



Interactive comment on “The effect of temperature and salinity on the stable hydrogen isotopic composition of long chain alkenones produced by *Emiliana huxleyi* and *Gephyrocapsa oceanica*” by S. Schouten et al.

L. Beaufort (Referee)

beaufort@cerege.fr

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Review of "the effect of temperature and salinity on the stable hydrogen isotopic composition of long chain alkenones produced by *E. huxleyi* and *G. oceanica*" by Schouten et al. By Luc Beaufort beaufort@cerege.fr

General comments : This is a very interesting experiment: the use of dD on alkenone appears to be in the near future an excellent marker of salinity, as several laboratories are developing it around the world. It is particularly important to conduct such an experiment on different species. This experiment is well conducted and the data appears to be of good quality. The paper is clear and concise. The results are significant. Be-

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cause of the importance of this work (there is no direct proxy for salinity at the moment) and its quality, it should be published with only minor revisions.

Specific comments : About the text However the style is often difficult to follow and the paper is written for specialists who know well the few articles cited. Some parts, such as the mathematical one, are tedious to understand. Adding some sentences of explanation would help.

About the data The regression between the fractionation factor and salinity is impressive, but I am curious about temperature : I do not understand why only few points of the fractionation / temperature are plotted in fig 2a. If I agree that there is no linear correlation between the two it appears however, that there is a large scattering of the data in particular at 25 PSU. Why that ? Does this scattering alter the salinity estimates ? does a non linear relation exist with temperature(a “U” pattern) ? Is it the same for the data that are not presented in this figure ? Does the scatter in the data are similar for *G. oceanica* and *E. huxleyi* ? (which species is plotted in fig 2a : the caption indicates it is for the two species but this is obviously not the case). I do not have the time to tabulate the table. I think a discussion on temperature should be extended.

About the discussion There is not only two species of Noelarabaceae in the present ocean, but at least 5 and certainly much more: 21 species of *Gephyrocapsa* have been described, many of them are synonymous but this show the large morphologic variation in this genus. Specialists agree on at least 4 species of *Gephyrocapsa*. *Gephyrocapsa ericsonii* and *G. carriberannica* can be extremely abundant in some place and/or in the past. All of them are potential alkenone producer. Their relative abundance varied significantly (from dominance to absence) in relation to environmental condition including salinity. If these species have different fractionation factors, as this experiment demonstrate for two of them, it will be a necessity to differentiate the relative contribution in alkenone of the different producers. I am afraid that even if this is possible, the error of such analysis will propagate into the salinity estimates.

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In the same vein: the analysis has been conducted on only 1 strain of each species, but there are at least 3 morphotypes of *E. huxleyi* (potential cryptic species); their fractionation factor should be tested for each of them to check intraspecific variation of the fractionation.

The last part (p.1690 I.16), discussing an “alternative approach” is not easily understandable. It should be either discussed in more detailed or withdrawn. In particular I do not understand how different species having different offset (which is the case for this experiment) could help in deconvolving salinity? Show an example from your data set !

Technical correction : Typo : Saches et al. (2005) in the text, instead of Sachse et al correctly spelled in the reference list.

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