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Interactive comment on "Temporal variability in foraminiferal morphology and geochemistry at the West Antarctic Peninsula: a sediment trap study" *by* Anna Mikis et al.

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We thank the reviewer for the positive comments, and excellent suggestions for how to improve the manuscript. We have responded to the points raised, highlighted in bold and italics below.

- p9 l1: Section 4.1: I do not understand why the authors assume that there is no vital effect whereas they have all the data to discuss it;

As we do not have the co-located water samples from where the foraminifera were growing, nor can we constrain the exact depth of calcification, we took

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the decision that it was not possible to measure the vital effect and, so, made the simplest assumption that this vital effect was negligible. This assumption allows the reconstruction of reasonable depths of calcification using seawater $\delta^{18}O$ profiles.

- p9 l1: Section 4.2: In the first sentence, the authors wrote: "there is no direct control of foraminiferal flux specifically due to seasonal changes in water column conditions" whereas in the Results section 3.1 they describe that "Nps test flux generally ranged over two orders of magnitude from zero in winter months to over 300 tests m-2 day-1 in summer", could you clarify this point?;

Apologies if this was not clear. Our point was that whilst there are seasonal changes in flux, there is also strong interannual variability, so that there is no simple relationship between season and foraminiferal flux. We have changed the first statement to:

"A qualitative view of our flux data reveals that, whilst there are generally fewer foraminifera in winter than summer, there is also pronounced interannual variability, indicating that there are complex controls on foraminiferal flux in addition to seasonal climatologies of water column conditions."

- p10 l4-6: Section 4.3.1: I do not understand why the authors discarded the size-specific kinetic/metabolic effects on δ18Onp ;

We have added to this discussion for clarification:

"There is a consistent size effect on both the $\delta^{18}O_{np}$ and $\delta^{13}C_{np}$ across all our data ($\delta^{18}O_{np}$ r = 0.52; $\delta^{13}C_{np}$ r = 0.23, n = 191) which is only weakly maintained for $\delta^{18}O_{np}$ when divided into the 150-250 μm (r = 0.28, n = 89) and >250 μm (r = 0.32, n = 102) fractions. In addition, there is no significant offset between the two size fractions with mean $\delta^{18}O_{np}$ values of +2.72 $\pm$ 0.59 and +3.21 $\pm$ 0.34 for the 150-250 μm and >250 μm size fractions respectively (1SD)."

- Section 4.4 : maybe the authors could add some informations about the seasonal cycle of the diatoms production in the area, is there any literature about that?

Given that we have Chl a data, not diatom data, we have decided not to include too much additional information about diatom seasonality. However, they form an important source of food for foraminifera, as stated in the text, and agree that an additional sentence would be useful, as - broadly speaking - the Chl a pattern does follow what would be expected from seasonal diatom production. We have added om page 4 line 7:

"The seasonal progression of Chl a in Antarctic fjords is consistent with a diatom-dominated phytoplankton (Cape et al., 2019; Pike et al., 2008 and references therein; Montes-Hugo et al., 2009)"

As more general comments:

- I think that the δ13C record could be better interpretetd/used: is there any relationships with the chlorophyll maximum ? With the nutrient proxies ? The primary productivity ?

We have now added to page 8:

"There were no significant seasonal differences in variance between samples in the single-specimen $\delta^{13}C_{np}$ dataset (Levene's test, p=0.076), and no links with indicators of primary production."

- You should discuss the potential impact of the carbonate ion concentration on the shell thickness as well as on the δ18O;

We have not discussed the impact of carbonate ion concentration on either shell thickness or $\delta^{18}O$ due to a lack of data with which to draw comparisons. However, this is a fair point as there are a few studies suggesting that there are subtle changes to both parameters with changing carbonate ion concentration, and we have added the following on page 7:

"Whilst temperature alone has no detectable influence, a combination of temperature and pH changes are known to impact calcification rate in both juvenile and adult Nps (Manno et al., 2012). However, as we do not have carbonate ion concentration data available for this location and time period, we will restrict the interpretation of grey value, and so shell thickness, to calcification changes relating to ontogeny."

And on page 8:

"Note that carbonate ion concentration has only a small impact on foraminiferal $\delta^{18}O$ ***(0.002 to 0.004 ‰ per*** $\mu mol/kg$ ***for Globigerina bulloides; Lea et al., 1999) and so cannot explain the range in values observed."***

- What consequences the results of this work can have for paleoclimate studies ?

We have included a brief discussion on the implications for paleoclimate studies in the synthesis section on page 15.

To finish, a very small error appears p7 l18: "peaks" written 2 times.

Done

Despite these few comments/suggestions, I think this paper is a very nice contribution to the better understanding of the carbonate productivity in this area, and to the ecological constrains on N. pachyderma.

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