

Interactive comment on “Algal constraints on the Cenozoic history of atmospheric CO₂?” by J. Henderiks and R. E. M. Rickaby

Anonymous Referee #1

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Comments on “Algal constraints on the Cenozoic history of atmospheric CO₂?” by J. Henderiks and R.E.M. Rickaby

General comments:

The very well written manuscript introduces a new methodology for placing an upper constraint on pCO₂ over the Cenozoic. The argument is based on literature data: pCO₂ reconstructions and culture experiments using four coccolithophore species. Although the manuscript does not contain new data it provides a new and interesting way of looking at past carbonate chemistry of seawater. Therefore, the manuscript should be published, but two crucial issues need to be considered:

Specific comments of major importance:

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Firstly, the authors discuss the upper constraint on pCO₂ only, but the data of the culture experiments also allow for placing a lower constraint. In the case of *C. pelagicus* this means that 63 Ma ago there might have been a pCO₂ of 150 μatm. That obviously contradicts “classic” pCO₂ reconstructions. Consequently other proxies, apart from the one presented in the manuscript, are needed to decide whether an upper constraint or a lower constraint makes sense. This is no flaw of the logic, but should be stated in the text explicitly.

Secondly (and most importantly), the response of *C. leptoporus* predicts a pCO₂ of roughly 360 μatm for the last glacial maximum, because the upper constraint equals the lower constraint. This prediction is clearly wrong. Therefore, in contrast to *C. pelagicus*, *C. leptoporus* cannot be used to reconstruct past pCO₂. The response of *C. pelagicus* to changing carbonate chemistry could well have been the same in the last 63 Ma, but the response of *C. leptoporus* must have changed since the last glacial maximum. Why? This question is crucial to the logic of the argument and needs to be addressed.

Specific comments of minor importance:

The following statement in the discussion is not plausible: Page 3, line 24-27 “Rather than a calcification dependency on saturation state, pH or [CO₃²⁻] alone, the critical factor determining the calcification tolerance of different species may be the photosynthetic capacity to harvest energy from CO₂”. Experiments using *E. huxleyi* and *G. oceanica* (Zondervan et al. 2001) show that these species increase POC production with increasing CO₂ while calcification is decreased. Hence the diminished calcification is not attributable to an energy deficiency. Moreover, calcification rate is correlated with coccolith morphology in all species tested so far (Langer et al. 2006, Riebesell et al. 2000). This points to a problem related to regulation of coccolith production rather than energy requirement.

Page 4, line 4 “Is evolutionary capability preserved without degradation?” The authors

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answer this question with yes. The argument is based on the observation that the modern counterpart of an “ancient” species, *C. neohelis*, still produces low Mg-calcite (in the high Mg/Ca modern ocean). However, the ability to produce low Mg-calcite is not determined by the Mg/Ca ratio of the seawater at the time of the species emergence, but rather by strong fractionation against Mg during Ca uptake, which is important for cell physiology. Some species have evolved in a high Mg/Ca ocean and nevertheless produce low Mg-calcite, e.g. *E. huxleyi* and *G. oceanica* (Stoll et al. 2001, G3). Hence, the preservation of evolutionary capability cannot be inferred from the data of Stanley et al. 2005.

Technical corrections:

Page 7, line 7. One of the authors is named “Kläs, J.” and not “Kls, J.”

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