We would like to thank the two referees and the editor for their time and effort. We appreciate the referees′ thorough reading of our article and their comments, questions and suggestions. In the following, we will first give a general answer addressing the main issues raised, then address the more specific comments by answering to them one by one (blue font), following the original referee′s statements (black font).

We understand that our explanation on our choice and construction of the forcing data sets was a bit confusing. We will add clarifications as well as a schematic figure to aid the understanding of the manipulations of the seasonal cycle. We will explain our choice of the featured experiments better and elaborate on our motivations.

We will also work on the clarity of the storyline and in general aim to improve our explanations to help the reader follow our line of argument and to improve readability of the article. Furthermore, we will provide more information on the forcings′ impact of surface fluxes. We will include an investigation of the Antarctic Slope Front in the 6 experiments presented in 3.2. While we will remove the Ross Sea from Results, and present the corresponding graphs as supplementary material, we will add the region of the Amery Ice Shelf in Discussion (graphs as supplementary material) to strengthen the evidence for universal validity of our found density balance as the determining factor for a regime shift on the Antarctic continental shelf.

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).

While the data set of HadCM3 21C-A1B has been used in previous studies (e.g. Hellmer et al. 2012.), the data only serves as basis for manipulation of the seasonal cycle of the ERA Interim data set in this study.

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.

Together with addressing the comments from reviewer #1, we will re-work the story to improve the clarity of the manuscript, especially where it comes to the forcings, their construction and the motivations behind the choice of the featured experiments. The aim of the study will be stated more clearly and we will work on improving the clarity of our argumentation.

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.)

We understand that the title of our article could be misleading. We plan to rename the article 'On the drivers of regime shifts in the Antarctic marginal seas exemplified by the Weddell Sea'

We thank the referee for making us aware of Comeau et al. (2022). Their results seem to agree with the statement that the initial bias of a model strongly influences the vulnerability of a continental shelf (as stated in our comparison between the Weddell and Ross Sea shelves). We will include the study in our Discussion.

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).

   This study is not a projection or prediction. The experiments with the perturbed forcing had the main goal of also perturbing the ocean state – in direction of a potential regime shift on the continental shelves. In this study there is no intent of predicting the regime shift precisely, but to understand why it occurs when it occurs. We will go through the text, however, and try to be clearer whether any given statement is about the true ocean or the model ocean (which may or may not come with a bias in any given property).

   The found trigger mechanism for WDW inflow onto the shelf in the Weddell Sea (density balance) remains valid independent of the forcing used and independent of biases (initial or other). However, these initial biases strongly influence the vulnerability of the shelf, so that they matter strongly when your intent is a projection. Actually, this is one of the take-home messages we aim to provide. We will insure the article is clear on that.

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.
   We will expand on the topic of surface fluxes from the different forcings and add a new figure to this purpose. However, rather than focusing on effective fluxes for the ocean (which include the reaction of the sea-ice model to the forcing), we will concentrate on the forcing variables themselves, since they constitute the original difference between the experiments.

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.
   We will include a more elaborate assessment on the differences between the

forcings in their effect on the ocean (surface fluxes). We have neglected this aspect to some extent, since our main finding, the density balance as trigger mechanism, is independent of the forcing used for the experiments. (We'll elaborate on that, too.)

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'.

Thanks for clarifying this, we'll keep the un-forewarned reader in mind. :-)

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.

We'll modify the title. It'll be adjusted to 'On the drivers of regime shifts in the Antarctic marginal seas exemplified by the Weddell Sea'.

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?

The sentence will be adjusted: '… does not exist and instead various ocean states can lead to an intrusion of off-shelf waters onto the continental shelf and into the cavities.'

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 …'

We do actually focus on the bi-stability later, so the study does cover more than one branch. Also, at this point of the introduction, we are still in "scientific background" space, not yet at what we plan to do for this paper, so we intend to stick to the original manuscript text here.

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

Thanks. Will be corrected.

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

We'll add a more detailed explanation.

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

Thank you for making us aware of the study, we will consider it in the review process.

L48. Typo with the " ' "
We'll correct this. Thanks.

Methods

Model

L66. Can you provide more detail about the vertical grid as relevant to your two ice shelves please
The model has 99 layers, 12 of which are in the uppermost 100 m and 57 in the first 1000 m. We'll add this information in the model description.

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

Will add to last sentence: 'The model is started from rest and spun up for 39 years using ERA Interim forcing (1979-2017; Dee et al., 2011).'

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

Noted. It serves here only as an initial condition.

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?

Yes, our model is likely to under- rather than overestimate melt rates especially close to the ice shelf fronts because it does not include or parameterize tides.

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.

It has become clear to us that a reader will need more explanations on the forcings and the choice of experiments presented in the article. We will put great effort into helping the reader understand our motivations.

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.)

The choice of ERA Interim as the fundamental forcing for our study is based on previous work: Timmermann and Hellmer (2013) showed that sea ice production is an important factor for warm water intrusions into the FRIS cavity and Štulić et al. (in prep.) tuned the model to match observation-based sea ice formation estimates under ERA Interim forcing.

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?

We'll add the sentence: 'This data set was also used in Hellmer et al. (2012, 2017) and Timmermann and Hellmer (2013) in projection studies.'

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: …

We'll move this part into the description of SA_G, where it is probably better placed, and make sure there is clarity of the purpose.

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

Caillet et al. (2022) used a different model (NEMO) to induce a different shift (warm to cold) in a different region (Amundsen Sea). They were successful with regional invariable changes to different forcing variables.

Although both studies manipulate the atmospheric forcing in order to effect a change on the continental shelf, they differ in methodology (in the featured experiments). We'll add more information on how the surface fluxes are affected in our forcing data and also elaborate on our choices (regional invariable changes of select variables were unsuccessful in triggering a regime shift).

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.

We will add a longer introductory paragraph here to clarify our motivations and give our reasons for choosing the individual forcings.

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.

We will do so in the 'introductory paragraph' for 2.3 Experiments.

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?
In my opinion, there is one great drawback to using a normal year: Anything that would be caused by something exceptional will not happen. And while the shock of looping was not treated in a special way (the variables are linearly interpolated for the 6-/12-hour interval in between) the timing of the regime shifts suggests they are not accidentally triggered by it.

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

Essentially, a look at the differences between the HadCM3 data and ERA Interim revealed large differences in the seasonal cycle. So, in order to trigger a regime shift, we tried a seasonal anomaly. We'll add our reasoning in the article.

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?)

In leap years the last (6th) 60th day was also repeated, resulting in 6 x 61=366 days, while the normal years left the last one out (5 x 61 + 60 = 365). We'll add mention of this to the article.

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?

We did not perform any sensitivity tests. Please keep in mind that this is merely an auxiliary step to calculate a seasonal anomaly, of a data set that has biases all of its own. Our experiments were never intended to be projections, but to move the ocean state far enough out of the present state to trigger a regime shift (while staying within a range of atmospheric change that could be expected within roughly 100 years) to then investigate what triggered the shift.

As stated before, we'll work on clarifying motivations, purposes, choices, etc. in the text.

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.

This is not a projection. We use the data in various ways (more and more removed from the original data source) in an attempt to trigger a regime shift on Antarctic continental shelves. Then analyzing the results of many "successful" and "unsuccessful" experiments, we found what sets those with a regime shift apart from those without one. In no way do we attempt to sell our results as a forecast whether or not this regime shift will happen.

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.

Yes, this is based on ERA Interim only. We agree that January is a summer month, so repeating it two times will prolong the summer. Then follows February, March, etc. but after June comes September. Thus, the year is 12 months long and has an extremely long summer and mild and short winter. We will add a figure on the construction of the forcings as a visual aid.

I notice I failed to mention that the shortwave radiation is exempt from this treatment in our experiments, since whatever the climate will be like in any future, Earth's rotation will stay comparatively constant. I will add the fact in the description.

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.

Since in SUMMER_S the Weddell Sea continental shelf remained stable, we added 'a little nudge' to the forcing. We kept the region small, we picked the wind, because we knew it mattered, we added some smoothing to the previously used seasonal anomaly to get further away from HadCM3 and we succeeded in triggering a regime shift in SUMMER_S_SAw_S.

Then we looked at the evolution of its ocean state, saw that it was something of a special case and that together with its sister experiment it would serve really well in explaining how the density balance worked as the deciding factor for a regime shift, so we featured it in the study.

Yes, the forcing is full of inconsistencies. It's wild. But it produces a rather unique case of temporary inflow into the Filchner Trough, a regime shift teetering on the edge and controlled by the density of the dense shelf water. And in our view it's certainly not less realistic as a hypothetic future case than simply adding 10°C of warming everywhere (which likely would have created tipping too, but leaving the realm of moderate perturbations).

We'll change the experiment name from 'SUMMER_S_SAw_W' to 'SUMMER_S+SAw_W' in order to help the reader's understanding.

L109. I would find it easier to follow with 'a seasonal anomaly is added to both wind components…'
We will alter the text to read: ' … a seasonal anomaly is added to both wind components. This seasonal anomaly is…'.

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.

We'll add a map showing the three regions and a schematic with the branch-off timings.

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

There is only a narrow transition zone of 0.75° width, where results are linearly interpolated between the two data sets.

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?

Yes. We'll adapt the description to increase clarity.

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)

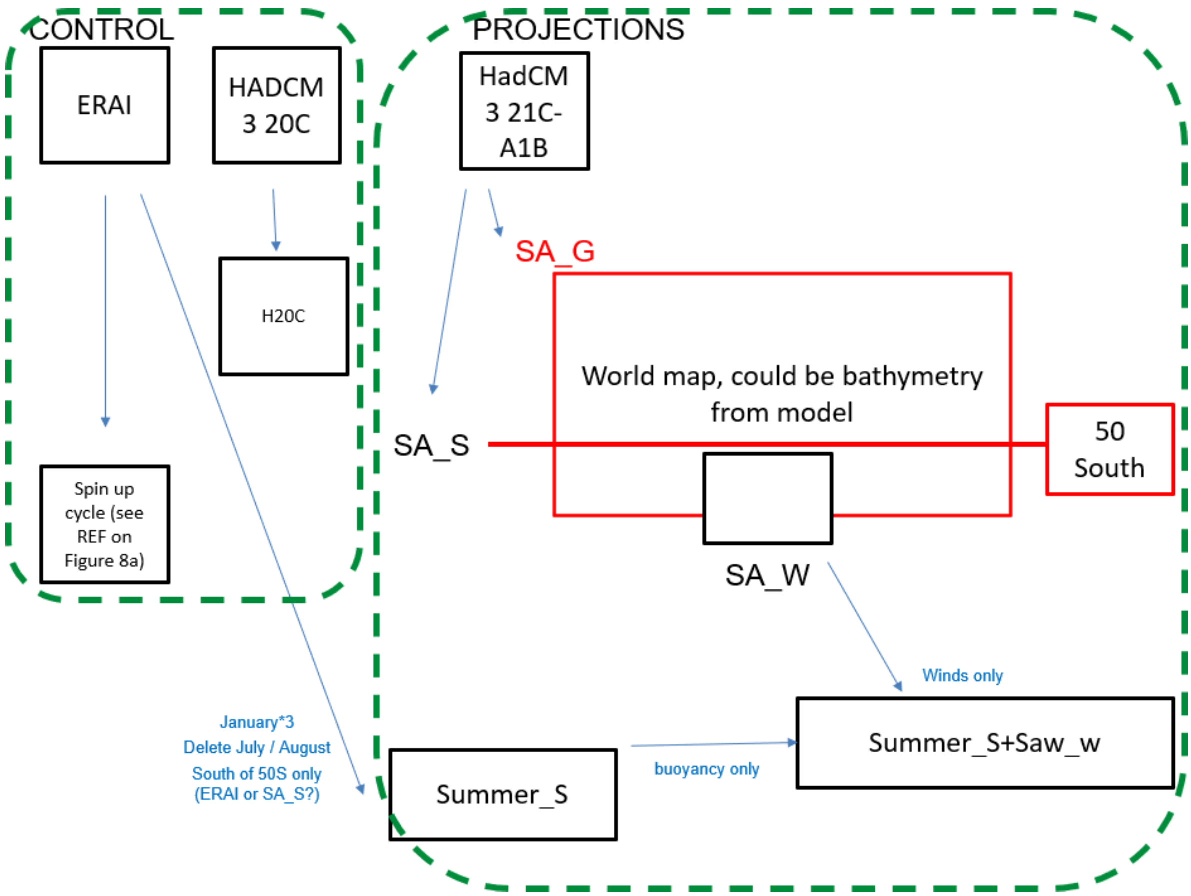
These three are experiments NOT using perturbation forcings, but forcings supposedly equivalent to current/last-century conditions. Two of them (R_ERAI and R_H20C, the actual reversal experiments) start from a state after a regime shift (warm state) and the last one (H20C) starts from 'current' conditions (cold state) and serves as a reference (control) run to R_H20C.

We'll be clearer in our descriptions.

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?

H20C is not called a reversibility experiment or a reversal run in the description. It is listed last since it is the last experiment to be talked about in the article. The main purpose of section 2.3 is to give an overview and provide a reference for specifics about the experiments. We'll add a introductory paragraph giving more information on the reasons for both, choice of forcings and choice of experiments represented in the article. We'll explicitly add a statement that H20C serves as a reference run to R_H20C.

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.

We'll add a figure (schematic) on the construction of the forcings and hope that together with better explanations given in the text there will be less confusion about the forcings.

Results

Warm inflow at FRIS

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

Because showing 15 will not make the article more readable. We will, however, address this point in the article and explain more clearly the choice of experiments presented.

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.

We will state the four experiments by name here. Yes, as we state in the discussion all experiments resulting in a regime shift on the Weddell Sea continental shelf are in various degrees linked to the HadCM3 21C A1B data set. Which is also the reason for our statement that the Weddell Sea was harder to 'tip' than expected.

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).
Will rephrase to 'Between 35 and 40 years after…'. Since the previous sentence was about SA_G, SA_S and SA_W *only* (stated so with explicitly naming them), this fact is also implied for this sentence.

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?

We'll add the information in the article. The Weddell Sea shelf here is limited by the longitudes 61° W and 25° W and the latitudes 78.5° S and 74° S. The upper 200 m are excluded as well as depths below 1000 m. The ice shelf cavity is not considered part of the shelf in this definition.

The Ross Sea is limited by 160° E, 140° W, 78.5° S and 75° S east of 180°E and 71° S west of 180° W. The depth is limited to 200-700 m. The cavity is excluded.

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

We'll add the comma.

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.

Yes, it is surprising. But the sudden increase in basal melt does not seem to be followed by a notable change in the salinity trend. As we explain, the discontinuation of the temperature increase (Fig. 3b) is more likely attributable to the limited heat availability. Furthermore, if a strong feedback from increased melt rates on mean salinity existed, we would expect an acceleration of the freshening trend (Fig. 3a) with increasing melt rates. What we found, however, is that temperatures increase and salinities decrease until about 40 years after starting the perturbation, and then remain largely steady after that, while melt rates jump to values higher by a factor of 20 at that point in time. There is certainly feedback from increasing melt rates on specific water mass properties, but the basin-scale trends (and their termination) are dominated by other processes (mainly atmospheric forcing and ocean dynamics / WDW heat supply)

We'll add this specification to the text: 'The data, as presented in Fig. 3, does not provide…'.

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.

Will change to 'SA_G, SA_S and SA_W'.

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

Will add the experiment name.

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?

The faster and initially stronger response can be explained by the faster warming the ocean experiences in SA_G. It causes stronger melt around Antarctica and much fresher surface waters. This affects the density on the continental shelf more than it affects the density of the deeper warm water. Thus, the density difference is reverted sooner and has a stronger initial effect.

The subsequent reduction of the melt rate below what is found in SA_S seems more surprising. Since the transport into the Filchner Trough shows the same behavior, our explanation is a slower gravity flow due to the decreased density of the (M)WDW. In SA_G the trend to lower WDW densities continues for another 20 years after the regime shift and the final densities are lower than in SA_S (Fig. 6).

L148. Exert?

Yes. Exert. Thank you. Will change the verb.

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.

Difference panels will be added.

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.

We'll provide more information on the differences in the forcing fields, namely downward heat and freshwater fluxes, wind stress. Since we see the sea ice model not as part of the forcing, but as part of the model itself, we'll provide information at the atmospheric boundary.

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?

The ACC is surely relevant to the Weddell Sea (as we also state in our study), but has no direct influence on the southern Weddell Sea. In our search for what decides whether a regime shift occurs, we found it of little help. The ACC is weakened compared to REF in all 6 featured perturbation experiments (least in SA_W, most in SUMMER_S_SAw_W, closely followed by SUMMER_S). The Weddell Gyre is strengthened in the perturbation experiments. Again, the least effect is seen in SA_W and the strongest in SUMMER_S_SAw_W, but SUMMER_S shows less effect on the volume transport of the Weddell Gyre than SA_G and SA_S.

We could not find a pattern in these large scale circulation features that would explain, or match, the occurrence of the regime shifts on the continental shelf.

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.)

We'll remove the Ross Sea from Results, mention it in Discussion (along with the Amery region as a new addition) and provide the figures as supplementary material.

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.

We forgot to mention that Fig. 3 also provides mean salinities and temperatures from WOA as a baseline for validation. We will add a comment to this in the manuscript.

We'll supply a list of studies included in the range of observation-based melt estimates in the figure caption. Also here, we will add a comment in the manuscript.

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)

We mean none of them. None of the entirety of them.

L165-166. This kind of statement would be very helpful much earlier. L166. Do any of your other ice shelves tip? (in either direction).

We see increasing melt rates for the Amery Ice Shelf. Since the representation of the bathymetry there is not reliable, we were hesitant to present it in the paper. To give further evidence of the validity of the density balance as the control mechanism of a regime shift, we will add the case of the Amery shelf in the supplementary material equivalent to the Ross Sea shelf.

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.
Yes. We think so, too. We come back to this in the Discussion.

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

All experiments starting with SA_ are talked about in this sentence. We'll explicitly list their names here.

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)

There is a map in Fig. 2 showing the relevant locations. Yes, it is a different metric. We used this metric because it explains which experiments experience a regime shift, and which don't. It is the only metric we found to do so. We'll go through the text and make sure to state that we are looking at single locations with in one case a varying position.

We are looking at annual means in Fig. 6 (the figure would be very hard to read otherwise). Still, the change in density over time is clearly visible as well as the fact that the two curves cross in the experiments with a regime shift and that this crossing closely coincides with the distinct increase in basal melt observed as a consequence of the regime shift. Apart from this temporal coincidence, the relatively simple physics behind the density balance is convincing to us.

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)

As described in the article, the main difference is that in SUMMER_S the winds along the southern coastline of the Weddell Sea are stronger and directed more offshore than in SUMMER_S_SAw_W. Therefore, there is less polynya activity (and a longer lasting sea ice cover in spring) in the southern Weddell Sea in SUMMER_S_SAw_W. The reduced sea ice production leads to less dense shelf water and thus enables the regime shift.

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.

The melt rate is mainly influenced by two quantities: the amount of heat available and the velocity of the current along the ice base. The latter in turn is influenced by the freshwater flux of the melting process itself. Inflow of MWDW leads to an increase in available heat and (at least through the feedback mechanism) increased velocities. The density difference in SUMMER_S_SAw_W in the period after year 70 switches sign several times and acts comparable to an on-/off-switch for the (M)WDW inflow.

This relationship is confirmed by a correlation of 0.81 between melt rate and density difference for the simulation years 75-117 (i.e. after the first inflow event). For the experiments SA_G, SA_S and SA_W, we find correlations of 0.53, 0.68 and 0.53, respectively, for the same period. All correlations have a significance level >99%.

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

There is a sentence mentioning salinity (no figure reference associated with it). Then follows a sentence mentioning temperature and basal melt with a reference to the respective subpanel of Fig. 3 (b). The contrast is emphasized by using 'however'.

L215. I thought the Ross analysis was removed?

We will remove Ross from the Results chapter.

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.

We'll add explicit mention that H20C serves as a reference run to R_H20C.

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

We will work on the color scheme of our line plots. Here, also resizing/rearranging the panels will help with visibility.

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.

We'll do so.

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

Yes, it should be singular here. The dark green line we talk about shows the results of R_H20C. (We'll change the colors of the plot.)

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?

For the same present-day data as in Hellmer et al. (2017), i.e. when using HadCM3-20C forcing, we also found hysteresis behaviour (cf. our experiments with H20C in their name). Also, the “irreversible” in Hellmer et al. (2017) stood for “not fully reversible”, not for “not reversible at all”. We try to avoid the term irreversibility, because (as seen in our results) it is just a matter of the forcing data set you pick (or: how far you move back on the x-axis in the scheme in Fig. 1).

We will add the information that the H20C data is the same as used in Hellmer et al. (2012, 2017). Apart from the data used as ‘present-day reference’ forcing in our experiments R_H20C and H20C, namely HadCM3 1900-1999, this study and the Hellmer et al. (2012, 2017) studies have very little in common.

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?
Yes, they are the same metric, but the presentation slightly differs for the sake of clarity, while keeping panel numbers within a reasonable limit.
For Fig 6 the red lines depict the maximum bottom density along the Ronne Ice Shelf front (there can be changes in depth depending on the exact location where the maximum is found); the blue lines show the bottom densities at L1 (Fig. 1) and do not vary in depth (-670 m). In Fig. 8, panel d) depicts the maximum bottom density along the Ronne Ice Shelf front (there can be variations in depth) and panel e) shows the bottom densities at L1 (no depth variation).
The values are comparable since the graphs do not show in-situ density, but potential density with reference to a depth level of 1000 m (sigma1).

Also, does the longitude of the maximum density along Ronne Ice front change much as applied?
In REF there is a seasonality, where (in most years) the densest water is found in the Ronne Depression in the west in the winter and in the summer the densest water is

found on the eastern side close to Berkner Island. There is, however, a strong interannual variability and for both sides of the ice shelf front, east and west, there are years when the location of the densest water remains on one side throughout the year (cf. Ronne mode vs. Berkner mode (Janout et al., 2021)). In the perturbation experiments, especially after the regime shift, there is a tendency away from the eastern or western corners toward the middle of Ronne Ice Shelf front. In SA_G for example, the Berkner mode is dominant at first, but after the regime shift, the densest water is found mostly between 58° W and 53° W.

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?
This metric simply samples the densest HSSW/DSW that could enter the cavity to compare it to the densest off-shelf water that could cross the sill and therefore enter the cavity. In the Weddell Sea, we are in the luxurious situation of an easy spatial segregation of the two inflow paths for HSSW/DSW and WDW/MWDW, so that the density time series can actually cross instead of meet. Even after the regime shift, the inflowing WDW/MWDW will not be present along the Ronne Ice Shelf front.

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)?

After the spin-up period the densest shelf water found at the Ross Ice Shelf front has a density (reference depth 1000 m) 0.11 kg m-3 higher than in the first year after initialization. This difference is less than the gap between the densities in any of our experiments at any time as seen in Fig. 7. We therefore assume that even without the bias the forcing in our experiments would not suffice to 'tip' the Ross Sea continental shelf. However, we know the ocean to be a complex system and we cannot exclude the possibility that feedbacks might just nudge the Ross Sea over the edge, esp. in SUMMER_S where the density gap is smallest. However, a stronger or simply a better-targeted forcing will induce tipping once the 'density gap' is closed.

Conclusion

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

We used the term Antarctic continental shelf because we view the trigger mechanism for a regime shift as being universal, but we have to admit there is little evidence so far for regions other than the Weddell Sea and will substitute with 'Weddell Sea continental shelf'. (In this context, we now plan to add a second case of a density-controlled regime shift at the Amery Ice Shelf into the Discussion, with Figures provided as supplementary material.)

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.

We stand by our statement, with the following concessions: The change from 'Antarctic' to 'Weddell Sea' in the beginning of the sentence and the substitution of the parentheses 'i.e. the ice shelf cavities' to 'and the ice shelf cavities below sill depth'. We wish to remark that the sentence states 'we identify … as the decisive factor', which states that it is our current scientific opinion. Of course, if we find, or are presented with, evidence of its inadequacy, we will adjust it accordingly.

By this we do not exclude the possibility of WDW/MWDW intrusions into the cavity at depths shallower than sill depth – but these will be too light to enter the deeper parts of the cavity. We will also add a statement to this purpose here and will add the explanation that we are only considering the case of gravity flows in the deeper layers where necessary in the article.

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.

Yes, thank you for pointing this out. We will correct the sentence: 'the higher on-shelf salinity in the Ross Sea leading to a larger density difference is more important'.

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

Using the term 'potentially', we acknowledge that it is an assumption that ERA Interim does adequately represent present-day atmospheric conditions. It is, however, not clear where exactly the actual present-day atmospheric conditions are located on the x-axis of the schematic in Fig.1.

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

Noted.

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

'cryosphere' will be replaced by 'ice sheet'.

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.

Yes, as you state there is a large amount of data associated with this study and the current data availability statement is on the small side. We will of course comply with the journal's policy. We can certainly share all the data behind the figures and annual mean temperature and salinity fields for the Weddell sector from all featured experiments.

Figures

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

We will improve readability of the line plots by resizing, a better color choice and/or thicker lines.

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

Most figures were intended to be page-width, we will work on their readability.

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

We will add the names of our experiments to do with reversibility (cf. Fig. 8) in the Figure, placing them in their respective zones with respect to the x-axis.

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.

We'll change the arrow color to orange (both in the figure as well as the description). The schematic seems to us an adequate first-order depiction of the circulation. The term 'warm' here is applied to water with T>0 °C or carrying heat toward the ice shelves. 'Cold' are waters containing a substantial amount of Ice Shelf Water.

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.

Actually, Figure 3 is mostly supposed to provide the baseline data from our experiments, not to convey the main message. The main result of the study in our view is in Fig.6. We understand that we need to be clearer on our motivation and the purpose of this study. We will work on the text with this aim.

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?

Yes, it does look very similar to bottom temperature. The choice to plot the maximum temperature was motivated by the experiments with the colder shelves in the bottom row. It is intended to avoid/answer the question: Well, the bottom is cold, but is there warm water at a shallower depth? We understand though that both are useful informations and we will add a comment on this in the text.

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.)

The motivation behind the SA_W experiment was to minimize the influence of the perturbed forcing (after the results of SA_G and SA_S had only shown a regime shift in the Weddell region) and see whether this would suffice to still induce this regime shift. It was therefore desirable to choose a relatively small region. The northern border was chosen due to the natural western border the Antarctic Peninsula constitutes, which ends at 63° S. Placing the eastern border at 0° kept the region small, while still keeping the direct effects of inconsistencies in the forcing out of the immediate inflow zone (Filchner Trough and adjacent upstream shelf break).

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.

This should not be too much of a surprise, since SUMMER_S_SAw_W and SA_W experience a very similar wind field. (The seasonal anomaly is smoother in SUMMER_S_SAw_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.

I would like the contrast between the two different density curves as high as possible. Plotting them in the same/similar colors, even with different line styles would not do this very well. Instead, we can color code the panels by printing the experiment names in the colors associated with the individual experiments.

The curves' colors will however be checked with view of color blindness (and adjusted if necessary) to give a good contrast.

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. Yes, it is the difference that matters. Still, if we plot the difference, we suspect that physical-oceanographer readers would ask for the absolute values to gauge the realism of the result. (In any case, we would.) Therefore, we decided to plot two lines (for each individual experiment) and leave it to the reader to gauge the difference. Admittedly there now are many lines in the plot, but the fact that none of the matching pairs of lines has a crossing should still be visible.

References

Barthel, A., Agosta, C., Little, C. M., Hattermann, T., Jourdain, N. C., Goelzer, H., et al. (2020). CMIP5 model selection for ISMIP6 ice sheet model forcing: Greenland and Antarctica. *The Cryosphere*, *14*(3), 855–879. https://doi.org/10.5194/tc-14-855-2020

Bull, C. Y. S., Jenkins, A., Jourdain, N. C., Vaňková, I., Holland, P. R., Mathiot, P., et al. (2021). Remote Control of Filchner-Ronne Ice Shelf Melt Rates by the Antarctic Slope Current. *Journal of Geophysical Research: Oceans*, *126*(2). https://doi.org/10.1029/2020JC016550

Caillet, J., Jourdain, N. C., Mathiot, P., Hellmer, H. H., & Mouginot, J. (2022, February 6). Drivers and reversibility of abrupt ocean state transitions in the Amundsen Sea, Antarctica. Earth and Space Science Open Archive. https://doi.org/10.1002/essoar.10511518.1

Comeau, D., Asay-Davis, X. S., Begeman, C. B., Hoffman, M. J., Lin, W., Petersen, M. R., et al. (2022). The DOE E3SM v1.2 Cryosphere Configuration: Description and Simulated Antarctic Ice-Shelf Basal Melting. *Journal of Advances in Modeling*

Earth Systems, *14*(2), e2021MS002468. https://doi.org/10.1029/2021MS002468

Daae, K., Hattermann, T., Darelius, E., Mueller, R. D., Naughten, K. A., Timmermann, R., & Hellmer, H.

H. (2020). Necessary Conditions for Warm Inflow Toward the Filchner Ice Shelf, Weddell Sea.

Geophysical Research Letters, *47*(22). https://doi.org/10.1029/2020GL089237

Hazel, J. E., & Stewart, A. L. (2020). Bistability of the Filchner-Ronne Ice Shelf Cavity Circulation and Basal Melt. *Journal of Geophysical Research: Oceans*, *125*(4). https://doi.org/10.1029/2019JC015848

Hellmer, H. H., Kauker, F., Timmermann, R., Determann, J., & Rae, J. (2012). Twenty-first-century warming of a large Antarctic ice-shelf cavity by a redirected coastal current. *Nature*, *485*(7397), 225–228. https://doi.org/10.1038/nature11064

Hellmer, H. H., Kauker, F., Timmermann, R., & Hattermann, T. (2017). The Fate of the Southern Weddell Sea Continental Shelf in a Warming Climate. *Journal of Climate*, *30*(12), 4337–4350. https://doi.org/10.1175/JCLI-D-16-0420.1

Jourdain, N. C., Mathiot, P., Burgard, C., Caillet, J., & Kittel, C. (2022, July 29). Ice shelf basal melt rates in the Amundsen Sea at the end of the 21st century. Earth and Space Science Open Archive. Retrieved from http://www.essoar.org/doi/10.1002/essoar.10511482.3

Naughten, K. A., Meissner, K. J., Galton-Fenzi, B. K., England, M. H., Timmermann, R., & Hellmer, H.

H. (2018). Future Projections of Antarctic Ice Shelf Melting Based on CMIP5 Scenarios.

Journal of Climate, *31*(13), 5243–5261. https://doi.org/10.1175/JCLI-D-17-

0854.1 Naughten, K. A., De Rydt, J., Rosier, S. H. R., Jenkins, A., Holland, P. R., &

Ridley, J. K. (2021). Two-

timescale response of a large Antarctic ice shelf to climate change. *Nature Communications*, *12*(1), 1991. https://doi.org/10.1038/s41467-021-22259-0

Neme, J., England, M. H., & Hogg, A. McC. (2021). Seasonal and Interannual Variability of the Weddell Gyre From a High-Resolution Global Ocean-Sea Ice Simulation During 1958–2018. *Journal of Geophysical Research: Oceans*, *126*(11), e2021JC017662. https://doi.org/10.1029/2021JC017662

Stewart, K. D., Kim, W. M., Urakawa, S., Hogg, A. McC., Yeager, S., Tsujino, H., et al. (2020). JRA55- do-based repeat year forcing datasets for driving ocean–sea-ice models. *Ocean Modelling*, *147*, 101557. https://doi.org/10.1016/j.ocemod.2019.101557

Timmermann, R., & Hellmer, H. H. (2013). Southern Ocean warming and increased ice shelf basal melting in the twenty-first and twenty-second centuries based on coupled ice-ocean finite- element modelling. *Ocean Dynamics*, *63*(9–10), 1011–1026. https://doi.org/10.1007/s10236-013-0642-0

Response References:

Not including References already given in the manuscript.

Janout, M. A., Hellmer, H. H., Hattermann, T., Huhn, O., Sültenfuss, J., Østerhus, S., et al. (2021). FRIS revisited in 2018: On the circulation and water masses at the Filchner and Ronne ice shelves in the southern Weddell Sea. *Journal of Geophysical Research: Oceans*, 126, e2021JC017269. https://doi.org/10.1029/2021JC017269

Štulić, L., Timmermann, R., Paul, S., Zentek, R., Heinemann, G., and Kanzow, T. (in prep.). Southern Weddell Sea surface freshwater flux modified by icescape and atmospheric forcing