Replies to the three reviews on cp-2021-103

Referees comment on "Abrupt climate changes and the astronomical theory" by Denis- Didier Rousseau et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-103,
2021

We would like to thank all three referees who evaluated our manuscript providing useful comments and suggestions on its original version. The revised one submitted here follows the reviewers' requirements including a re-organization of the manuscript by including a "Material and Methods" section as well as providing supplementary material. We also have changed the title, corrected both figures and tables, updating the list of references.
We wish that in its present new version, our manuscript will fit the reviewers and editor's expectation.

Anonymous Referee #1

We thank Reviewer 1 for his comments. Our replies are in blue below.

In this manuscript, Rousseau and colleagues present a small review of millennial scale variability in the North Atlantic, with a particular focus on DO events. They use recurrence plots to determine the main transition times in the past 3 million years, and also to link millennial scale variability in the last glacial period to Bond cycles.

The review section is unexpected in CP, but could make sense as a contribution for this special issue. I have no major comments on that section and will leave this to the editor.

Although the title of the manuscript is "Abrupt climate changes and the astronomical theory," we didn't intend to submit a complete review paper on the astronomical theory, which is the object of the entire Special Issue. Instead, we just wanted to sketch the evolution of ideas on the specific aspect of abrupt climate changes through a selected subset of papers, while injecting some of our own thinking and recent results. Such a selection cannot be entirely devoid of personal preferences.

On the research side, the use of recurrence plots to identify specific transition is an interesting approach. The advances presented are not very substantial when compared to standard CP papers;

Our paper is a contribution to the CP Special Issue dedicated to the celebration of the centennial of MM's 1920 book. As such, this paper is based on the invited presentation given during the centennial symposium and is part of a series of papers on the detection of abrupt transitions by using two distinct but complementary methods: a modified Kolmogorov-Smirnov (KS) method and the recurrence plot (RP) method used in the paper at hand.

in my second major comment I suggest some aspect that could be fleshed out a bit. My main problem with this manuscript is the seemingly arbitrary way in which the transitions in the recurrence plots are determined (see major comment below). I wonder if the results are robust against small changes in parameter selection. I would like to see a sensitivity analysis before I recommend publication of this manuscript.

Thanks to Reviewer #1 for pointing out this important issue, which was not included in the original manuscript. Following their recommendation, we have performed a recurrence rate (RR) analysis, which corresponds Reviewer #1's request for a sensitivity analysis. The results were plotted along with the original recurrence plot. The plotted values correspond to the mean for different window lengths ranging from 1 kyr to 15 kyr. The selection of the transitions of interest relies therefore on the definition of a threshold that we choose to be the standard deviation of RR prominence, which is 0.089 for the U1308 benthic $d^{18}O$, 0.127 for the U1308 bulk carbonate $d^{18}O$ and 0.173 for NGRIP

$d^{18}O$. The modified figure is attached to this reply and will be included in the final manuscript if the paper is accepted.

Major comments:

The choice of transition in the recurrence plots is not explained or justified. In line 166 and 167, the authors refer to Eckmann et al., 1987 and Marwan et al., 2013 to determine "sufficiently close". That is not acceptable, as a publication should include all necessary information to replicate the results. The authors should explain in detail what choices they made to produce the red lines in the recurrence plots.

We agree with Reviewer #1 that just referring to these papers — while necessary and useful to the reader unaware of the recurrence analysis literature — is not a sufficient explanation of the choices made in identifying the abrupt transitions we discussed. To explain these choices, as mentioned previously, we have performed an RR analysis using different windows, and plotted the mean values under the recurrence plot. The minima of the RR plot correspond to the abrupt transitions of interest and applying the RR prominence analysis, we determined the major rapid changes to be discussed. They are marked on the figure by pink crosses. The statistics of the RR minima are given in a new table, also attached to this reply and to be included in the final manuscript, if accepted.

An emblematic example of this problem is Figure 4. Looking at the recurrence plot in figure 4b I see no justification for the line at 32 kaBP, it seems very arbitrary. The same goes for the line at 78 kaBP; between 70 and 78 kaBP there seem to be three more transitions that could reasonably have been drawn. The question arises about the sensitivity of the results to small variations in the parameters of the algorithm chosen to identify transitions. An uncertainty/sensitivity analysis needs to be added for each RP.

See above. The RR analysis allowed us to refine the dates of the major transitions previously identified in the original manuscript. Concerning NGRIP $d^{18}O$, the RR analysis has identified 7 major transitions (RR prominence above the standard deviation highlighted in yellow in the new table) and 6 more could be considered in the discussion (RR prominence close to the standard deviation in highlighted in green). This leads us to remove the former lines at 32 ka and 78 ka, which correspond to minima with an RR prominence that is too low compared to the standard deviation; see the new table attached. Between 70 and 80 ka, the RR analysis identified one major transition at 72.3 ka, and two minor ones at 74.2 and 76.4 ka, respectively.

The sentence in line 305 is unclear. Are the authors defining new GIs based on the recurrence plot? If so how are they defined?

No, we don't define new GIs based on the recurrence plot.

If instead they are talking about the GI numbers in Figure 4a, which ones do they mean? There are several numbers in each interval defined by the red lines. I think the authors may have missed an opportunity to make a clear contribution here.

We have rephrased the sentence using the results of the RR analysis. Still, the longest GIs from the NGRIP $\delta^{18}O$ record are labeled in the upper panel of Figures 4 and 5.

This paragraph is the only one of the chapter that appears to be more than a review, and the relationship between GI duration and sea-level is very interesting. A scatter plot of sea-level (or sea-level trend) vs. GI duration would make their point much clearer and add a bit more results to this chapter.

We have redrawn Figure 4: first by presenting it in a format similar to Figures 2 and 3, i.e., with the original record in the upper panel (a), the recurrence plot in the middle (b), and the recurrence rate in the lower panel (c), with the pink crosses identifying the selected minima with a prominence threshold higher than the standard deviation. Moreover, in Figure 5 we changed the RR curve to the global mean sea-level curve to argue in favor of our hypothesis of longer GIs being linked to "stable sea levels". Thank you for these very constructive suggestions.

Minor comments:

Lines 12-13: "relatively" used twice in one sentence

Changed "relatively short time" to "rather short time"

Line 16: "constant" is the wrong word here since these are periodic variations. Maybe "regular"?

OK, changed; thank you.

Line 98: It is unclear what "those" stands for in the second part of the sentence. I imagine it must refer to the shorter periodicities mentioned in the first part? Please clarify.

"Those" here refers to "transitions". We have changed "those" accordingly.

Line 99: I'm not sure "affected" is the right word here. Maybe something like "the frequency of abrupt changes is in part modulated by…"

We have changed the sentence as follows: "We show that abrupt climate changes are still resulting, albeit indirectly, from changes in insolation and […]"

Line 131-132: As I understand this sentence, it now says that during the late Pliocene the ice sheets over Greenland and Scandinavia were larger than during the Quaternary. That is not the message of the Naafs et al. 2013 paper. Please clarify.

Sorry for this statement's lack of clarity. Indeed, ice sheets over Greenland and North America were not larger during the late Pliocene than during the Quaternary. The sentence should read instead "Naafs et al. (2013) report the occurrence of minor IRD events attributed mainly to Greenland and Fennoscandian glaciers, indicating that the ice sheets over these regions were more prominent than during the later Quaternary, when North American ice sheets were considerably larger"

Line 141-143: Yes, but Barker's record starts at 800 kaBP without any information about the occurrence of millennial scale variability before that. I think it is important to make clear that we don't know if millennial-scale variability (i.e. DO events) started during the MPR or not.

We have corrected the sentence, which reads now as follows: "At about the same time, the synthetic Greenland $\delta^{18}O$ reconstruction — which starts, however, at 800 ka — indicates the occurrence of millennial variability expressed by DO-like events (Barker et al., 2011)."

Line 154-155: This sentence is too vague, as ice sheet extent was very large also during MIS6 and LGM. It also doesn't convey much important information. I suggest rephrasing it or deleting it.

We have rephrased the sentence as indeed the southern edge of the NH ice sheets reached, during MIS 12 and 16, a position similar to that reconstructed for MIS6. Indeed, the Batchelor et al. (2019) reconstructions show that the Laurentide (LIS), Eurasian (EIS) and Greenland (GIS) ice sheets had areas that were fairly similar during MIS 16, 12, 6, and again the same values during MIS 2 for LIS and GIS (Batchelor et al. 2019 Suppl. Data).
The sentence reads now as follows: "During the interval 1 Ma – 0.4 Ma, Northern Hemisphere ice sheets reached a southernmost extent during MIS 16 and 12 that was similar to the one reached during MIS6 (Batchelor et al., 2019)".

Line 184: I think it would be helpful to explain in one or two sentences what a "drift topology" is here, with deeper insights being referred to Marwan et al.

The end of the sentence explains what the drift topology relates to. In fact, this is a particular pattern introduced by the Eckmann et al. (1987) paper cited above. However, we have updated the sentence as follows: "recurrence analysis shows a drift topology (Marwan et al., 2007) that characterizes a monotonic trend in time,[...]".

Line 186: Please refer to Figure 2a at the end of this sentence already.

Done.

Line 196-197: This sentence seems unnecessarily complicated. I suggest "Our analysis further identifies the steps at 0.9 Ma, 1.25 Ma, and 2.75 Ma (with 1.25 step also noticed in the d^{18}O)."

We have corrected the sentence following Reviewer #1's suggestion.

Line 204: The sea-level change increased since the value of the change is not higher

Correct, thank you. We rephrased the sentence as follows: " After 1.25 Ma, the sea level changes increased to about 70–120 m below their present day values,"

Lines 208-211: That is only true for the glacial maxima. The glacials themselves have all kind of different orbital configurations due to their long duration.

Correct, thank you. We rephrased the sentence as follows: "The "Milanković glacials," which correspond to the odd marine isotope stages determined in the U1308 core and in many others, have maxima that are characterized by low eccentricity and obliquity,.."

Line 221: You could reference Figure 2 here since it's the same plot.

No. The paragraph relates to IRD and therefore we must refer to Figure 3, which shows the recurrence plot of the bulk carbonate d^{18}O.

Lines 224-225: Not every cold period is a Heinrich Event.

We agree with Reviewer #1's statement, which is consistent with our manuscript's sentence that "The former are manifested by IRD events, some of which are significantly stronger, and represent the previously mentioned HEs and correspond to massive discharges of icebergs into the North Atlantic." Hence we keep the sentence as it is.

Line 261: In the text above, the "canonical" DOs were those described by Dansgaard.

We rephrased the sentence as follows: "[...] all the canonical events described by Dansgaard et al. (1993) and identified in Rasmussen et al. (2014),..."

Line 333: "event" is included in HE. Please provide a definition separating HEs from regular IRD events.

We have removed "events". HEs are defined previously in lines 224-225 and IRDs are defined line 118, where we added after ice-rafted debris "continental detrital material eroded by the ice sheets,"

Line 350: "prevailed" may not be the best word choice here. How about "existed"?

"Prevail" is correct English but maybe less familiar, since Reviewer #1 doesn't like it. So we accept the suggestion.

Line 354-355: It's not clear what "these results" refers to here. I'm guessing the authors mean the relationship between sea-level and GI duration? Please clarify.

We were referring to the 0.9 Ma or 1.5 Ma dates. Therefore, we have rephrased the sentence as follows: " Whether a younger start date of 0.9 Ma or an older one of 1.5 Ma is posited, these dates show that the Northern Hemisphere ice sheets played a significant [...]"

Line 368-373: This mechanism was already posited by Shaffer et al. in 2004 (https://doi.org/10.1029/2004GL020968)

Thank you for mentioning this reference. The model of Shaffer et al. (2004) did propose already that ocean subsurface warming during the DOs may be at the origin of ice rafting events, due to ice shelf melting and break up. However, these authors did not model the Northern Hemisphere ice sheets, which are the iceberg providers. We repeat here their conclusion, as follows: "Clearly, more simulations with more comprehensive models and more high-resolution paleodata are needed to test the proposed mechanisms for coupling of DO cycles and ice rafting events. For example, a better understanding of ice sheet–ice shelf dynamics is needed, not only to predict the future of the Western Antarctic ice sheet (Oppenheimer, 1998], but also to better interpret past climate variability". This is exactly what Ziemen et al (2019) did by modeling the evolution of the Northern Hemisphere ice sheet dynamics during HEs as a two-stage mechanism described in much greater detail and comparing with proxies in a way that Shaffer and co-authors did not do." This is the reason that we wish to maintain our description of the state of affairs.

Figure 1: In paleoclimatic sciences and in this manuscript as well for most of the figures, the "Age" scale on the x-axis increases in values towards the right. I would advise the authors to either flip the figure around to make it consistent with the rest of the figures, or to use "time" instead as an x-axis with negative numbers if you want to keep the present on the right side.

We have homogenized all the x-axes of the figures; thank you.

Figures 5 and 6: x-axis has again been reversed, please flip the figure around or use "time" with negative numbers.

Done; thank you.

Table 2: "Last" is already included "LGM".

Corrected.

Anonymous Referee #2

Rousseau et al. provide a short review of astronomical theory and abrupt climate change. They plot some selected climate records using recurrence plots, and discuss the findings. They argue that DO oscillations are a type of internal oscillation, and that Bond cycles are formed through interactions with ice sheet volume.

It is unclear what the purpose of the manuscript is. Are the authors providing a review study, or original research? Unfortunately, the paper does not live up to the standards of either type of paper. It is not comprehensive enough for a review paper and does not provide an unbiased overview of relevant work. It further does not provide the kind of novel results and insight that would be the hallmark of a research paper. The two aspects are also not clearly separated. In both sections 2 and 3 one finds RP analysis and historical review mixed together. Given the shortcomings of the manuscript, I think the authors need to resubmit a very different paper for it to be suitable for Climate of the Past.

Thanks to Reviewer #2 for their detailed comments and remarks, which will be taken fully into account in the revised version that is in preparation. Although the title of the manuscript is "Abrupt climate changes and the astronomical theory," we didn't intend to submit a complete review paper on the astronomical theory, which is the object of the entire Special Issue. Instead, we just wanted to sketch the evolution of ideas on the specific aspect of abrupt climate changes through a selected subset of papers, while injecting some of our own thinking and recent results. Such a selection cannot be entirely devoid of personal preferences. Still, we thank Reviewer #2 for pointing out several important papers that were missing from our selection.

With respect to the presentation of the paper's complementary aspects of literature review and original research, separating them entirely is one possible approach but not necessarily the best or only one. The combination of review with novel results in Sections 2 and 3 represents an approach that is followed in many review-and-research papers. We will attempt, though, to introduce appropriately titled subsections in order to improve the paper's legibility.

The introduction (section 1) as currently written bears no relationship at all to the main topic of the paper. It provides a short historical introduction to astronomical theory – given the short length of the section, it is necessarily incomplete. The overview stops in the 1970s, and it does not give the reader an idea of the recent ideas and challenges.

Once more, as indicated previously, although the title mentions the astronomical theory of climate, we didn't intend to submit a complete review paper on this theory. In Section 1, we just sketch the evolution of ideas, roughly until the broad acceptance of the astronomical theory in the 70s by the paleoclimatic community.

The authors suggest they are interested in the relationship between orbital and millennial- scale climate change. Then why not write an introduction / overview of the literature written on that topic instead? The authors do not acknowledge that there is a long history of such studies; these earlier studies should be evaluated and discussed instead. The first such study is probably (McManus, Oppo, & Cullen, 1999), who linked DO variability to sea level.

We were aware of the McManus et al. (1999) paper and thank you for noticing the missing reference. We have added it now, in the context of stating that millennial variability prevailed during the past 0.5 Ma, as recorded already in marine records prior to the publication of the EPICA results.

More recently, the Dome Fuji community members have presented a detailed study of the link between DO recurrence times and background climate (Kawamura et al.,2017). While these are probably the most important,

The Kawamura et al. (2017) paper presents the long-expected Dome Fuji results. This is indeed an extremely valuable source of data that we are going to cite in the revised version. However they mainly refer to the results by Steve Barker when trying to reconstruct the synthetic Greenland $d^{18}O$, while focusing on the Antarctic isotope maxima. They cite McManus et al (1999) just to indicate that "Proxy studies have suggested that climate instability and the associated bipolar seesaw become active in glacial periods".

many other studies should be listed also – my list is by no means complete: (Schulz, Berger, Sarnthein, & Grootes, 1999; Schulz, 2002; Schulz, Paul, & Timmermann, 2002; Sima, Paul, & Schulz, 2004; Buizert & Schmittner, 2015; Lohmann & Ditlevsen, 2018, 2019). Most of these studies are not cited.

We have checked these references, and will be including the most relevant ones in the revised version.

The review given of abrupt climate change (mostly sections 2 and 3) are likewise not very comprehensive or complete. They authors seem mostly interested in highlighting their own contributions. For example, the 2020 and 2021 papers by Bagniewski et al. (the same as the authors on the present paper) are given a detailed description (L255-263), while their method is not even used in the manuscript.

Thanks to Reviewer #2 for pointing out this issue. We have referred to the Bagniewski et al. (2021), which is now in press in Chaos, as it formulates and applies an abrupt- jump detection method that is sharper and more robust than the recurrence plot (RP) method used herein, namely an augmented Kolmogorov-Smirnov test. We intend to add Supplementary Material to the revised version that shows how, using this method, we are able to detect the various transitions described in the two records studied in the present paper; see the supplementary figures.

Likewise, Boers et al. (2018) (with several of the current authors) is cited extensively throughout, while many seminal / standard papers on DO variability are ignored.

Boers et al. (2018) gave a detailed list of references covering DO variability, which was the topic of their paper. We have added to this manuscript "see Boers et al. (2018) and references therein."

The interpretation of the recurrence diagrams is very subjective.

We have corrected this apparent subjectivity by conducting a recurrence rate (RR) analysis allowing one to precisely select the major transitions; see also the final Reply to Reviewer #1, labeled AC4 in the Discussion of this paper, https://doi.org/10.5194/cp-2021-103.

The authors appear to visually identify "steps" in the RP diagrams, that are listed. However, it remains unclear what criteria were used to select these steps.

In the revised version of the manuscript, we refer to the RR and the selection among the detected major transitions by using the value of the standard deviation of the various analyses performed of the RR using different window sizes; ; see also the final Reply to Reviewer #1, labeled AC4 in the Discussion of this paper, https://doi.org/10.5194/cp-2021-103.

What is the significance of such "steps"? Are these times that the climate system undergoes some transition? From looking at the records, in can just be a period of below-average variability. In most cases the steps from the RP diagrams are not meaningfully evaluated. By looking at the diagrams, it is unclear that I would have picked the same "steps", adding to the sense of subjectiveness. The RP terminology is further not clearly defined. Terms like "drift topology" are used throughout, but not defined. Doesn't this simply mean that there is a long-term trend in the underlying dataset? I am unclear what new insights, if any, have been gained using the RP.

Thanks once more for emphasizing the need for clarification on the RP terminology. We did revise the text accordingly; ; see also the final Reply to Reviewer #1, labeled AC4 in the Discussion of this paper, https://doi.org/10.5194/cp-2021-103.

Last, the paper has several statements that are either incorrect, or simply not supported by the available evidence. Most of the bullet points in their conclusions fall in the latter category.

We trust that the revision of our manuscript does provide the evidence that Reviewer #2 was missing.

For the benefit of revising their manuscript for future submission, I provided some minor suggestions by line number.

Line 17: "these processes varied considerably during the past 2.6 Myr" Where does this claim come from? I don't think we know

This line is part of the abstract of the paper (Lines 12 to 32) and it addresses part of the content of its main text, rather than previous knowledge. We have changed the sentence in question to read: "Abrupt changes, however, appear to require fast processes that are internal to the climate system; such processes were active during the past 2.6 Myr, and yielded climate fluctuations that were more irregular than those that can be directly attributed to changes in the Earth's orbit."

Line 88: that ARE dominant

No, we speak about the "intriguing transition" between the 40kyr and 100kyr cycles. Therefore, we keep "is"

Line 96: "Recent" perhaps only compared to studies of the orbital theory.

Of course. We have therefore rephrased the sentence to read as follows: " Although the broad astronomic framework for past climate changes seems to be widely accepted, high-resolution investigations over the past two decades in ice, marine and terrestrial records"

Line 106: the structure of this section is somewhat unclear. The section provides more review-type writing, but also presents the methods used, the results, and their discussion.

Thanks for the request for clarification. We have restructured the section by first introducing the methods and the material that we are using in this paper with a classical "Method and material" section (#2), and then the proper "Past 3.2 Myr history of Northern Hemisphere climate" (#3).

Line 108- 112: This section adds little. Consider removing?

This part of the text places our study within a broader perspective on climate change and observed abrupt climate transitions. We think it is helpful for the less expert reader of this Special Issue and prefer to keep it.

L142 to 144: The Barker record is artificial, and not a good reference for the onset of DO variability. DO-like events have been observed 1.3Ma ago (Birner, Hodell, Tzedakis, & Skinner, 2016).

We agree with Reviewer #2 that the Barker record is a synthetic one based on the EPICA $\delta^{18}O$ record and the bipolar seesaw model. However, it yields a continuous reconstruction over the past 800 ka. We are referring to it because we are addressing our third date "close to MIS 22-24 $\delta^{18}O$ optima. The Birner et al. (2016) paper relates to a marine record on the Iberian Margin but investigates the millennial variability during the time interval 1235–1220 ka, MIS41-37, therefore much older. Although detecting variations in planctonic $\delta^{18}O$ that are comparable to the MIS3 DO events in intensity and evolution (sawtooth like), Birner and coauthors indicate that "However, identifying further Bond-like cycles in MIS 38 and 40 is ambiguous. Although the lack of additional cycles might be due to the short duration of glacials in the 41 ka world, the occurrence of Bond-like cycles in the early Pleistocene would not necessarily be expected, owing to their intrinsic relationship to Heinrich events [Bond et al., 1993] that have not been observed in the early Pleistocene [Hodell et al., 2008].", especially because the closing stadial of these cycles does not show a particularly massive IRD discharge as present in the Heinrich events as described during the last climate cycle.

Line 157: "mere visual inspection"; isn't that exactly how you evaluate the recurrence plots also? Visually?

No, we just indicate that a visual inspection of the record could lead to proposing some supposedly major abrupt transitions that would require justification. This is what we indicate in the following sentence with " To gain further insight into the climate story the records tell us, we performed a quantitative, objective analysis of these time series of proxy variables, based on the recurrence plots..."

Line 184: "recurrence analysis shows a drift topology". Isn't this just a fancy way of saying that there is a long-term trend in the data?

We have rephrased the sentence to clarify this expression. However, there is nothing fancy, just using the proper terminology, established by Eckman et al. (1987).

Drift topology is not formally defined. What does it mean in this context?

See previous reply and also the final Reply to Reviewer #1, labeled AC4 in the Discussion of this paper, https://doi.org/10.5194/cp-2021-103.

Line 187: What are these 5 steps based on? It seems to be a somewhat arbitrary pick. What are the criteria for selecting a pick? How robust is the number of steps to the selection criteria?

We have revised the manuscript by providing the RR values, their prominence values and the standard deviation deduced from the analyses using various window sizes. These data allow us to define robustly 5 steps, with a prominence higher than the standard deviation, and discuss one more possible step.

Line 195: Again, what is a drift topology precisely?

Rephrased

L204: we don't know the CO2 concentrations during this interval very well. Van de Wal is cited as if it were a true reconstruction, which it is not of course.

We agree with Reviewer #2 that the CO_2 concentrations prior to 800 ka from the ice cores are subject to debate. However, there seems to exist some agreement about the trends in the reconstructions during this time interval.

L230: "This return generally happens in two steps, thus forming DO cycles of variable duration that does not exceed a millennial time scale (Broecker, 1994; Boers et al., 2018; Boers, 2018)." I don't know what the authors are trying to state, and why these references are used. The studies by Boers et al. don't present any original data, and any estimates of DO timescales have been given by earlier authors.

We apologize if the sentence wasn't clear enough. We have attempted to describe what a Dansgaard-Oeschger (DO) cycle is as there can be some confusion between DO events and DO cycles. More importantly, the sawtooth-like shape of the DO cycles is similar to the Bond cycle one.

In Broecker (1994), the author indeed doesn't provide any precise DO timescale but indicates "climate cycles averaging a few thousands of years in duration". In Boers et al. (2018) there is, once more, no exact value for the DO time scales, but Figure S1 in their appendix indicates the durations of the stadials and interstadials in the NGRIP record.

Numerous DO timescales have been published by Rasmussen et al. (2014), Wolff et al. (2010), Rousseau et al. (2017) and, most recently, Capron et al (2021). However, no DO cycle timescale has been published yet. We have corrected the text accordingly.

L242: The 1982 Dye 3 core already confirmed the rapid events seen in Camp Century

Not exactly in these terms and it is only in Johnsen et al. (2001) that all Greenland records are correlated.

Dansgaard et al. (1982) indicate that "When the details in the two $\boldsymbol{\delta}$ profiles in Fig. 1 are studied, it appears that essentially all of the $\boldsymbol{\delta}$ oscillations in the Dye 3 core down to y = 50 m can be correlated with the previously mentioned features in the Camp Century core down to y = 75 m [...]."

L255 – 263: I don't understand the goal of discussing this. The KS analysis is not used in the manuscript, is it?

The point here is to outline how abrupt transitions are detected and we report that some of the detected sub-events given in Rasmussen et al. (2014) do not withstand the KS test. As the Chaos paper is in press, we have added a supplementary figure illustrating this result.

L263: "with Southern Hemisphere warmings occurring prior the Northern Hemisphere ones." A better way to describe their phasing is an integrator / integrand relationship. Also, the Antarctic and Greenland ice cores are not representative of their respective hemispheres of course.

The quoted phrase is in line 268 but Reviewer #2 is right in terms of a more exact explanation. But such an explanation is not within the scope of our paper and we leave the reader with the references given in the paragraph under discussion.

L271: It appears here that the authors confuse the ideas or propagation direction of the climate signal, and the direction of the heat transport. Oceanic heat transport in the N Atlantic is northward, but that does not mean that DO events originate in the Southern Ocean. One of the few studies suggesting a true South-to-north direction is Knorr and Lohmann (2003); most others all suggest N-to-S, despite the direction of heat transport being S-to-N.

Reviewer #2 is correct and we merely focused on the AMOC variation. We will add the Knorr and Lohman (2003) paper in the discussion and rephrase the paragraph.

L278: There are many good models of DO dynamics, this is a case of self-citation.

That was definitely not the intent. It is merely the case that Boers et al. (2018) were the first, to the best of our knowledge, to effectively link the variabilities of sea-ice, ice shelf and AMOC. The paragraph now starts by stating:

"Many DO models — e.g., Buizert & Schmittner (2015), Dokken et al. (2013), Ganopolski & Rahmstorf (2001), Lohman & Ditlevsen (2018), Peltier & Vettoretti (2014), Shafer et al. (2004), Klockmann et al. (2018), Menviel et al. (2014; 2020), or Timmermann et al. (2003) — have not specifically addressed the issue of the interhemispheric signal's direction. To address this issue, Boers et al. (2018) recently [...]"

L305: "the length of the GIs appears to be related to the mean sea level.". Variations on this observation has been made several times by various authors. See my list of suggested papers at the beginning of this paper. Also, I don't see how or why this is derived from the RP.

Thanks to Reviewer #2 we have added the "missing references". The RP allows us to show changes in the system's regime of behavior as identified by transitions detected by analysing the RR values. Using the appropriate thresholds for both the relative sea level and NGRIP curves, one can observe that the longest GIs occurred when the sea level was relatively stable while the shortest GIs occurred during strong changes in the sea level. Reviewer #1 suggested preparing a scatter plot to better illustrate this point. We have followed that suggestion; see the supplementary figure.

L317: The first naming of the Bond cycles comes from this paper (Lehman, 1993), and not from the papers cited.

Reviewer #2 is correct. We have added the Lehman (1993) News and Views item published in Nature. Our mistake comes from Broecker (1994), who was referring to the cycles named "Bond Cycles" without any reference to Lehman (1993). Amazingly, Wallace Broecker thanks Scott Lehman for his comments on the 1994 paper, while Lehman could have insisted on the paternity of this term, as Reviewer #2 suggested.

L350: it goes back to at least 1.3 Ma, and perhaps further (Birner et al., 2016).

See our reply above to Reviewer #2's comment regarding lines 142–144. The observations we reviewed allowed us to report on a start date at 0.8Ma or 0.9Ma. We will consider how to integrate this reference and its results in the revised paper.

L371: How can a 2011 study confirm something suggested in a 2018 paper? In this case the 2011 paper already suggested it.

This is exactly what we were reporting: Marcott et al. (2011) reported deep water temperature estimates based on Mg/Ca data values from benthic foraminifera and ocean model results suggesting a subsurface warming of Northwest Atlantic prior to the Heinrich events. Using "a coarse-resolution (T31/GR30) setup of the CMIP3 Max Planck Institute climate model ECHAM5/MPIOM/LPJ (Mikolajewicz et al., 2007b) coupled with a 20 km Northern Hemisphere setup of the modified Parallel Ice Sheet Model (mPISM) version 0.3 (Bueler and Brown, 2009; Winkelmann et al., 2011 for PISM) […]", Ziemen et al. (2019) were studying more specifically the Heinrich event dynamic. They found, among other results, similar sub-surface warming prior the Heinrich events, as observed by Marcott et al in (2011).

L398: "This fairly well-agreed-upon fact leads support to the interpretation of the enhanced millennial variability during glacial times as arising from an internal oscillation of the climate system — as proposed by several authors". I don't follow (or agree with) this line of thinking. Yes, the DO events are not forced by orbital variability, but it could still be forced by something else – such as internal ice sheet variability.

We don't have any problem with Reviewer #2's conjecture. We are just not able, based on the evidence we review, to infer an internal forcing from ice sheet variability.

Line 410: aren't abrupt changes identical to these phenomena?

We have rephrased the first bullet point to avoid possible confusion.

Line 412: none of the material presented provides any evidence for an internal oscillation. While the internal oscillation is certainly the most commonly held view in the field, this is not proven – certainly not by the authors in the present paper.

Contrary to Reviewer #2's statement, we maintain that analyzing the NGRIP and U1308 records, including the RR analysis, allowed us to depict internal oscillation.

Line 415: This is again pure speculation. There is no evidence provided that Bond cycles are linked to the NH ice sheet extent. For example climate model experiments would be needed to prove this.

Once more, we beg to disagree with Reviewer #2's statement. The classical Bond cycles end by massive iceberg discharges into the North Atlantic ocean mainly from the Laurentide ice sheet, but also from the Fennoscandian, Greenland, Iceland and British ice sheets. IRD releases to the North Atlantic have been documented as well during every stadial. Therefore a link to the NH ice sheet extent appears evident in order to allow iceberg calving into the North Atlantic, whatever the magnitude of the calving event, whether IRD events or HEs.

Line 419: It is observed as early as 1.3 Ma, but possibly earlier (Birner et al., 2016)

The statement "at least since 0.9-0.8 Ma" does not contradict the possibility of a start "as early as 1.3 Ma." We simply feel more confident about the statement as it stands.

Line 427: I agree that it modulates their period, but I have not seen any evidence for their amplitude being modulated.

We are adding some more information in the description and discussion of our results.

Line 429: I am not sure this is supported by the available evidence. Birner et al. (2016) suggests for MIS 38 and 40 the DO-type variability is indistinguishable from MIS3, despite presumably very different ice sheet sizes in the NH (40ka world vs. 100 ka world). So much for NH ice sheets being important!

Our study leads us to the conclusion as formulated. We do not feel that repeating its contradicting the Birner et al. (2016) inference adds much to the matter. It could be that on the Iberian margin the similarity with earlier variability was, indeed, greater than for the records we studied in substantial detail. It is not exactly clear to us at this point how to settle the differences between their study and ours. Typically, high-end climate models used in multi-millennial paleoclimate studies do not have the requisite spatial resolution to exhibit great differences between one small region and another one.

References used in this review:

Birner, B., Hodell, D. A., Tzedakis, P. C., & Skinner, L. C. (2016). Similar millennial climate variability on the Iberian margin during two early Pleistocene glacials and MIS 3. Paleoceanography, 31(1), 203-217. doi:10.1002/2015PA002868

Buizert, C., & Schmittner, A. (2015). Southern Ocean control of glacial AMOC stability and Dansgaard-Oeschger interstadial duration. Paleoceanography, 30(12), 2015PA002795. doi:10.1002/2015pa002795

Kawamura, K., Abe-Ouchi, A., Motoyama, H., Ageta, Y., Aoki, S., Azuma, N., . . . Yoshimoto, T. (2017). State dependence of climatic instability over the past 720,000 years from Antarctic ice cores and climate modeling. Science Advances, 3(2), e1600446. doi:10.1126/sciadv.1600446

Lehman, S. J. (1993). Ice sheets, wayward winds and sea change. Nature, 365(6442), 108-110.

Lohmann, J., & Ditlevsen, P. D. (2018). Random and externally controlled occurrences of

Dansgaard–Oeschger events. Clim. Past, 14(5), 609-617. doi:10.5194/cp-14-609-2018

Lohmann, J., & Ditlevsen, P. D. (2019). Objective extraction and analysis of statistical features of Dansgaard–Oeschger events. Clim. Past, 15(5), 1771-1792. doi:10.5194/cp-15-1771-2019

McManus, J. F., Oppo, D. W., & Cullen, J. L. (1999). A 0.5-Million-Year Record of Millennial- Scale Climate Variability in the North Atlantic. Science, 283(5404), 971-975. doi:10.1126/science.283.5404.971

Schulz, M. (2002). The tempo of climate change during Dansgaard-Oeschger interstadials and its potential to affect the manifestation of the 1470-year climate cycle. Geophysical Research Letters, 29(1), 2–1-2–4. doi:10.1029/2001gl013277

Schulz, M., Berger, W. H., Sarnthein, M., & Grootes, P. M. (1999). Amplitude variations of1470â€year climate oscillations during the last 100,000 years linked to fluctuations of continental ice mass. Geophysical Research Letters, 26(22), 3385-3388.

Schulz, M., Paul, A., & Timmermann, A. (2002). Relaxation oscillators in concert: A framework for climate change at millennial timescales during the late Pleistocene. Geophysical Research Letters, 29(24), 2193. doi:10.1029/2002gl016144

Referee #3, Dr. Linda Hinnov

OVERVIEW

Thank you to reviewer #3, Dr. Hinnov, for their comments and suggestions that we are using in the revised version of our manuscript.

The authors carry out recurrence analysis of two paleoclimate proxies from the North Atlantic core U1308, covering the past 3.2 million years. One proxy represents global ice volume and deep ocean temperature, and the other proxy represents ice raft debris deposition. They also analyze the NGRIP water ice d^{18}O record representing temperature at the top of the Greenland ice sheet for the past 110,000 years. The authors identify thresholds in RP topology that coincide with previously inferred paleoclimate transitions.

RECOMMENDATION

The authors attempt to accomplish two things at once: (1) review the history of Cenozoic climate change research,

Not so at all. If we intended to do so, we would have covered much more ground than is covered in the present manuscript.

and (2) newly analyze the past 3.2 million years of climate change with recurrence plots. This does not work very well,

Of course, as we only focused on the past 3.2 Myr and not at all on the entire 66 Myr of the Cenozoic era.

and in my estimation (1) should be abandoned, and (2) should be singularly pursued with its novel and illuminating possibilities.

We beg to differ on Reviewer #3's point (1), as per above, and do not exactly understand what they mean by their point (2).

There are dozens of vaguely relevant facts and factoids

Factoids are not well defined in scientific terminology and sound a bit insulting, which we're sure was not the intent of the reviewer.

mentioned only once and never tied to anything else in the paper, such as the long commentary on the history of the astronomical theory of climate change, and the description of the unused change-point method of one of the co- authors. These could easily be removed from the paper.

What Reviewer #3 names "vaguely relevant facts and factoids" correspond to the observations we have made after performing the analysis of the U1308 and NGRIP datasets using a method that appears to be rather novel in paleoclimatology, despite several papers published as lead or co-author by N. Marvan. As indicated in our reply to Reviewer #1, although the title of the manuscript is "Abrupt climate changes and the astronomical theory," we didn't intend to submit a complete review paper on the astronomical theory, which is the object of the entire Special Issue to which this paper was submitted. Instead, we just wanted to sketch the evolution of ideas on the specific aspect of abrupt climate changes through a selected subset of papers, while injecting some of our own thinking and recent results. Such a selection cannot be entirely devoid of personal preferences.

The CENOGRID recurrence analysis by Westerhold et al. (2020) provides a convenient starting point, in that the RP topology there is governed by major climate reorganizations. The authors could pick up on that as a lead into their discussion of climate reorganizations and thresholds over 0-3.2 Ma and over the Last Glacial Cycle (0-100,000 years bp).

We thank Reviewer #3 for this remark, since this is exactly what we did. In fact, the CENOGRID recurrence analysis by N. Marvan in Westerhold et al. (2020), does show major climate reorganization, but in Fig. 2B of that paper, the recurrence plot (RP) in the upper right corner corresponding to the last 3.3 Myr doesn't show any particular pattern. This is the reason why we decided to investigate the last 3.3 Ma through the high-resolution core U1308, as well as the NGRIP $\delta^{18}O$ record.

It would be helpful to discuss the meaning of the various patterns in the recurrence plots. For example, in the NGRIP recurrence plot (Figure 4), there is a region with a highly periodic signature, from 30 ka to 60 ka, that does not occur anywhere else. Turning one's head 45º so that the diagonal is in the vertical position has a powerful effect on visualization. It would be interesting to point out this and other features along all three of the analyzed time series.

We are now discussing the RPs according to the analysis of the recurrence rate (RR), following up on the suggestions for clarification of Reviewers #1 and #2. Doing so allows us to robustly distinguish the significant thresholds from the others. In particular, our RR analysis allows us to identify 58.9 ka and 47 ka (b2k) as significant transitions, and discuss 38.3 ka, too.

Finally, much is discussed about modeling DO and Bond cycles, but the recurrence plots have a very limited role in these discussions. It would be helpful to show how these recurrence analyses supplement our knowledge about millennial-astronomical climate change connections.

Thanks to Reviewer #3 for this valuable suggestion that we are using in revising our manuscript.

COMMENTS

The comments below are linked to line numbers. Additional editorial suggestions (blue and red markings) and comments (yellow sticky notes) are provided in the annotated version of "cp-2021-103-LAH.pdf".

Abstract

Most the text here is information that is better suited for the Introduction. The results of the present study should be summarized here.

Lines 12-18: Rephrase perhaps as follows: Abrupt climate changes are defined as sudden climate changes that took place in tens to hundreds of years and/or recurred at millennial time scales, involving processes that are thought to be internal to the climate system. By contrast, astronomically forced climate changes involve parameters that are external to the climate system and whose multi-millennial quasi-periodic variations are well known from astronomical theory.

Thanks to Reviewer #3 for this valuable suggestion. We have rephrased these lines accordingly.

Line 26: What is a Bond cycle? This should be defined earlier in the text, prior to the introduction of "amended Bond cycle".

We have removed the word "amended" and the sentence now reads as follows: " Combining the HE, IRD and DO observations, we study a complex process giving rise to the observed millennial-scale variability that subsumes the abrupt climate changes of the last 0.9 Myr. This process is characterized by the presence of Bond cycles, which group DO events and the associated Greenland stadials into a trend of increased cooling, with IRD events embedded into every stadial, the latest of these being an HE

1 Introduction

The Introduction includes an unexpected and long historical commentary on the astronomical theory of climate change, which can be abbreviated considerably by deleting Lines 49-95 in favor of focusing on the intersection of abrupt and astronomical climate changes.

As we indicated in our replies to the two previous reviewers, we did not mean to provide a complete review of the astronomical theory of climate: such a review is given by other papers in this Special Issue. Instead, we just wanted to sketch the evolution of ideas on the specific aspect of abrupt climate changes through a selected subset of papers, while injecting some of our own thinking and recent results.

We prefer to keep the presentation here as is and have modified the Abstract as suggested by Reviewer #3.

Most of the Abstract as presently written would be relevant here.

Please see the previous relevant replies.

Other topics that could improve the introduction:

- Explain the connection between CENOGRID, U1308 and NGRIP (referring to Fig. 1)

Figures 1a,b,c are discussed in detail in Sec. 2 and in the caption of the figure. We fail to see the point of lengthening the Introduction by bringing the figure into it.

- Comment on the recurrence plots of CENOGRID by Westerhold et al. (2020) as a way to introduce recurrence analysis, and application of the methodology in the work here.

Following Reviewer #2's suggestion and your own later on, we will restructure the paper, if accepted, by including a Materials and Methods section, in which both the datasets and the methods used are described. Doing so clarifies the paper's structure. Concerning the recurrence analysis, we think it better to guide the reader towards the key papers describing the method, namely Eckman et al. (1987) and Marvan et al. (2007, 2013).

• Previous work on astronomically paced ice volume link to millennial scale climate variability.

Lines 39-43: Change to something short like this (with apologies for suggesting some of my publications): Geological data indicate that the Earth has experienced astronomically paced climate changes throughout its history (reviews by Hinnov, 2013, 2018).

We have added these two references after the citations of Berger (1977) and Laskar (2011). Now the sentence reads as follows: "These changes reflect the variations in the Earth's axis of rotation – namely in its precession and tilt — and in the geometry of the Earth's orbit around the sun, i.e., in its eccentricity, driven by gravitational interactions within the solar system (Berger, 1977; Laskar et al., 2011; Hinnov, 2013, 2018)"

2 The Past 3.2 Myr History of the Northern Hemisphere Climate

Lines 115-116: Briefly describe the "new relationship" between the carbon cycle and climate proposed by Turner (2014).

Following Reviewer #3's suggestion, the sentence now reads as follows: " which corresponds to a new relationship between the carbon cycle and climate. Indeed, the negative excursions in $\delta^{13}C$ were associated with negative ones in $\delta^{18}0$ during most of the Cenozoic since 66 Ma. A shift occurred, though, in the Plio-Pleistocene, at about 5 Ma, with negative $\delta^{13}C$ excursions associated with positive $\delta^{18}0$ ones (Turner, 2014). Such a shift appears to be related to a dichotomy in the response of the marine and terrestrial reservoirs of the carbon cycle dynamics to orbital forcings."

Line 118: Include the geographic coordinates of the U1308 site (49.87N, 24.24W) and NGRIP (75.1 N, 42.32 W).

Thank you. The sentence now reads as follows: "... particularly well described in the North Atlantic core U1308 at (49.87N, 24.24W) (Hodell and Channell, 2016), while the last climate cycle is well represented by the Greenland NGRIP ice core at (75.1N, 42,32W) (NGRIP community, 2004). The U1308 core...".

Lines 120-123: Rephrase, perhaps as follows: ...and reflect the benthic marine $d^{18}O$ record stack of 57 marine cores from the world's oceans (Lisiecki and Raymo, 2005).

We have clarified our sentence as follows " The variations in the benthic $\delta^{18}O$ mostly indicate varying periodicities through time that correspond to periodicities in the orbital parameters of Earth's climate (Hodell and Channell, 2016; see suppl. fig. S1), as also pointed out by Lisiecki and Raymo (2005) from the stack oxygen isotope record they produced using 57 marine records from the world's oceans".

Lines 156-158: The statement here is perplexing, when there are hundreds of quantitative studies of Cenozoic paleoclimate proxy records, especially of the past 3 million years, that have been made over the past 50+ years. These lines could be deleted.

We acknowledge that the statement could be confusing and therefore we have removed these lines.

Line 158: At this point a new section should be started entitled, "Materials and Methods" which should continue from Lines 158-181, and add text here about the time series being investigated. It looks like NGRIP is also analyzed (Fig. 4) and should be introduced here.

As mentioned earlier and following the recommendation of Reviewer #2, too, we will restructure the manuscript, if accepted, including such a new "Materials and Methods'" section. Thank you.

Line 182: This should be the start of a section entitled "Results".

Correct. Thank you.

Lines 201-205: The meaning of "sea level variations of about 25-50 m below the present-day" is unclear. Does "below" mean that sea level was below present-day sea level (of 0 m)

Yes, exactly this is what the available dataset used by van de Wal et al. (2011) shows. We consider that it is worth reproducing the figure published in Climate of the Past. However, checking the available dataset, we changed 50 m to 70 m. The sentence now reads as follows: "The interval 2.8 to 1.2 Ma shows glacial–interglacial sea level variations of about 25–70 m below the present-day value."

or that the sea level variations during 2.8-1.2 Ma were 25-50 m smaller in amplitude compared to 1.2 Ma to present? Same confusion for "After 1.25 Ma...etc".

We have added "value" after "present day" and the sentence reads now: "After 1.25 Ma, the sea level changes decreased to about 70–120 m below the present-day value",

3 Millennial-Scale Variability

Line 220: Is there a reliable reference for the increase of IRD at 1.5 Ma?

We are referring here to our own results and to those published by Hodell and Channell (2016) about the bulk carbonate $\delta^{18}O$.

Lines 229-233: Strong DO cycles found in GISP2 and GRIP ice $d^{18}O$ (also in marine core MD95-2042) over the Last Glacial Cycle occur in a very narrow frequency band centered on 1/(1470 yr) (Hinnov et al., 2002). Schulz (2002) and Rahmstorf (2003) likewise noticed that DO cycles have a persistent 1470 yr period, and that if a DO warming event was missed, one then occurred in the future at a multiple of 1470 yr. So, the phrase "DO cycles of variable duration" needs further explanation/qualification. When/where are there submillennial DO cycles? Or, is this meant to indicate the short events that occur within DO cycles (shown in Fig. 5)?

If one considers the original DO cycles, including the DO interstadials and the associated Greenland stadials, one can notice that these DO cycles don't have the same duration. Computing the duration of these original cycles by using the limits published by Rasmussen et al. (2014), one gets an average of 4045 yr ± 3179 yr. Therefore, it is difficult to trust the persistent 1470-yr period claimed by Schultz (2002) and Rahmstorf (2003).

Lines 253-263: This passage describes ongoing work by Bagniewski et al. on a new method using a

Kolmogorov-Smirnov test to identify abrupt transitions ("change points") in time series, available as a short presentation at https://www.essoar.org/pdfjs/10.1002/essoar.10506097.1
However, it is unclear if the test has been used anywhere in this paper. Is it used to identify the transitions shown by the vertical "threshold" lines in Figs. 2, 3 and 4? (Apparently not.) I propose to remove this from the paper.

The new method is in press in Chaos and was indeed used to determine the major transitions in U1308 and NGRIP. In the marine core, the method proposes objective dates for the marine isotope stratigraphy, which Hodell and Channell (2016) linked to the astronomical parameters. In the NGRIP record, this method allows one to also objectively determine the abrupt transitions as presented by Rasmussen et al. (2014). However, the complementary recurrence analysis allows one to select among these abrupt transitions the ones that are related to important changes in the system, allowing us therefore to associate the astronomical theory of climate and the millennial variability. We will clarify this point in the new "Materials and Methods" section of the revised version. Moreover, we are adding 2 supplementary figures showing the abrupt transitions detected by the new method in both the two U1308 records and the NGRIP δ18O record.

Lines 264-266: The work of Hinnov et al. (2002) on methane-linked GISP2 and Byrd included statistically constrained spectral coherency analysis to demonstrate the global reach of the DO cycles, as well as what at the time were called the "Antarctic warming" cycles (with a 4.44 kyr period), each with interesting lead-lag relationships.

We are adding the reviewer's reference. "Moreover, Hinnov et al. (2002) also carried out [...]"

Lines 268-269: How will this statement that Antarctic warms before Greenland - who said that; Hinnov et al. (2002) did, but who else? –

Well, the seminal paper of Blunier and Brook (Science, 2001) did but this lead-lag relationship was better supported by the WAIS Divide project members (Nature, 2016), by comparing the NGRIP and WAIS ice cores.

be reconciled in the next paragraph that argues that Greenland climate leads Antarctica by approx. 200 years.

In the same WAIS (2016) paper, the authors indicate that "We find that on average the DO cooling signal is transmitted as fast to Antarctica as the DO warming signal is (our sensitivity study suggests a difference in propagation time of 10689 years). This implies that the north-to-south propagation time is independent of the AMOC background state; that is, it is independent of whether the AMOC is in the weak or strong overturning state."

Lines 273-275: Consider mentioning the results of the never-cited Hinnov et al. (2002): coherency analysis reveals a time lead of Byrd DO (Antarctic) cycles over GISP2 DO (Greenland) cycles by 384±70 yr (2s level), and of the North Atlantic benthic DO (AABW) cycles over planktonic DO cycles by 208±33 yr (2s level).

Yes, pls. see above.

Line 285: What is meant by "subsumed by the 65ºN summer insolation"?

Since the word "subsumed" does not seem to be familiar to Reviewer #3, we will drop the end of this sentence, "whether subsumed by the 65ºN summer insolation curve or not." What is meant is that orbital forcing acts in complex ways at different seasons and different latitudes, which are not really subsumed — see Merriam-Webster or other standard English dictionary — by this particular curve.

4 DO events and Bond cycles

Lines 300-301: The recurrence plot of NGRIP shows very pronounced well defined patterns, whereas those of the U1308 d^{18}O records are harder to understand.

We have revised the recurrence plots of NGRIP and U1308 by adding the plot of the recurrence rate below the recurrence plot, and identifying the significant transitions as defined by their prominence. Please see the replies to Reviewers #1 and #2, as well as the attached figure.

Lines 343-344: It is not clear what the difference between the traditional Bond cycle (e.g., Alley, 1998) and the "amended" Bond cycle proposed here. A figure contrasting the two models would be helpful, e.g., add Figure 1 of Alley (1998) next to current Fig. 6a.

In the traditional Bond cycle as sketched by Alley, the DOs show a clear decreasing trend in the warming intensity and the last stadial includes an HE. This is precisely what we have reproduced in our Figure 5, with a reference to the long cooling trend indicated in the Bond et al. (1992) paper. In the "amended" Bond cycle, we include the fact that all stadials include an IRD event whose amplitude culminates in the last stadial and corresponds to an HE, the massive iceberg discharge, as illustrated in Fig. 6a.

5 Concluding Remarks

Lines 407-408: Here it is stated that this paper is an "overview of millennial-scale climate variability over the last 3.2 Myr." But the authors really only discussed millennial scale climate for the past 0.1 Myr (the Last Glacial Cycle).

Not quite so. We have tried to demonstrate with our analysis that millennial-scale climate variability started at least at about 0.9 Ma, when DO-like events are clearly present, although Reviewer #2 suggested to let such events start at about 1.3 Ma.

Lines 420-421: Here it is indicated that millennial scale variability has been observed in glacial periods previous to the Last Glacial Cycle, "at least since 0.8-0.9 Ma." There was mention of EPICA modeling of Greenland suggesting that DO cycles should have continued throughout the past 800 ka – but has geological evidence been recovered yet?

Yes, please see previous reply and comments by Reviewer #2, cp-2021-103-AC5+Suppl.

There is a new report of millennial scale variations in MIS 19 Interglacial, 0.760 to 0.790 Ma (Head, 2021), so limiting the discussion to glacials only may not be sufficient or accurate. Finally, where did HEs first appear in the record?

We will indicate more clearly in the revised paper, if accepted, that we are addressing the millennial climate variability during glacials only, as related to the occurrence and expansion of NH ice sheets, while millennial variability during interglacials relies on other mechanism(s). Concerning HEs, they first appear by 0.65Ma, as indicated in the original manuscript at lines 144-154 and 335-341.

Figures

Figures 2 and 3: In Figure 2, the top of the figure has the title "Recurrence U1308 benthic δ18O"; in Figure 3, the top of the figure has the title "Recurrence U1308 bulk carbonate δ18O". These are helpful designations and could be included in the figure captions. In both figures, there is a blue curve called "Cibicidoides sp. δ18O bulk (VDP)" and presumably this is analyzed only in Figure 2 and is the U1308 benthic δ18O. There is a green curve called "δ18O bulk carb (PDB)" and this is analyzed only in Figure 3. I recommend showing only the relevant time series in these two figures (blue curve in Figure 2 and green curve in Figure 3).

These figures have been corrected and are included in the reply to Reviewer #1, cp-2021-103-AC4+Suppl. We have decided to keep both benthic and bulk carbonate $\delta^{18}O$ curves to help the reader locate where the selected transitions occur in the record under study and to what features they correspond in the companion record.

Figures 2-4: Remove the sentence: "The RP web site is http://www.recurrenceplot.tk" and add once in the text where the recurrence plot method is discussed.

Done, thank you.

Figure 6a: This "amended Bond cycle" illustration should be enhanced by adding the "traditional Bond cycle", e.g., Figure 1 of Alley (1998) to highlight the differences.

The traditional Bond cycle is presented in Figure 5.

Figure 6b: Add locations of U1308 and NGRIP.

Done, thank you.

Other

Upper vs. Late, Lower vs. Early is always confusing: geologists are very particular in the use of this terminology: Upper and Lower refer to rock, and Late and Early refer to time. In this paper, there is no comprehensive description of a stratigraphic section in terms of a rock/sediment formation, therefore only the "time" terms should be used, i.e., Early and Late. (Middle is conferred to both rock and time.)

Done, thank you.

NEW REFERENCES

Hinnov, LA, 2018. Chapter 1: Cyclostratigraphy and Astrochronology in 2018, in Montenari, M., ed., Stratigraphy and Timescales, 3, 1-80.

Hinnov, LA, 2013. Cyclostratigraphy and its revolutionizing applications in the Earth and Planetary Sciences, 125th Anniversary Volume, Geological Society of America Bulletin, 125, 1703-1734.

Hinnov, L.A., Schulz, M., Yiou, P. 2002. Interhemispheric space-time attributes of the Dansgaard-Oeschger oscillations between 0-100 ka, Special Volume: Decadal to Millennial Climate Change, Quaternary Science Reviews, 21, 1213-1228.

Head, M, 2021. Review of the Early–Middle Pleistocene boundary and Marine Isotope Stage 19, Progress in Earth and Planetary Science, 8:50, doi:10.1186/s40645-021-00439-2

Rahmstorf, S., 2003. Timing of abrupt climate change: a precise clock, Geophysical Research Letters, 30:10, 10, 1510, doi:10.1029/2003GL017115

All these references have now been included. Thank you.