Anonymous Referee #2

Referee 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-RC2, 2021

Review of "Abrupt climate changes and the astronomical theory" by Rousseau et al.

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 δ profiles in Fig. 1 are studied, it appears that essentially all of the δ 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.

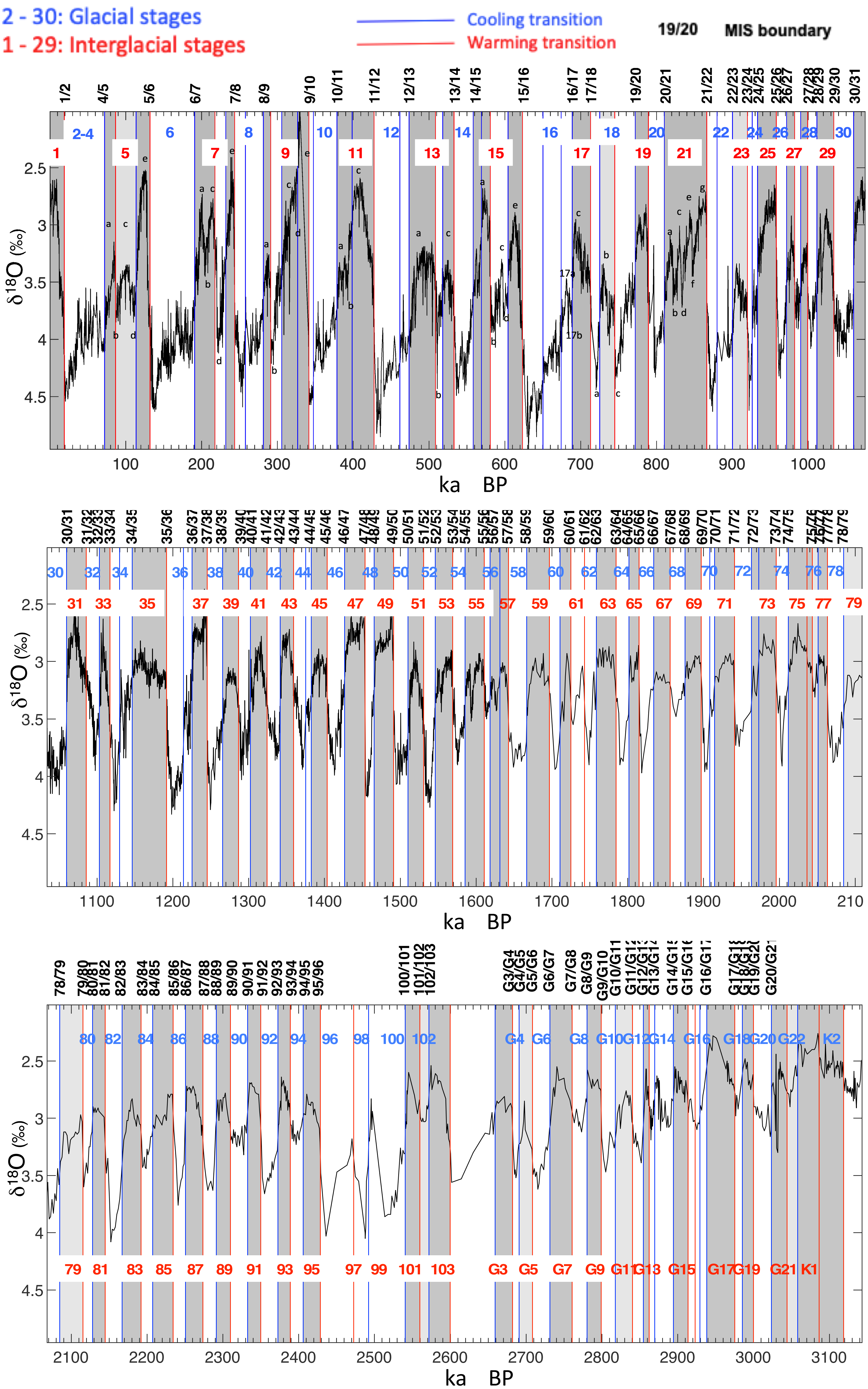
Schulz, M., Paul, A., & Timmermann, A. (2002). Relaxation oscillators in concert: A framework for climate change at millennial timescales during the late Pleistocene. Geophysical Research Letters, 29(24), 2193. doi:10.1029/2002gl016144

Sima, A., Paul, A., & Schulz, M. (2004). The Younger Dryas—an intrinsic feature of late Pleistocene climate change at millennial timescales. Earth and Planetary Science Letters, 222(3), 741-750.

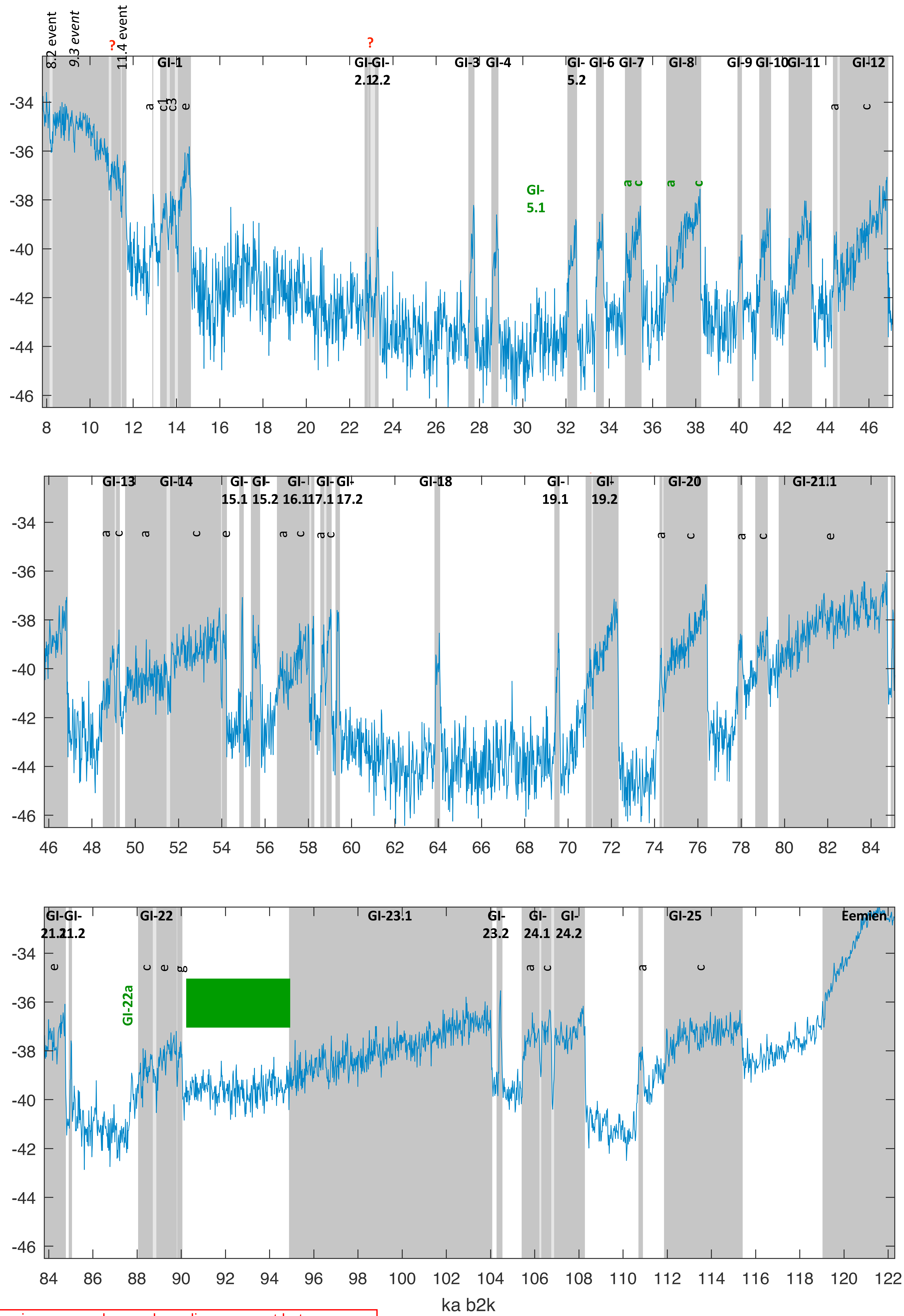
Supplementary figures

- *KS augmented test applied to*
- *U1308 benthic $\delta^{18}0$ record*
- *NGRIP $\delta^{18}0$ record*
- *Scatter plots of DO duration and DO relative sea level vs time expressed by the DO number*

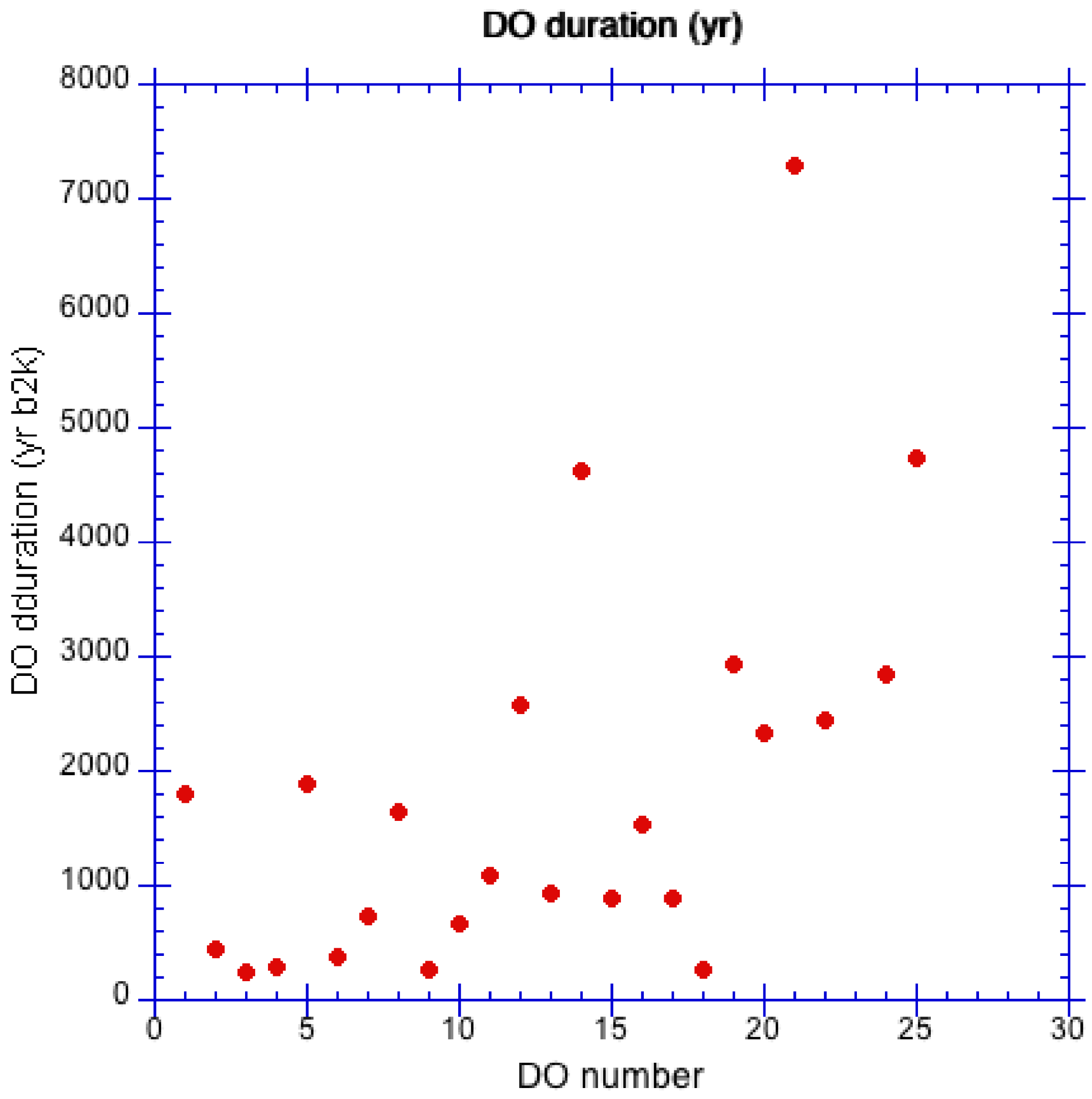
Detection of Abrupt transitions from benthic δ¹⁸O in U1308 with the K-S method

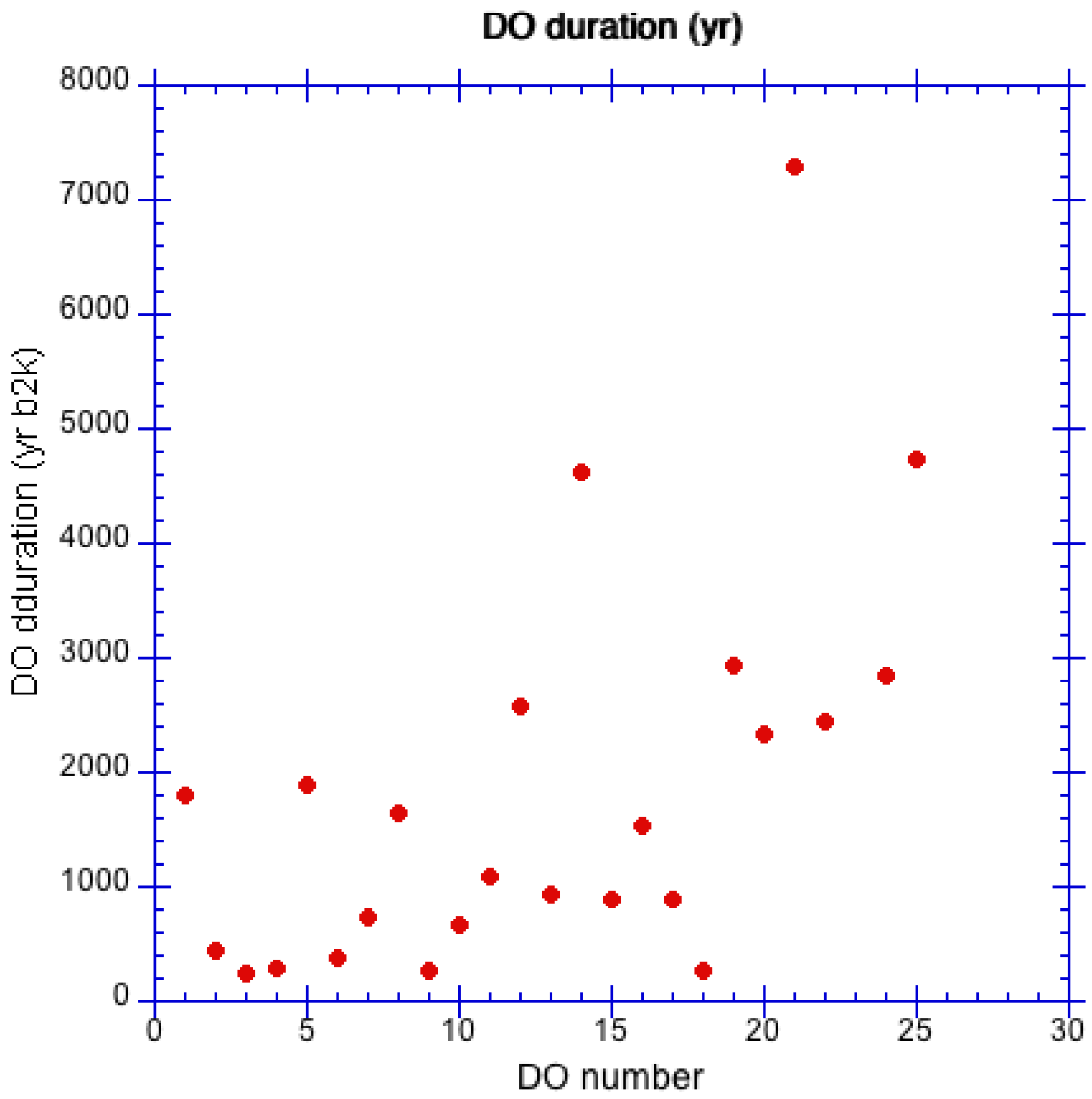


MIS boundaries from Lisiecki & Raymo (2005). Paleoceanography, PA1003, doi:10.1029/2004PA001071 and odd MIS from Hodell and Channel (2016) Climate of the Past, 12, https://doi.org/10.5194/cp-12-1805-2016.
Substage numbering from Railsback et al. (2015). Quaternary Science Reviews 111. https://doi.org/10.1016/j.quascirev.2015.01.012.



Annotation in green and green box: disagreement betwen Rasmussen et al (2014) observations and the KS test results


DO duration (yr)
DO dduration (yr b2k)
DO number
0
1000
2000
3000
4000
5000
6000
7000
8000
0
5
10
15
20
25
30


DO duration (yr)
DO dduration (yr b2k)
DO number
0
1000
2000
3000
4000
5000
6000
7000
8000
0
5
10
15
20
25
30