Response to Reviewer Comments

We would like to thank the two reviewers for their additional insights and constructive comments on our manuscript. We have copied their reviews below, in black, and our responses are in blue.

Reviewer #1

Here is the paper I referenced in my review: Thompson, S. S., Kulessa, B., Luckman, A., Halpin, J. A., Greenbaum, J. S., Pelle, T., ... & Blankenship, D. D. (2023). Glaciological history and structural evolution of the Shackleton Ice Shelf system, East Antarctica, over the past 60 years. The Cryosphere, 17(1), 157-174. Apologies for providing the incorrect year in the original review.

Thank you for providing the citation to the paper. We have added this into the citations on line 52.

Reviewer #2

Dear Authors,

Thank you for your meticulous responses to the reviewer comments. There is a clear improvement of the manuscript, with the figures and the organization of the text. I believe you have adequately responded to most of the comments that I raised on your initial submission.

However, there are a few key things that I would like to see addressed before final publication of the manuscript. Once the comments are addressed, I believe the manuscript will be a valuable addition to the scientific community, particularly those interested in East Antarctica.

The major comment I have remaining is how the mCDW is discussed throughout the text. In some cases, it reads as if the mCDW is new in this region, thus causing the grounding lines to retreat in the recent decades. However, the data do not clearly support a change in the presence of mCDW. Therefore, I highly suggest going through the text and making sure the writing clearly articulates a characterization of the ocean properties, and not a change in properties. Please also see the comments on specific sections below. *We have addressed this throughout the paper (see below). We are in agreement with the reviewer that we have no evidence for a change in the presence of mCDW and have edited the manuscript to avoid any potential confusion. We are simply arguing that the observed changes in ice dynamics are consistent with its presence.*

I also suggest including the calculated uncertainties in the results and discussion sections. *All of the uncertainties associated with each dataset are shown explicitly in the Supplement. Adding uncertainties to each data point in the results would make some of the Figures look very cluttered and break up some of the fluency of the text. However, we now frequently cross reference to the uncertainties in the Supplement and, as the reviewer notes, we now only focus on broader patterns of change that lie outside the uncertainty range (see also response below in relation to Section 3.3.)*

The other comments are minor:

Line 138 – typo firm should be firn. *Corrected.*

Line 153 – again, firm should be firn. – Please check throughout the document (line 514 also). *Corrected.*

Line 155 – what do you mean by ground finding errors? *Ground finding errors are the errors in measuring the exact distance between the satellite and the ground, which we have*

clarified in the revised manuscript. This term is used here to distinguish from interpolation errors in the sentence.

Line 165-168 – I'm not quite sure what you mean by this sentence: "Ice surface velocity profiles were extracted along the central flowlines using the 2021 annual velocity mosaic for each outlet glacier". Do you mean that you used 2021 to determine what the central flowline was and then used that line for the rest of the years? Please clarify in the text. *This has been replaced with "For each outlet glacier, the central flowline was extracted from the 2021 annual velocity mosaic and used to position the sampling boxes."*

Line 171 – Can you add the mean error as well? *Here we have referred the reader to the Table S2, which provides the error for all four glaciers across all years.*

Line 193 – there is an extra period in this sentence. *Removed*

Line 225 – missing "line" next to "grounding". *Corrected.*

EN4 and MEOP data – In the reviewer responses, you mention that the MEOP casts are used to build the EN4, and in the paper, you say that you show them to supplement the EN4. I would be more explicit about how the two datasets are related and what they uniquely show. *We have edited the manuscript to make it clear that EN4 data are a reanalysis product that uses available observations and processes and interpolates these into a gridded data product. Due to potential interpolation errors, we also show the empirical observations from MEOP casts and Argo floats separately. This has been made clear in the revised manuscript.*

Additionally, I'll insist on plotting the time series of the MEOP data (even though the data varies in space). This will improve your assertation that there is the intrusion of mCDW and clarify if there are indeed any ocean warming trends in the region. Please also see comment below.

As noted above, our data do not show an increasing presence of CDW over the time period, but simply that CDW is likely to be present periodically on the shelf since the 1990s. The data do not support any ocean warming trends. We cannot plot the MEOP data against EN4 because it is restricted to a few years and is at a few depths. A new study (https://iopscience.iop.org/article/10.1088/3049-4753/ae7113) provides more information on the different datasets used for EN4, which we now cite

Regarding Figure 12 – What this figure is showing (and that strange line) is still unclear. The figure caption says "Oceanic properties derived from the MEOP casts…" – this makes the reader believe that the data is seal tag temperatures collected from the MEOP program - but your reviewer response says "The line of data points is likely from an Argo float. When the float is under ice, it cannot transmit its location; the position is typically interpolated between the last known location before ice cover and the first transmitted location after the float resurfaces. In this case, the float was likely under ice for a prolonged period and collected multiple profiles during that time." So, is it an ARGO float or is it seal tag data? *Good point and we apologise for any confusion, but we are now confident that these observations are both MEOP casts and Argo floats and that the straight line is from an Argo float. We have included a few sentences in the Methods about our use of Argo data in the study and cited Wong et al. 2020 throughout. The data we use is a combination of MEOP and Argo float data and this information has been updated in the Methods.*

Section 3.3. – Can you add the uncertainties of the calculated elevation changes for each glacier for both datasets here? *The reviewer makes a good point here. We have removed the sentences in Section 3.3 that referred to elevation changes falling within the uncertainty range reported in the Methods.*

Given uncertainties are stated in the methods section, here we decide to remove any quantification of this and simply note that there is little change at this site but also that measurements are subject to high uncertainties compared to the signal. By doing this, we avoid any quantitative over-interpretation of rates of change at this location.

We have retained the uncertainty discussion in the Methods because the Results section is focused on the overall pattern of change, rather than the significance of individual values. Since the data in Figures 5 and 6 are plotted as anomalies with a 24-month rolling mean, the reader can see the spread and variability in the data through time. We have adjusted our interpretation to focus on the broader thinning pattern at HE and HW, rather than interpreting each individual value separately or overemphasising the mean change through time. This approach is consistent with Picton et al. (2022), where uncertainty was not defined for the mean elevation changes.

Table 3 – I suggest changing the word "statistic" in the table caption. These are not actual statistics but rather characteristics of the glaciers that you have investigated. "*Statistic" changed to "characteristic."*

Line 520 – How do you make sense of the East GL retreating the most, but having only a minor thinning rate, especially when taking into the account the uncertainty associated with the elevation data? Also, in line 348 it states "Schröder et al. (2019b) and Nilsson et al. (2023) observed elevation change at an average rate of −0.41 and −0.40 m yr−1 for Holmes East Glacier", which is different than what is written on line 520. Please check for consistency throughout. The assertion in this paragraph that there is a relationship between grounding line retreat and ice surface thinning rates does not align with what you have in Table 3 and in fact, brings up many more questions. I suggest removing this paragraph entirely. *The end of Section 4.1 focuses on directly comparing period of rapid GL retreat with enhanced thinning, so these more rapid thinning results were taken from 1996 and 2001 rather than the longer averages that are stated in above sections. We have retained this section and have altered the language to improve clarity.*

Section 4.2 – Here, are you suggesting that your results show that mCDW has been there all along? Since your results do not show increasing temperatures at depth, do you think mCDW has been there since the 90s – the start of your EN4 data? Please expand on this! *Our data are not able to show a temporal change in ocean temperature through time. We have further clarified this throughout the paper by including sentences such as "Although we find no evidence of a temporal change in ocean temperature (Fig. 11), recent observations of mid-depth CDW along the continental slope off East Antarctica (80–160° E), and including offshore Porpoise Bay, show warming since the 1990s…" (line 556) and "Though we acknowledge the uncertainties, EN4 data suggest the presence of mCDW at depth across the continental shelf since the 1990s…" (line 565).*