Response to the comments from Reviewer #2

(NB *italicized text in box* is comments from the reviewer.)

GENERAL COMMENTS

The manuscript fits within the scope of the journal. By bringing a long-term perspective (70 years) based on a new bathymetric compilation that is considerably more realistic than in earlier modeling studies, it represents substantial progress beyond our current scientific understanding of this region. The purpose of the work is clearly articulated in the text, I did not find substantial flaws in the methodology, and the conclusions of the study are adequately supported by the figures/tables of the manuscript. The results of the manuscript are appropriately discussed in the context of the existing literature and the authors abundantly cite other related work. Overall, this is a substantial contribution providing considerable detail on the melt of ice shelves in a region storing the equivalent of 3.5m of global sea level, which is obviously relevant to the large fraction of the population living along the world's coastlines.

SPECIFIC COMMENTS : MAJOR

(1) *The one major flaw that I'm noticing is that the authors apparently made no effort in limiting the number of figures and tables (24 figures, 3 tables, which is something I've never seen before). So the manuscript fails the journal's requirement of "scientific results and conclusions presented in a clear, *concise*, and well-structured way". This being said, if the authors are reasonable, it should be easy to address this problem by following some, or all, of the following suggestions:*

(a) *This journal allows for a "Supplement", and I would encourage the authors to use it extensively. While the new bathymetric compilation is a major outcome of the study, I would think that Figure 2 represents more information than the average reader is interested in seeing, and so it could be moved to "Supplement". Figure 4 is mostly methodological in nature and could be moved to "Supplement". Figures 5-6 demonstrate the realism of the model but they are not necessary to support the scientific results (and therefore Figs.5-6 could go to "Supplement"). The timeseries of Fig.9 look so much like a sinusoid that one could say that they are already appropriately described by the panels inside Fig.8 (making Fig.9 a bit redundant). Figure 11 is barely discussed in the text (most of the text about Fig.11 is spent describing the Methodology behind it) and could be moved to "Supplement" as far as I'm concerned. Figures 19-22 appear (to me) to describe the same story in different ways, with Figs.19-20 being only very briefly mentioned in the text; given that, it's hard to believe that all of Figs.19-22 deserve to appear in the body of the manuscript.*

(b) *Couldn't Tables 2-3 be combined together? This would facilitate a comparison between this study (Table 2) and the earlier studies (Table 3).*

(c) *Another possibility is to use "(not shown)" when a result is interesting enough to be mentioned but it isn't critical to the scientific demonstration. For example, I felt like the 8 year cycle apparent in Fig.11(a) was a bit interesting and worth briefly mentioning in the text, but I didn't feel the need to see it as one of the manuscript's figures.*

(General comments)

Thank you very much for your careful reading of our manuscript and your constructive comments. We are pleased to hear that you find our work interesting, and that you recommend publication in TC. Your feedback is invaluable for improving the quality of our paper, and we are committed to addressing your comments and suggestions.

(1-a) (1-c) Following your suggestion, we will shorten the manuscript by moving several figures to Supplement. Figures 4, 5, 6, 9, 11, and 22 in the previous manuscript will be moved to Supplement, and Figure 2 will be moved to the Appendix. In the following response, we use new figure numbers in the revised manuscript.

(1-b) Following your suggestion, we will merge the tables.

SPECIFIC COMMENTS : MINOR

(2) *The journal requires the "experiments and calculations" to be "sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)". Based on the authors' "Data availability" statements, they plan to share the model results supporting their conclusions "after acceptance". So I wasn't able to access the model results, but the authors seem to be willing to share them, eventually. The editor can decide whether this is good enough (or not).*

(3) *Another requirement of the journal is that the language be "fluent and precise". Overall, I thought the whole manuscript was well-written. My main concern is that some portions of the text felt unecessarily long. For example, lines 45-75 are fairly general and would fit better in an introductory paper (or a graduate student's thesis) than in a research paper where conciseness is key. Another example is line 591-616, which acts as a repetition of what the reader already saw inside Sections 3-6. I personally don't see the need for lines 591-616; the Abstract already provide a summary of the key results.*

(4) *Line 52: "...and iceberg caving at ice-shelf fronts..." (Typo: "caving")*

(5) *Line 89: "Previous observational and modeling studies have inferred that the interannual variability in glacier/ice sheet variable is strongly controlled..."*

Something is wrong in the sentence; maybe delete the word "variable"?

(6) *Line 127: "...heavily influenced by their artificial lateral boundaries, the conditions which were often derived from a different coarse-resolution ocean model."*

The sentence could be interpreted by a reader as "all regional models are flawed by having artificial boundaries", which would be inaccurate. (The literature is filled with successful regional model implementations using highly-realistic lateral conditions.) Removing the word "artificial" from the sentence would make it a lot better; it gives more weight to the second part of the sentence and ties the criticism to something specific and quantifiable (the coarseness of the oceanic dataset used for the lateral conditions).

(2) We have uploaded data to a data repository (Mendeley Data). Please find the link in the revised manuscript.

(3) Thank you for your thoughtful suggestions regarding the length and structure of the “Introduction” and “Summary and Discussion” sections. We deeply considered your advice. However, we have decided to retain the sections as they are for the following reasons. Given that The Cryosphere encompasses a broad spectrum of topics within the cryosphere, our intention in the Introduction is to navigate a diverse readership from general concepts to the specific topics of our study. As for the “Summary and Discussion” section, it serves to provide more detailed correspondence with the figures presented in the paper, which we feel is not fully covered by the Abstract alone.

(4), (5), and **(6)** We will correct them.

(7) *Section 3 (Sea-ice extent and production): I could not find where the "observed" "sea ice concentration and extent" were from. What datasets were used? SSMI? Something else? This needs to be specified.*

(8) *Line 393: "The upper panels in Fig.13 show the net inflow volume transports..."*

The word "net" is doing more harm than good in this sentence. "Net" is often used to represent "inflow minus outflow", while in this case the authors are really referring to the inflow. I assume you added "net" to refer to the fact that the inflow was summed across the ice shelf front, but I believe it is creating more confusion than anything. Could you take out the word "net"?

(9) *Line 514-516: "We calculated the transports of the surface current by integrating the westward transport in density layers lighter than 27.7 kg/m3 and that of the undercurrent by integrating the eastward transport in density layers denser than 27.6 kg/m-3."*

Why would you have such an overlap in your definition of the surface/undercurrent? As things stand, the density range 27.6-27.7 kg/m3 is accounted in both the surface and the undercurrent. Is it because these two currents reach their maximum magnitude at different times in the seasonal cycle, and the overlap was necessary to capture the full magnitude of the surface/undercurrents at those moments? Please clarify.

(10) *Caption of Figures 7, 15: In an ideal world, the reader would be able to look at the figures and understand them without having to read the full manuscript. In the caption, could you replace the acronym "SD box" by "Sabrina Depression (SD) box"?*

(11) *Caption of Fig.10: Please define acronym "CKDRF" in the caption; this particular acronym only shows up a few times in the whole manuscript, so we shouldn't expect the reader to know or remember what it stands for.*

(7) We will add the following sentence in the sea-ice section.

L247–249

Figure S2 shows the observed and modeled seasonal cycle of regional sea-ice concentration and extent from 108°E to 128°E. We used observed sea-ice concentration derived from satellite passive microwave data using the NASA team algorithm (Cavalieri et al., 1984; Swift and Cavalieri, 1985).

(8) This is correct with “net”. As you correctly understood, we calculated the difference between the inflow and outflow transport in each bin, and accumulated it when the net inflow is positive.

(9) Based on the vertical structure of the east-westward velocity observed in Figure 15 (Fig. 21 in the old manuscript), we identified the density layer of 27.6–27.7 kg m^{-3} as a transition zone. This is because the upper portion of the undercurrent can also be observed within this transition zone. Since the direction of the velocity has been accounted for in our calculations, this overlap does not introduce any errors in our calculations for volume and heat transports. We will add the sentence to clarify the transition layer.

L491–494

We calculated the transports of the surface current by integrating the westward transport in density layers lighter than 27.7 kg m^{-3} and that of the undercurrent by integrating the eastward transport in density layers denser than 27.6 kg m^{-3}. We treat the density layer in 27.6–27.7 kg m^{-3} as a transition layer between the ASF and the seasonally-formed undercurrent. The surface current transport becomes …

(10) and (11) Following your suggestion, we will modify them.

(12) *Figure 10: As the authors point out in their manuscript, this is one of the first studies to provide a multi-decadal (70 years) perspective on glacial ice loss, so there's an opportunity to provide new insight to the community. If we assume the 69-72.6 Gt/yr grounding line flux of Rignot et al. 2019 is valid over the period 1979-2017, and that the modeled basal melt of Fig.10 is accurate, then what fraction of the grounding line flux is due to calving? How does this fraction compare to the one reported in Fig.1 of Rignot et al. 2013 (although the time periods are different)?*

Similarly, if we assume that the calving is constant over time, how does the trend apparent in Fig.10a over 2008-2018 compare with the trends in Miles et al. 2022 over the same period?

(13) *Caption of Figure 12: Something is wrong in the sentence: "...velocity along a 5-km off along a line section of...".*

(14) *Caption of Figures 12, 17: Specify the units for potential density anomaly (kg/m3).*

(15) *Caption of Figure 13: See my earlier comment about "net".*

(16) *Caption of Figure 14: Clarify that the stream function is computed from laterally-integrated velocities, clarify that the units for the stream fucntion are mSv.*

(17) *Caption of Figure 24: Mention what geographical area the "wind speed" is representative of (presumably the yellow box of Figure 3, but this needs to be clarified).*

(12) We consider that estimating the calving volume/fraction is beyond the scope of this study, as it requires consideration of the dynamical processes of ice sheets and ice shelves. Following you suggestion, we will add comments about comparison with recent oceanographical and ice-sheet studies.

L566–576

Importantly, the model has a long-term integration spanning more than 70 years making the output suitable for examining seasonal, interannual to decadal variability. While a direct comparison between our model's TIS basal melting (Fig. 5a) and observational data is challenging, we find our model's interannual variability to be consistent with the observed variability and trends, including the temperature rise over the continental shelf from 2015-2022 (Rintoul et al., 2016; Hirano et al., 2023), the decrease in ice discharge after 2010 within the 2008–2018 observation period (Miles et al., 2022), and the long-term TG acceleration since the 1960s (Li et al., 2023). The model is subject to constraints and uncertainties, including assumptions such as a stable ice shelf shape, sub-ice shelf topography, and the ice-ocean parameterization. Additionally, uncertainties related to factors like the time lag in ice sheet/shelf response to ocean forcing through basal melting make direct comparisons across different variables more challenging. However, within these limitations, our findings do not contradict existing observations and provide new insights into interannual-to-decadal variability.

(13) We will correct it.

caption in Fig. 6

Vertical profiles of (a) potential temperature, (b) salinity, and (c) velocity along a line section located 5 km off the Sabrina Coast.

(14) and (17) We have modified the captions.

(15) Please refer to the reply to (8).

(16) We will add the explanation in the caption. ("The stream function is determined through the lateral integration of volume transport along consistent distances from the ice shelf within equivalent density layers. This integration process accumulates values from denser to lighter water masses.")