Letter to the Editor: 2nd Resubmission of Manuscript

Dear Bernadette,

Many thanks for your continued support towards our manuscript submission. Please see below our responses to the second reviewer's comments and the corresponding changes we've made to the manuscript.

Best regards,

Jennifer Cocks

We thank the reviewer for taking the time to consider our manuscript and for their positive and constructive response.

Major comment:

Reviewer comment	**Author's response**	**Changes to manuscript**
I think introduction would benefit from a bit more information about why computing steric height from satellites is difficult in the Southern Ocean. Is it just the Steric and barystatic height that is challenging? It feels like the thing that makes this study novel, and I feel like this isn't highlighted enough, especially in the intro. This paragraph seems like a good place to reference to figure 2.	We agree – thank you for picking this up.	We developed a paragraph in the introduction to give greater detail on previous work on this subject and the difficulties of transferring it to the Southern Ocean, please see lines 69-81.
When calculating geopotential height, I'm concerned about the choice to reference to only 500 dbar. I understand that changes in water mass properties will preferentially materialize in the thermocline, but I would still probably reference to 2000 or 1800 when using Argo. Why did you choose 500? You should have enough profiles in	We chose 500 for the following reasons: - Many of the profiles come from tagged elephant seals, who rarely dive beyond 500m. - The Southern Ocean follows an equivalent barotropic circulation model, and the steric	We have added a paragraph into the methods section to explicitly explain this reasoning, please see lines 156-163. We refer to the papers Meijers et al., 2011 and Killworth and Hughes, 2002, who show that the difference between surface GPH change and that at depth is minimal.

the Southern Ocean so that you still have sufficient data even when eliminating shallower profiles. Especially when comparing to steric height anomaly which will represent the entire water column. Also, in the introduction, you motivate this work by discussing changes in deep water which you won't capture by only referencing to 500. Can you either justify your choice or show that it doesn't change your results to use 500 dbar as opposed to 2000.	height changes in the top 500m are highly correlated with the steric height changes at depth (we have performed our own check of this on the Argo floats to check it holds true for our data – please see Figure 1 below). - We would like to use the same validation procedure both on and off the shelf, so a shallower reference depth is required.	
Figure 6: the overall trend is really interesting. You say the trend is predominantly negative but there are large regions where it's positive. I feel like the manuscript is missing some discussion of what possible physical drivers are causing positive and negative steric height trends. I guess I'm surprised that the trend isn't positive everywhere and curious why that would be.	We think the reviewer might mean Figure 4. We agree there should be a clearer discussion of this as it is unexpected!	We have rearranged and expanded the discussion (Section 4.1) – we now begin by discussing the trend, before looking at SAM and SOI. We've added more in-depth discussion around the widespread decreases to SHA relating to the MOC, please see lines 60-68.
The seasonal cycle should be removed before calculating the EOF in figure 5. It looks like it dominates the first and second modes. It probably also wants to be removed from SOI and SAM analysis.	Thank you for these suggestions. We have experimented with removing the seasonal cycle from the EOF analysis (please see Figure 2 below) and have decided to retain it for the following reasons: By including the seasonal cycle, we are able to draw conclusions on the role of the seasonal cycle towards the overall variability, for instance, we were able to show that the seasonal cycle is dominant in the first mode, with a much larger amplitude than interannual changes. Thus, we can conclude that the seasonal cycle has a dominant influence on overall SHA variability, and we can explore the spatial pattern of this in the EOF plot. We think this is informative to show. Furthermore, removing the seasonal cycle seems to have the effect of removing the first mode, so that Mode 2 becomes Mode 1, and Mode 3 becomes Mode 2 (minus the low-amplitude	We include a line (L277) to summarize our reason to retain the seasonal cycle in the EOF.

	seasonal cycle in the time series). Modes 3 and up have a very low Explained Variance Ratio and lack coherent spatial patterns, so there is no benefit to removing Mode 1 (i.e., removing the seasonal cycle). We also experimented with removing the seasonal cycle from the SOI and SAM composite plots. However, since we're already smoothing the SHA used here, removing the seasonal cycle has a negligible effect (a maximum of 0.1cm, the difference was not visible in the plots).	
You reference a Kolbe 2024 paper that used GLORYS to do a similar analysis but I can't find the paper in your references. There's a Kolbe 2021 paper though. Please check that all your citations are correct and included in your references.	2024 was an error – it should have been 2021. Thanks for picking this up.	Reference corrected.
Line 463: GLORYS is a reanalysis product that assimilates satellite altimetry and Argo so it isn't surprising that your results are similar here. I think you should expand on this paper in more detail, discuss what they found and how your results add to our understanding beyond what this paper finds. Should probably include some discussion of this in the intro as well (probably all papers in section 4.3). See above note about reference.		We have elaborated our reference to Kolbe et al. (2021) in Section 4.1 with more information on the mechanisms resulting in the decreasing steric height trend. We've added a line in Section 4.3 (L498). We have added a discussion of previous work to the introduction, please see lines 60-68.

Minor comments:

Reviewer comment	**Author's response**	**Changes to manuscript**

Sentence starting on line 32: A bit awkward and very long. Split into two. Maybe be a bit more specific about what has changed about deep water and the overturning circulation.		Line has been modified and broken into two.
Line 44: maybe change to, including the marginal sea ice zone? Or including sea ice regions. Maybe just 'sea surface height of the Southern Ocean, and more accurately'		Changed to 'marginal ice zone'.
Section 2.1: Was there a reason you did your own gridding as opposed to using a pre-gridded product that might extend beyond 2018?	The data were gridded by Dragomir (2023), but this wasn't clear in the text.	Clarified that we did not process the DOT data.
Line 131: This is a bit awkward and seems repetitive. Considering possible changes in steric height, would your SSHA calculation be more accurate if you computed it from, for example, a 180-day low pass filter rather than the time mean?	We must compute anomalies relative to a time mean since the GRACE data are provided as anomalies against an arbitrary time mean. Computation against anything else is impossible, since there is no reference.	We have shortened this section and removed repetition.
Figure 2 is referenced before figure 1. Should probably reference figure 1 somewhere in the intro.		Figures have been swapped.
Section 2.4: You should include how many Argo floats/profiles were used, the years your float records span and maybe something about their distribution in space and time? I think profiles were gridded? That information should be included too.	Agreed.	We've added the total number of profiles (L152) and a supplementary figure to show the distribution of profiles in space and time (see Fig. 5 below). We refer to this in the text at L153. We also moved the explanation of the gridding from Section 3.1 into Section 2.4.
Line 175: follows from above comment, you say you subtracted mean geopotential height from July to June 2018 but you don't have any Argo profiles from 2002. Probably worth including some discussion about when you have a high enough degree of freedom to accurately validate using the Argo dataset.	We discuss this in Section 3.1 and describe the following measures which have been taken to ensure sufficient data are available for validation: - Coarsening of the grid by a factor of 6 - Excluding grid cells where we have fewer than 36 months' worth of data	Supplementary figure S1 (please see Figure 5 below) added to show temporal and spatial distribution of profiles. We added a line (L200-202) to clarify that these measures were taken to achieve a good enough degree of freedom for validation.

	- Performing local comparison in a location where we have a high density of data	
Figure 3 caption: you say the red box in panel d outlines the region from which the data in e have been taken but I think you mean the data in f.	Quite right, thank you for spotting this.	Changed to (f).
Figure 4 is never referenced – should include somewhere in paragraph near line 235	Thanks for spotting this.	Added references to Fig. 4a and b.
Figure 4 (and elsewhere): maybe consider adding dots or some indication of where validation with Argo data was low/negative or not possible	We show in Figure 3e (yellow area) where validation has not been possible and state in lines 220-226 that we include these areas, and those of negative and low correlation, in the results. We do not mark these areas on the other plots so as not to obscure the results.	We have added a line to the captions of Figures 4-7 to direct the reader to section 3.1 for information on regional variation in data quality.
Line 267: Would change this to “temporal component is dominated by an annual fluctuation reflecting steric height changes resulting from the seasonal cycle”	Thank you for the suggestion – nicely worded	Line changed to this.
Figure 7: The years identified as negative SAM years don’t really seem representative of true negative years, and I don’t think enough years are included to consider a reliable composite. Considering the trend, you could just not show a composite of negative SAM years. Alternatively, instead of negative SAM years you could change to weak SAM years. In this case, I would remove the increasing trend from the time series, probably reduce the filter size (12 months max, maybe even less than that) and identify years from there.	We agree that there are very few -SAM months and the few months which are highlighted may not be representative. We experimented with detrending the SAM signal and reducing the filter to 12 months (please see Figure 3). Since the amplitude of SAM appears to increase over time, this emphasises the latter portion of the time series, and considering the trend, we see a -SAM response which is similar to the +SAM response.	We removed the -SAM plot, and retained only the +SAM. The text in Section 3.3 has been modified to reflect this.

	We also experimented with reducing the filter size to 12 months for the non-detrended signal (please see Figure 4). We found a weak signal which seemed to indicate that these were not significant - SAM events, and the algorithm had just found a more random assortment of months.	
Line 307: this could be better worded. It's not really a problem that the SAM index is positive – it's the signal.	Noted	This has already been changed in response to a previous comment.
Section 4.3 I feel like an introduction to these studies should go in the intro.	Agreed	This has already been added in response to a previous comment – please see lines 60-68.

References:

Killworth, P. D., & Hughes, C. W. (2002). The Antarctic Circumpolar Current as a free equivalent-barotropic jet. Journal of Marine Research, 60(1), 19–45.

Meijers, A. J. S., Bindoff, N. L., & Rintoul, S. R. (2011). Estimating the four-dimensional structure of the Southern Ocean using satellite altimetry. Journal of Atmospheric and Oceanic Technology, 28(4), 548–568.

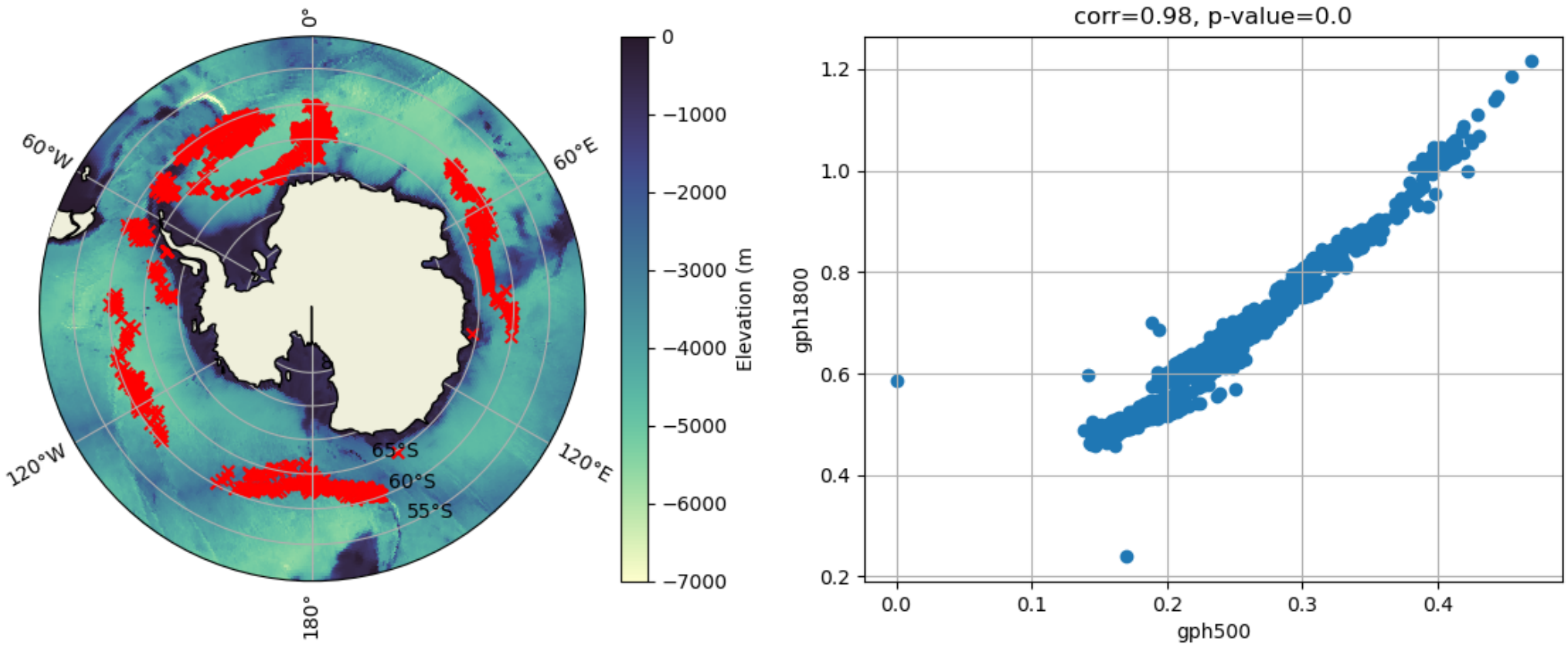

Figure 1: Comparison of geopotential height (GPH) referenced to 500 and 1800 dbar from a sub-selection of Argo float data. (left) The geographical locations of the floats in the sample (red, 'x') plotted over the GEBCO bathymetry. (right) The GPH of each profile referenced to 500 dbar plotted against the GPH referenced to 1800 dbar.

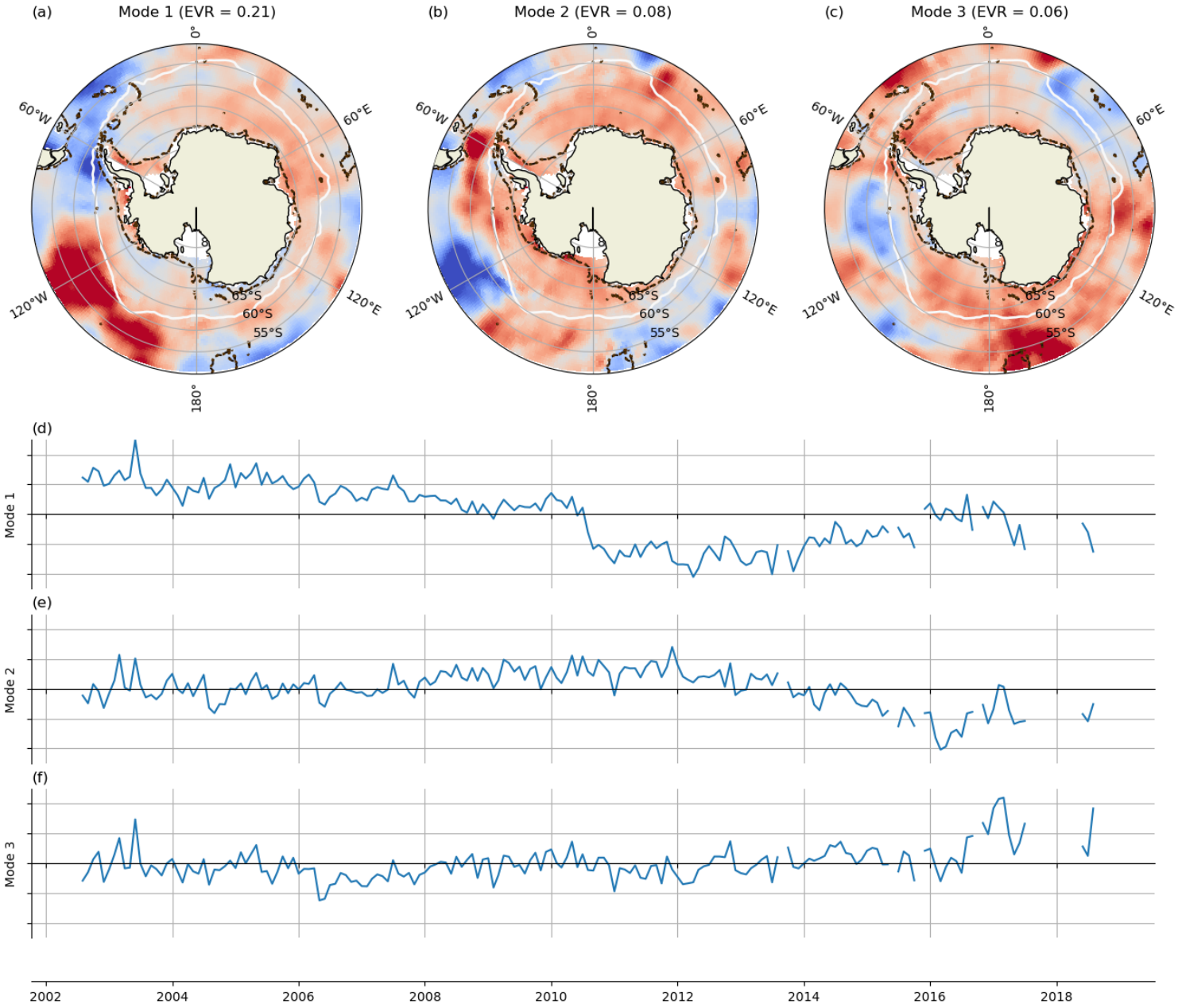

Figure 2: EOF analysis with seasonal cycle removed. Spatial signatures of Modes 1 (a), 2 (b) and 3 (c). The colour scale is arbitrary but consistent across (a), (b) and (c) and centred at zero. The Explained Variance Ratio (EVR) is shown in the title. The sea ice maxima is demarcated in white (solid), and the -1000m isobath in black (dashed). Temporal signatures of Modes 1 (d), 2 (e) and 3 (f). The scale on the y-axis is arbitrary but consistent across (d), (e) and (f) and centred at zero.

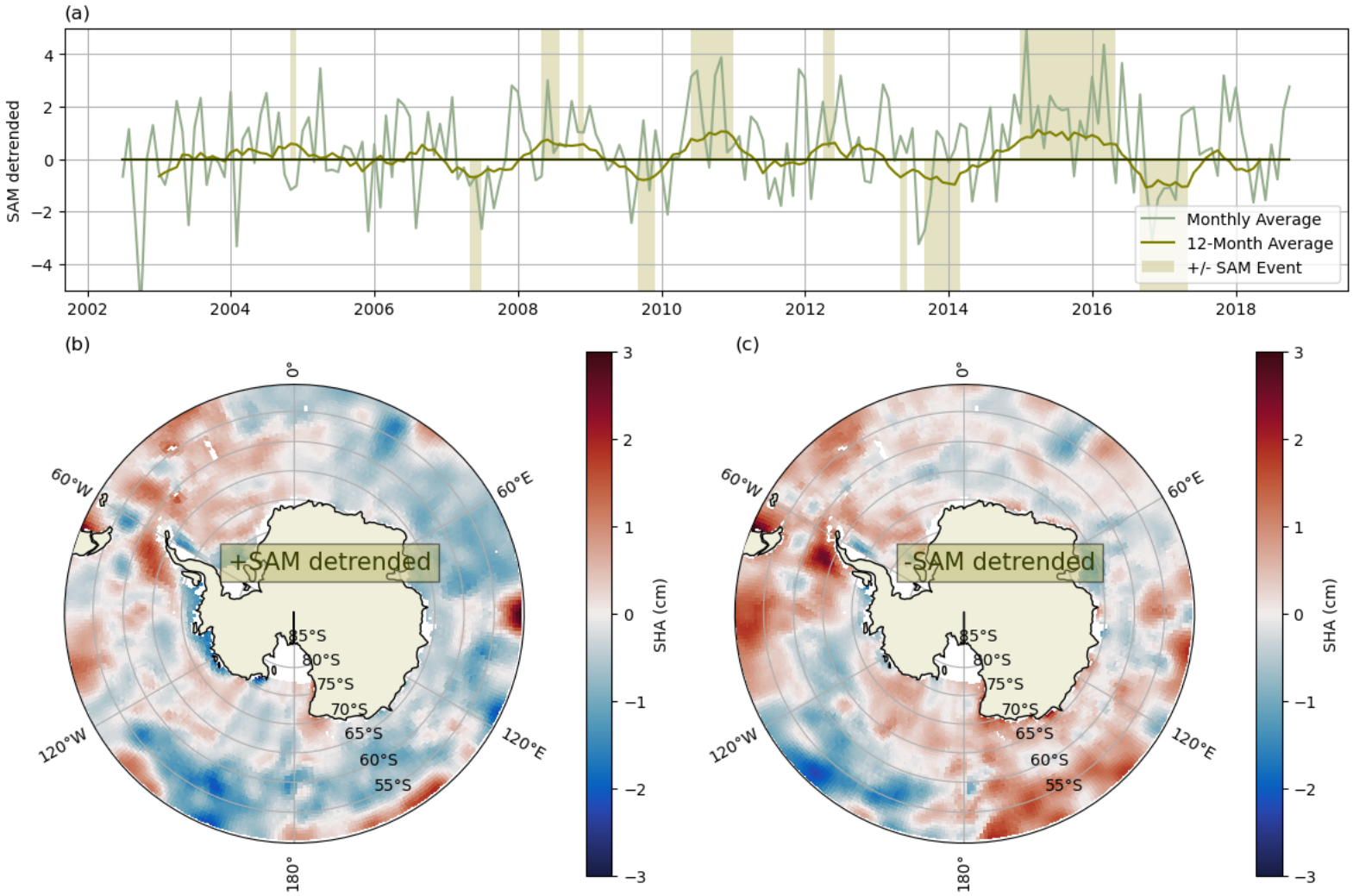

Figure 3: a) Timeseries of SAM index detrended and smoothed over 12 months. Months showing a SAM index greater than 1 standard deviation and less than -1 standard deviation highlighted in pale green. b) Composite plot of SHA within months where the SAM index is greater than 1 standard deviation. c) As b), but where the SAM index is less than -1 standard deviation. SHA has also been smoothed over 12 months.

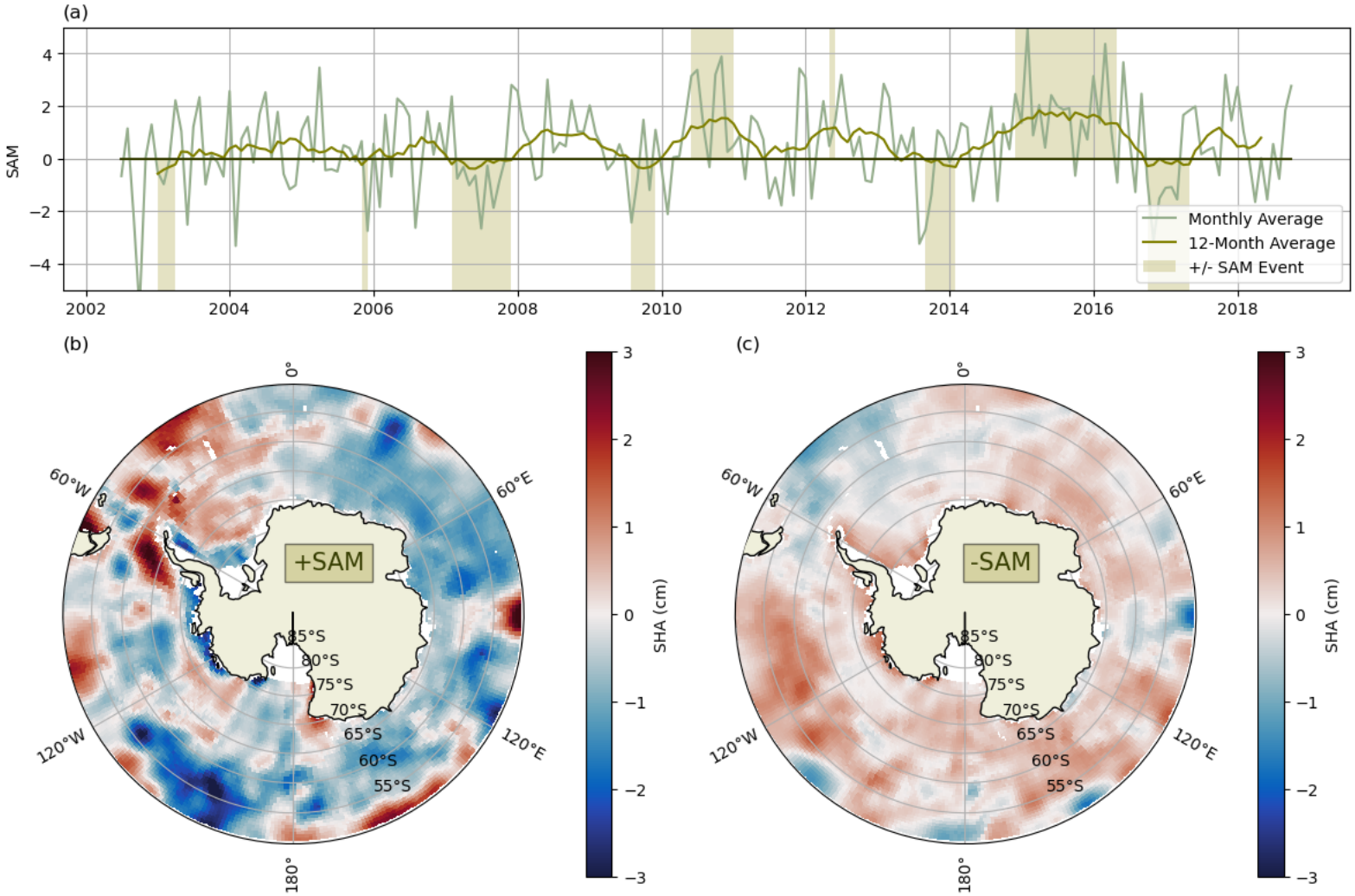

Figure 4: Timeseries of SAM index smoothed over 12 months. Months showing a SAM greater than 2 standard deviations and less than 0 highlighted in pale green. b) Composite plot of SHA within months where the SAM is greater than 2 standard deviations. c) As b), but where the SOI is less than 0. SHA has also been smoothed over 12 months.

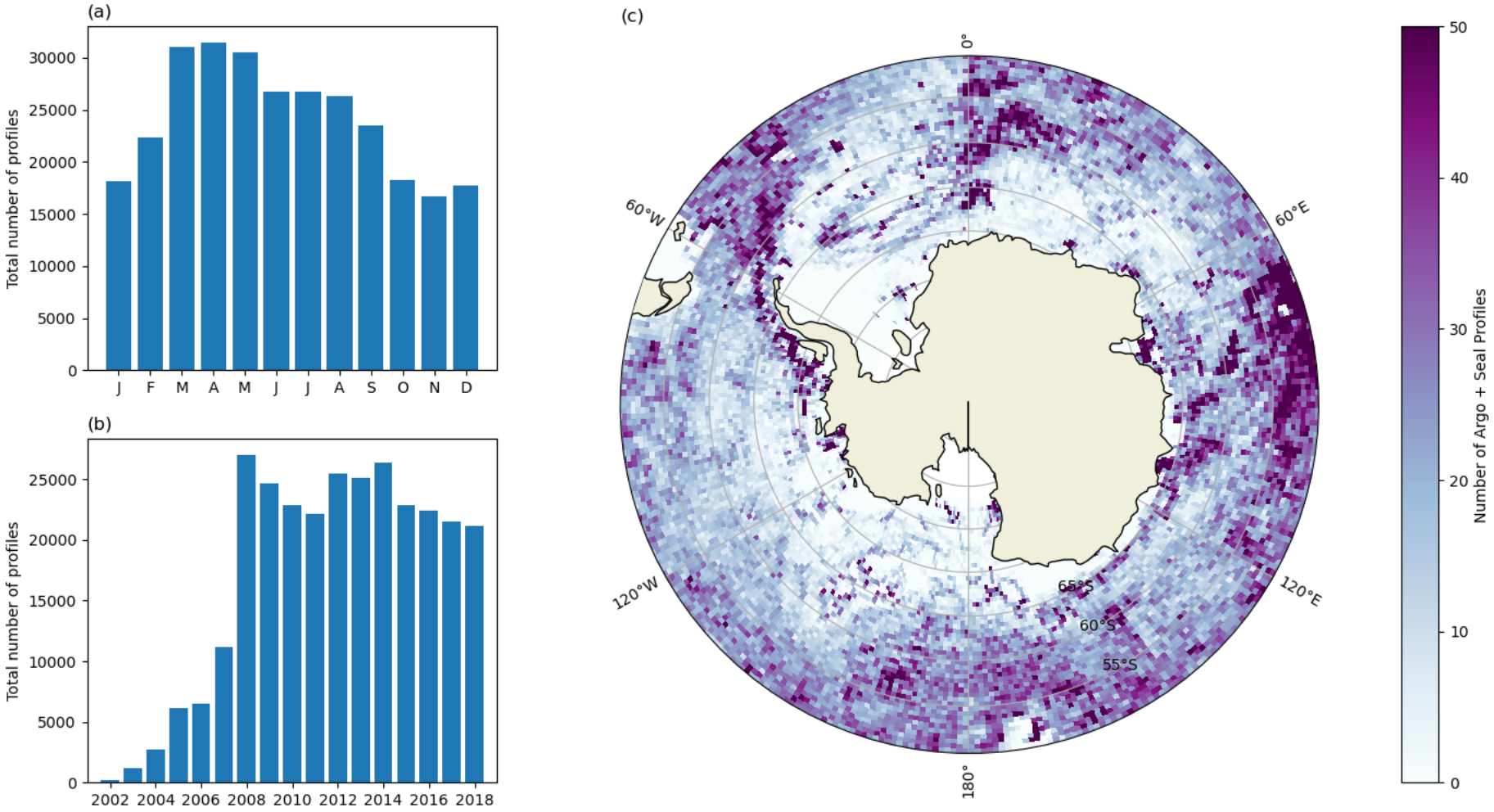

Figure 5: (a) Monthly distribution of Argo and Seals profiles. (b) Yearly distribution of Argo and Seals profiles. (c) Geographical distribution of Argo and Seals profiles.