

Reviewer 1:

We thank the reviewer for their detailed consideration of our submission and for making useful suggestions which we have now incorporated in the updated manuscript.

Major comment:

Reviewer comment	Author's response	Changes to manuscript
The title appears broadly misleading judging by the content of results. Heat and freshwater changes are not estimated, so their contribution to steric height changes is discussed only in a speculative way. This is fine, but the title should reflect core results, not discussion.	We agree with this.	The title has been changed.
A more quantitative estimate of errors would be useful. You are merging information from two very different products. Altimetry has a better spatial resolution; GRACE has some problem of "leakage" (contamination by continental surfaces); temporal resolution between the two products might also differ. What would be the combined uncertainty on steric height estimates and is this expected to be sufficient to capture different scales of variability?	We agree with this in theory, however, the uncertainty on GRACE in this area is poorly understood. Global and regional estimates of GRACE uncertainty are available in the literature but it would not be appropriate to use those here, as it is thought to vary dramatically depending on bathymetry, proximity to land, presence of ice shelves and short-term barotropic mass movement.	We have added a section (Section 4.2) in the discussion which addresses the uncertainty in more detail. We have also added a paragraph (beginning Line 209) discussing the margin of error in our Kerguelen Island validation and comparing against the seasonal and interannual variability. We conclude that a comprehensive calculation of uncertainty is out of scope for this study.
There is a problem with the smoothing applied on temporal curves in Fig. 4, 6 and 7. Judging by eye, the rolling mean is not centered, so it appears lagged by half the smoothing window (i.e. 6 months). This need to be checked, and I apologize if my eye is inaccurate.	You're correct.	This has been corrected in Figures 3, 6 and 7 (Figure 4 has been removed).
The potential of the presented dataset appears under-used in the study. Why not add a map of steric height anomaly RMS? EOF of steric height changes would also be useful. A regression of steric height changes to SAM and SOI would help demonstrate a potential statistical link, together with some quantification of significance. At the	Thank you for these suggestions. We do not show regressions of the steric height against the climate indices as the composite plots are more illustrative of the response.	We have added a section (Section 3.2) in the results showing the variation and results of an EOF analysis. We discuss these further in the Section 4 and tie into the SAM and SOI analysis and the existing discussion.

moment, we struggle to see any clear novelty in the analysis, despite the obvious value of the dataset and potential of the method.		
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Minor comments:

Reviewer comment	Author's response	Changes to manuscript
<p>I. 10: "The Southern Ocean *circulation* plays ..."</p> <p>I. 27: Reference Haumann et al 2023 absent from reference list. Check all references.</p> <p>I. 29: "The South Atlantic [...] Oceans *waters* ..."</p> <p>I. 31: "extreme and unpredictable", vague and potentially misleading. The weather might become more extreme, not sure about the climate. Not sure about the future of climate predictability either.</p>	Noted, thank you for highlighting these.	These have been corrected.
I. 172: I do not find the agreement between SHA and GHPA particularly striking in Fig. 3e. Fig. 3b seems more convincing. Can you be more specific/quantitative?	We acknowledge more quantitative comparison was needed.	We have re-worded Section 3.1 to include quantitative comparison and clarify the significance of Figure 3f (previously 3e).
We politely suggest the authors to read and incorporate the citation Kolbe et al. 2023 in their work (a paper that went unnoticed as so many other studies from the Covid era), as they will find much information about steric height variability in the Southern Indian Ocean during a period overlapping their period of interest, that may be compared with their results. Notably, the relative importance of heat and freshwater changes is discussed in detail.	Thank you for the suggestion, this paper has been an excellent resource for the revisions.	We have plotted the SHA trend (Figure 4a) as it is useful for validation via comparison against the Kolbe et al study (Line 468). We also refer to the latter in our Discussion when interpreting the trend (Line 378) and drawing links to interannual climate modes.

•There is too much information in the Appendix. Appendix A is useful to interpret the main results. Appendix B could be shortened and put back into the main text. The validation of GRACE against ocean bottom pressure is useful and could be part of the main text. Fig. S4 should also be in the main manuscript. Section 13.5 has nothing to do in an Appendix.	Noted.	We have removed Appendix B and included a section (Section 4.2) in the results assessing the uncertainty of GRACE in context of the present study. The validation of GRACE was simply a repeat of what had previously been performed by Hayakawa et al. (2012) so we have removed this and refer to that study instead.
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References:

Hayakawa, H., Shibuya, K., Aoyama, Y., Nogi, Y., & Doi, K. (2012). Ocean bottom pressure variability in the Antarctic Divergence Zone off Lützow-Holm Bay, East Antarctica. *Deep Sea Research Part I: Oceanographic Research Papers*, 60, 22–31.

Kolbe, Marlen, Fabien Roquet, Etienne Pauthenet, and David Nerini. “Impact of Thermohaline Variability on Sea Level Changes in the Southern Ocean.” *Journal of Geophysical Research: Oceans* 126, no. 9 (2021): e2021JC017381. <https://doi.org/10.1029/2021JC017381>.

Reviewer comment 2:

We thank the reviewer for their detailed consideration of our submission and for making useful suggestions which we have now incorporated in the updated manuscript.

Major Comments.

Reviewer comment	Author's response	Changes to manuscript
I agree with Anonymous Referee #1 that title is not appropriate. The manuscript does not assess the Southern Ocean heat and freshwater changes, it does estimate the steric height changes.	We agree with this.	The title has been changed.
I suggest the authors revise how information is written in the manuscript. It is important that clear and concise language that correctly describes the applied data methods is used. For example, but	Noted, thank you for highlighting this.	We have re-written Section 2.4 to clarify the procedure. We have also proof-read the Methods

<p>not limited to, section 2.4 explains how in-situ data are used to calculate geopotential height anomaly. It is unclear what is the value of the statement “ in which there are multiple pressure level” when the depth criteria are used to determine what profiles are used ($P_{\max} > 500$ dbar and $P_{\min} < 25$ dbar and quality flag 1). Rather than beginning the sentence with “The maximum depth of 500 dbar.....”, perhaps it is better to begin with “ We calculate GHA relative to 500 dbar, where 500 dbar is sufficiently deep.....”</p>		<p>and Results and re-written any areas where extra clarity could be needed.</p>
<p>Another example, in figure 3 b the correlation coefficient is mostly greater than 0.25 but there are region where the correlation coefficient is less than 0 indicating strong negative correlation, and regions of 0 correlation coefficient (no relationship between SHA and GHPA).</p> <p>The white color in figure 3b may not actually show regions of no relationship but rather region with less than 6 in situ profiles. Here it is important to use a mask for regions where the Pearson correlation coefficient was not calculated.</p> <p>Similarly, in 3c the color bar axis should have a minimum value of 6 and a mask applied to regions with less than 6 profiles. While these are rather small correction, the accurate display of data is important.</p> <p>Finally, what is the value of figure 4d and 4e. A reader may ask why you show a region with anomalous in situ observations. Also, for 4d there appear to be data that in water depth less than 500 m.</p>	<p>The purpose of Figure 3f (previously 3e) is to demonstrate the agreement between the satellite and in-situ data in a region where we have temporally abundant in-situ profiles. The correlations we derive on large scales (i.e. whole southern ocean, Figure 3e vary in quality between grid-cells based on the temporal availability of in-situ profiles. For example, some grid squares have only a few profiles collected in summer, meaning that the correlation shown may not capture the year-round relationship. Thus 3f is intended to show a higher quality, temporally-comprehensive comparison.</p> <p>It was previously unclear that we were excluding grid cells with less than 6 months’ worth of gridded monthly GPHA data. This is a different quantity to the total number of profiles in a given cell as shown by Figure 3d (previously 3c), i.e. there could be many profiles within a single month. We have explained this difference in the text and thus have not modified the scale on Figure 3d or added a mask.</p>	<p>We appreciate the detailed suggestions and have revised Section 3.1 with the following changes:</p> <p>Addition of comparisons of seasonal cycle of SHA, GPHA (these were previously shown in a different figure which has been removed as per another review comment) and sea ice concentration to support the validation analysis.</p> <p>Removal of Figures 3a and 3d.</p> <p>Coarsened grid cells for the correlation analysis.</p> <p>Clarified the gridding procedure (Line 188).</p> <p>We now use 36, rather than 6, months of data as the cutoff point which has been scaled in keeping with the coarser grid cells.</p> <p>Added a quantitative analysis of Figure 3e (correlation plot, previously 3b) (Line 195).</p> <p>Added masked region on Figure 3e (previously 3b) where there were fewer than 36 months’ worth of data.</p> <p>Clarified the significance of the Kerguelen Island study (i.e. Figure 3f (previously 3e)) (Line 199).</p>

Lines 176 to 180 need to be revised. How you validate the SHA is the sea-ice zone is important. What is meant by “ open Southern Ocean”? 60oS to the seasonal sea-ice zone?	Thank you for highlighting this.	These lines have been removed, along with any reference to the ‘open Southern Ocean’ and replaced with more specific regions.
<p>While the interannual SHA variability shows a potentially interesting signal, it is somewhat complicated, and the analysis undertaken needs further investigation and refinement. This more detailed consideration of the causes of the variability would enable the authors to reach much more conclusive findings.</p> <p>I would suggest that the authors consider only the SHA and the relationship with the large-scale SAM and ENSO forcing for this current manuscript and continue to investigate the interannual and seasonal signals for inclusion in another manuscript.</p>	Thank you for the suggestion, this has helped a lot with the revisions.	We’ve removed results concerning the SHA time series and seasonality, and instead focussed on additional statistical analysis which ties in with the SOI and ENSO analysis.