figure 10 in (Neme et al., 2021), e.g. Weddell gyre strength, surface buoyancy flux and surface stress curl. Additional suggestions given below.
4. Improved context and critical assessment of tipping behaviour in the Weddell Sea here as compared to past work in this space (for an example and suggestions, see Discussion in (Comeau et al., 2022) and comments below). Given that this is now the seventh paper on tipping in the Weddell Sea that I am aware of, the authors are in the difficult position of trying to provide synthesis to a soup of models, initial conditions and forcings. One kind of progress on this problem would be a process-based understanding of the elements that drive tipping in the Weddell, whilst this study does noble work towards that, it falls short because of point 3 above.

Detailed comments are provided below where the 6 comments indicated by a **bold*** are key concerns.

Detailed comments

General comment: I initially read the paper, start to finish and while several of my comments were no longer relevant with subsequent explanation, I've kept the comments in as I hope the authors might re-phrase to help a time pressured reader. These instances are indicated with an 'update'.

Abstract

General comment. On the first read through, I got confused what the focus of the study was because it talked only about the Weddell Sea but the title suggested something more general.

L10 and L316. 'does not exist'. This should be re-phrased, I appreciate the authors did many experiments (>70!) but how can we be sure?

Introduction

L24. Slightly awkward as you've just talked about bi-stability but now you only mention one branch. Suggestion 'from a cold to a warm and… Here, we focus on …'

L25. 'cavity-shelf-sea'? (see **bold**)

L29. I'm not sure how these papers (Darelius and Ryan) support this idea, can you please clarify in-text.

L33. Not a regional model but I would have thought the findings of (Comeau et al., 2022) is worth a mention here?

L48. Typo with the “ ’ ”

Methods

Model

L66. Can you provide more detail about the vertical grid as relevant to your two ice shelves please

L67. At this point I was wondering about spin up, can you provide a small comment here (like what is on L88). E.g. 1979-2017 with ERAI are used as spin up

L67. WOA2013 is quite old now, for some evaluation (requested elsewhere) I’d suggest using a newer version.

L55. Tides are known to be important for FRIS, are they represented or parameterised here? I note your melt rates are lower than the observations that you compare to, is that related?

Forcing data sets

L69. I felt like there was a missing sentence here, note that the first-time reader at this point in the paper isn’t sure what you’re looking to do with these different forcings.

L69. There’s also no commentary that I found on why you’ve chosen these two forcings (not JRA, ERA5 etc). In particular, please comment on how these choices relates to the existing literature (e.g. study xx that tipped used.. we choose ..) would be great. (L95 is an example.)

L73. So I believe this is the same as used in (Hellmer et al., 2012, 2017) (see Table 1)? It’s a missed opportunity that you don’t mention this either here or in the introduction? Or perhaps I missed it?

L77. I got lost here. ‘For our purposes…’ As above, at L69. Please include what these purposes are. Something like: we will use this forcing method (e.g. bulk formula) for FESOM which requires variables: …

L77. How does this compare to (Caillet et al., 2022)?

Experiments

General comment: I’m sure the authors gave a lot of thought to the choice of presented experiments and both the region and variables perturbed. Please add some comments about each of the experiments based on why the authors chose those variables/regions and what we (naively) expect the model’s response to be.

L86. ‘extensive suite’ Here and elsewhere you mention that you did other simulations. I’m not asking for more runs to be included but if you wish to make these kinds of statements (e.g. L10 ‘does not exist’) then what can you tell the reader what you learnt from these other kinds of perturbations. E.g. we thought changing the buoyancy flux by xx would tip yy but it did not.

L88. Why was a fully varying forcing used here? I would have a thought a normal year (e.g. (Stewart et al., 2020)) would be more appropriate. Was anything done to reduce the shock when looping between 2017 and 1979?

L92. What was the thinking behind applying a seasonal anomaly? (As compared to say a time-mean shift?)

L93-94. Given the leap year differences across those two datasets, how was this handled here? (I think this is a distinct issue to the L98 60th day correction right?)

L98. 'every 60th day of …' this feels like a fairly extreme kind of hack (I appreciate the authors honesty in their method), were any sensitivity tests performed to see how much it influences any of the variables/regions of interest?

L100*. Here and elsewhere am I right in thinking no bias correction was performed on HADCM3? How different is the ocean output of HADCM3 to your REF mean state? I note in this paper (Barthel et al., 2020) that from CMIP5, the best performing models are: "top three CMIP5 climate models are CCSM4, MIROC-ESM-CHEM, and NorESM1-M for Antarctica". I appreciate you want to be comparable to (Timmermann & Hellmer, 2013) but is this still the best projection in 2022 to be using? Any sense of how this compares to UKESM as used in (Naughten et al., 2021)? I note two points from that paper:

- "In this domain, the only significant bias of UKESM's historical simulation compared to the ERA5 reanalysis is the coastal winds around Antarctica… We counteract this bias using a coastal wind correction, which is applied to the UKESM output … With this correction, present-day conditions simulated by ÚaMITgcm in the FRIS region largely agree with observations, as shown in Supplementary Note 2."
- "previous modelling studies using two closely-related ice-ocean models advanced directly to Stage 2 and did not appear to simulate a discernible Stage 1. We hypothesise that these studies may have been overly sensitive to WDW inflow, as they were all forced with the same climate model projection, now two generations old, with no bias corrections."

Note on the second quote that the papers and forcing being referenced are the ones relevant to this study.

L105. I'm confused by this. Austral seasons I think are: December to February is summer; March to May is autumn; June to August is winter; and September to November is spring. Here, December is unchanged, January is run three times (31 days * 3). So is that overwriting Feb, March and a bit of April? 3 days in April as Feb has 28 days. Then what is happening when you say 'July and August are eliminated?' And to be explicit your using ERAI, right? As elsewhere, a Figure and some careful additional text could help improve my understanding of the experiment design.

L108. As I don't follow SUMMER_S I'm confused here too. What is the rationale behind not changing the winds consistently but instead using SA_W?

Update: I was confused about the naming of this experiment, I would find it clearer if it was called SUMMER_S+SAw_W. It's weird that the non-wind changes are south of 50 but the wind is south of 63. So you're changing the buoyancy forcing of your ACC but not the winds.

L109. I would find it easier to follow with 'a seasonal anomaly is added to both wind components…'

L90-117. Can you please present all the experiments in a Figure. With a map showing regions and arrows indicating which experiments are based on other experiments. For the last three, branch off timings would be helpful too.

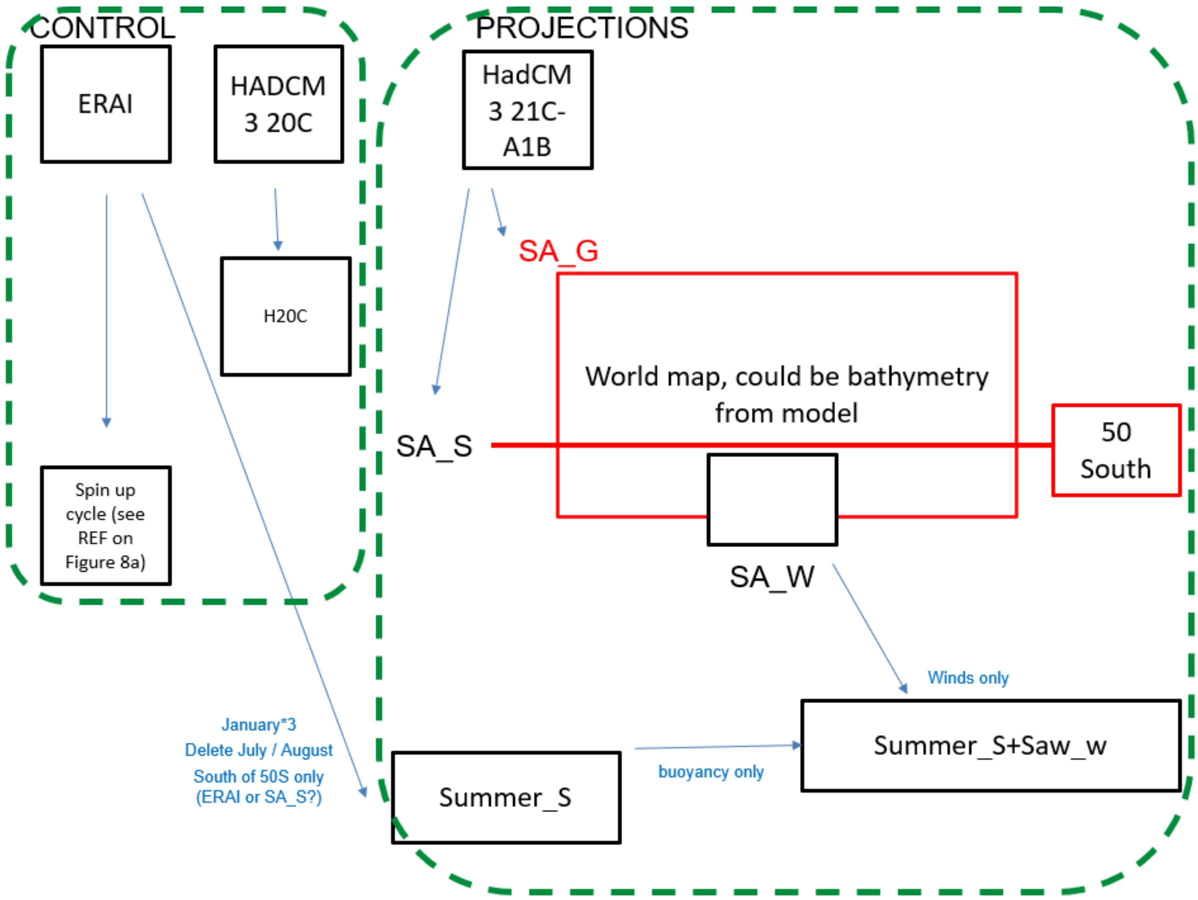
L102,104*, etc: ‘forcing remains unaltered’ what did you do in the transition zone? E.g. figure 2 from (Caillet et al., 2022)

L114 and L116. ‘starts from a warm-state’ and ‘starting from a cold-state’. As in the branch off point was chosen because it was a time period when the cavity was warm/cold?

L112-L117. On a first read through, I was puzzling on why these three had been chosen for reversal (at a guess because they are the perturbations that causing tipping). Thus on L111, please make it clearer what these ones are and where it’s important. (Equally, I’d be open to not including them at all at this stage and mentioning them only when they become relevant later)

L116. I think it’s confusing to frame H20C as a reversibility experiment. It’s really a control experiment to understand reversibility. I would suggest listing after REF. What was the rationale for spinning it off after one cycle of REF?

General update: as comments above, now that I’ve finished most of my review, here’s a suggested Experiment design Figure, I’m sure you can make it look much nicer:



It took me a long time (more time than most reader’s have I suspect) to understand your experiment’s forcing. You can see my remaining confusion with the forcing for Summer_S, I would guess it uses SA_S but as written I think it implies ERAI.

Results

Warm inflow at FRIS

L120-122. This is intriguing "several of our… of these, four". So why only show four?

L121. What are the four? Going off L123, three are based on SA and (at this point) I'm naïvely thinking that it's the local forcing (SA_W) that relevant. So you can see a comment like this, if relevant, would be helpful for this reader.

L125. 'About' or after? Moreover, it should be explicit that you mean the SA_G, SA_S and SA_W *only* as the SUMMER_S_S… has a weaker response at around y75 (as discussed later).

L126. For the following two comments about temperature and salinity what is the definition of the shelf? And is that what is being used here for both?

L130. This reader wished for a comma after 'maximum'

L131. Huh, that's surprising isn't it? So you're saying despite FRIS melting 20 times more, it has no influence on the water masses on the shelf? Given the size of FRIS, that's surprising. Please detail what evidence was looked at, the 'data does not provide strong evidence' is unclear to the reader.

L133. 'SA_*' please be explicit. If it were me, I would be inclined to compare GA_G and GA_S. This seems to be implied in L136 but if that's true it's clearer to be explicit, earlier.

L135. 'Global forcing manipulation' again, please reference specific experiments as it allows the reader to assess your evidence. (See previous comment about L136.)

L135-138. I'm not sure of what to make of SA_G being faster to respond, are you? I guess it could be: not significant (as it reverts back), an artefact of SA_S' regional forcing method, a highlight that we can't only focus on regional changes in forcing as there are feedbacks. If it's the first one, it's probably not worth highlighting but if you feel it's something compelling than please provide a hypothesis and evidence… L190 seems connected?

L148. Exert?

L148-149. Perhaps two difference panels could be added to this figure. I find it hard to see where the westerlies differ as mentioned in text.

L148-149*. I'd like to see more plots similar to this, but showing bulk metrics of the differences in felt ocean surface stress (wind + sea ice) and buoyancy fluxes. So what the simulations effectively *felt* from the perturbation. At the moment, I have a hard time understanding how the effective forcing between the nine runs differs.

L143. Why zero out the ACC here? And if so, have you looked at changes to say drake passage ACC transport? Surely changes in the Weddell gyre could also be relevant? Were these looked at?

L157. So we are now going to talk about the Ross Sea but this is in the section titled 'Warm inflow at FRIS'. Can we have a new subsection please. Having said L165-166, if you think it's uninteresting because it does not tip then perhaps the paper would be clearer if the Ross Sea analysis was removed entirely? (A comment could still be made in the discussion.)

L161-2*. This is the first model evaluation that I could find in the paper. At minimum, this study needs at least one Figure with a plan view map comparison of REF and FESOM as forced by HadCM3 20C (H20C^{+}) to observations. Relatedly, please add text to the WOA and melt estimates on Figure 3.

And state where the observations for melt are coming from (if they are coming from WOA2013 then this is not evaluation it's more a measure of drift, please use an independent dataset).

Update: [+]Note my confusion about this run in comment about L116. Because I thought it was a 'reversibility' experiment, the first time I thought about it was in Figure 8 and L243.

L163. The section on Ross would be better started with something like this. I suppose that's the main point, right? When you say 'none of our experiments' do you mean the ones shown in the study or all the ones you did? (As you allude to them elsewhere)

L165-166. This kind of statement would be very helpful much earlier.

L166. Do any of your other ice shelves tip? (in either direction).

L193. Curious. So I guess it implies that upstream far field changes are important? Which I guess is consistent with some of the papers you mention in the introduction.

L195. Which experiment is it though? (SA_G via L136?)

L198. So you're looking at density difference between on and off-shelf? Again, a map of these regions would be very helpful. I think this is a different metric to what was used in (Bull et al., 2021; Daae et al., 2020; Naughten et al., 2021), please comment on why you've used your metric here. Perhaps you feel it's more relevant? My impression, please correct me, comparing Figure 3c and Figure 6 is that the changes do not always occur at the same time?

Update: As I was focusing mostly on the text, I've been confused thinking you're doing regions but actually they are mostly points or sections..? Based on the Figure captions I think you're doing ice fronts and L1/L2 but it's still confusing because Figure 2 shows multiple ice shelf fronts. Thus, can you update your language to reflect if you're doing a region (Figure 3 and 8 maybe?), a section (e.g. Figure 6/7), a single location (e.g. Figure 6/7)

L201. 'it only takes a change in the regional wind pattern'. This feels like a key point. What is this change? (See my questions regarding SUMMER_S and SUMMER_S_Saw_W above)

L207. 'can be clearly..' I'm not convinced by this, see also my above comment about L198. What's the correlation like? To me, comparing Figure 3c to Figure 6; SA_G, SA_W, SA_S all have a consistent change of density between Ronne IF / L1. Their melt rates however have similar wobbles to SUMMER_S_Sa' but they don't have to change their density difference like SUMMER_S_Sa' does? Or did you mean something else? Perhaps that's what you're getting at in L210, see next comment.

L210. I'm not sure what I'm supposed to take from this 'however'? Also Figure 3b is temperature, perhaps you meant 3a?

L215. I thought the Ross analysis was removed?

L224. As written (see L237 below), I don't think you can frame it as a 'reversal experiment' because REF used ERA Interim data. To put this another way, what do you see as different between ERAI and HadCM3 20C *forcing* fields that could be important for the differences here? And if you want the reader to compare H20C and R_H20C please be explicit.

L237. I find that green hard to see, and is it teal?

L237. As my comments regarding L224 and 161, if H20C is what I think it is, please add a comment here that that is what we should be comparing R_H20C to.

L238. I think you mean singular 'green line'. H20C looks red, see above comment.

Discussion

L247. General discussion comment. So as above, supposing we are using the same forcing as used in (Hellmer et al., 2012, 2017) (see Table 1). What is the same / different here? I think those two studies used BRIOS? I wonder why Hellmer et al. (2017) found that it was irreversible? Given that many of the same authors are involved in this study, this seems like a good opportunity to explore these differences?

L248*. Throughout the paper, the key metric appears to be the density gradient in the Wedell as determined by

- Figure 6 "Annual mean values of maximum density \sigma_1 (potential density anomaly with reference pressure of 1000 dbar; reference density 1000 kg m−3) along the Ronne Ice Shelf front (red) and bottom density at 670 m depth at the continental shelf break at location L1 ... The depth of L1 is determined by the z-level closest to the Filchner Trough sill depth, which in the model is 640 m, slightly lower than in reality."
- Figure 8 "maximum density along the Ronne Ice Shelf front, and … bottom density near the Filchner Trough sill (Location L1)"

I believe the motivation is this (L180):

"The crucial criterion for the warm water to cross the continental shelf and fill the ice shelf cavity is the balance between the density off-shelf at sill depth and the densest water produced by sea ice formation on the continental shelf."

Are these two metrics from Figure 6/8 the same? Figure 8 looks like it can vary with depth whereas I think Figure 6 is fixed at ~1000m? Also, does the longitude of the maximum density along Ronne Ice front change much as applied? And like I asked at L198, how do you physically justify and interpret this metric? If the density can vary both in longitude and depth and one is comparing a section to a point, I find it hard to visualise. Plan view plots would help, something like a barotropic stream function between the cold and warm tipped runs. Or perhaps a longitude – depth section along FRIS with stratification and temperature / velocity?

L273*. 'by the bias'. So if the Ross didn't have the bias and you applied the same forcing you think it would tip? My impression is that previous tipping papers (e.g. (Comeau et al., 2022; Hazel & Stewart, 2020)) suggest that initial conditions are important. As above, some more detailed evaluation of your model would be very helpful. Perhaps you tried tipping the Ross with SA_G and something closer to WOA2013 (L67)?

Conclusion

L304. Here as elsewhere you could be more precise with 'Antarctic continental shelf' when I think you really mean Weddell and Ross shelf.

L305. 'as the decisive factor determining…' I accept you've shown that for the Weddell but as the Ross doesn't dramatically tip, can you really say that? Relatedly 'is possible' (L307) implies that there is no perturbation that exists that will allow off-shelf water in and I appreciate you did lots of experiments but I'm not willing to say no perturbation exists.

L309. 'the higher salinity in the Ross Sea is more important', I got the impression that it was the density difference (not the absolute numbers) that you felt was relevant? My suspicion is that your referring to the 'higher salinity threshold' in L274 but I encourage you to write for a time-pressured reader.

L320. 'potentially' what do you mean here? Maybe that ERAI is the 'present' and FRIS is currently cold?

L321. This paragraph would be improved with a topic sentence (this study is limited because…) as the transition is a little awkward.

L329. Instead of 'cryosphere' I think you mean ice sheet? (The ice shelves are part of the cryosphere)

L333. I note the following from the cryosphere pages:

"We recommend that any data set used in your manuscript is submitted to a reliable data repository and linked from your manuscript through a DOI."

https://www.the-cryosphere.net/submission.html

See also: https://www.the-cryosphere.net/policies/data_policy.html

I imagine your outputs are quite large but does 'available upon request' comply with the above guidelines? Perhaps a portion of the outputs/process scripts could be made available? Given the complexity of the forcing, some kind of documentation for how those experiments were created would help reproducibility in the future.

Figures

All line plot figures. Can the lines be made a bit thicker please.

In general, all figures with subpanels could have less whitespace between them which would increase the size of the panels making them more readable.

Figure 1. Would it be possible to annotate this, later perhaps on a new figure with the actual experiments?

Figure 2. I'm not sure what is meant by the brown (warm) water pathways. The large-scale arrows look red to me but if they are the warm pathways that's surely not applicable out in the open ocean.

Figure 3. Depending on how you end up re-structing the paper, I think this Figure is your 'main result' Figure and should be focused as such. In the present format, when I was reading top to bottom for the first time, I only wanted REF, SA_*. I appreciate you refer to the other experiments later but it was confusing at a glance.

Figure 5*. I find this plot helpful. It's the only spatial plan view plot of output in the whole paper (which is odd to me). So this is max temperature anywhere in the water column? Does it look similar to bottom temperature?

It's interesting that SA_G / SA_S are so similar when SA_W has a similar but weaker response. 'maximum in the water column' concern aside, SA_G / SA_S has more maximum warm water at the

Ronne side then SA_W but the whole Weddell sea is cooler in SA_W. Reviewing these forcings we have:

> SA_S: The same alterations to the forcing data were applied as described for SA_G, but only in the region south of 50°S. North of this line the ERA Interim forcing remains unaltered.
>
> SA_W: The same alterations to the forcing data were applied as described for SA_G, but only in the region of the Weddell Sea (south of 63°S and between 0° and 61°W). Outside of this area the ERA Interim forcing remains unaltered.

Why did you choose 63S for SA_W? And stopping at 0° is a little odd in that your chopping the Weddell Gyre in half (see Figure 1 in (Neme et al., 2021)) so I'd expect a weird response in the Weddell gyre. The ACC winds are likely also strangely modified? (See earlier question regarding transition zones.)

Curiously, SUMMER_S_Saw' has that fiddled wind field (see earlier question) and has a rather different gyre to SUMMER_S, perhaps looking more like SA_W.

Figure 6. Thanks for keeping the colors the same across the experiments in the other figures. Can we please also do that here, with say dashed or some kind of line style change. The panel layout is fine as is, just the colors.

Figure 7. As mentioned elsewhere is the difference or the absolute numbers that is of interest? If it's the difference then you could try plotting that here to make it less busy.

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