Climate of the Past Discussions

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Reply on RC2

David A. Hodell et al.

Author comment on "A 1.5-million-year record of orbital and millennial climate variability in the North Atlantic" by David A. Hodell et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2022-61-AC2, 2022

The manuscript of Hodell et al provides not only a set of high-resolution data for the last 1.5 Ma from a climate sensitive region, but also a quite comprehensive, up-to-date and insightful review for the MCV. The characteristics of the MCV and their links with orbital forcing, glacial state and CO2 are discussed for different periods of the 1.5 Ma. The discussions are made not only based on the U1385 data but also on a wide collection of important literature. I also agree with the authors that "In addition to documenting MCV, the planktic and benthic isotope records from Site U1385 provide unprecedented detail of the amplitude and shapes (waveforms) of the glacial cycles on orbital time scales for the last 1.45 Ma". I believe it will be a very useful paper for researchers and students in paleoclimate study. I would recommend its publication after some minor revision. Please find here under my comments and questions which I hope will help to clarify a few things and potentially make the paper more attractive.

We are thankful to the reviewers for their thoughtful comments and apologies for the slow response. I am currently on IODP Expedition 397 at Site U1385 ("Shackleton site") where we are deepening the sequence to recover the older part of this remarkable archive. I'm pleased to report that we have lengthened the record to 400 mbsf into the early Pliocene.

- Figure 6 seems very noisy. There are many peaks between the blue and red shade. I wonder why these are not discussed in the paper. If these peaks are considered as noise, how can we tell the blue and red shaded parts are not noise? I would suggest to try with another software to check whether the result of the spectral analysis is affected by the software used.

A similar point was raised by reviewer 1. We will remove the power spectrum of the millennial variability (Figure 6) and its associated discussion from the paper because the inaccuracies of the age model smears the peaks in the millennial band over a wide range of frequencies, resulting in a noisy spectrum. As the reviewer points out, this makes it difficult to distinguish which peaks are significant and which are not. We have tried different methods of spectral analysis and the results are similar.

- The evolutive power spectrum figures (Figures 16 and 17) are not sharp nor nice, which

is a pity for such a nice paper where all the other figures are of very high quality. A color bar is also missing. Similar to my comment for Fig.6, I wonder whether the result is affected by the software used, and I would suggest to test with another software to perform wavelet analysis and compare with the results shown in Figures 16 and 17 and to improve the quality of the figures.

The quality of Figures 16 and 17 was severely degraded upon conversion of the manuscript to pdf. The originals of the evolutive power spectra are clear and crisp and we will ensure the figures are high resolution in the published version of the paper. We will also add the colour bar.

- Lines 733-736: In addition to the 41-kyr signal, Figures 16 and 17 also show strong signal around the low-frequency 0.005-0.01 (200 - 100 kyr cycles) between 1450-900 ka, but I don't see this is mentioned anywhere in the manuscript. Could the authors comment on this and add some discussions in the paper?

We didn't comment on the 200-100 kyr cycles because the periods are long relative to the size of the window (300 kyrs) used for the evolutive spectra. We will note in the text that the longer periods exist, but caution they may not be significant given there are only 1-3 cycles in a given window.

- The 28-kyr cycle has been suggested to originate from the non-linear interactions between eccentricity and precession/obliquity, or between the 41-kyr cycle and its multiples or from a non-linear response of the glacial cycle to obliquity (lines 32, 739-748, 934, 1056-1058). I would like to draw attention that 28 kyr is one of the important periodicities of obliquity although its magnitude is only one eighth of the magnitude of the 41-kyr periodicity (see Table 1 of Berger, 1978, Journal of the Atmospheric Sciences), so the possibility of a direct and more linear response to obliquity can not be excluded, although the reason why the 41-kyr is switched to 28-kyr at ~800 ka BP remains to be explored. I would suggest to mention this in the manuscript to open more possibilities.

We will mention the theoretical obliquity signal contains a secondary peak at ~29 kyr. The spectral power seems rather weak so it is uncertain if it has any direct climatic significance, but it is important to note. See also response to comment of Reviewer 1 about the 28-kyr cycle. We will rephrase the discussion about the ~28kyr modulation following the suggestions made by both reviewers.

Lines 752-753: I wonder to which extent the good relationship between obliquity and MCV depends on the way to build chronology, and to which extent the obliquity threshold 23.5 is affected by age uncertainty. Could the authors add some discussion on this? The obliquity minimum around 1180 ka seems not playing a role, what might be the reason?

The chronology was built by tuning the color (L*) to precession which should be independent of obliquity, which was not used in the orbital tuning procedure (Hodell et al., 2015). The obliquity minimum around 1180 ka falls in the middle of MIS 35, which has always been a puzzle. Shackleton et al. (1990) assigned Stage 35 to two obliquity cycles. Although it is possible that the lack of response to the obliquity low at 1180 ka indicates a problem with the time scale, nobody has found a better alternative to Shackleton's proposal that MIS 35 spans two obliquity cycles. We will add some discussion of this point to the text.

Line 803: It would be more accurate to say "obliquity through its effect on the mean insolation but mainly on the total summer insolation at high latitudes (see Berger et al., 2010, Quaternary Science Reviews, https://doi.org/10.1016/j.quascirev.2010.05.007),

because a large part of the mean insolation is depending on precession.

Yes, that's a very good point. We will change the sentence as suggested.

- Lines 816-818: It is a repetition of lines 804-805, not related to the subharnomics as discussed in this paragraph, and is better to be integrated with lines 800-805.

Agreed. We will move this sentence and integrate with lines 800-805 as suggested.

- Line 932: Figs. 10 and 11 should be Figs. 16 and 17?

We will correct this.

- Zr/Sr is less familiar at least for me. Adding some explanation on its paleoclimate interpretation would be welcome.

We're happy to add an explanation of why we chose Zr/Sr to represent stadial events. It is essentially a detrital/biogenic proxy. Sr is biogenic and it is highly correlated to Ca except that it has a lower number of counts/sec that is more similar to Zr than is Ca. Both Zr and Sr are relatively heavy elements and are less sensitive to surface effects than other lighter elements (e.g., Al, Si). Increases in Zr/Sr during the last glacial period clearly capture the stadial events, and particularly the strong Heinrich stadials, in a suite of piston cores from the Iberian margin (see supplementary materials in Channell et al. (2018), Quaternary Science Reviews 191, 57-72).

- Both ka and kyrs are used through the paper. Better to use only one?

We have followed the guidelines provided by the journal for the use of ka and kyr:

"ka stands for "kilo-annum" and literally means thousands of years ago, thus referring to a specific time/date in the past measured from now. In contrast, "kyr" stands for thousand of years and is used to reference to duration."