

***Interactive comment on “Surface circulation patterns and the pathways of sea surface carbon dioxide (CO<sub>2</sub>) off northern Chile (~27.5° S) between 30 and 10 kyr BP: global and/or local forcing?” by J. A. Placencia et al.***

**Anonymous Referee #2**

Received and published: 11 June 2010

Placencia et al. aim to reconstruct CO<sub>2</sub> fluxes in the coastal upwelling waters of northern Chile from carbon isotopes in alkenones. The authors go through great lengths of trying to tie this record into other local and global climate records. However, the carbon fractionation into alkenones is affected by many parameters, including the carbon isotopic composition of seawater, nutrient levels, growth rates and cell size, where the influence of the physiological factors is often summarized in a constant factor “b”. The authors acknowledge this factor and calculate an average value from literature values and assume the modern seawater phosphate concentration was constant in the past.

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I am concerned about this approach and the result for several reasons:

1. The authors argue that upwelling intensity has changed throughout their record, thus resulting in a higher source of CO<sub>2</sub> to the atmosphere. If the upwelling intensity changed, I would expect higher nutrient concentrations, thus leading to higher growth rates and a decrease in photosynthetic carbon isotope fractionation. It is hard to distinguish between the nutrient and PCO<sub>2</sub> effects without a nutrient proxy, and therefore the absolute CO<sub>2</sub> estimates and variations through time are uncertain.
2. For estimates of the carbon isotopic composition of seawater the  $\delta^{13}\text{C}$  measured in shells of the deep dwelling planktic foraminifer *N. pachyderma* is applied. The comparison between  $\delta^{18}\text{O}$  of *N. pachyderma* and alkenones revealed that the temperature estimates are not comparable, and the authors argue that stratification may explain the difference. This argument implies that *N. pachyderma* and the alkenone producers did not share the same water mass, which makes me question the applicability of these foraminiferal  $\delta^{13}\text{C}$  values to estimate the  $\delta^{13}\text{C}$  of the seawater that the alkenone-producers have lived in. This adds to the uncertainty of the CO<sub>2</sub> estimates.
3. A recent alkenone CO<sub>2</sub>-reconstruction by Pagani et al. 2010 produced 6 Plio-Pleistocene CO<sub>2</sub>-records from various core sites in oligotrophic and mesotrophic regions of the Atlantic and Pacific Oceans. Of those records only 2 yield CO<sub>2</sub>-estimates in the general range of Pleistocene ice core measurements, whereas all others (including the oligotrophic sites, which should be in equilibrium with the atmosphere) show much higher CO<sub>2</sub> estimates. Although the general trends of those records are similar in that they all suggest higher Pliocene pCO<sub>2</sub>, the difference between the records makes it clear that our understanding of the alkenone-PCO<sub>2</sub> proxy is insufficient to allow for interpretation of absolute atmospheric CO<sub>2</sub> or surface seawater CO<sub>2</sub> from a single site. In this regard it would be extremely important to test this new Chilean record relative to the preindustrial, i.e. extend the record throughout the past 10 kyr. Such an extension would provide greater confidence into the reconstruction, if the coretop estimates matched preindustrial estimates for this site. With Figure 1 the authors seem to make an attempt to test the data relative to modern observations but it falls short in many ways: Panel A and B: Arrows have

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been drawn into these figures to suggest similarities but the timing and magnitude of change is actually very different. In addition, modern SST at the core site is 3°C colder than the alkenone estimate at 10 kyr. How can that be explained? Overall the SST and PCO<sub>2</sub> estimates do not seem to agree with the upwelling interpretation: Upwelling would bring colder temperatures and higher PCO<sub>2</sub> to the surface but the SST estimates are warm when the PCO<sub>2</sub> estimates are high. Also, modern atmospheric pCO<sub>2</sub> is not the relevant quantity to compare the aqueous PCO<sub>2</sub> estimate to, as this is an upwelling region where PCO<sub>2</sub> is typically higher than atmospheric pCO<sub>2</sub>.

In summary, I find this reconstruction not very convincing but the conclusions that are drawn from it potentially misleading. It would be useful to see cross plots of CaCO<sub>3</sub> flux versus foraminiferal δ<sup>13</sup>C, *G. bulloides* abundance and SST, to gain a better sense of the significance of these observations. For instance, Figure 2 does not seem to show any correlation between inorganic/organic carbon vs. CO<sub>2</sub>. The suggestion that “marine DIC was captured intensively by the carbonate system, enhancing the carbonate precipitation of the calcifying organisms in the plankton assemblage” has important consequences for the ocean acidification debate. If this observation were correct, it would suggest that ocean acidification has a positive rather than a negative effect on marine calcifying plankton. This would be an important result but such a statement should be based on solid arguments, and not on a weak basis such as the underlying reconstructions.

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Interactive comment on Clim. Past Discuss., 6, 347, 2010.