

## ***Interactive comment on “Fatty acid carbon isotopes: a new indicator of marine Antarctic paleoproductivity?” by Kate Ashley et al.***

**Anonymous Referee #1**

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Ashley et al. present an assessment on the usefulness of  $\delta^{13}\text{C}$  of fatty acids to assess paleoproductivity in an Antarctic coastal setting. The manuscript is well-written, the data appropriate and extensive, and the research question interesting and relevant. The rationale for this work is fully explained, the introduction is clear and the methodology is sound. The main results and discussion section is generally clear, but not enough attention and focus is given to linking the data to productivity. At present, it almost looks like productivity was chosen because the trends could not be explained by anything else. I am sure this is not the case, but it needs to be made clearer for the reader as well.

There are a few criticisms I have which ought to be addressed before this manuscript is ready for publication.

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1. The manuscript is focusing on one specific site, and while the observed links to productivity are observed here, the site is very particular and in no way is this ready to be extrapolated at all to any other sites in Antarctica or any other settings. Hence, the title is a little presumptuous, while at the same time the phrasing as a question makes it vague. The phrasing of “fatty acid carbon isotopes” won’t be valued by some in the isotope community as it can sound a little bit colloquial. I would suggest changing to “d13C of fatty acids trace paleoproductivity off the coast of Adelie Land, Antarctica” or something along these lines. 2. The manuscript gives a lot of space for trying to pin down a single, or majority, producer, for fatty acids such as C18. I think this is impossible as so many organisms produce C18 FA, and thus this discussion can be shortened and focused. 3. The changes observed in d13C are very small and some comments on how significant changes of 1‰ really are would be useful. 4. I can see a number of analytical issues that should be addressed. First of all, there is no explanation on how the correction for the methyl-group 13C values was carried out. This needs to be explained, or, if the C used for methylation has not been analysed for 13C and is not available anymore, and it is thus impossible to make this correction, it needs to be clearly acknowledged that values are not absolute. The second issue is that the standard used (C19) is not the best for FAME as it is an n-alkane, and was only added post-extraction, hence analysis is semi-quantitative at best which needs to be made clearer. 5. Throughout the manuscript, often words such as “extremely”, “very high”, etc. are used – I would recommend a thorough edit removing these descriptions and replacing them with actual values that allow the reader to put them into context.

Line 68: Give a number instead of “extremely high” – how high? Line 70: “highly productive” as above Line 94: See comment 4 on internal standard – when was it added? Does it really allow quantification at this point? Line 97: Are these values corrected for Me? Are these errors subsequently appropriately propagated? What is the significance of a change of just above 3 x SD (0.26 vs 1 ‰)? Line 102: Which internal standards? Line 194: Saying that a marine source is “entirely possible” sounds strange – do you want to say likely? Lines 213-214: There are more novel studies on FA, Wakeham and

also Hilary Close Line 291: What do you mean by weaker coherence? Lines 547-549: We know that there are many algae that make these FA so this is not likely to be resolved. At the same time, the non-distinctive nature of these molecules will make it difficult to apply this proxy to other settings where there are likely other producers. The whole paragraph is not particularly relevant and I would shorten and/or delete or move up so the work does not finish on a weak statement.

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