Review on: “Southern Weddell Sea surface freshwater flux modulated by icescape and atmospheric forcing”

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R: Referee comments, in black
A: Our answers, in blue
N: Notes about changes in the revised version, in orange; referenced line numbers are from the file highlighting the changes (diff file)

We thank the two reviewers for their valuable feedback and comments which certainly help to improve our paper. In the following you can find our detailed response to the reviewers comments.

N *: In the final version, I used a version of my surname without diacritics, as it was brought to our attention that more than one diacritic in the surname seriously limits the searchability of the paper with the search engine on the EGU site. Both versions are linked to my ORCID profile. LS
N **: Minor typos and grammar mistakes were corrected throughout the text.

Response to Referee #1:

General comments:

R: The model setup and simulations are well described and the resulting ice growth and freshwater fluxes agree with observations. The figures and tables are comprehensive and the text is well written. Given the importance of coastal polynyas in this region for ice shelf basal melt and deep water formation, the study is relevant and interesting. However, I suggest some changes mainly in the structure of the paper for easier readability and for an improved highlighting of the relevance of the study.

The paper feels at times very much focused on an evaluation of the model and very packed with comparisons with previous studies and different values, which could be shortened or summarized (e.g. when the values themselves are also in a table, or considering to first provide the results from this study, then comparing with previous results f.ex. lines 220-226). Some parts could be considered to be moved to the discussion section (e.g. lines 212-216 or some of the comparisons such as lines 245-272). Instead, there could be a stronger focus on the physical results, such as the implication of the salt production on HSSW production, circulation inside the cavity/ on the continental shelf and basal melt. The impact of the polynyas on HSSW seems a very important part of this paper, especially in regard of the new studies about the Berkner and Ronne mode (Janout et al and Hattermann et al). The impact on HSSW is described in the discussion and nicely summarised in the conclusions, but in my opinion most of the discussion section is actually a result and would gain more weight by moving it to the results.

A: - We thank the reviewer for these helpful comments. We will improve readability of the section comparing different sea-ice production rates by following reviewers suggestion and presenting first the results of our study and than comparing with the previous results. We will also add a short summary of our findings at the end of the section. Furthermore, impact of polynyas on HSSW will be better introduced and emphasized as suggested by the reviewer. The separate result and discussion sections will be merged in a section “Results and discussion”, therefore the HSSW results will be presented with the equal weight like the rest of results, while still allowing the reader to follow the paper topic after topic.

N: “Results” and “Discussion” are merged into one section “Results and discussion”

Ln 50 – 55: the new studies about the sub-ice-shelf circulation modes included in Introduction (Janout et al and Hattermann et al)

Ln 79 – 86: effects of the icescape on the sea-ice production, dense water mass formation and basal melt explained

Ln 251 – 260: our results presented first (old Ln 212 –226)

Ln 285, Ln300 : Tables 2 and 3 referenced in the text

(summary paragraph at the end of 3.1.2. was already included)

Specific comments:

R: Title: It might be useful to have the term polynya in the title, as people looking for sea ice production might search for the term polynya in google scholar.

A: - We would like to keep the title as it is, given the wider surface freshwater fluxes context presented in this study. However, we will try to add “polynya” as a key word, which might help with some search engines.

R: Abstract: It could be condensed or restructured to better highlight the main results (e.g. some details such as the MODIS satellite in line 10 is enough to have in the methods, and line 5 can be merged with line 8-10..) and there could be a larger focus on the impact of the polynyas on the HSSW production (which is mentioned in the last sentence), as it currently sounds as if the last sentence is more of a general statement rather than an actually conclusion based on the model simulations and calculations done in this study.

Line 5 and 8-10: It is unclear from the abstract what an icescape is. In line 5 is seems like an alternative term for the grounded iceberg rather than the ice bridge between the ice shelf front and the grounded iceberg. Also, it is unclear to me what the difference between an icescape and an ice bridge is…

Line 15: i) treatment of the icescape – not clear what you mean with treatment; better “existance of the icescape”?

A: - We follow these suggestions and clarify accordingly. We will clarify definition of the icescape (i.e. as used for the stationary ice features such as grounded icebergs, ice shelves, and land-fast sea ice) when first mentioned in the text. “treatment of the icescape” is exchanged for “representation of the icescape” as it refers to the implementation of the icescape features in the model rather than just its “existence”

N: Ln 8-10: the term icescape as used in this study is clarified

Ln 11–13: old line 5 merged with old 8-10

Ln 10–13: MODIS details omitted

Ln 17: treatment of the icescape → representation of the icescape

Ln 24: emphasized conclusions about the impact on HSSW and basal melt

R: Introduction (Line 66-67/ line 71): Since this study has a strong focus on the icescape and the sensitivity of the freshwater fluxes on its existence, it would be easier for the reader to understand the motivation for these sensitivity experiments, if there was a description of the icescape – is it a permanent feature, does its shape change, what do we know from satellites? etc... I suggest to move (parts of) the first paragraph of 2.3 into the introduction.

A: - We included these suggestions.

N: Ln 65: added detail about the grounded iceberg

Ln 77–78: added information about the iceberg and ice-bridge formation

R: In general, the introduction could benefit from a more physical motivation of this study. There is currently a strong focus on what other studies have not managed to resolve/look into, but there is much more value in your simulations: How have icescapes/ fast ice/ icebergs influenced coastal polynyas and salt fluxes in other regions around Antarctica? Why is it important to study the impact of the icescape in the Weddell Sea in detail? The switch between the Berkner and Ronne mode for HSSW production is well described later, but it provides a great motivation for this study and could already be mentioned in the introduction.

A: - We included these suggestions.

N: Ln 50 – 55: the new studies about the sub-ice-shelf circulation modes included in Introduction (Janout et al and Hattermann et al)

Ln 77 – 86: details of the icescape and effects of the icescape on the sea-ice production, dense water mass formation and basal melt explained

Line 107: Is there a reason to use ERA-Interim instead of ERA5?

- ERA-Interim was used to accomplish consistency with the CCLM boundary condition.

N: Ln 350–352: this information added to the text

R: Lines 109: Reading the description of the sensitivity experiments the first time, it was not very clear to me that BRIDGE means an experiment with the existence of the time-varying icescape as observed from satellites (the name seems arbitrary if you otherwise call it icescape).

A: - It is clarified as the reviewer suggested.

N: Ln 134–135: the experiment BRIDGE related to the formation of ice bridge

R: Line 113: “without prescribing the blocking effect of the varying icescape” – noBRIDGE is an experiment where the ice melange between the iceberg and the ice shelf is removed, right? It would be useful to describe a bit more what is done in the noBRIDGE experiments (either here or in section 2.3).

A: - It is clarified. We add a sentence explaining noBRIDGE is an experiment where the ice melange between the iceberg and the ice shelf is removed in section 2.3.

N: Ln 167–168: this sentence added.

R: Line 116: The experiment statBRIDGE is listed in Table 1, but not mentioned here.

A: - It is added.

N: Ln 139–141: information about statBRIDGE added.

R: Line 120: “until beginning of 2022” – until present (?); it reads as if it eventually got ungrounded in 2022, which I believe was not the case?

A: - Actually, the iceberg got underground indeed (based on the iceberg locations from the Antarctic Iceberg Tracking Database, https://www.scp.byu.edu/data/iceberg/database1.html , last access March 22, 2023, or Antarctic Iceberg Data (USNIC), https://usicecenter.gov/Products/AntarcIcebergs, last access August 15, 2023). The low sea ice concentration signal associated to its movement can also be seen in the monthly sea-ice concentration products from the beginning of 2022, e.g. The Sea Ice Index (Fetterer et al, 2017). We will make a stronger point about this in the text.

N: Ln 145–147: the iceberg location an movement described in more detail.

R: Line 183: Is the 70% threshold or 20cm ice thickness a common criterion? Do you have any reference?

A: - These thresholds have been used in other studies, as the strong ocean to atmosphere heat fluxes occur both over the open ocean (sea-ice concentration threshold) and thin ice (ice thickness threshold) areas. Similar threshold for the sea-ice concentration (75%) was used in the early satellite-based study of the East Antarctica polynyas (Massom et al. 1998). 20 cm thickness threshold has been used in the satellite based studies of coastal polynyas using thin-ice thickness products (e.g. Nihashi and Ohshima, 2015a , Paul et al., 2015, Preußer et al., 2019). 70% threshold for the sea-ice concentration and 20cm threshold for the ice thickness has been used by Haid and Timmermann (2013) and Haid et al. (2015) in modeling studies of the Weddell Sea polynyas. A sentence is added clarifying that these criteria have been used in other studies.

N: Ln 211–213: A sentence with the references added.

R: Line 192: A more recent basal melt rate study is Adusumilli et al 2020, how does the pattern compare with it?

A: - Thank you for this suggestion, we added comparison with Adusumilli et al., 2020 . The mean (2002-2017) ice shelf basal melt flux from FRIS in the BRIDGE experiment was 5.2 mSv. Converted to Gt/year using ice density of 910 kg m^3 , this corresponds to 150.9 Gt/year. This result falls in the range of the satellite-based estimate from Adusumilli et al., 2020 (81.4 ± 122.9 Gt/year, 1994-2018 mean). The pattern of melting and freezing in BRIDGE is rather similar as in Adusumilli et al., 2020 , with the exception of a weaker freezing signal in the northeast Filchner cavity and stronger melt signal instead. The difference could be result of the too strong HSSW inflow in the Filchner cavity in BRIDGE. Moreover, while the melt contribution of the Filchner Ice Shelf in BRIDGE (66.8 Gta −1) is slightly higher than the range given by Adusumilli et al., 2020 (34.2 ± 29.6 Gt/year), the experiment without the representation of the ice bridge, noBRIDGE, yields results quite higher (80.3 Gta −1) than both the BRIDGE experiment and the satellite based estimate, as a response to the stronger circulation under the Filchner Ice Shelf in noBRIDGE.

N: Ln 223, 229–231, 442: Comparison with Adusumilli et al., 2020 discussed.

R: Fig 4c: Are the values and average over the whole are shown in Fig. 4A?

A: - Values and averages in Fig. 4c are shown for the Weddell Sea control area enclosed by the violet line in Fig. 4a.

R: Line 242-244: It is really nice to have these values of ice production in the different regions... How about adding a figure with the ice production for each of the regions? Either the annual production for each experiment or maybe a figure like 6a split into the different regions to also see the interannual variability.

A: - Thank you for this suggestion, we will add these figures in the supplement materials.

N: sea-ice production time-series from 6 regions is included as a supplement figure

R: Line 270: “tends to reduce polynya area and ice production”... Possibly explaining the smaller values in Paul et al.

A: - Thank you for this suggestion, we included it.

N: Ln 306: included “Possibly explaining the smaller values in Paul et al.”

R: Line 318: Fig. 3b shows that ERA is also in general warmer. How can you distinguish between the changes in ice production due to temperature vs. wind?

A: - The analysis of the heat fluxes over polynyas from the sea-ice model indicates the most significant differences between the experiments are found for the sensible heat flux, which is 10-30% stronger in noBRIDGE compared to cATMO (in locations with the strongest wind differences). Radiative heat flux does not show significant differences. We added this information in the section about atmospheric forcing.

N: Ln 354–356: the comment about the sensible heat flux being is added

R: Lines 378-380: If there is an output of velocity from the model it could be useful to add velocities/ streamlines in Fig. 7

A: - The bottom velocity field (and bottom velocity anomalies) will be included Fig. 7.

N: Fig.7. was replaced with the new figure including the bottom velocity, and caption was edited accordingly.

Ln 434– references added to the bottom velocity.

R: Line 425: This is not clear... You mean “Using a high resolution/ more realistic region atmospheric forcing …”

A: - Yes, this is now corrected.

N: Ln 479: corrected

R: Fig. 5a & b: Are only the velocities from August 2009? Why are you showing that months rather than an average?

A: - The Fig 5a and 5b show a typical case of the ice bridge fully formed as found in the monthly satellite data shown in Fig 2 c. This connection will be made clear in the text and figure caption.

N: Fig.5. caption clarifies this, as well the added text in Ln 329--330

Technical comments:

R: Line 120: hyperlink does not work with the line break.

A: - This is now corrected.

N: Ln 146 corrected

R: Line 162-163: remove “by”: ERA has 2C ... while it is 0.5C ... and 2C cooler…

A: - This is now corrected.

N: Ln 189, 190 corrected

R: Line 367: “too strong salinity” -> too high salinity

A: - This is now corrected.

N: Ln 419 corrected

R : Line 371: “is in BRIDGE is 3.2Sv” -> remove one “is”

- This is now corrected.

N: Ln 422 corrected

Line 376: reference in brackets

- This is now corrected.

N: Ln 428 corrected

Response to Referee #2

General comments:

R: This is a clearly presented assessment of the impact of the stationary ice features on sea ice production in the southern Weddell Sea region. The results provide convincing evidence that the relatively small-scale details of fast ice and grounded icebergs can impact the spatial patterns of sea ice production, bottom water export, and ice shelf melting. I believe this study has the potential to become a valuable addition to the Weddell Sea literature. However, I would like to highlight a few issues for the authors to consider and address.

- The manuscript motivates the importance of the southern Weddell Sea shelf by highlighting its outsized role in sea ice and HSSW production, which have far-reaching impacts on the global ocean overturning circulation. Though the manuscript makes a convincing argument that the details of the icescape are important for local ice production and water mass formation, it does not make a compelling case that these processes matter much away from the southern Weddell Sea region. From Figure 6a, one could argue that none of the sensitivity experiments had a meaningful impact on the total amount of sea ice leaving the region. While this is certainly a valid result worthy of publication, it is tempting to conclude that the nuances of the icescape in these high sea-ice production regions do not have an appreciable impact on the circulation and water mass properties of the Weddell Sea and the broader Southern Ocean. If the authors dispute this conclusion, I encourage them to clarify this point in their abstract and conclusion.

A: - Thank you for this suggestion. The cumulative sea-ice production results from Fig.6a show little influence of the sensitivity experiments on the total ice production (and consequently sea-ice export out of the region, not shown). However, we show that regional distribution (location of sea-ice production) and not just the amount of sea-ice production in polynyas are important for HSSW production and properties, and consequently the basal melt of the Filcnher-Ronne Ice Shelf. By additionally comparing properties of the water mass export, we will make a stronger point about the influence of the sensitivity experiments out of the region and emphasize it further in the abstract and conclusion.

N: The analysis of the water mass export was not conclusive. The export of dense shelf water out of the region seems to be dominated by the dense HSSW of Ronne origin (variability and magnitude of differences resembling that shown in Fig 8a). At the same time, the sensitivity to the icescape is most pronounced for the properties of dense water masses on the eastern part of the continental shelf (Fig8b).
While we show that properties of dense shelf water that sources Weddell Sea Deep Water and influences modified Warm Deep Water are affected by changes in the icescape, settings of our experiments (e.g. horizontal and vertical resolution, duration of the experiment) have not been constructed to follow the influence of the dense shelf water on the water masses outside of the continental shelf. Therefore, our ability to assess the effects on the large-scale Weddell Sea circulation and bottom water production is limited. Future studies that represent these processes well while also realistically representing the variability of the surface freshwater flux on the continental shelf would be beneficial to investigate this question.

R: Compared to the sensitivity analysis of the icescape, the assessment of the atmospheric forcing feels like an afterthought. While Section 3.3 highlights many differences in the experiments where the ice-ocean model is forced with ERA-interim versus a high-resolution local atmospheric model, the significance of these discrepancies is not clear. It is well-established that sea ice production within coastal polynyas is sensitive to local winds. Thus, it is not surprising the two experiments yield different sea ice production rates. The value of this comparison is further diminished by the fact ERAinterim is an outdated global reanalysis. Further, since neither experiment is rigorously compared to observations, this assessment does not provide any validation of CCLM. I do not see much value in discussing the cATMO experiment.

A: - We agree with the reviewer that sensitivity of polynya sea-ice production to local winds is a well established fact. However, beyond presenting the expected (difference in wind → different ice production), the construction of the noBRIDGE and cATMO experiments allowed us to assess the influence of the large-scale and local forcing, especially in the light of the recent study (Hatterman et al., 2021). In particular, ERA-Interim was used outside of the Weddell Sea region in the noBRIDGE experiment, and across the whole domain in the cATMO experiment to accomplish consistency with the CCLM boundary conditions and therefore exclude differences in the atmospheric large-scale influences between the two experiments. Detailed comparison between the two forcings was presented in another study (Zentek and Heinemann (2020)). Following your comment, we explained better set-up of the experiments in the introduction part and emphasized better throughout this section the significance of the comparison between cATMO and noBRIDGE for separating influence of large-scale (out of the region) and local forcing, as well as for simulation of the recently observed changes in the sub-ice-shelf circulation (Janout et al.,2021, Hatterman et al., 2021).

N: Ln 50 – 55: the new studies about the sub-ice-shelf circulation modes included in Introduction (Janout et al and Hattermann et al)

Ln 348–353: separating influence of the large scale and regional forcing explained

Ln 370–373: correcting and emphasizing the different trends
Ln 377: emphasizing effects of the large scale forcing on the recent sea-ice production variability

R: - This is a somewhat minor point, but it would appear that the use of the term "icescape" in this study differs from its common usage in the literature. Here, icescape refers to stationary ice features, such as grounded icebergs, ice shelves, and land-fast sea ice. However, in other studies, the term is used as a catch-all descriptor of all forms of ice, including snow and transient sea ice, regardless of its motion or lack thereof. For consistency and precision, I recommend that the authors clarify that their study explores the influence of stationary ice features on sea ice production.

A: - Thank you for this recommendation. We clarified definition of the icescape (i.e. as used for the stationary ice features such as grounded icebergs, ice shelves, and land-fast sea ice) when first mentioned in the text.

N: Ln 9 (Abstract), Ln82 (Introduction) clarified that we refer to the stationary features in the icescape

Detailed Comments:

R: - Lines 31-32: I generally associate basal ice shelf melting with the intrusion of modified CDW or Warm Deep Water. In some studies (e.g., Hazel and Stewart 2020), the presence of HSSW within the ice shelf cavity is described as a "cold state" that is characterized by low levels of ice shelf melting. Please clarify.

A: - As the pressure decreases the freezing point of sea water, at depths of the grounding lines (up to 1500 m below the sea level), freezing temperature can be 1 °C below the temperature of HSSW (−1.9 °C) (Nicholls et al., 2004). Therefore, while being colder than modified CDW or Warm Deep Water, HSSW still causes the basal melt due its positive thermal forcing. Currently, HSSW is blocking intrusions of modified CDW or Warm Deep Water, keeping the basal melt levels of the Filchner-Ronne relatively smaller compared to the cavities dominated by the warmer warmer masses (warm state). We add a sentence clarifying the role of the HSSW for basal melt in part of the text pointed by the reviewer (Lines 31-32).

N: Ln 35: sentence added.

R: - Lines 80-90: While referring readers to previous studies that have used FESOM is appropriate, more details need to be presented here. In particular, a description of the ice dynamics scheme and specifications along the open ocean boundaries should be provided.

A: - Regarding the latter, FESOM is a global ocean general circulation model. Regarding the former, we specified our choices of the ice dynamics schemes. The following information is added: the elastic-viscous-plastic rheology (EVP; Hunke & Dukowicz, 1997) is used for computation of ice (and snow) drift, while sea surface tilt force is computed as a function of the sea surface height from the ocean model.

N: Ln 119–120: details of the ice dynamics scheme added

R: - Line 87: How does using a time-varying S_ref impact freshwater conservation?

A: - The choice for the treatment of a virtual salt flux in a sea ice-ocean model is between a global, constant reference salinity and using the local (simulated, i.e. time-varying) salinity at each of the grid nodes in question. While a global, constant reference salinity enables a global conservation of freshwater, using the local salinity leads to a more accurate conversion from freshwater flux to salinity change. The extreme case that helps to illustrate the problem is the assumption of a very small or zero salinity at any given surface grid point, which conceptually should not be affected by adding more freshwater, while adding freshwater to a very saline environment is bound to entail strong changes in salinity. Given that this is a study of regional rather than global processes and that the timescales addressed do not exceed a few decades, we chose to prioritize local accuracy over global conservation.

R: - Line 93: Please specify that "A" represents the sea ice area.

A: - We have specified.

N: Ln 108–109 : this was already defined ("percentage of the grid covered by ice (A)")

R: - Line 100: Please comment on how well this horizontal grid spacing represents mesoscale eddies.

A: - With a resolution of between 3-12 km in the region of interest, the model is clearly not eddy-resolving, but circulation snapshots indicate that it is eddy-permitting.

N: Ln 122–123 : this comment added.

R: - Line 107: Why not use ERA5?

A: - ERA-Interim was used to accomplish consistency with the CCLM boundary conditions.

N: Ln 350–352: this information added to the text

R: - Line 110: At this point in the paper, it is not apparent why these experiments are referred to as BRIDGE and noBRIDGE. Perhaps add a sentence clarifying the names are in reference to an ice bridge that will be described later.

A: - We added a sentence clarifying the names of the experiments as suggested by the reviewer.

N: Ln 134–135: the experiment names related to the formation of ice bridge

R: - Line 113: Please clarify what is meant by "blocking effect." Blocking has various meanings in oceanic and atmospheric literature.

A: - We clarified that "blocking effect" in this case means blocking the movement of ice.

N: Ln 136: blocking effect clarified

R: - Line 132: I suggest the authors delete "best possible" or provide evidence that you have rigorously optimized your methodology.

A: - “best possible” has be omitted.

N: Ln158: “best possible” omitted

R: - Line 138: How can one distinguish between warm surface temperatures caused by surface melting versus ice thinning? It seems like this approach may misclassify a region with substantial snowmelt over thick sea ice.

A: - While, in general, it is difficult to differentiate between surface melting and thinning, this is not an issue in this study since all investigations of the satellite ice-temperature data take place during Antarctic winter time when surface melt can be neglected.

R: - Line 188: Or, more precisely, "where the vertical entrainment of Warm Deep Water into the surface mixed layer drives basal melting."

A: - Thank you for this suggestion, we rewrite the sentence including the suggested more precise explanation.

N: Ln 218–219: this suggestion added

R: - Line 208: Here and elsewhere, I suggest explicitly defining non-SI units when they are first introduced (e.g., mSv and Gt/a).

A: - This suggestion is included.

N: Ln 240: this suggestion added

R: - Line 218: Delete a^{-1} or "annually."

A: - This suggestion is included.

N: Ln 251,253: deleted “annual”/”annually”

R: - Line 219: Same issue as above. "With the annual net sea-ice export of 1041 km3 a−1..." Using a^{-1} or "annually" is redundant.

A: - This suggestion is included.

N: Ln 251,253: deleted “annual”/”annually”

R:- Line 236-238: "substantially higher ice-growth rates..." This sentence is hard to follow. Please re-write for clarity.

A: - It has been rewritten.

N: Ln 271–274: sentence rewritten

R: -Lines 245-260: It would be helpful to reference Table 2 somewhere in this paragraph.

A: - It is referenced.

N: Ln 285, Ln300 : Tables 2 and 3 referenced in the text

R: -Line 276: "where" should be "were."

A: - It is corrected.

N: Ln 312 corrected.

R: Line 290-291: In addition to the triangle icon showing the grounded iceberg, it would be helpful to add contours or shading outlining the time-averaged area where the ice velocities are modified.

A: - Thank you for this suggestion, it is added.

N: Mean fast-ice area contours are added to Fig. 5 a and c, and figure caption modified accordingly

R: - Line 293: I don't fully understand the ice thickness and production anomaly maps in Figure 5b,d. From ice velocities, one would expect less ice accumulation upstream (to the west) of the ice bridge when the blocking effect is removed. However, Figure 5b shows the opposite pattern. Perhaps related is the fact that the ice velocities in Figure 5a are almost 90 degrees to the right of the surface winds in Figure 3c.

A: - In the presence of the ice bridge, polynyas are formed west of it, the ice bridge blocks movement of ice into this area, and causes accumulation of ice east of the ice bridge. It is not clear why would less ice be expected west of the ice bridge area when the blocking effect is removed. However, note that sea-ice production shown in Fig. c,d,e,f is due to the thermodynamic ice growth only.

R: - Line 293: Also, why do Figures 5a and 5b only show values from August 2009? What is unique about this period?

A: - The Figures 5a and 5b show a typical case of the ice bridge fully formed as found in the monthly satellite data from Fig.2c. This connection is now made clearer in the text and figure caption.

N: Fig.5. caption clarifies this, as well the added text in Ln 329–330

R: Line 319: Please clarify what the 19% refers to.

A: - It is clarified.

N: Ln 350: clarified that it is “19% of the total ice production”

R: Line 320: It is not apparent that these ice production values are statistically different.

A: - It is clarified.

N: Ln 367: it is clarified that total ice-production estimates are rather similar

R: Line 325: A few words appear to be missing. "For 2002-2017, we find polynya ice production [to be]…"

A: - It is corrected.

N: Ln 366: corrected

R: Line 330: Are these trends for the 2002-2017 period?

A: - Thank you for this question, it helped to notice and correct the small error in values of the polynya-based trends 2002-2017. The correct is: Small decreasing trends are found for the polynya-based ice production in noBRIDGE (-1.4 km^3 a −1) and cATMO (-3.3 km^3 a −1). We have as well added an estimate for 1992-2017 trend from the long ERA run (cATMO) to compare with the recent publication (Zhou et al., 2023).

N: Ln 366: trend numbers corrected (Ln 372–373) and multidecadal trend discussed (Ln 378–386)

R: Line 361: Please specify the density range used to define HSSW.

A: - It is now specified also at this point.

N: Ln 415: range for HSSW specified

R: Line 384: It would also be helpful to show the circulation anomalies of HSSW.

A: - The bottom velocity field (and bottom velocity anomalies) will be included Fig. 7.

N: Fig.7. was replaced with the new figure including the bottom velocity, and caption was edited accordingly.

Ln 434– references added to the bottom velocity.

R: Line 387: Please see my earlier comment about the heat supply associated with HSSW.

A: - We add a brief explanation connected to our earlier answer on the HSSW heat supply.

Ln 35: HSSW thermal forcing of basal melt explained

R: Line 400: From the results presented thus far, it is not evident that the density variations in the southern Filchner cavity reflect an increase in the volume flux of Ronne-sourced waters or a change in the property of waters produced at Berkner Bank or something else entirely. It might be helpful to show the shelf cavity circulation in an earlier figure.

A: - The bottom velocity field (and bottom velocity anomalies) will be included Fig. 7.

N: Fig.7. was replaced with the new figure including the bottom velocity, and caption was edited accordingly.

Ln 434– references added to the bottom velocity.

Figures and tables:

R: - Figure 4a: please note in the caption that the colormap does not linearly scale with the numerical data.

A: - It is noted.

N: added in the caption

- Figure 5: (a,b) Why only show values from August 2009 here? (b, d, and f): Note in the caption that the colormap does not linearly scale with the numerical data.

- The Fig 5a and 5b show a typical case of the ice bridge fully formed as found in the monthly satellite data shown in Fig 2 c. This connection will be made clearer in the text and figure caption. We added the note in the caption regarding the colormap.

N: Fig.5. caption clarifies the reasons, as well the added text in Ln 329–330; comment about colormap added in the caption

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References:

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