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Biogeosciences Discussions

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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.

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This manuscript by Meier and colleagues offers an impressive, 12-yr sediment trap time series from the Gulf of Lions (Mediterranean Sea), focused on the variation in coccolith size and mass of the most common species Emiliania huxleyi. Interesting patterns emerge from the in situ "monitoring" of this taxon. The coccolith data are compared to several physicochemical parameters, measured (or inferred) over the same time interval, December 1993 to January 2006. The data demonstrate highly interesting seasonal (monthly average) patterns, and longer-term trends.

The present manuscript will need major revision to clarify data treatment, expand on statistical analytical methods and substantiate the main conclusion that "change in the surface water carbonate chemistry is the most likely parameter that has caused the decrease of E. huxleyi coccolith weight below Holocene and industrial values in the western Mediterranean Sea" (p. 19715, lines 9-11).

Figure 5 is probably the most informative representation of the 12-yr time series data, revealing the monthly mean variations in coccolith weight and all investigated physico-chemical parameters, and yearly deviations (anomalies) from the monthly mean (thin bars). The raw time series data reveal that seasonal (month-to-month) variations are in general larger than the inter-annual variations recorded for the abiotic parameters. The interannual variation in coccolith mass and size, however, appear larger than seasonal variation (compare Fig 2 B/D and Fig 4).

Major concerns 1. Statistical treatment of the data

The authors need to clarify (1) how they interpolated values for data gaps due to lack of sample availability, and (2) how they dealt with the data gaps created by themselves, by selecting out certain time intervals of "low counts" and "extremely low average weight" (these data are shown in Fig. 2, but omitted in the Supplementary database).

The authors describe the relationships between abiotic factors and E. huxleyi mass – by visually evaluating smoothed data and single spectrum analysis (SSA) of the raw data series. This is another major concern: the manuscript lacks any statistical testing of covariation/correlation between the presented time series.

For example, statements like "oceanographic features of the NW Mediterranean Sea are well reflected in the weight and length of E. huxleyi coccoliths collected by the sediment trap investigated here (Fig. 4)." (p. 19711, section 4.3), and "The seasonal variability in temperature, salinity, nutrient concentration, and the carbonate system parameters is clearly expressed in both the raw measurements and the SSA extraction of the seasonal signal (Figs. 4 and 5)" (same page, l. 11-13), are not substantiated by statistical tests, determining covariation/correlation, but by wordy descriptions of

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how various parameters "possibly" relate to coccolith length and/or mass (or, rather how previous studies have interpreted results). Plotting paired boxplots comparing the latest 2,5 yrs to the decade prior to these, in Fig. 6, may summarize the pooled data, but doesn't constitute a valid test either.

Bottom line: all parameters investigated show seasonal patterns and inter-annual variations – the question is how each of these relate to another in this particular, very interesting data set.

The fact that seasonal (month-to-month) variations in physicochemical parameters are much larger than the inter-annual variations calls for an in-depth analysis of these, before jumping to interpreting the "long-term" trends (which, in the end are cooked down to (12-yr) "trends from the SSA", Figure 7).

2. Missing out on alternative interpretations?

The information that was culled from the final data analysis ("low counts" in years 2000, 2004 and 2005) may indeed reveal some very interesting clues on ecological dynamics in addition to (seasonal) abiotic influences on E. huxleyi (see Fig. 2 and p. 19709, l.16-29). Could this time series potentially reflect an overall "regime" shift after the summer of 2000, as mean weight and size became overall lower/smaller and you infer "decreased seasonality" (yellow bars in Fig. 2) since then (and not before)?

The frequency of such "disturbance" periods is arguably increasing. What were the conditions at the start of the trap deployment, 1993/94, when you record similarly low counts?

Could we be looking at some threshold/step changes, rather than a "long-term trend" as implied in your conclusions?

Also, please define what you deem the "normal" seasonal development of E. huxleyi in this area (p. 19710, l. 5-13).

Minor points and recommendations (at this stage non-exhaustive) 1. "Thinning" of E.

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huxleyi coccoliths (p. 19709, l. 1-15)

In order to substantiate your conclusion that E. huxleyi is becoming lighter in mass through "thinning" (rather than size decrease alone), you could calculate the theoretical shape factor (ks= mass/(2.71*length^3); cf. Young and Ziveri, 2000) for each sampled population and show these data either as an additional time series and/or cross plots.

2. Abstract Shorten. Focus on the fact that this study offers a (unique) 12-yr sediment trap-derived time series from the Mediterranean (Gulf of Lions), what's special about the Mediterranean, and your main findings. Most of lines 1-16 could be integrated in the Introduction (you already do).

3. Figures

Fig. 3: rephrase: "coccolith concentration vs. total mass flux in the trap material." Reading the figure caption alone, it is not clear what the point of this comparison is (section 4.2. Sediment transport, explains a sediment resuspension argument). Nor is it clear what is meant by the grey "intervals". The phrasing "indicating small influence of mass flux on coccolith weight" raises the questions "why would it?" and "is this a plot of coccolith weight, or coccolith concentration (as on y-axis)?"

Very hard to read all the details contained in Figure 4. I also prefer the "seasonal" data presented in Fig. 5 over those in Fig. 4 Right-handside. I understand the latter holds all the raw scatter and additional water depths for some parameters, but this is not explained in the caption (nor discussed in the text). You highlight the period Feb-May in Fig. 4, but not in Fig. 5 – what is the rationale (interpretation) behind the highlights? May want to explain this in the caption as well.

Figure 5 is very informative on the monthly (seasonal) AND interannual variations (through the annual deviations from the 12-yr mean).

Fig. 6: shows paired box plots, comparing data from different segments of the time-series; before August 2003 (from Dec 1993; 10 yrs, 8 months of data) and after (until

Dec 2005 (or Jan 2006?); 2 yrs, 4-5 months). This is not time series analysis, but a comparative approach to illustrate average shifts in the distribution of data (mean and variance). I am not convinced that the comparison between a decade vs. 1/4 decade worth of data is a fair comparison.

Fig. 7: Comparing and correlating long-term trends is very different from actual time series analysis (see e.g. Hannisdal et al., 2012; Reitan et al., 2012). Again, this representation of data leaves the authors (and readers) only with visual speculations, no statistical test of the relationships between the various parameters in these SSA plots.

Fig. 8: This is a nice overview figure (note that the blue shading for the pre-industrial Holocene range is not visible in b&w print). However, what is your explanation (or point) for the overlapping range between the pre-industrial Holocene and the mean weight values during 1994-1999 (clearly part of Industrious Times and CO2 values 355-370, according to Fig. 7)? It is interesting that the most recent trap samples reveal even lower mass than the Gulf of Lions Recent sediments. However, data from 1993-1994 revealed similarly low values (but are omitted here – compare Fig. 2A).

References Hannisdal, B., Henderiks, J., and Liow, L.H., 2012, Long-term evolutionary and ecological responses of calcifying phytoplankton to changes in atmospheric CO2: Global Change Biology, v. 18, p. 3504-3516.

Reitan, T., Schweder, T., and Henderiks, J., 2012, Phenotypic evolution studied by layered stochastic differential equations: Annals of Applied Statistics, v. 6, p. 1531-1551.

Young, J., and Ziveri, P., 2000, Calculation of coccolith volume and its use in calibration of carbonate flux estimates: Deep-Sea Research II, v. 47, p. 1679-1700.

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