

## Referee 2

This article is an interesting and valuable contribution to our understanding of seasonal sea-ice zone dynamics across a full glacial-interglacial cycle. The palaeoenvironmental conditions are reconstructed from a marine sediment core located further south than previous reconstructions of a full glacial-interglacial cycle and thus represents a valuable new data point. The authors use a combination of sedimentological and diatom species assemblage analyses, alongside statistical analysis, to reconstruct the palaeoenvironment of the continental slope region off Adelie Land. This multi-proxy data set is used to investigate the variations in environmental conditions between glacial and interglacial periods, as well as during the glaciation and deglaciation transitions, back to MIS 6.

Overall, the authors do a good job presenting and interpreting the diatom data and show a good appreciation of the limitations and challenges. Particularly those associated with transport and dissolution of diatoms and establishing robust chronologies for Southern Ocean marine sediment cores. Whilst I think this manuscript should be published, there are some areas of concern that I would like to see addressed, and think would help strengthen the manuscripts conclusions.

Thank you for your comments, and your work.

### Specific Comments

1. How did the authors determine which age model details were presented in the main manuscript and which were only in supplemental? For example, in section 2.5 biogenic silica, Si/Al, and IRD are listed as some of the primary data used in age model construction but only the first two have detailed methodologies in the main manuscript. I appreciate that the authors probably don't want to spend too much of the manuscript detailing all of the sedimentology, but I think the current separation could benefit from reassessment.

Answer: Yes, I agree with this point and have shifted the methodology and results for IRD data in the main manuscript (line 155 and 201, respectively). I have added IRD data into the age model figure (Fig. 2) to species distribution (Fig 3.) and to the final Fig. 5, instead of cell counts.

2. Robust age models for Southern Ocean marine sediment cores located so far south are often challenging and I largely agree with the logic used by the authors for the chronology in core Tan\_44. However, I think the age model would benefit from additional biomarker evidence (e.g., the last occurrence of *Rouxia leventerae* at the MIS 6-5e boundary). The authors themselves mention the problems with Antarctic ice sheet advance removing the deposited sediments, and the addition of biomarkers would help establish that the interglacial identified as MIS 5e isn't actually an older interglacial.

Answer: *Rouxia leventerae* wasn't identified in any of the slides. All slides (5-350 cm) were thoroughly analysed. However, I have added this fact in the Age Model section (line 222).

The solution to the question of how we can really know that MIS 5e isn't older, is to undertake additional diatom analysis of the deeper section of core Tan\_44, that is, from 350-630 cm, which includes older MIS 6 and MIS 7 interglacial.

3. I have a couple of points on the diatom preparation and counts. Firstly, the authors mention that for species that are highly fragmentary, only the ends were counted, was the same process applied to other pennates? Or were they only counted if >50% of the valve was present? If the latter, how did the authors ascertain they had >50% of the valve for broken valves of species such as *Fragilariopsis cylindrus*, which are linear and isopolar? Secondly, the counts are detailed as >400 valves but it is unclear when the count was stopped, did the entire slide need

to be counted, or did the count just continue until the 400 point had been passed? Without details on this it is hard to know how to interpret the diatoms per slide values given in Figure 2. Either way, I would still advise removing this metric from figure 2 and the discussion as it is highly qualitative given the method of slide preparation. Thirdly, I am somewhat confused by the criteria used to include or exclude species/groups from the analyses. Lines 202-3 imply that only species with >2% abundance throughout the core are included in the analysis, but figure 3 and the discussion clearly include species for which this isn't the case (e.g. *Actinocyclus ingens*)? For groups, seemingly the dominant species only needs to have >2% abundance in a single sample, which seems rather inconsistent. I would also caution the authors against grouping by morphology, for example within the *Thalassionema* genus there are substantial differences in environmental preference despite very similar morphologies.

Answer:

3.1 The ends of diatoms were counted only in case of *Thalassiothrix* group, this is written (line 246). For all other diatoms, including pennates, the valves were counted only if they were >50% of the whole (line 245). This means that in the case of *Fragilariopsis* species, the valve needed to be over >50% the length. Due to the small curve on the outer edge of the *Fragilariopsis* valve, this wasn't a problem to determine. The isopolar *Fragilariopsis cylindrus* was really rare but also the size of the valve can give us some clue about whether it is >50% of valve.

3.2 I agree the counts are relative, I have counted >400 per slide (line 245). Some slides I counted all of the slide while others I had stopped at a certain point well above 400. I have removed the number of valves per slide from Fig. 2, Figure 3 and from discussion (line 679-682), however I have left the discussion on barren intervals (line 678) and intervals where pyrite is found (line 685-690). I think both are important to mention due to the content/ that is, no content.

3.3 *Actinocyclus ingens* was found 11% and 3% abundance, within two different samples.

3.4 Species *Thalassiothrix antarctica*, *Thalassiothrix longissima* and *Triothoxon reinboldii* were grouped together due to very similar morphologies. They all constitute open ocean species with some difference in preference. I present this in Table S1 – for each of these species. However, in the results I present the group is dominated by *Thalassiothrix antarctica* which in number is probably highly underrepresented due to the inability to count its broken very elongated valves. I say this because this species occurs in very high numbers in the sample, in relation to others seen in the core- at 40 and at 270 cm (line 349-353).

4. For section 3.4 the authors argument would be strengthened by the inclusion of some p values to show the statistical significance of the regressions. Especially as, to me at least, the  $r^2$  values seem rather low for all of the regressions.

Answer: This analysis was changed from regression to correlation, which is more appropriate in this case as we are interested in the strength and direction of the relationship between variables, not the predictive ability of the specific relationship. The p values were added to indicate the significance of the correlation (line .468).

The paragraph in lines 408-24 feels rather contradictory. The authors seem to suggest both that there is significant reworking of the diatom assemblage, and that the assemblage is a faithful reconstruction of the overlying environmental conditions. The justification for why the authors consider this assemblage to be truly autochthonous needs to be made clearer. Otherwise the

reader is left questioning whether the PC1 assemblage can really be trusted any more than the PC3 for reconstructing environmental conditions.

Answer: All assemblages especially on the continental slope and shelf, are reworked to some extent. However, commonly the completely reworked assemblages contain only robust valves of certain species, and these have been defined in sediment, by Taylor and McMinn (1997), and Truesdale and Kellogg (1979). Assemblages which contain other species are therefore considered to contain in situ sedimentation as well as to some extent reworking.

#### Technical Corrections

Line 23 - It isn't specified whether it is a high or low *Eucampia* terminal/intercalary ratio associated with PC2. Corrected to 'high *Eucampia antarctica* index' (line 23).

Line 29 - Should be *oliveriana* not *oliverana* (mispelt throughout manuscript). Corrected in text (line 29; line 335; line 434; line 544, 546), Fig 3. And Fig. S5.

Line 130-1 - Are the anomalous spikes identified by statistical comparison to surrounding data or just by eye? 'By eye' is added to text (section 2.3; line 145)

Line 150 - Core site Tan\_68 is shown in Figure 1 but not reference at all in the manuscript. Tan\_68 removed from Fig. 1.

Line 157-8 - The lines showing the average position of the monthly sea-ice edge are not explained in the figure caption. I assume the lines are sourced from Fetterer et al. (2017) and the blue shading from Spreen, Kaleschke & Heygster (2008) but this also isn't made clear. This has been made clear in Fig 1. caption and in text (line 109-110).

Line 165 - There is no explanation in the main manuscript on what the D and R in the %microfossil row stand for. Explanation added in caption of Table 1.

Line 169 - Only two radiocarbon dates are mentioned but Figure 2 and Table S2 both contain 4. This has been explained in text (Age Model; line 214-217).

Line 374 - The PC3 and biogenic silica regression has an  $r^2 > 0.1$ . This section is changed – see answer to question 4, above.

Line 451 - I would consider *A. ingens* to also be fairly robust so don't think the except is necessary. This has been corrected 'except' is replaced by 'including' (line 549).

Line 589 - Should be "pyrite is". Corrected (line 688).

Line 607 - kyrs as one word. Corrected in text (line 705), and figures Fig. 2, Fig. 3 and Fig. 5.