

Biogeosciences Discuss., author comment AC2  
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## Reply on RC2

Pauline Cornuault et al.

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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-AC2>, 2022

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

Reviewer #2 has pointed to a lack of information leading to our conclusions that we are here adding for a more solid interpretation.

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).

There needs to more evidence to indicate that changes in carbonate AR are not affected by dissolution, especially for Site 927. There also needs to be more discussion on the link between the carbonate production and the driving mechanisms (e.g., light, temperature, nutrients-upwelling processes).

- a) I found the first part of the discussion section "carbonate preservation" rather weak. Although this does not mean that I necessarily disagree with author's arguments, but as this section is very important for the next parts of the paper, I recommend to provide more evidence indicating that the observed changes are (not) driven by dissolution. A series of Scanning Electron Microscope (SEM) images for instance could be helpful, or/and comparison with other available data (e.g., biogenic siliceous productivity) (maybe add a figure in supplementary material).

We realise that the aspect of carbonate dissolution was not treated sufficiently and we appreciate the comments by both referees who request a stronger case for the claimed absence of dissolution during Miocene and Pliocene and/or the fact that it is not the driving factor of the observed changes in carbonate accumulation rate. To this end, we

propose to restructure and expand the dissolution section. First we will separate the Quaternary part, where dissolution does occur, and where the discussion only deals with the identification of the parts of the sequence that are not affected, from the Pliocene and Miocene part. We agree with the referees that a stronger case and more explicit support will allow us to better substantiate the claim that the observed variation is due to changes in export production. Whilst we agree that visual evidence for dissolution in the coarse fraction is a useful and instructive means to support our claims, SEM images, as proposed by the referee, can only be used to document the state of a few individuals, which makes them less representative. Instead, we propose to document the preservation state of key samples, representing the highest and lowest carbonate accumulation rates for each period, by high-resolution optical images from a digital microscope. Those can be provided in the supplement and an example is shown below. Next, in the course of our project, where we are trying to identify the exact mechanisms responsible for the observed changes in carbonate accumulation, we have in the meantime generated for the Pliocene and Miocene data on fragmentation of planktonic foraminifera shells, a commonly accepted proxy for the extent of carbonate dissolution. We propose to introduce the data in this paper either in the main part or in the supplement. As shown below, the fragmentation varies, but remains low, indicating no evidence for dissolution and, most importantly, the fragmentation does not correlate with carbonate accumulation rate at all, indicating that the observed changes in carbonate accumulation must reflect processes other than dissolution.

- b) Moreover, the authors infer in several places in the text that Leg 154 sites remain either above or below lysocline based on their modern depths. Given that depth is a crucial parameter for dissolution/preservation, I recommend to provide information on the paleodepths of the sites, especially for Site 927 for all studied time intervals.

□ As explained in the response to the previous comment, an argument about lack of dissolution control on carbonate accumulation based only on a depth-related argumentation is likely not sufficient. Therefore, we will use more explicit data to constrain and quantify a possible effect of this variable. With regard to the paleodepth, we believe that the variations since mid-Miocene have been negligible. Paul et al. (2000) note that the exact subsidence history is unknown, but assume minimal subsidence since early Miocene. Similarly, sea-level differences among Quaternary interglacials and Pliocene and Miocene were likely in the order of 10s of metres. Therefore, the largest changes in paleodepth would have been due to sediment cover, which would make the studied mid-Miocene interval about 300 m deeper compared to the present one. We will provide a brief description of these facts in the revised version.

- a) The authors propose that changes in light, temperature and nutrients driven by upwelling, forced the observed changes in the export of flux of pelagic biogenic carbonate. These could be plausible mechanisms, but I would like to see a more detailed discussion on this. The authors could use available published data (e.g., SST; Herbert et al., 2016) to back up their hypothesis. Additionally, in Lines 22-23, they state "These results imply that the pelagic carbonate production in the tropical ocean, buffered from large temperature changes, varied...." Are there available data that shows that?

□ It would be indeed very interesting to study what exactly caused the changes in carbonate production. However, none of the pertinent data are available at high resolution, allowing a direct correlation with the new carbonate accumulation data that we present. Also, the key parameter we would really need is palaeoproductivity and this is very hard to derive from proxy data. The list of potential driving parameters as presented in the paper is meant to specify the options (what could potentially affect production), and should not be seen as an opening for an extensive discussion, which we admittedly would also like to engage in, but cannot due to lack of key data. We will modify the sentence to make it clear that we list these parameters as options, but cannot at present resolve which was more important for the observed changes in carbonate production.

In terms of the claim that the tropical ocean was buffered from large temperature changes, we can provide references showing modest SST variation compared to higher latitudes and highlight the fact that we mean buffered compared to higher latitudes. Low-magnitude tropical SST variability in the Atlantic in the Pliocene and the Miocene was reported by Herbert et al. (2016) and Curry et al. (1995).

- b) I also recommend to include a final figure (conceptual model) summarizing the main conclusions: changes in carbonate AR for the different time intervals, orbital variability, as well as potential mechanisms (e.g., light, temperature and nutrients).
  
- I recommend to add a section of modern hydrography of the region.

□ We do think that adding a conceptual model figure will be too much (especially because we do not consider the temperature and nutrients), but we will provide a more extensive description in the introduction at the place where we introduce the site (line 129).

- In line 239, you state that “the CaCO<sub>3</sub> AR, on the contrary, does not show any obvious temporal trend (Fig. 4), indicating that the increase in SR is compensated by decreased carbonate content in the sediment”. Maybe I’m confused, but when I’m looking for example Site 927 in Figs 3 and 4, I see that increased SR coincide with increased carbonate content between 16 and 3 Ma. Could you explain this better?

□ 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.

- I’m missing a section in the result part for the new  $\delta^{18}\text{O}$ ,  $\delta^{13}\text{C}$  data generated from this study. Moreover, these data can provide additional information that can help the part of discussion.

□ 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.

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

Lines 20-21: "..., but that each interval was characterized by large orbital-scale variability"  
Although I understand what you mean, reword if possible.

This sentence will be reworded as: "We observe that the highest carbonate accumulation rates occurred during the Pliocene but that each of the studied intervals was characterised by large-magnitude orbital variability".

Lines 23-24: "...on orbital time scales similarly or even more than on longer time scales".  
Rephrase.

This sentence will be reworded as: "These results imply that pelagic carbonate production in the tropical ocean, buffered from large temperature changes, varied on orbital time scales. The magnitude of the orbital-scale variability was similar or even higher than the long-term mean differences among the studied intervals".

Line 71: "plankton had no opportunity to responds to the climate cycles by migration" Add a reference.

This sentence will be reworded as : "[...] where the plankton could not [...]".

Line 80: "... to assess the spatial coherence of long terms

The "s" at the end of "terms" will be deleted.

Line 111: "...is also characterised as wetter" wetter compared to today? clarify

This sentence will be clarified, specifying that indeed, it is compared to today.

Figure 1: Add scale for bathymetry

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

Line 130: "This aseismic ridge rises several km above..." Give depth

The depth will be added in the text : in this location, the average depth of the seafloor is at 4500 mbsl. The Ceara Rise has a maximum thickness of 1900 m of lithogenic and biogenic sediments (the minimum depth at the Ceara Rise is about 2600 mbsl) (Curry et al., 1995, initial report).

Figure 3: This is a nice figure. 3b: I recommend to add also a small key-scale showing the values of the colors.

Yes, a colour scale will be added on this figure.

Line 273: "... and times of fastest sea-level change..." What do you mean by fastest sea level change?

Specification will be added in the text as follow: by the fastest sea level change (coinciding with the fastest ice volume change), we mean the inflection points of the d18O curve (327.55 mcd to 15605 ka and 331.5 mcd to 15930 ka).

Line 506: "a largely similar overall trend..." I cannot see that - reword this part.

This will be reworded in the revised version of the manuscript as follow : Our record is showing similar absolute values as Lyle et al (2019) and Drury et al. (2020) (an AR between 0 and 5 g cm<sup>-2</sup> ka<sup>-1</sup>) and a largely similar overall trend with highest values in the late Miocene/early Pliocene and similar values in the early Miocene and Quaternary.

Lines 507-508 you state "Clearly, the overall of carbonate accumulation at the Ceara Rise supports the existence of a late Miocene carbonate maximum also under tropical conditions". However, in lines 15-16 you note that there is "a systematic increase in sedimentation rates since the late Miocene, but carbonate accumulation rate does not show a clear trend", which is what your data show. Therefore, these lines in the discussion need rewording.

Indeed, what needed to be reworded was the claim that there is no trend in CAR at Ceara Rise (line 239), which we have now corrected, following a similar comment to this end by both referees.

Lines 515: "... The two shallow sites consistently..." Add sites in a parenthesis to remind them to the reader.

The site's references will be added in parenthesis in the revised version of the manuscript.