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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.

A. Benthien (Referee)

Albert.Benthien@awi.de

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This manuscript presents the distribution of lipid biomarkers and their carbon isotopic composition in particulate matter from the South East Pacific. The samples were taken at six different sites with contrasting trophic environments along a transect between Tahiti and the Chilean coast. The authors demonstrate that the upwelling areas are characterised by a dominance of diatom-related lipids whereas the in the oligotrophic areas haptophyte lipids (as the authors call them; see comment below) were proportionally more abundant. The carbon isotopic fractionation (ep) is discussed with respect

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to the effect of CO2 concentration, nutrient availability and growth rates. The paper provides an impressive data set and the results are discussed solidly. However, there are a number of issues which should be thoroughly addressed before the manuscript can be considered for publication.

1) In order to estimate the growth rates from alkenone producing haptophytes the authors are applying equation 5 found in Bidigare et al., 1997 which is effectively (the calculation of) the b-value divided by 138. The authors should be extremely cautious to do so since this equation is based on the result of a nitrate-limited chemostat culture grown under continuous light conditions and cannot be simply extrapolated to field conditions with varying light and nutrient levels. This becomes evident by looking at the resulting growth rates listed in Table 5. For the sites upw and upx (both upwelling areas), the authors estimated growth rates of 1.5 to 1.7 d-1 for alkenone producers in 40 to 100 m depth. This is more than unlikely! Even though there is no information about the light conditions at these locations one can assume that in an upwelling area the lower end of the euphotic zone (defined by 1% light level) is between 30 and 50 m, if at all. Thus, at these depths I would expect growth rates close to zero. The high growth rates presented here are mainly the result of the high CO2 values at these depths inserted into Eq. 5 and, to a minor extend, of the low ep-values. Low ep-values in E. huxleyi, however, could not only be the result of high growth rates but also caused by low light levels which in turn cause low growth rates (cf., Rost et al., 2002, L&O 47, 120-128). That changes in nutrient- and light-limited growth rates have opposite effects on certain patterns of isotopic fractionation in marine phytoplankton has also been shown in a theoretical model by Cassar et al. 2006 (GCA 70, 5323-5335).

2) In the context of the comments above I suggest to avoid the use of equation 3 which is based on the assumption that marine phytoplankton obtain CO2 (as the only carbon source) solely by passive diffusion. This was the state of knowledge 5-10 years ago. In the meantime, however, various laboratory studies as well as theoretical considerations have demonstrated that carbon isotopic fractionation is affected by a variety of factors,

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including growth rate and CO2 concentration but also by the kind of growth limitation, active uptake of bicarbonate and CO2, various forms of CCMs an so on. As to my knowledge, there is no investigated marine phytoplankton species which does not use a CCM or take up bicarbonate. If the authors want to use an equation to describe the overall effect on carbon isotopic fractionation (ep), I suggest using the model of Sharkey and Berry 1985 which was later extended by Burkhardt et al. 1999 (GCA 63, 3729-3741). In this model ep is determined by the isotopic composition of the carbon source and the leakage (L) defined as the ratio of carbon efflux to carbon influx (L = F out/F in):

ep = a*es + ef*(F out/F in)

where a = fractional contribution of bicarbonate to total C uptake, es = equilibrium discrimination between CO2 and bicarbonate, F out = carbon efflux, F in = carbon influx.

3) Regarding the calculation of ep it is unfortunate that the carbon isotopic compostion of DIC has not been measured (p. 4661). To deal with this problem, the authors use a constant value of 2.2 per mill for the 13C of bicarbonate. However, is it reasonable to assume a constant value for the different oceanographic regions (upwelling vs. oligotrophic) and different depths? I suggest that the authors discuss how the potential errors of this assumption would affect the calculated ep values. In the context of estimating ep, the authors should also take into account that the isotopic difference between the lipid biomarker and biomass is not always constant. Does this uncertainty have an effect on the interpretation of the results?

4) In Fig. 7 the authors show that the estimated temperatures based on the UK37 index were overestimated by 2-3°C. They argue that this phenomenon might be the result of nutrient limitation as found by Epstein et al. 1998 in E. huxleyi batch cultures (p. 4669). Also using batch cultures, however, Prahl et al. 2003 (Paleoceanography 18, doi:10.1029/2002PA000803) found the opposite effect, namely decreasing UK37

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values (0.11 units or ˜3.2°C) under nutrient limitation. But in the same study Prahl et al. observed increasing UK37 values in light-limited cultures of E. huxleyi (+0.11 units). Thus, according to the results of Prahl et al. the observed overestimation by alkenone unsaturation in the present study might be the result of light limitation rather than nutrient limitation. These findings should be discussed in the context of the oceanographic conditions. In this regard I am wondering why the authors did not estimate or present UK37 values for the other sites of this study (mar, hnl, egy, upw, upx) since they were able to measure 13C on alkenones.

Minor comments:

1) Often in the text (e.g. p. 4663, l. 19; p. 4670, l. 27) huxleyi is capitalised.

2) p. 4668, l. 21: UK37 is not a growth index.

3) p. 4669, l. 19: fractionation instead of fixation

4) p. 4670, 4695, and 4696: in the text the authors use the correlation coefficient r (uncapitalised), in the Figures (8, 9) they use R-squared.

5) p. 4667/4668, l. 23ff: Alkenones are not a marker for haptophytes in general but for very few haptophyte species, namely E. huxleyi and G. oceanica (at least in open marine environments). So, why should cellular alkenone concentrations vary with the species composition of the coccolithophorid assemblage

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