REF 1 comments

Comments to Lyle and Olivarez Lyle

I have reviewed this manuscript and found very interesting! The new dataset is clearly novel and the discussion is clear and interesting. I have just few remarks and questions and thus ask for minor revisions.

Sincerely

Baptiste Suchéras-Marx

Introduction

l28: Why using Corg rather than POC?

We shall be happy to change Corg to POC to conform to the preferred abbreviation (POC = Particulate Organic Carbon)

l30-31: You never talk about bioturbation throughout the manuscript. Why so? Could you add argument to exclude this process as consumption of organic matter within the sediment.

We are unsure what the concern about bioturbation is. All samples are well below the surface sediment mixed layer, where bioturbation primarily occurs. For the most part, bioturbation will smooth the measured parameters of a sedimentary record, reducing the high frequency change. However, it is possible that POC taking longer to pass through the surface mixed layer of 5-10 cm might be exposed to higher degradation, thus reducing the amount of POC that is ultimately sequestered. In the equatorial Pacific there is evidence that more labile POC components are degraded in the upper cm (Stephens et al., 1997, Geochimica et Cosmochimica Acta, Vol. 61, No. 21, pp. 4605-4619), so it is possible that longer residence in the mixed layer might cause higher POC degradation. Because time spent in the mixed layer is proportional to sedimentation rate, we would expect a correlation between high POC and high sedimentation rates. To examine this possibility, we plotted the POC vs sedimentation rate at Site U1338 and found no correlation (see below). This suggests that bioturbation was not a determining factor in the POC % of these samples.

We can add a paragraph similar to the one above to the manuscript if you deem it important.
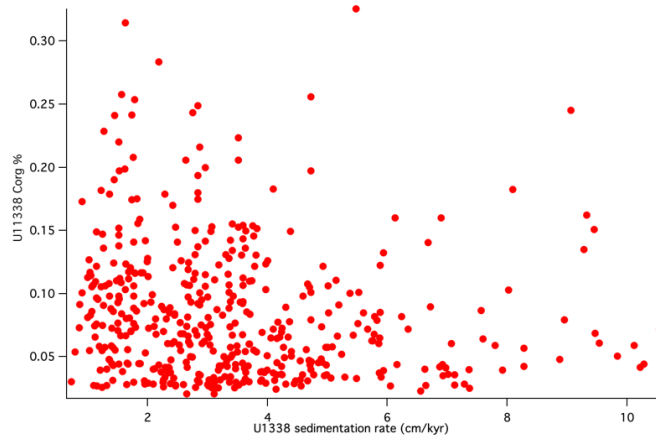
Analytical Methods

l120: Could you please add a sentence about uncertainty (2 sigma) for wt%CaCO3 and wt%Corg in order to evaluate the reliability of variations in your record.

This can be added. We have checked consistency by running an in-house sediment standard with each carbon run ("Midway" standard, 2.64±0.02 wt % total carbon, n=523, 0.85±0.01 wt% n=570). We also repeated analyses of every 4th unknown sample during each run day. We monitor both total carbon and organic carbon. The average difference between repeated unknown samples for organic carbon is <0.01 wt%, and if the differences exceeded 0.02 wt % we re-analyzed the sample.

l128: Concerning site 884, the error is up to 0.4wt%. Those data are not shown but comparing to Fig.2, the variations are within this error. Could this bias the use of this site for your interpretations?

This sentence was written poorly in our manuscript. We were stating that we used shipboard analyses in addition to our own analyses when CaCO3 was under 15 wt% (1.8% carbonate carbon). The POC contents at Site 884 ranged from 0.1 to 0.4 wt% so the amounts of carbonate carbon and POC were much nearer to each other than if the sediment were 80% CaCO3 (9.6% carbonate carbon) and 0.2 % POC. Shipboard carbon analysis for ODP samples at this time used a coulometer to measure CaCO3 carbon and then subtracted that value from the total carbon measured on a CHN analyser to determine POC. At high CaCO3 values, significant errors often resulted. We added the shipboard analyses because our lab analyses started at about 5 Ma and we wanted to also report on the upper section of the site. The analyses that we did in our lab for Site 884 did not use the shipboard method, but a much-improved method of measuring organic carbon directly. They have the same analytical precision as reported for Sites U1337 and U1338. The lab samples were also 80% of the reported samples in the interval older than 5 Ma.

We propose to rewrite this section to correct the wrong impression made upon the reviewer. The error in the data from our laboratory is not 0.4wt%.

l149-151: Could you please explain how you estimate the proportion of terrigenous, authigenic oxide and authigenic clay-based Ba at site U1338 in order to evaluate the reliability of the 93% +/- 4% biogenic BaSO4 you have calculated?

The estimate is based on normative analyses of clay and ferromanganese oxides from our XRF studies of the sites (see IODP Exp 320/321 Proceedings for more detail on the XRF studies). We assigned a Ba content to each normative component and compared that value to the total Ba measured by XRF. The lowest biogenic Ba occurs where there are the most clays and manganese oxides, which is at the tops of Sites U1337 and U1338.

Age Models and Mass Accumulation Rates (MAR)

l198: You say that "*ages should still be good to +/- 0.2 Ma*". How can you tell? Please describe how you made this estimation.

The age model for Sites 806 and 807 were based on biostratigraphic events, not paleomagnetic datum levels. There are 3 possible errors associated with biostratigraphic datums: mis-identification of species, poor age control on the bio-event, and low resolution in average sample spacing of the biostratigraphic study.

We used bio-events that have proven robust as our age control but had to use the shipboard stratigraphic data and postcruise studies to build an age-depth curve. The sampling density of the data were about 1 sample for every 3 m for foraminifera, and 1 sample for every 4.5 m for calcareous nannofossils. Given sedimentation rates of about 20 m/Myr, the potential error from sampling alone is around 0.14 to 0.22 Myr. We used what we believe is a realistic estimate of total error to be 0.2 Myr.

l203-206: How the sampling density – which is often relatively low resolution for magnetostratigraphy – could influence the reliability of your age model?

The magneto-stratigraphy is based on shipboard measurements using a pass-through magnetometer supplemented by shorebased studies. For Sites U1337 and U1338, see IODP Exp 320/321 Proceedings, where a measurement was made every 2.5 to 5 cm. For Site 884, see ODP Leg 145 Initial Reports and Science Reports, where measurements were made every 10 cm. Magnetostratigraphy on ODP Leg 130 was limited to the Pleistocene.

Results

l220: Your data "*averaged 0.043+/-0.014 wt%*". This is extremely low. Coming back on previous comment, what are the uncertainties of measurements of Corg?

As previously explained, the C_{org} (POC) measurement has sufficient level of precision and accuracy to make that claim.

Fig. 2: Panel B is in mg/cm2/kyr but in section 3, you said it would be in g/cm2/kyr. Please use

the same unit throughout the manuscript. By the way, I would recommend to use g/m2/a, which is more coherent in term of unit (the former Bubnoff unit).

The standard MAR units are either mg or g/cm2/kyr. Using g/m2/yr might make sense for very surficial sediments and comparison to water column fluxes. However, the sedimentation rates are measured cm/kyr or m/Myr, so using the units the reviewer suggested (g/m2/a) would imply much higher precision than we would claim. We prefer to use the standard paleoceanographic units presented in the manuscript. Fig 2 can be changed to g/cm2/kyr if you prefer.

Still in Panel B, DSDP Site 574 show a peak at 12 Ma. Why so? Is it related to a major change in sedimentation rate and thus does the age model is reliable for this interval?

This is a good observation. It is important to note that the same 12 Ma peak can be found in the Site U1338 POC MAR profile as well. Sites U1337 and U1338 have a sedimentation rate peak at about 12 Ma, and since Site 574 is correlated to Site U1337, it does too. Site U1337 was the master site for the U1338 correlation as well because it had a stable isotope record from 20 to 0 Ma. It is very clear where the end of MCO at 13.8 Ma lies based on the stable isotopes, but the carbon and oxygen isotopes are not as distinctive in the 12 Ma interval. We checked the correlation to the CENOGRID isotope stack and did not see any obvious miscorrelation.

Discussion

l373: You site Honjo et al., 1982. With all the respect I have for pioneer work of Honjo, maybe more recent estimation and complex studies have been donw since then.

The settling rate of particles was a minor point to describe how long a typical particle remained in the water column, but we can add newer papers like Berelson (2001), Honda et al (2002), and McDonnell and Buessler (2010). A different settling rate does not change the interpretations we made.

l438: You mentioned that "*modeling and observations of plankton distribution point to a loss of Corg primarily within the surface ocean layers*". This statement about model is circular because models are designed this way, increase temperature = more organic oxidation. Please reevaluate the use of modelling studies in your argumentation.

This is not circular because the studies we referred to were trying to match the past water column stable isotope distribution, and were able to do so only by having temperature dependent oxidation.