Climate of the Past Discussions

Clim. Past Discuss., referee comment RC1
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Comment on cp-2022-15

Anonymous Referee #1

Referee comment on "Compilation of Southern Ocean sea-ice records covering the last glacial-interglacial cycle (12–130□ka)" by Matthew Chadwick et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2022-15-RC1, 2022

This is a well-written paper that compiles sea ice records from 24 sediment cores around Antarctica covering the past 130 ka. The contribution is novel and important as it covers a much longer time frame than previous compilations. The authors use a creative method for comparing disparate types of records by normalizing the winter sea ice, annual sea-ice duration, and sea-salt sodium records to identify periods of higher or lower sea ice at each site. This method, coupled with record stacking, allows the to make comparisons about sea ice expansion and retreat on glacial-interglacial and finer timescales. The authors are careful not to overinterpret changes in timing, keeping in mind the error behind the cores' age models, but they are able to discern important differences between the 5 sectors of the Southern Ocean, noting both that the output regions of the Weddell and Ross Sea gyres are more sensitive to changes in sea ice and also that the Pacific and Indian sectors lead the Atlantic in sea ice retreat during Termination II, which the Pacific sector leads all others during Termination I.

However, there are a few areas where additional clarification is needed both in the text and in the figures. I detail the major issues below followed by my line-by-line comments.

When I read the abstract, I was not entirely sure what the authors meant by "output regions." It is eventually defined in section 3.2, but the term is used several times before this section. This could be clarified by maybe referring to these regions as "regions of high sea ice outflow" and/or including the ocean currents on at least one of the figures.

It is unclear why the PCA figures are only included as supplemental information. I'm guessing it is because it doesn't add much information beyond the fact that glacial/interglacial cyclicity is the dominant forcing? It might be useful to plot PC1 along with the stacked sea ice records in Fig. 4.

In section 3, there is a great deal of attention paid to the differences between MIS 5 through MIS 2. However, I had a really hard time seeing the MIS 5 substages referenced in lines 203-208. Perhaps some delineation of the plots in Figs. 1-3 would aid in this (i.e., colored bars indicating glacial/interglacial stages and substages). The changes during the MIS 5 substages are significantly more subtle than the difference between MIS 5 and MIS 4. The authors also refer to "the prominence" of the substages in Fig. 2 for cores PS75/072-4 and PS1768-8. However, for PS1768-8 there is only one small increase in sea ice around 100 ka. This does not depict the prominence of MIS 5b and 5d. PS1778- is much more prominent in Fig. 2. Is there a typo?

It would be helpful if the authors briefly reiterated that positive values in Fig. 3 (note, Fig. 3 axes are unlabelled) indicate times when the cores were south of the winter sea ice extent in the past.

Lines 211-212 state, "Standardised sea-ice records also show that, despite sea-ice expansion in most regions as early as MIS 4 (Figures 1, 2 and 4), the mean WSI edge was located south of the majority of core sites in this study until MIS 2 (Figure 3)." However, only the W. Indian sector agrees with this statement. When I look at Fig. 3, I see that many cores do not extend back to MIS 4. There is no information in the Scotia Sea sector about MIS 4, In the Atlantic sector, the only core that extends to MIS 4 shows that the site was south of the WSI edge in MIS 6 and MIS 4. In the Pacific Sector, two sites show similar magnitude expansions of sea ice in MIS 4 and MIS 2 (SO136-111 and the SID in TAN1302-96), whereas the WSI estimate in TAN1302-96 shows that the site was south of the WSI edge in MIS 6 and MIS 2. While, as the paper rightly points out PS58/271-1 shows highly fluctuating sea ice and was probably close to the WSI margin. I ask the authors to re-evaluate and revise their quoted statement above.

Line 225: This sentence is confusing because the authors say that cores from the western Pacific sector show little sea ice signal outside of MIS 2-4, but the western Pacific cores (PS75/072-4; SO136-111; TAN1302-96) are the ones that earlier were described as having variability in the MIS 5 substages. It's unclear whether this is a typo and the authors meant the Scotia Sea or if there's something about these western Pacific cores that is not obvious here.

In line 241, the authors estimate how much farther north the WSI edge was in the Scotia Sea during MIS 2. It took me a while to parse that this was probably estimated because there is a lot of sea ice in the cores during MIS 2 and they are currently up to 3.6 degrees north of WSI. It would be helpful if a line or two describing this logic could be included in the text. While on the topic of the Scotia Sea, I also wonder if the sediments below iceberg alley artificially show increased sea ice because sea ice diatoms are transported by the flux of ice bergs?

Short Line by Line Comments:

Lines 80-81: I was confused by this, and initially thought that the sites north of the WSIE also should be in abyssal depths. I looked up a bathymetric map of Antarctica and was surprised to learn that there is a network of ridges north of Antarctica, roughly in the same location as the WSIE boundary. I suggest mentioning the water depth of cores in lines 76-79 somewhere (and potentially in Table 1?) to underscore that the cores mostly come from shallower depths. It's not necessary for this paper, but if you're interested in a modern analysis of sea ice extent and water depth, Nghiem et al., 2016; doi:10.1016/j.rse.2016.04.005 is quite interesting. I found this paper while deciphering Lines 80-81.

Lines 82-83: The authors write, "dissolution of the more lightly silicified diatom species (generally sea-ice related species) increases, which biases the preserved diatom assemblage to reflect warmer and lower sea-ice conditions." This is a commonly written idea, but in my experience, finding actual data to back this up is challenging. I urge the authors to find a reference to support this idea of biased diatom assemblages (i.e. sea ice diatoms selectively dissolved) and/or increased dissolution of diatoms in general.

Line 147: Journal requirements differ, but as a reader, I would appreciate to be reminded of what the acronyms FCC, WSIC, SID, APF, and WSI stand for in this figure caption.

Line 217: It's unclear which cores are in the output region of the Weddell Sea Gyre. Is it the Atlantic Sector cores? Please include a notation.

Lines 296-305: I suggest you spell out iron throughout this paragraph.

Lines 314-320: This paragraph relies fully on non-peer-reviewed papers. If the Green et al., 2021 and Chadwick, et al submitted papers are In Press by the time this manuscript is submitted, this paragraph is fine (in fact it's great). But I just wanted to highlight it in case they're not.

Figures:

Figures 1-3:

Please label major places referred to in the test including (but not limited to) Ross Sea, Weddell Sea, Scotia Sea…it would also be helpful to include very basic current patterns since you mention output regions repeatedly. It is very difficult to discern the difference between the grey and black lines (September sea ice vs Antarctic Polar Front). Perhaps the grey line could be lighter?

Each of the records is numbered in Figs. 1-3, however the numbers change from figure to figure. I realize that this is because the number of records decreases, however, it makes it confusing to compare between figure. I suggest the authors number the records in Fig. 1 and leave the numbers consistent even though it means that Fig. 3 will not have record numbers 1, 3, 5, 6…

In the paper you refer to studies as reconstructing winter sea ice concentration and abbreviate it WSIC, but in the figures you call it WSI (%). Please make these consistent.

Figures 2 and 3 do not have the x axis defined. Is it the same for every core? It should be. Is it +/- 1? Even though it's normalized, it should still be labelled. Actually, in the legend, it says that the axes are still WSI (%), SID (months/year), etc. But, if this data is normalized, shouldn't it be unitless?

Figure 4: Please label Terminations I and II.

Supplementary Material

Figures S5 and S6 are not referred to in the text. Please either include a discussion of them or remove them from the supplemental material.