Biogeosciences Discussions

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Reply on RC1

Pauline Cornuault et al.

Author comment on "Nature and origin of variations in pelagic carbonate production in the tropical ocean since the mid-Miocene (ODP Site 927)" by Pauline Cornuault et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-81-AC1, 2022

We thank the reviewers for the comments they gave on our work, that helped us to review and improve our manuscript.

Reviewer#1 has highlighted unclear points that we here take care to clarify.

We have considered all the different comments and here they are all addressed/discussed as follows (the arrows are our suggestion to the reviewer comment written above).

Line 85: add age range for each of the four intervals examined.

Figures: I suggest to give ages in Ma rather than in ka, so one can get rid of all the zeros.

☐ The ages of the studied intervals will be added in brackets in the text;

☐ Since we have two intervals in the Quaternary, we think it is easier to stay in ka;

☐ This is an important comment, because it made us realise that this sentence is ambiguously formulated. We investigate four intervals, which occurred during the four listed periods (not the four listed periods entirely. Therefore, we will not only add the exact time brackets for the four intervals, but also specify the above in the text.

Line 234: How about productivity changes? Sites 928 & 929 are farer to the coastline compared to other sites. Can the higher distance to the Amazon fan result in lower nutrient deliver and thus lower biological production at those sites?

☐ Indeed, Curry and Cullen (1997) show for the late Quaternary an effect of distance from the Amazon Fan on sediment composition on Ceara Rise, but this change is only manifested by differences in the accumulation rate of terrigenous (non-carbonate) sediments. This is seen in patterns of carbonate content of the sediment (their Figure 2), but not in changes in carbonate accumulation. Also, there is little evidence that the Amazon discharge plume reaches far enough offshore to induce changes in productivity over the plateau. At present, the discharge is strongly deflected northwards and stimulates productivity mainly along a narrow coastal stripe (Gouveia et al., 2019). The same authors note that some of the Amazon discharge may be deflected into the North Brazil Current, but this affects productivity only little and mainly north off the Ceara Rise. We will add a brief statement explaining the possible role of productivity in the revised manuscript.

Line 239: "The CaCO3 AR, on the contrary, does not show any obvious temporal trend" I do not agree. In my opinion the CaCO3 AR generally increases until ~4 Ma, and then it slightly decreases.

☐ We did not intend to insinuate that there is no trend in the CAR at all, but wanted to highlight that the observed changes are much less obvious than the strong increase in overall sedimentation rate. We will improve the statement accordingly.

Lines 272-273: what do the authors mean with "fastest sea-level changes"? Do they mean that they interpret the Site 927 d18O record as reflecting sea-level changes? If so this needs to be stated and the motivation for such an interpretation needs to be explained.

☐ Indeed, the benthic stable oxygen isotope record from Site 927 published by Bickert et al. (2004) during the Quaternary reflects chiefly sea-level changes. This is why it could be included in the L&R stack and why it should be interpreted and used for correlations as such. We will include a statement at this place: because the benthic stable oxygen record reflects mainly global sea level change (Bickert et al., 2004), the tuning was based on…

Line 342: I think it is necessary to add a figure, perhaps in the supplemental information, to show the results of the spectral analysis.

☐ We plan to add the corresponding MTM figure in the supplements

Lines 349-352: this is confusing and difficult to follow as written. Can you add to Figure 7 the correlation lines between d18O and E+T-P?

☐ We suspect that this is a misunderstanding. We have not used the isotopic curves for tuning. We only used them to position the interval that is to be tuned such that we can then tune the colour data to the correct ETP target. For this, the correlation lines are all shown. Also, the resulting effect on the isotopic curves is then shown in supplementary Figure S3. We understand where the misunderstanding arose and propose to change the sentence as:

☐ Therefore, after the alignment with the younger isotopic minimum, we used the E+T-P signal as a target …

Line 374: "the CaCO3 AR is driven by both the carbonate content and the SR" I disagree with this statement. The correlation between CaCO3 AR and SR during KM5 has a R2=0.089. This means that there is no correlation between the two parameters.

☐ Yes, but still significant according to the p-value (1.1 x 10^-2), therefore, we cannot reject the hypothesis.

Figure 9, panel a: apart from the MCO, the panel shows that:

i) there is no correlation between CaCO3 AR and SR;

ii) at one single SR value corresponds different CaCO3 AR values. Can this result from the method used to build the age model? Or is there an oceanographic reason instead? I think the authors need to discuss this in the text. It seems to me that the fact that SR are linear plays a significant role in the relationship between CaCO3 AR and SR.

☐ The presence or absence of correlation is tested by calculating the significance of the correlation coefficient. This reveals that the hypothesis for a higher-than-random correlation between SR and CAR can only be rejected for MIS5, but not for MIS9 and the Pliocene. We fully agree that the differences in the R value are enormous, and interpret the data accordingly, but we cannot ignore the results of the statistical test.

With respect to the second comment, we see an interesting point here, which is likely affecting the Quaternary sections. Here, there are simply too few values of sedimentation rate, because the studied intervals are short and the tuning cannot be carried out on much higher resolution than orbital. In fact, in such a situation, it would have been more appropriate to treat the CAR data as four groups of observations, each representing a different mean SR, and test for differences using ANOVA. However, the number of observations for some of the intervals is too low, to run the test in this way. Either way, this phenomenon could well explain the apparently significant relationship for MIS9, which we, like the referee, also do not consider convincing. We will include a statement to this end to the discussion of the results.

Figure 9, caption: regression lines of MIS 5 and MIS9 are difficult to distinguish. I suggest to add regression formula to the figure legend, to better represent the slope of regression lines.

☐ Yes, the regression formula will be added in the legend of the figure.

Discussion: I found the discussion a bit difficult to read and not well organized (see comments below). In addition, I couldn't find any discussion and interpretation of the new stable isotope records, which is a bit of shame considering that they can provide important information for the interpretation of the other records presented. In my opinion, a discussion on the stable isotope records and on how they correlate with sedimentation and accumulation rate records needs to be added.

☐ We understand that the referees would like to see a more thorough discussion of the stable isotope record, but we believe this is beyond the scope of the present paper. Also, a more detailed discussion would result from a further extension of the record, which we are currently working on and which will be presented in another study. However, we concede that we could comment on some features of the curve already here and we propose to modify the discussion section around line 348 as follows:

We do have a similar signal as the world signal (cf. Westerhold et al., 2020), with maybe even higher resolution and one isotope excursion not visible that much in the other records.

Paragraph 4.1: The discussion in this section is difficult to follow and needs rewriting.

☐ Indeed, as mentioned above, we will entirely restructure this section.

Lines 402-403: How can you reconcile your observation of dissolution in Pleistocene sediments with the fact that Site 927 has been located above the lysocline?

☐ Above the lysocline TODAY (cf. point 1.b) from R2 at the beginning of this document) but the Antarctic deep water formation and circulation during the glacial intervals is causing dissolution during these cold intervals (Curry et al., 1995; Frenz et al., 2006).

Lines 411-414: deleted this sentence.

☐ This sentence will be reformulated in the new version anyway.

Lines 424-425: I do not agree. Pelagic carbonate AR can indicate both carbonate production and carbonate dissolution. So how can carbonate production be assessed by carbonate SR without considering carbonate dissolution?

☐ We will modify the first sentence, to make our point clearer, by beginning the sentence with "Assuming dissolution did not play a significant role in the observed variations in CAR…". We will then continue in the next sentence: under the same assumption, the new record…

Figure 12: It is not clear what the dashed curves are. I suggest to change the names of curves in the legend.

☐ The dashed curves are corresponding to the significance at 90% intervals. The names of curves in the legend will be changed for : BT CAR-Insolation, BT CAR-Obliquity, BT CAR-Obliquity, BT CAR-Eccentricity.

Line 503: Please describe briefly the main observations made by the cited studies.

☐ Yes, we will begin this paragraph by first describing the main trends in cited studies.

There are 2 studies (fig. 14) available and they show [….] And our results are consistent with [….].

Line 506: in my opinion it cannot be said that the new record has a similar long-term trend as Lyle et al. (2019).

☐ We agree that it is not appropriate to talk about trends when describing the new data for the four time periods. The overall trend at the Ceara Rise is similar and our new data are consistent with it. That is indeed a more appropriate description of the observations.

Conclusions: I suggest to shorten the conclusions which are extensively long.

☐ We believe the main messages should be briefly summarised here and we also believe the conclusions are not longer than in other studies of this kind. We have considered this comment carefully and we feel that the only aspect that could be deleted is the sentence on line 520-522.

---------- Technical Corrections ---------- Minor comments

Lines 43-44: quantify short-term and long-term.

☐ "[...] geological (Ma) and orbital (ka) [...]" will be added in the text.

Line 124: add colour scale for the bathymetry next to the map of Figure 1.

☐ Yes, a colour scale for the bathymetry will be added on this figure.

Line 131: add "modern" before "regional". Add lysocline depth.

☐ This will be modified in the revised version of the manuscript.

Line 161: What does "loess" mean in the plot vertical axis? If a detrending function was applied to the record, state it in the figure caption.

☐ Yes, this is a smoothing method, and it will be added in the figure caption.

Line 164: substitute "Stable oxygen isotopes" with "Oxygen stable isotopes".

☐ Yes, this will be modified in the revised version of the manuscript.

Line 168: delete "Next,".s

☐ Yes, this will be modified in the revised version of the manuscript.

Line 193: substitute "S3" with "S1".

--> No, it is well the table S3 of the Westerhold et al. (2020) paper.

Line 200: substitute “For the high resolution 4 intervals” to “For the four high resolution intervals”.

☐ Yes, this will be modified in the revised version of the manuscript.

Line 223: state that the graphs in panel a are box plots.

☐ Yes, this will be modified in the revised version of the manuscript.

Line 253: the Leg number can be removed.

☐ Yes, the Leg number will be removed in the revised version of the manuscript.

Lines 255-256: it is difficult to understand which color is which. I suggest to add a legend next to the panel.

☐ Yes, this will be modified in the revised version of the manuscript.

Line 258: substitute “blue” with “light blue”. Apply the same to figures 6 and 7.

☐ Yes, this will be modified in the revised version of the manuscript.

Figure 6, panel f: I suggest to use another color instead of the light purple for the MS record of the middle core because it is difficult to distinguish from the MS record in dark purple.

☐ Yes, this will be modified in the revised version of the manuscript.

Line 280: add corresponding colour for the MS record and the MS smoothed record.

☐ Corresponding colour for the MS record and the MS smooth record will be added in the revised version of the manuscript in brackets (black and grey).

Line 298: add a brief explanation of why this insolation curve has been used.

☐ A brief explanation will be added as follow: The spliced MS signal (Fig. 6b) has then been tuned with the daily insolation on 21st of June 65°N. This is because this representation of orbital forcing of global climate, shows the best pattern of influence from both obliquity and precession (Laskar et al., 2004) (Fig. 6a) and has been used for tuning at the studied location in previous studies (e.g. Zeeden et al. (2013), who also provide arguments for why the MS and Insolation are co-varying without lag.

Line 213: which curve is obliquity and which is E+T-P?

☐ Obliquity in grey, ETP in black, this will be specified in the legend.

Line 230: define "LAD".

☐ Last appearance datum, this will be added in the revised version of the manuscript.

Line 358: substitute "local" with "Site 927".

☐ This will be corrected in the revised version of the manuscript.

Line 368: substitute "periods" with "intervals".

☐ This will be corrected in the revised version of the manuscript.

Line 381: "carbonate AR appears to decrease with time". Do the authors mean with increasing age?

☐ Yes, with increasing age. This will be specified and this sentence will be reworded in order to make it clearer in the revised version of the manuscript.

Line 443: delete “On the other hand,”

☐ This will be deleted in the revised version of the manuscript.