Biogeosciences Discuss., 10, C7782–C7785, 2014
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Biogeosciences
Discussions

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10, C7782–C7785, 2014

Interactive
Comment

Interactive comment on "The role of ocean acidification in *Emiliania huxleyi* coccolith thinning in the Mediterranean Sea" *by* K. J. S. Meier et al.

Anonymous Referee #2

Received and published: 9 January 2014

Meier and co-workers investigate the weight of E. huxleyi coccoliths collected in sediment traps and taken from sediment cores in order to understand if weight has already been affected by environmental change. Sediment traps were deployed for approximately 13 years (1993-2006) in the north-western Mediterranean Sea. Sediment cores were collected close to deployment site and contain material dating back 10,000 years into the past. The development of coccolith weight over time presented in this study is interesting and a highly valuable contribution to this research field. I am not entirely convinced, however, by the authors' major conclusion that ongoing ocean acidification is already reducing coccolith weight in this ocean basin.

Major concern: I wonder if the data fully support the authors' major conclusion. i.e.

(e.g. page 19715, lines 22-23): “This natural E. huxleyi assemblage shows a response to OA”. I find evidence in the dataset which are not in support of this conclusion.

1) Changes in carbonate chemistry conditions during the 13 years of deployment are extremely small. pH changes by 0.02 units, CO2 by 1 μmol kg-1, and CO32- by only 7 μmol kg-1 which seems to be too small to explain the prominent decrease in coccolith weight from 5 pg to almost 3.5 pg. Beaufort et al., (2011) report much smaller weight differences over the same CO32- range.

2) Measurements from November 1993 to April 1994 reveal similarly low coccolith weights as has been measured at the end of the deployment in 2006. Low weights were also found in summer 2000 and 2004 as well as autumn 2005. It is argued that the three low weight periods in the early 2000s are caused by unusual mixing events and can therefore be excluded. But this does not seem to be the case for the low weight period in winter 1993/1994. So why should these datapoints be excluded from the analysis? There is no sound explanation for that given in the text. If you include these data points in the analysis than there seems to be a fluctuation from low weight until 1994 to higher weight from 1994-2000 and back to low weight in 2000. A consistent negative trend can only be seen when excluding the 1993/1994 data.

3) Fluctuations in carbonate chemistry conditions are much more pronounced on a seasonal scale. CO32-, for example, fluctuates by almost 30 μmol kg-1 from summer to wintertime which is four times more pronounced than the change from 1993 to 2006. Still, coccolith weight does not follow these fluctuations. In fact, it shows the opposite trend with higher weight at lower CO32- concentrations.

4) The sediment core data support conclusions by Meier and co-workers because pre-industrial coccolith weight is consistently lower than it is today. However, this evidence seems to be too weak to allow such a critical conclusion. In fact, this conclusion should be backed up by the sediment trap data but this does not seem to be the case as outlined in 1-3.

Minor concerns: Abstract.

1) Lines 4-5. "The release of several thousands of petagrams of carbon over a few hundred years will overwhelm the capacity of the surface ocean reservoirs to absorb carbon." This is not fully correct. The ocean cannot be "overwhelmed" with carbon but will always equilibrate with the atmosphere no matter how high the atmospheric pCO2 will become.

2) Line 9. The term "calcification" should be specified. Do you mean calcification rates or coccolith weight? Please be precise on this throughout the whole manuscript.

3) Line 24. What is meant by coccolithophore production? Please specify.

Introduction.

1) Page 19703, Line 17. Riebesell et al., (2000) investigated two different species but not two different strains.

2) Page 19703, Lines 17-23. It is argued in this section that a particular haplotype is responsible for increasing calcification rates measured in some experiments. I think this description is overly simplistic. Thinks are probably much more complex than that (compare e.g. Iglesias-Rodriguez et al., 2008 and Hoppe et al., 2011).

3) Page 19703, Line 28. Beaufort et al. (2011) did not measure calcification rates. The reference should not be used here.

4) Page 19704, Line 25. "These oceanographic features. . . ." I think these are rather chemical than "oceanographic" features.

5) Page 19704, Lines 24-26. "These oceanographic features make the Mediterranean a natural laboratory to study the effect of anthropogenic acidification on calcifying organisms." In fact, CO2 invades the ocean not only in the Mediterranean. That is why it is misleading to call it a "natural laboratory". Furthermore, the Mediterranean is a rather unrepresentative ocean basin to study the effect of OA on marine organisms

because of its high total alkalinity. This sentenced could be rephrased. Material and methods.

1) Page 19707, Lines 21-22. “Missing measurements were replaced with values obtained from linear regression of the measurements from above and below.” I do not understand what is meant by “above and below”. Calculations of the carbonate system should be explained in more detail. They are not described particularly well in the current version of the manuscript.

Results and discussion.

1) Paragraph 4.3.1. The detection of a regular seasonal pattern in coccolith weight is highly interesting and deserves to be highlighted. The correlation between coccolith weight and nutrient/productivity patterns is also good enough to speculate that there may be a connection. I wonder, however, if the Authors could extend their discussion on this topic. Seasonality is a very broad term. What could specifically be responsible for the regular pattern. Are there different E. huxleyi populations with one dominating in spring/summer and the other dominating during autumn/winter time?

2) Paragraph 4.3.2. Page 19714, Lines 12- 18. “ The SSA analysis conducted on this data, coccolith weight and other environmental data reveals significant trends only for coccolith weight and carbonate system parameters, whereas temperature, nutrients, and salinity present a limited variability (Fig. 7). Therefore, the most likely cause for the observed mas loss of E. huxleyi coccoliths is the observed change in surface water carbonate chemistry.” I wonder why changes in carbonate chemistry are the most likely cause for the observed decrease in weight? There can be many other parameters (which have not been measured in this study) which can explain the decreasing trend.

3) Paragraph 4.3.1. See major concerns.

Interactive comment on Biogeosciences Discuss., 10, 19701, 2013.