



Review of Rousseau et al.

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*.

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.

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. 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, 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.

The review given of abrupt climate change (mostly sections 2 and 3) are likewise not very

comprehensive or complete. The 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. 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.

The interpretation of the recurrence diagrams is very subjective. 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. What is the significance of such "steps"? Are these times that the climate system undergoes some transition? From looking at the records, it 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.

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.

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

Line 88: that ARE dominant

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

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.

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

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).

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

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?

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

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?

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

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.

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.

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

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

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.

L271: It appears here that the authors confuse the ideas of 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.

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

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.

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

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

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

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.

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

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.

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.

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

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

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!

References used in this review:

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