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Biogeosciences Discussions

Interactive comment on "Ecology and biogeochemistry of contrasting trophic environments in the South East Pacific by carbon isotope ratios on lipid biomarkers" *by* I. Tolosa et al.

Anonymous Referee #1

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This manuscript describes distributions of lipids in particulate matter from the eastern South Pacific along a transect from the Marquesas to the coast of Chile, along with stable carbon isotope measurements of selected biomarkers used to evaluate isotope fractionation effects and growth rates. The conclusions (p 4673), that upwelling systems have high biomass, specialized carbon concentration mechanisms and high growth rates, in contrast to oligotrophic areas, are not novel, yet the data presented are for the most part sound and do demonstrate these features. There are a number of points that the authors need to strengthen, as listed below.

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The abstract should list those biomarkers that represent the different taxa (rather than just saying “the diatom marker”, etc).

In the methods section, two sizes of PM were collected, on a Nitex screen and on a microquartz filter. Were both size fractions analyzes, together or separately? This is important since diatom aggregates might be preferentially collected on the Nitex whereas coccolithophorides and dinoflagellates (if not incorporated into large aggregates) might be enriched on the microquartz filter; same with a potential discrimination of bacterial biomarkers (preferentially on the microquartz?) and zooplankton biomarkers (preferentially on the nitex?).

A major concern is the presentation of concentration data for the biomarker lipids, but without reference to some normalizing factor, such as POC. Are concentrations peaks shown simply because there is more biomass (or POC) at certain depths, or because certain compounds are specifically enriched in the POC? In addition, reference is made throughout to how a profile for a specific biomarker is related to the chlorophyll a profile, but it is not clear that the chl-a profiles are shown anywhere. Likewise, except for the 19’-hex, what about the other diagnostic pigments that are referred to?

On p 4663, is should be made clear that C25-HBIs are not markers for all diatoms (i.e. there might be an offset between diatom sterols and the HBIs depending on the diatom species composition), nor do all haptophytes produce alkenones. More to the point, later in the paper, relative abundances of biomarkers are used to estimate relative abundances of phytoplankton taxa. This is actually quite difficult since the origins of some biomarkers are diverse, and in fact the abundances of compounds in different algae might vary considerably. So really all one can say is that abundances of the biomarkers vary and this might suggest more or less of the source alga. For example, from alkenone abundances, can one really extrapolate to relative abundances (in the sense of more haptophytes vs fewer diatoms) of haptophytes, or really only E. huxleyi; and vice versa?

BGD

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As noted above, the Summary and Conclusions needs to be strengthened – what is really new and exciting?

Once these items are considered, then a revised manuscript might be ready for final publication in Biogeosciences.

Interactive comment on Biogeosciences Discuss., 4, 4653, 2007.