

Clim. Past Discuss., referee comment RC3
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Comment on cp-2021-107

Anonymous Referee #3

Referee comment on "Sea ice changes in the southwest Pacific sector of the Southern Ocean during the last 140 000 years" by Jacob Jones et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-107-RC3>, 2021

Review of the paper « "Sea Ice Changes in the Southwest Pacific Sector of the Southern Ocean During the Last 140,000 Years" by Jacob Jones, Karen Kohfeld, Helen Bostck, Xavier Crosta, Melanie Liston, Gavin Dunbar, Zanna Chase, Amy Leventer, Harris Anderson, Geraldine Jacobsen submitted to Climate of the Past,

The authors present new data of sea ice cover and sea surface temperature for the Southern Ocean. These data are obtained using diatom assemblages from a low sedimentation rate core that extend back to the last interglacial period. They compare their results with cores from the same sector of the Southern Ocean to discuss the role of sea ice in atmospheric CO₂ and ocean circulation changes. Evolution of Southern Ocean sea ice is of great importance and these data will be welcome despite the low sedimentation rate of core TAN1302-96. However the comments only concern the methods and results sections because the chronology of the new sediment core studied here must first be corrected.

Sedimentation rate in core TAN1302-96 is much higher during interglacial/warmer period than during glacial period and MIS 2, 3 and 4 are represented by less than 30cm in that core from ~90 to 120cm. The period that the authors discuss as MIS 3 is part of MIS 5. The evidence comes first from the d¹⁸O stratigraphy measured on *N. pachyderma* (senestre? should be indicated by the authors). The values measured between 120 and 170cm are clearly too low to represent MIS 3. They indicate that from 120 to 300cm the sediments were deposited during MIS 5. This is also indicated by the 14C data:

measurements at both 130 and 170cm indicated dates undistinguishable from background because both are older than 70kyr. Further evidence of the "extended" MIS 5 and shrunk MIS 2, 3, 4 could come from the carbon isotopic record of *N. pachyderma* but they are not presented in the paper. The authors could/should compare their isotopic record to the isotopic record of core SO136-111 that they are also using in this paper if they want further evidence. Correcting the chronology for the studied core TAN1302-96 will make it possible to reconcile the sea ice record of this core with those of the 2 cores from the same area: core E27-23 and core SO136-111. It is not clear if MIS 6 is represented in the core. There is no corresponding isotopic value but it might be due to the low resolution of the isotopic data.

Following are a few comments on the methods and results:

-Chronology: As the authors indicate, significant MRA variability occurs over a glacial cycle, specifically in the southern high latitudes. They should use as a minimum ± 100 years for the uncertainty on the MRA as it is the variation indicated by Paterne et al., 2009, for the last century. The authors do not indicate the uncertainty they evaluate for the tie points used to correlate the planktic isotopic record to the LR04 benthic record. From figure 3 it seems that they also choose a too small uncertainty. Anyway the authors should give more details. Furthermore as they present a planktic isotopic record and a SST record and as their goal is to discuss the impact of sea ice extent on atmospheric CO_2 , it would make more sense to establish the chronology of MIS 5 comparing their records to EDC deuterium record, following Govin et al., 2015, Capron et al., 2014. Anyway the record resolution is pretty low (partly due to the low sedimentation rate of the core) so the real uncertainties are large and this comment is not that important.

- SST and wSIC: the authors should give more details: how many analogues have been used for reconstructions? Is the error indicated on the figure the standard deviation between the different analogues? The tables should be available to reviewers.

-Results: what is indicated in the text is not what is presented on the figures. Some examples: line 253, the SST increase seems to be $\leq 3^\circ\text{C}$ on the figure. Taking into account uncertainties ~ 1 to 4°C would be precise enough. Line 254: on the figure the 2 methods indicate ~ 22 to $\sim 33\%$ wSIC for the oldest point. Where does the 48% comes from? Line 256: I do not see a rise in SST during MIS 5e, only variability.