

Interactive comment on “Validation of carbon isotope fractionation in algal lipids as a PCO_2 proxy using a natural CO_2 seep (Shikine Island, Japan)” by Caitlyn R. Witkowski et al.

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We thank the reviewer for the comments and recommendation for publication. Below we will respond to each of the comments (italics), which will improve the manuscript.

Line comments: ...I would like to ask the authors to use continuous line numbers in the future, as is standard practice.

We used the Biogeosciences format which specifies using the numbering shown in this manuscript (though we also prefer continuous line numbering).

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Page 4, 1: Why were the filters combusted only at 300 C for 3h? Standard practice is 450 C for 5h or similar.

This temperature is sufficient to remove all background molecules which potentially can contribute the lipid pool we investigated. Indeed, higher temperatures are needed in case one wants to have a completely carbon-free filter.

Page 5, 6-8: Unclear if the reported pCO₂ values (is this dissolved CO₂?) are taken from the literature or are original data. If these are original data, the authors need to state in detail how pCO₂(aq) was calculated. If these are literature values, and not measured from the same samples as the d¹³C-DIC values, the authors need to state why they consider these values to be adequate for comparison with their samples (both in a spatial and temporal sense).

This data is reported in other studies (e.g. Agostini et al., 2015; 2018; Harvey et al., 2018) where it is specified how they came to these values, i.e. calculated PCO₂ based on the carbonate chemistry parameters of the bay (using the program CO₂sys). We will briefly expand on this section.

Page 5-6: The authors should include all data as either a main text table or supplementary table/data file, containing d¹³C-DIC, d¹³C-CO₂, d¹³C of biomarkers etc.

We will include a main text table with the values used to reconstruct PCO₂ at this site.

Page 8, 11-12: Is it reasonable to assume a constant temperature? Is there no seasonality in primary productivity at this site?

Here, we use a constant temperature because the surface sediments are an integrated accumulation of all primary productivity over the year. Although primary productivity is higher in the spring and summer, this site has some (observational) productivity throughout the year.

Page 8, 25-Page 9, 21: Here you could discuss the recent paper by Badger et al.

(Climate of the Past, doi. 10.5194/cp-15-539-2019) suggesting insensitivity of alkenone ^{13}C at low-mid $p\text{CO}_2$ levels.

Badger et al. shows an insensitivity to the alkenone proxy at low CO_2 values (<400 μatm), but here our general biomarkers do reconstruct the correct (low) control values and rather show insensitivity to the higher PCO_2 sites. We will briefly discuss this citation in the revised manuscript.

Page 9, 18: “annually”

This will be changed to “annually”

Page 9, 28: I would suggest being more cautious with the wording (“likely”) here. Can you provide evidence to support your argument for allochthonous input? Where would this come from?

We use the word “likely” here as we do not have independent support for the input of allochthonous organic matter. That material would come from surface sediments transported from the edge or outside of the bay where CO_2 levels are much lower than near the CO_2 seep. Since this is not a very large distance (500 meters) we can imagine that strong circulation events like typhoons would resuspend surface sediments and transport them to near CO_2 vents.

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