Responses to review comments 1:

AC: The authors would like to extend our thanks to Xylar Asay-Davis for the in-depth and insightful review comments provided. We have done our best to address them in their entirety and believe that the paper has been enriched and improved by these suggested changes. The review comments are listed here in italics and preceded by "RC" and the authors' response in regular font preceded by "AC".

RC:

Reviewer: Xylar Asay-Davis

I wish my name to be relayed to the authors, as I feel I am always a better reviewer when I am not anonymous and I encourage others to consider reviewing non-anonymously whenever they feel able.

General Comments:

This paper presents two 1-degree NEMO simulations, one with closed ice-shelf cavities and another with open cavities for the three selected ice shelves: Filchner-Ronne, Ross and Larsen C. The authors validate the simulations using gridded climatological data, CTD sections and satellite-derived products. They show that the simulation with three open cavities ("Open") leads to important improvements in water mass properties compared with the reference simulation without any open cavities ("Closed"). They propose that the configuration they present is an important stepping stone toward full representation of Antarctic ice sheet-ocean processes in a coupled Earth system model (ESMs), and that NEMO is an especially good model for this work because it is used in several ESMs.

The paper was a pleasure to read, is well organized and makes a strong argument for why the inclusion of open cavities could significantly improve ESMs. I enjoyed the writing style of the introduction in particular: I found it to be far less dry than typical introductions because of the lighter and more literary writing style.

I have some minor recommendations for improving the paper, as I will go into in more detail in my specific comments and formatting suggestions below. I want to comment more generally on four of them here. First, I suggest including some discussion of the lack of tides in this model configuration, as the higher resolution NEMO configurations that you compare to seem to all include tides (or compare configurations with and without tides, finding that configurations with tides are better). Does this configuration include any attempt to capture the effects of tides on melting/freezing below ice shelves, given that tides aren't modeled explicitly? Could you *speculate on what the effects might be if tides were included, and comment on the feasibility of including tides in global ESM simulation with NEMO?*

AC:

Thank you for raising this, it certainly is worth clarifying how tides are treated in NEMO eORCA1 and this was missing in the original manuscript. The following paragraph has been added under methodology along with a section in supplementary material (Sect. S2):

"The effect of tides on vertical mixing (through breaking of internal waves) is taken into account in NEMO using the energy constrained parameterization of de Lavergne et al. (2020). This mixing parameterization does not, however, represent trapped waves at high latitudes or any tide-induced internal-wave mixing below ice shelves, and does not include the effect of tides on basal friction and thus melting of the ice shelves. To address this, by default there is a parameter (rn_ke0) representing the background kinetic energy associated with tides which is set to a constant of 2.5 x 10^{-3} m^2 s^{-2} everywhere. We tested another methodology of parameterizing the impact of tides on melting according to Jourdain et al. (2019) using CATS2008 two-dimensional tidal velocities; as summarised in Supplementary Material Sect. S2 and Fig. S1, this alternative parameterization brings marginal changes in the simulated melt patterns and bulk melt rates (< 10 %). The explicit representation of tides is not advisable in a configuration designed for climate applications due to the high levels of numerical mixing induced."

RC:

Second, and somewhat related, I think there is a little too much reliance on the namelist options provided in the Zenodo link for the model configuration. That file is 6 GB, which makes it quite a monster to download for a model developer that is interested in knowing more details about your configuration. The Zenodo package is wonderful for a NEMO developer interested in reproducing your work or using it as a stepping stone for future work. But it is considerably less helpful for developers of other models that might just want to know a bit more about what parameterizations and parameters you are using. For one, we may be unfamiliar with NEMO's namelist options. I took a look and I didn't find it super easy to wade through. The namelist filenames (namelist_ref and namelist_core_ia_cfg) don't make very clear which is "Open" and which is "Closed". Diffing the namelists show hundreds of differences, making it hard to know which are relevant. All this is to say that I think some more details (e.g. in a table or in the supplementary information) would go a long way.

AC:

Thank you for raising this. The description on the data repository has been updated to clarify the difference between the namelists provided and point the user to the relevant NEMO reference manual where detailed information on all namelist parameters can be found. Additionally, the following section has been added under Supplementary Material to assist readers in understanding the various files provided:

“In the Zenodo data repository associated with this manuscript (10.5281/zenodo.7561767), the NEMO reference namelist (namelist_ref), “Open” configuration namelist (namelist_core_ia_cfg) and sea ice namelists (namelist_ice_ref and namelist_ice_cfg) are given. The reference namelist is the default provided with the NEMO code. Unless stated otherwise in the “cfg”, the simulation uses the choices selected in the “ref” namelist. The namelist_core_ia_cfg is specific to a global ocean configuration (with modifications adapted to eORCA1) forced by interannual core winds. For more information on all the parameters included in these namelists, please refer to the NEMO reference manual available on Zenodo (10.5281/zenodo.6334656). Of specific interest may be Chapter 6.10 on “Interaction with ice shelves (ISF)” where the various options to represent ice-shelf/ ocean fluxes, heat and salt exchange coefficients and melt parameterization choices are explained.

The differences in namelist_core_ia_cfg for the “Open” and “Closed” cavity runs are listed in Table S1. Note that these differences are minor as the adaptations are made mostly to the input files (explained under “DOMAIN FILES AND INITIAL CONDITIONS” in Zenodo data repository description 10.5281/zenodo.7561767).”

** See table in Supplementary Material **

RC:

Third, this may just be my ignorance but isn’t Amery also thought by some in the community to be a major source of AABW? This may just be an area where I’m out of touch but I think I recall hearing several talks that made this claim over the years. In the literature, what I’m able to find is Williams et al. (2016), which seems to suggest that it isn’t as major a player. In any case, it might be worth including a little more explanation about why Amery was not included even though it’s a larger ice shelf than Larsen C (60,000 km$_2$ vs. 46,000 km$_2$ according to Rignot et al. 2013). Maybe that explanation is simply that you deemed it to be less relevant to AABW productions than the three cavities you included. In the conclusion, you mention that Amery along with Riiser-Larsen and Fimbul would be candidates for inclusion as a next step, so I think it might be worth saying a bit more about why they weren’t included yet here.

AC:

Indeed, Prydz Bay and Cape Darnley are considered together as an important site for AABW formation. However, as mentioned in Williams et al (2016), Amery Ice Shelf itself only preconditions shelf water and it is instead the Cape Darnley Polynya that is thought to be important for AABW. This polynya is not represented in eORCA1 as its presence is driven by landfast ice triggered by a group of grounded icebergs, processes not yet represented in NEMO.

The following paragraph explaining this has been added to the introduction:

“ RIS, FRIS and LCIS were chosen due to their direct role in the formation of the parent waters of AABW (Kerr et al., 2018; Bowen et al., 2021), and due to their large size and thus practicality of realistically simulating their sub-ice shelf cavities in a global ocean 1° setup. We choose to keep all other ice shelf cavities closed with prescribed melt rates injected at the mouth of the front using the method described by Mathiot et al. (2017). This includes the relatively large Amery Ice Shelf cavity, despite its role in preconditioning bottom water formation in the Cape Darnley polynya (Williams et al., 2016), because this polynya is absent in our configuration (due to the absence of icebergs and landfast sea ice). ”

RC:

Finally, all the figures in this paper have some gray artifacts along the boundary. In most figures (e.g. Fig. 1), these show up as if the panels have white backgrounds but they are on top of an overall figure with a gray background. In some figures (Fig. 6, 7, 8 and S5), there appears to be a dotted gray boundary around the figure. All of this should be cleaned up for final publication. I don't think this is my viewer, as I used several viewers and see the same issues.

Thank you for pointing this out. This was not evident to us. The figures have now been cleaned and there are no more gray artifacts.

RC: I hope my comments are helpful to you in revising the paper. I feel it is in good enough shape that I do not need to review it a second time and will be happy to leave it up to you and the editor to decide which of my comments to address.

AC: Thank you for these insightful and constructive comments. We have amended the manuscript accordingly.

Specific Comments:

RC: l. 18, 305, 308, 315, 317, 321 and perhaps elsewhere: You use “melt rates” to describe quantities in GT/yr that I would refer to as “total melt fluxes”. Perhaps this distinction isn’t well established in the community but to me a “melt rate” is in m/yr (or could be in kg/m$_2$/s) and a “total melt flux” is in GT/yr.

AC: This phrasing has been amended as advised.

RC: l. 44-45 and 128-130: Is your conclusion that the Amery region is not a major contributor to AABW? As I said, I may be a little out of touch with the latest literature there but I had a sense it was considered another contributor.

AC: A paragraph addressing the role of Amery in contributing to AABW precursors has been added to the introduction, as mentioned above.

RC: Fig. 1: Very nice figure!

AC: Thank you!

RC: l. 159-160: "...we decide to scale horizontal eddy viscosity south of 65°S according to grid cell size." Can you say more about this? What form of eddy viscosity are you using (del2, del4, something else?)? Does the viscosity scale linearly with the grid-cell size or some other way?

AC: We use laplacian viscosity and scale linearly. The text has been amended to state: "To account for the decrease in the horizontal size of grid cells at high latitudes, we decide to linearly scale the laplacian eddy viscosity south of 65° S according to grid cell size."

RC: l. 164-165: I think it's probably necessary to expand these 3 acronyms: ETOPO2v2, IBSCO and TEOS-10.

AC: These acronyms have been expanded.

RC: l. 168-170: "For more information regarding the choices of advection and diffusion schemes, mixing coefficients, and eddy parameterizations, please refer to the copy of the namelists provided in the accompanying data repository." As I mentioned in my general comments, I don't think this is sufficient or very accessible. Could you spend a paragraph here or in the supplementary information (and perhaps including a table) describing each of these in a little more detail for a non-NEMO expert?

AC: Supplementary Section S1 explaining the namelists in greater detail has been added along with a table showing the differences in namelist choices between the "Open" and "Closed" cavity runs.

RC: l. 182-184: "For the reference "Closed" cavity configuration, a fixed freshwater flux corresponding to the volume of basal meltwater estimated by Depoorter et al. (2013)...": You say later that the melt fluxes from Depoorter et al. (2013) are lower than other satellite-derived estimates of melt fluxes and call them into question. It might be worth mentioning why you chose to use these as opposed to newer estimates (e.g. Adusumilli et al. 2020). Maybe these were available from previous work by Mathiot et al. (2013) so it was more convenient? That's an acceptable explanation if that's what happened.

AC: The Depoorter et al. (2013) freshwater estimate for ice shelf melt is the default used for the IPSL climate model and so the files were already available for eORCA1, we just needed to remove the fixed fluxes for FRIS, RIS and LCIS. Additionally, Adusimilliet al. (2020) only extends to 81.5 °S so that RIS and FRIS are not fully covered. The following line has been added under methodology:

" The fixed freshwater flux is based on Depoorter et al. (2013) melt estimates as this is the same file used for the IPSL climate model. Furthermore, the ice shelf area surveyed by Adusumilli et al. (2020) only extends to 81.5 °S so that RIS and FRIS are not fully covered and therefore don't have the full melt flux."

RC: l. 184-185: "...for each ice shelf is added into the ocean evenly between the ocean floor and the base of the ice shelf at the location of the ice shelf front...": I believe this is the case but can *you explicitly state if the melt flux is also uniform horizontally along the calving front? From this sentence, it was only clear to me that it is vertically uniform.*

AC: Yes, it is horizontally uniform across the ice shelf front. This has been stated in the text, thanks for pointing that out.

RC: l. 190: "the temperature, salinity and velocities are averaged over a fixed boundary layer thickness of 30m...": Could you say how the 30-m thickness was chosen?

AC: 30m was chosen according to the study of Losch (2008). This is now stated in the text.

RC: l. 191-193: If you make a table with other model parameters as I suggested above might be a good idea, please move these 3 parameters to the table for easier readability.

AC: It was decided that a table is out of scope as the NEMO reference manual goes into great detail to explain the namelist parameters, and the namelists themselves have comments next to each parameter for clarity. The parameters mentioned here are those specific to simulating ocean - ice shelf interactions which is the focus of this study and so are explicitly stated in the main text.

RC: l. 206: Did you use CORE interannual forcing because it was more convenient than more up-to-date alternatives like JRA? If so, that's fine but it's probably worth stating.

AC: The authors and local NEMO team had less experience with JRA than with CORE forcing and so were less familiar with the inherent biases.

RC: Sec. 2.3: I'm not sure if this is the right place or the previous section but somewhere here I think you need to have a discussion about not having tides here and any parameters that were used to mimic or parameterize tides in the ice-shelf flux calculation.

AC: This has been added along with a section in supplementary material.

RC: *l. 213-214: "For all simulations, global ocean properties were initialized using the 1981-2010 climatology of World Ocean Atlas 2013 (WOA2013; Locarnini et al., 2013; Zweng et al., 2013).": Later on, you compare to WOA18 for validation. So it seems odd to initialize with WOA13. Perhaps this was convenient because it had been used in previous studies. If so, please state this.*

AC: Yes, you are right, the initialization files were available on our supercomputer as they are used for the IPSL climate model and so WOA2013 was used to initialize the model as it was more convenient. The following has now been stated in the text:
"For all simulations, global ocean properties were initialized using the 1981-2010 climatology of World Ocean Atlas 2013 (WOA2013; Locarnini et al., 2013; Zweng et al., 2013) as this dataset is used for the IPSL climate model and so was a convenient choice."

RC: *l. 214-230: I really appreciated the care you took in this process. I know from my own experience how tricky choices can be about initializing these cavities and this seems like a simple but very clever and effective method. Thank you for providing these details.*

AC: Thank you for recognising the effort behind this initialization procedure.

RC: Fig S2: Is this showing the Open or Closed simulation?

AC: This is showing the closed cavity run and the difference Open-Closed. It has been updated in the new supplementary material.

Formatting, Typographical and Grammatical Suggestions:

RC: l. 18 "36 + 7" and "112 + 22" should be "36 ± 7" and "112 ± 22"

AC: Oops! Thanks!

RC: l. 26-30: Maybe EGU sphere gives guidance on these plain language summaries. I did a quick search and didn't find anything helpful. My sense would be that the terms "lower limb", "salinity bias" and "water mass" might be too specific to oceanography to count as "plain language". Maybe these can be reworded?

AC:
"Lower limb" has been replaced with "bottom half"
"Salinity biases" has been replaced with "salt biases"
For water mass, there is no succinct and appropriate replacement (we need to fit within the word count).

RC: l. 62: I would change the word "valuable" to something a little less subjective.

AC: This has been changed to "important"

RC: *l. 95 and 618: This is obviously a stylistic choice but I think references to "the authors" are a little strangely indirect, and I would suggest just using "we".*

AC: Here as a stylistic choice we prefer to leave it as "the authors".

RC: *l. 96 "Then, work needs to be done..." (a comma after "Then".)*

AC: Corrected

RC: l. 127: "We choose to keep all other ice shelves closed..." I would change "ice shelves" to "ice-shelf cavities".

AC: Corrected

RC: l. 128-129 "RIS, FRIS and LCIS were chosen due to their role in the formation and setting of properties of the parent waters of AABW..." I would suggest rewording "setting of" to something like "influence on".

AC: This has been amended

RC: *l. 132: "geometry, as coupling can introduce..." (a comma after "geometry").*

AC: Corrected

RC: *l. 153: "For this study version 4.2 of NEMO is used..." I suggest active voice – it reads much better and gives credit where it's due: "For this study, we use version 4.2 of NEMO...".*

AC: Corrected

RC: *l. 166: "...absolute salinity, which, for the purposes of this study, were converted to..." (3 missing commas).*

AC: Corrected

RC: *l. 181: "Results from two configurations are presented here..." Again, a great opportunity for active voice: "Here, we present results from two configurations..."*

AC: Thank you for the advice, this has been amended.

RC: *l. 182: "...prescribed in a way to mimic the ice-shelf overturning...": "to mimic" should probably be "that mimics".*

AC: Corrected

RC: l. 223-225: "[-2 °C and 34.76 for FRIS (Janout et al., 2021), -1.95 °C and 34.74 for LCIS (Nicholls et al., 2004; Hutchinson et al., 2020), and -1.94 °C and 34.76 for RIS (Bergamasco et al., 2003; Budillon et al., 2003)].": A pet peeve of mine (and I will admit a losing battle) is nested parentheses. I'd ask you to consider using square brackets for the outer parentheses here. Regardless, you are missing a second end parenthesis or bracket.

AC: Square brackets have been added

RC: l. 234-236: "To assess the existing biases in the representation of dense water properties in NEMO v4.2 eORCA1 standard configuration ("Closed"), we compare full depth temperature versus salinity plots along with bottom temperature and salinity are compared with World Ocean Atlas (WOA 2018)": Again, my preference for active voice.

AC: We tested this re-phrasing and it becomes quite cumbersome here.

RC: *l. 237: "Weddell and Ross Seas, respectively": comma after "Seas"*

AC: Corrected

RC*: l. 242: "(Figs. 2a and 2b)": I think this should be "(Figs. 2b and 2c)"*

AC: You are right, this has been corrected.

RC: *l. 248: "...in the model output (ISW box Fig. 2d), as in this...": comma before "as"*

AC: Corrected

RC: *l. 261: "...CTD results from Hutchinson et al. (2020; **their** Fig. 3b), we find the Closed configuration to be too saline...": comma missing before "we" but also I think you must mean "their Fig. 3b" since Fig. 3b in this paper shows temperature, not salinity, and doesn't seem to be relevant to this discussion.*

AC: You are right, this has been corrected.

RC: Figs. 2 and 3: I found that the resolution of these figures was too low to be able to see important details in the T-S diagram panels. The text labels on density in these panels are also too small to be readable (even with zooming on my tablet).

AC: The text size of the density contour labels have been increased. Please note that for final submission high resolution pdfs will be provided (there seems to be some reduction in quality when conversion of the docx to pdf takes place).

RC: *l. 287: "...there is no ISW in this standard configuration, as there is no explicit model representation...": comma after "configuration"*

AC: Corrected

RC: l. 315: "melt rate for RIS, while being higher than observational studies...": reads better without the word "being"

AC: Corrected

RC: l. 344: "Opening the sub-ice shelf cavities in eORCA1 allows for the establishment of...": comma should be removed before "allows".

AC: Corrected. Thank you for the high level of attention you have paid to the text. It is much appreciated.

RC: *l. 356-358: "Comparatively warm and salty HSSW enters via the Ronne Depression, circulates from west to east, **melts** the base of the ice shelf mostly along the grounding line (cold, fresh signatures in Figs. 5c and 5d), and exits via the Filchner Trough as ISW.": I think you might want "melts" rather than "melting" but it might be fine either way.*

AC: Corrected. Thank you.

RC: *l. 362-363: "It is therefore encouraging that eORCA1 captures these, as they could play an important role...": comma needed after "these".*

AC: Corrected.

RC: *Fig. 5: This figure needs to be higher resolution to be able to make out the size and direction of the arrows in the quiver plot.*

AC: This has been done.

RC: l. 373: "Here, we notice a strong anticlockwise circulation..." comma needed after "Here". l. 376: "...far east (Fig. 5g), which is not seen..." comma needed before "which".

AC: Corrected.

RC: l. 377: "...speeds are extremely slow.": I would use something less subjective than "extremely slow" here.

AC: Corrected. This has been changed to "are slow".

RC: *l. 402-403: "...be seen in the volumetric T-S plot (supplementary material Fig. S1a), where explicit ocean-ice shelf interaction...": comma needed before "where".*

AC: Corrected.

RC: l. 408: "...conditions in the west, where in the reference run HSSW..." comma needed before "where".

AC: Corrected.

RC: l. 428: "...using PAGO, a pre-existing tool to analyze gridded ocean datasets (Deshayes et al.,2014)." I would take out the word "pre-existing" since the citation makes it clear that you didn't write this tool yourselves.

AC: Corrected.

RC: *l. 438-439: "While the model struggles to capture the coherence of this sub-surface temperature maximum, the counterclockwise circulation cell* ***set up*** *on the central continental shelf in the open cavity...": "setup" should be "set up".*

AC: Corrected.

RC: l. 443-445: "...while we cannot directly compare with the simulation output as the CORE forcing ends in 2009, evidence for the presence of a tongue of ISW focused on the western bank of Filchner Trough is evident in Fig. 3 of Janout et al. (2021)...": redundant "evidence for...is evident".

AC: Corrected to state : “the presence of a tongue of ISW focused on the western bank of Filchner Trough is evident in Fig. 3 of Janout et al. (2021)”.

RC: l. 483: “...both on the continental shelf adjacent to FRIS, where the depth of the base of the mixed layer...”: comma needed after “FRIS”.

AC: Corrected.

RC: l. 517-518: “But the question remains regarding the transfer of these now more realistic dense shelf waters offshore, to feed the globally important AABW...”: No comma is needed after “waters”.

AC: Corrected.

RC: l. 519: “...is too short to explore the impact of these changes far afield...”: Neither hyphen is needed in “too short” and “far afield”.

AC: Corrected, not sure why those hyphens are there!

*RC: l. 524: “The thermohaline and velocity **salinity** cross sections of Filchner Trough...”: This panel shows salinity, not velocity.*

AC: Corrected to simply state “thermohaline” as this covers temperature and salinity.

RC: *l. 538-541: “A cross section of the Challenger Trough (Fig. 9), reveals depth-varying thermohaline changes as opening the sub-ice shelf cavity has allowed for the water adjacent to the ice shelf to advect into the cavity leaving the bottom properties here slightly warmer, while the layer immediately above experiences cooling and salinification due to the outflow of ISW driven by the ice pump (Fig. 9c).” This is a lot to try to follow in one sentence. Maybe break it up?*

AC: This has now been split into 3 sentences: “A cross section of the Challenger Trough (Fig. 9), reveals depth-varying thermohaline changes. Opening the sub-ice shelf cavity has allowed for the water adjacent to the ice shelf to advect into the cavity leaving the bottom properties here slightly warmer. The layer immediately above conversely experiences cooling and salinification due to the outflow of ISW driven by the ‘ice pump’ (Fig. 9c).”

RC: *l. 619: “...compare the model simulations with local in situ observations...”: “local” and “in situ” mean the same thing in this context.*

AC: “Local” has been removed.

RC: *l. 626-627: “Antarctic ice shelves and, while they are responsible for the formation of the majority of the parent waters of AABW, interactions with...”: comma needed before “while”.*

AC: Corrected.

RC: *l. 627-628 "...interactions with remote unresolved ice shelves* ***are*** *missing..."*

AC: Corrected.

RC: *All your references: You seem to have the publisher listed instead of the journal title. Also, one reference is in a weird typewriter font (but I guess the typesetter will fix that).*

AC: Thank you for spotting this, it was an artifact from the referencing software. This has been corrected. As for the strange typewriter front, not sure how this happened, it is only in the pdf, not in the original word document.

RC: *Figs. S3 and S4: I found it a little confusing that you used the same cmocean colormap for percentage and m/yr of ice production that is rightly used for depth in Fig. S2. Maybe use different colormaps for these other fields? "amp" or "matter" could work for a percentage, my group has used "dense" for sea-ice production but "ice" might also be appropriate.*

AC: These figures have been amended and no longer show m/yr and %.

RC: *Fig. S5: "Density" should presumably be "Density difference" and "kg/m3" should be "kg/m^3" (superscript).*

AC: Corrected.

RC: *S1: I appreciated this discussion, though I agree that it was appropriate for the supplementary information. The comparison with observations is nuanced enough to be a little messy and the conclusion is that Open and Closed both have large biases, with neither clearly more realistic. So it does not refute, but neither does it really support, the main conclusions of the paper.*

AC: Thank you for raising this. The sea-ice supplementary section has been revised and re-focused. It is now Supplementary Section S3 ("An evaluation of sea ice production and polynya activity in the NEMO simulations"). We believe that the revised version is now better aligned with the main text.

RC: *l. 1002-1003: "It is hard to conclude on more or less realistic polynya activity in the Open simulation." I would suggest rewording this as "It is hard to conclude whether the polynya activity in the Open simulation is more or less realistic than in Closed".*

AC: This sentence no longer exists in the new supplementary material.

RC: ***References***

Williams, G., Herraiz-Borreguero, L., Roquet, F. et al. Tƒ Nat Commun 7, 12577 (2016). https://doi.org/10.1038/ncomms12577

AC: This reference has been added, thank you.

Responses to review comments 2:

AC: The authors would like to thank anonymous Reviewer 2 for the time taken to comment on our manuscript and provide the helpful suggestions listed below. We have addressed each comment and where we have been unable to implement the recommended amendments, we have provided an explanation as to why. The revised manuscript is much improved, especially the supplementary section which has been rewritten following the feedback provided from both reviewers. The review comments are listed here in italics and preceded by “RC” and the authors’ response in regular font preceded by “AC”.

RC: The study “Improving Antarctic Bottom Water precursors in NEMO for climate applications” by Katherine Hutchinson and colleagues is an exciting demonstration on feasibility and importance of including ice shelf–ocean interaction in coarse resolution (1˚), global ocean and climate models. Over the past two decades a growing body of literature has documented the processes of ice melt in ice shelf cavities around Antarctica, their implications on water mass transformation and modelling approaches thereof. It is an ongoing debate, how this is best implemented in coarse resolution models used for multi-centennial climate simulations. Hutchinson et al. show that even on a 1˚ ocean model grid one can include and simulate the three largest Antarctic ice-shelf cavities interactively using complex physic.

The manuscript is well written and composed, the arguments supported by mostly clear and matching figure. It would be desirable though if the authors could provide a little more background in the introduction on modelling approaches for ice shelf melt and ice cavity circulation, reference early studies such as Hellmer (2004, doi:10.1029/2004GL019506) and other approaches like the PICO model (Reese et al., 2018, doi:10.5194/tc-12-1969-2018), just to name a couple of examples. By the way, Hellmer 2004 is listed as reference but not cited in the main text. Introduction and discussion are sufficient to introduce the subject and to validate the results but do not really reach beyond. The authors make a great effort to properly validate their simulations, clearly state weaknesses of their model (and its strengths) but also where observations are sparse and thus differences may not necessarily mean a model bias.

AC: Thank you for bringing it to our attention that more context regarding approaches to modeling ice shelf melt could be useful. The authors decided to not do an extensive literature review on all the other models simulating sub ice shelf cavity circulation because it is a study in and of itself and to do so comprehensively would be its own review article. Instead we present a summary of all models using NEMO. To satisfy this review comment we have added the following to the introduction:

“The simulations with a fixed freshwater flux parameterization at depth perform well in terms of mimicking the vertical overturning and associated entrainment of ice shelf melt, but do not allow for interactive ice-ocean exchange that evolves with ocean properties. Parameterizations of ice shelf melt using far field temperature (outside of the cavities) exist and an extensive comparison was undertaken in Burgard et al. (2022). Here they found that none of the available parameterizations yield a negligible error, and so parameterizing basal melt still remains a challenge. Furthermore, these parameterizations do not solve the need to allow for circulation underneath the ice shelves in order to produce the horizontal variability observed on the continental shelf. For this, it is necessary to open the sub-ice shelf cavities in the simulation (Mathiot et al., 2017; Storkey et al., 2018; Comeau et al., 2022) ”

Please note Hellmer et al., (2004) is listed in Table 1 summarizing the net melt rates for various ice shelves. That is why the reference is in the bibliography even though it is not directly cited in the main text.

RC: The authors provide bathymetry and initial conditions used for the ice shelf cavities along with some simulation output, which is very much appreciated. Information missing but likely of interest to the community would be the additional computational effort caused by including, “opening” the cavities.

AC: Thank you for pointing this out, yes this information is useful. We have added to following line to the methodology: “In terms of computing cost, the “Open” cavity configuration costs 11% more than the “Closed” cavity simulation (mostly due to addition of cells as the model grid is extended further south; only 0.3% of this is associated with the cost of the ice shelf routines themselves)”.

RC: While the manuscript overall is strong and certainly merits publication with GMD being a very suitable journal, I see three weak spots that could be revised prior to publication:

1) Arguments are typically well made and supported throughout the text. Only towards the end of subsection 4.4 and in subsection 4.5 the text makes the impression that the authors want to address the given topics but the text is rather short, referring to several supplementary figures (e.g., lines 494-501) without really being able to pinpoint the interaction with open ocean deep convection and sea ice and the role of the ASC.

AC:

We appreciate the reviewer raising this as the sea ice supplementary material was added shortly before submission and, indeed, improvements can be made. The sea ice supplement went into a level of detail and was not needed for this study and so the validation against observations has been removed and will be the focus of a separate study. Supplementary Section S3 (“An evaluation of sea ice production and polynya activity in the NEMO simulations”) has been revised and re-focused, and the following text has been added to the end of subsection 4.4 to summarize the relevant sea ice production findings:

“ High ice production is seen on the southwest continental shelves of the Weddell and Ross Seas in Supplementary Figs. S2a and S2b. Opening the cavities slightly reduces the magnitude of ice production in the Ronne Depression (Fig. S2c) and at the location of the Terra Nova Bay Polynya (Fig. S2d) and increases the production of ice further east. There is no overall change in the principal location of polynya activity and the slight west/east decrease/increase in sea ice is presumed to have a negligible effect on the total amount of HSSW generated. As such, the reduction of the highly saline HSSW signature seen in Figs. 2g and 3g when cavities are opened is likely due to a conversion to ISW (and not from a decrease in HSSW production itself). Please see the Supplementary Information Sect. 3 for an evaluation of simulated polynyas near the studied ice shelves and a diagnosis on the effect of opening the cavities on ice production.”

RC: *A particular issue is that in the “Open” case one can see an offshore displacement of the deep convection region (Fig. S2e) but not really an overall weakening/shoaling, which seems to contradict studies simulating ice-shelf melt impacts by freshwater release (Beadling et al., 2022, doi:10.1029/2021JC017608; Bronselaer et al., 2018, doi:10.1038/s41586-018-0712-z). The authors show that the ASC is weak in their simulation, which would rather facilitate a freshening of the interior Weddell Gyre and hence decrease MLD. Please discuss.*

AC: The MLD figure which was in Supplementary material has been moved into the main text and is now Fig. 8. The following paragraph has been added to the end of Section 4:

“Once a model is able to explicitly form the parent waters of AABW in the right locations on the continental shelf (and export this dense water), it will become necessary for modelers to tone-down open ocean deep convection as this workaround will be longer relied upon to form the totality of AABW. Here we explore the impact that opening the cavities has on MLD to diagnose the extent of vertical convection in the model. Some reduction in MLD is seen on the continental shelf and slope in the Filchner (Fig. 8e) and Challenger Troughs (Fig. 8f) due to the increase in stratification as a result of the greater bottom densities associated with outflowing ISW (Fig. S4a and S4c). The presence of ISW appears to promote slightly increased ice production in these areas, as discussed earlier. In this case, it is therefore the ocean properties that drive sea ice and the brine rejection associated with elevated ice production is found to have a minor effect on water properties. Within the region of exaggerated MLDs off the Weddell continental slope, the MLDs deepen in the “Open” cavity experiment (positive anomalies Figs. 8e). We hypothesise that this deepening is associated with an overall cooling of the subsurface layers due to a horizontal mixing of ISW, unimpeded by a relatively weak and diffuse Antarctic Slope Current (discussed in the following section). Overall, in wintertime, mixed layers are on average 19 m deeper over the whole Weddell Sea region in the “Open” cavity experiment compared to the reference “Closed” simulation. This reinforcement of the high MLD bias highlights the need for work to be done on reducing wintertime deep convection, together with better representing dense water overflows”

RC: *2) Section 5, the discussion mostly reads like a summary and I suggest to actually rename the section accordingly ("Summary and Discussion"). This is okay as the model results are intensively discussed and validated already in Sections 3 and 4, which reach beyond the typical presentation of results. In addition, results of other modelling approaches could be discussed, like with the UKESM (Smith et al., 2021, already cited) or the PICO model mentioned above. This would be particularly helpful as the authors use a forced ocean model but understand their study as a contribution to improving climate models as they state in the introduction.*

AC:

Done, section re-named "Summary and Discussion".

As for comparison with other model results: We actually struggled to find similar studies where the effects of opening the cavities in a global NEMO configuration were compared extensively and validated against observations. For example, while the paper of Smith et al. (2021) goes to great effort to document the technical steps undertaken to couple the ocean and ice-shelves, it does not explore impact on water masses. We do directly compare our results with Bull et al (2021) and Hausmann et al. (2019) which are respectively 1/4° and 1/12° configurations of NEMO for the Weddell Sea with open sub-ice shelf cavities. In fact Figure 5 is designed to match the aspect and colorbars of Bull et al. (2021) Figure 4 to facilitate easier comparison. As for a discussion on other coupled climate model results, we summarize the net melt rates in Table 1 but have decided that it is out of scope in this paper to embark on a detailed literature review on all forced and coupled ocean models who simulate sub-ice shelf cavity simulation. We feel this would dilute the focus of the paper.

RC: *3) The sea ice production and polynya activity analysis in the supplementary material goes to some length but without clear results. There is no significant difference in sea ice production due to opening the cavities (table S1) except the discussed spatial displacements. The latter are rather simple responses to changes in surface water mass properties. The authors should be careful not to contradict arguments made in the main text and overinterpret the role of sea ice. This seems to be the case in the summary section S1.6, where in particular references to figures of bottom(!) salinity (Figs. 2i, 3i) are more confusing than supportive.*

AC: Thank you for raising this. The sea ice supplementary material has been completely revised and is now better aligned with the main text. Please see Supplementary Section 3.

RC: *The list of references needs to be properly checked for consistency.*

AC: The references have been checked again, thank you for flagging this.

RC: *While extensive, all comments are rather minor and should be addressible without requiring an additional round of review.*

AC: Thank you for the time taken to provide this valuable feedback.

RC:

Minor comments by line:

Line 1, the title: I wish "by opening ice shelf cavities" would somehow be included. The formulation "AABW precursors" appears kind of indirect and less appealing to me -- though less technical, I admit.

AC: This was debated amongst the authors before initial submission and as only the 3 sub-ice shelf cavities thought to play major roles in the formation of AABW parent waters were opened, we wanted to state this clearly in the title. "By opening ice shelf cavities" could be misleading by insinuating that all cavities are open, which they are not.

RC: *l.33: add references like Fröhlicher et al. (2015, doi:10.1175/JCLI-D-14-00117.1) and maybe Bourgeois et al. (2022, doi:10.1038/s41467-022-27979-5) and*

AC: Frölicher et al. (2015) is found to be relevant and has been added. Thank you for the recommendation.

RC: *l.35-38: introductions of both HSSW and ISW could use a reference each where the interested reader could find more detail on formation processes*

AC: Indeed, references to Jenkins (1991) and Jacobs et al. (1979) have been added.

RC: *l.175-177 details of sea ice advection scheme etc. is provided here. Please provide similar information on ocean model in prev. paragraph. Focus is the ocean model.*

AC: A section has been added in supplementary material (S1) explaining the ocean model namelists with reference to the NEMO manual.

RC: *l.187, 199, etc: term "cold-core ice shelves" or "cold ice shelves" needs introduction or at least a reference; maybe in addition refer to "dense shelf" as defined in Thompson et al. (2018, doi:10.1029/2018RG000624)*

AC: This expression has been amended to 'cold water' and 'cold cavities' to be clearer and in line with existing nomenclature.

RC: *Section 2.4 The entire approach of attaining the initial conditions is wonderful, very thoughtful and a great example. I would be curious though, how much the model state still* *drifts in the ice shelf cavities after starting the main 100+ year long run. Could be added to the supplementary.*

AC: Indeed, it would be useful to quantify whether the model retains information from initialization after a few decades. Unfortunately, performing such an assessment is not possible with the present 124 year run as we force it with 2 consecutive cycles of CORE atmospheric conditions that include interannual and longer-term variability (related to climate change). In order to have access to model drift, we should run a long simulation forced by climatological present-day conditions, which we have not done so far. This is work that is being considered for future investigation.

RC: *l.241 and 251: Figure S3 is addressed before S2. Switch figures in supplementary.*

AC: Figure S2 has been moved into the main text and is now Figure 8, Figure S3 has now become S2.

RC: *l.303: add "ice shelf" in "The average ice shelf melt rate pattern …"*

AC: Added.

RC: *l.313: careful with the resolution statement. I assume the earlier studies use NEMO3.x and potentially also different settings complicating a direct comparison.*

AC: Correct, both these studies use older versions of NEMO. The impact on model results delivered by improvements in functionality in NEMO are, however, minor compared to the impact of moving from a 1° to a 1/12° resolution. Nevertheless, the statement only mentions "possible impact" and highlights the fact that these are regional configurations thereby explaining that a direct comparison is, of course, not possible. We are simply stating that of the other studies mentioned in the table, as these are both based on NEMO, they are the most relevant to compare eORCA1 with (within reason).

RC: *Table 1: this is an awesome overview, excellent!*

AC: Thank you!

RC: *l.325f add information on figure number in Rignot et al and Haussmann et al.*

AC: This has been added, thank you.

RC*: l.348 & 370: drop subsection header; subsections consist of 1 paragraph each only.*

AC: Okay, they have been removed.

RC: *l.362 remind the reader here that eORCA1, i.e. 1 degree, in fact means 22km actual resolution*

AC: This has been edited to read: "It is therefore encouraging that eORCA1 (with an effective horizontal resolution under FRIS of 22 km) captures these, as they could play an important role in the evolution of shelf circulation in future climate scenarios (Naughten et al., 2021)."

RC: *l.403f & 416f: please provide an average difference estimate for "fresher, slightly cooler" and "cooler fresher values"*

AC: The following has been updated: "A change in signature of AABW can, however, be seen in the volumetric T-S plot (Supplementary Material Fig. S3a), where explicit ocean-ice shelf interaction results in a small shift in volume towards cooler fresher AABW (Open - Closed weighted average shift in AABW volume by -0.008 °C and -0.003 psu)."

And: "The volumetric T-S plot for the Ross Sea found in Supplementary Material (Fig. S3b) indicates that opening the RIS cavity has moved the core of AABW towards slightly cooler fresher values, accompanied by a 0.34 % decrease in volume of AABW as defined by the original water mass limits (delineated in green in Fig. S3b; Open - Closed weighted average shift in AABW volume by -0.001 °C and -0.005 psu)."

AC: *l.437: MCDW, abbreviation is not introduced*

AC: Corrected

RC: *l.498: Figures 8 and 9 are only properly introduced in line 525; referring to their panels (d) is more confusing than helpful here*

AC: This paragraph has been re-written and reference to Figures 8 and 9 is now later in the next, after they have been introduced.

RC: *l.528, "little indication of a coherent cascading": cascading is a rapid process, is it possible that the time mean over 10 years masks such process?*

AC: While in other places the downslope flow of dense water is intermittent and extremely difficult to observe, adjacent to the large ice shelves of the Weddell and Ross Seas, the dense water overflows are considered to be large-scale, highly active and to permanently contribute to AABW formation (Ivanov et al., 2003). The cascading of dense water from the Filcher and Challenger troughs are both considered "active" cascades by Baines and Connie (1998) as they were directly observed by summertime CTD sampling (Foldvok et al., 1985; Jacobs, 1991).

To clarify this in the text, the relevant references have been added.

RC: *l.551: please also reference Colombo et al. (2020, doi:10.5194/gmd-13-3347-2020) on overlow in z-coordinates in NEMO*

AC: This reference has been added.

SUPPLEMENTARY TEXT

RC: *l.929: "An evaluation of sea ice production …" (not to confuse ice shelves and sea ice)*

AC: Corrected

RC: *l.933ff: rewrite: "Sea ice growth, melt and transport exert an influence on water mass properties. Polynyas are large areas of open water or very thin ice forced by local winds or heat from the ocean. Near the Ronne and Ross ice shelf margins, large polynyas source dense saline waters to the surface ocean during the cold season."*

AC: This section has been completely re-written..

RC: *l.948f: rewrite: "… concentration outputs, of which the 2003-2009 average in the is displayed in Figs. S3b and S4b for the "Closed" simulation."*

AC: This section has been completely re-written.

RC: *l.1018 & 1020: you refer to temperature and salinity, which are shown in panels (h) & (i) in Figures 2 and 3 (not only panel (i)).*

AC: This section has been re-written and now refers only to salinity..

FIGURES

RC: *Figure 1: switch panels (a) and (b) as Weddell Sea (currently panel b) is addressed first in the text. Also, please increase line thickness of the circulation arrows for visibility.*

AC: This has been done.

RC: *Figure 2 (and all other contour/pcolor plots): use discrete colors and fewer color levels (max. 20 or even only 10) often help to improve readability of the graphic.*

AC: The authors understand this point and have discussed it. We have used cmocean to plot all temperature and salinity plots as this package is standard and fit-for-purpose having oceanographic specific colorbars for "thermal" and "haline". Unfortunately cmocean functionality is fixed as being perceptually uniform, and so it is not possible to split the colorbars into discrete intervals. Doing so would require a change of colormap, and the authors feel that this would be a greater disservice to readability than having perceptually uniform colorbars.

For more information on the cmocean toolbox please refer to: Thyng, Kristen, et al. “True Colors of Oceanography: Guidelines for Effective and Accurate Colormap Selection.” Oceanography, vol. 29, no. 3, The Oceanography Society, Sept. 2016, pp. 9–13, doi:10.5670/oceanog.2016.66.

RC: *Further, please increase line thickness of the gray dashed line of freezing point (panels a,d,g).*

AC: This has been done.

RC: *Caption: add “of WOA and “Closed” after “…bottom temperature and salinity” in line 272. And add last “Panels (a) and (g) exclude ice shelf cavity data matching the “Closed” configuration of panel (d).” (as stated later in line 388)*

AC: Thank you for these suggestions, the caption has been amended accordingly.

RC: *Figure 4: panels should have some location (lat/lon) or distance labelling along frame.*

AC: This has been done.

RC: *Figure 5a: velocity arrows are way too small, reduce number and increase length and thickness; also red arrows on green shading is not color-blind friendly, black arrows would do*

AC: This has been done.

RC: *Figure S1, caption: mention that scatter dots are placed in T-S space according to “Closed” and coloring shows “Open”-“Closed”; also this should be Figure 3 according to the sequence in which supplementary figures are addressed in the main text*

AC: The caption has been amended accordingly.