

Interactive comment on “Millennial and sub-millennial scale climatic variations recorded in polar ice cores over the last glacial period” by E. Capron et al.

Anonymous Referee #1

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This paper presents some new data and insights into millennial and sub-millennial scale climate variations. The authors present an interesting analysis and discussion of the occurrence of precursor and rebound events associated with the well-known interstadial events of the last interglacial-glacial period (although I am not completely convinced by their discussion of an insolation control on the occurrence of these events given that the posited relationships do not seem consistent – see below). Their analysis of temperature change amplitudes between north and south in the context of the bipolar seesaw is very interesting and highlights extreme glacial conditions as distinct from intermediate states (this section needs more discussion in the text). The paper is well written and generally clearly presented. In general I would like to see more

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contextual discussion of climate records from other archives, particularly from the marine environment. There is also some ambiguity (understandably) as there is a mixture of discussion about sub-millennial variability (precursors and rebounds) and D-O type variability itself (assuming that the two variants are actually distinct, which I feel is unlikely).

I have outlined my comments in order of their relevance:

Abstract line 9: The authors refer to the abrupt changes within MIS 5 as D-O events. It strikes me that the term D-O event might better be reserved for those quasi-periodic and more frequent oscillations observed during MIS 3. This does not mean that the terms stadial and interstadial should not be used for events within MIS 5 but then we should be clear that the occurrence of stadials and interstadials does not define the background state (glacial or interglacial). This comment should be discussed more generally by those with interests in this field.

It is not clear what new measurements this paper represents. In table 2 it states that d15N measurements for DO 21 and 22 are new yet from the text and Fig 3 it suggests that that d15N for DO 21 was published by Capron et al., 2010.

P139 line 28: MIS 5 includes MIS 5e, which was of course an interglacial period, not part of the glacial inception. The authors should be careful with their use of these terms.

P140 para 2: It would be useful to be given some context as to how the timing of GIS within MIS 5 relate to the sub-stages MIS 5a-e (see also next point). For example an oxygen isotope stack could be added to Fig. 1 rather than just the isotope boundaries (this would require careful consideration of relative age scales – e.g. via Shackleton's MD95-2042 records?).

P142 line 21: The authors state that their timescale can be compared with independent chronologies from other paleoclimatic archives – I think it would be very useful for them

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to show such a comparison here. Furthermore it would be very useful if they would include more discussion on how these records compare in terms of their implications for climate reconstruction.

Section 3.1: Here the authors describe the ‘singular case’ of GIS 24 but it seems to me that GIS 24 is only unique if one considers it as a single event. For example, GIS 15-17 within MIS 3 include between 6 to 8 individual warming events depending on what one counts as a distinct warming event.

Section 4.1: The authors need to make clear what they are discussing here i.e. an explanation for the transitions between stadial and interstadial state per se or ‘just’ the occurrence of precursor (and rebound) events.

P151 line 6: Can a ‘rapid’ event have a warm ‘phase’? Use of the term rapid here implies that the event is short-lived.

P152 line 1: Do the authors mean ‘more probable’ or ‘easier to test’ (they do not reject the CO2 control hypothesis based on mechanism but rather lack of data)?

P152 line 10: The authors have mentioned the occurrence of similar events during MIS 3 when ice sheets were larger than MIS 5. Can they define what that mean by large and small ice sheets?

P152 lines 14-23 and Fig 6: This section is a little confusing as the authors discuss possible controls on the occurrence of precursor events but only concentrate on those events which have a rebound event. So they discuss e.g. GIS 11 and 12 (which don’t have precursor events) but many other events also don’t have precursors. My query could be rephrased; why should the occurrence or lack of a precursor event only be of interest if the GIS has a rebound event?

P152 line 16: Following my last point, if we are more generally interested in why only certain GIS events have a ‘precursor’ event we should include other events such as GIS 24, which starts when insolation is as high as for GIS 23 and yet no precursor is

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observed. Do the authors have an explanation for this? We could also question the Bølling transition and the end of the YD, which occur when insolation is 'high' but do not have precursor events.

P153 lines 2-9: The authors describe 6 GIS events as having rebounds yet 3 of these (GIS 11, 12 and 16) do not follow the pattern suggested by the authors (i.e. decreasing insolation leading to rebound event). This does not lend support to their argument. Also, insolation is increasing before the 'rebound event' of GIS 23. Can the authors' mechanism account for this? It is interesting that rebound events tend to be associated with longer GIS intervals. Could a rebound event 'simply' be a subsequent GIS event that the preceding event 'runs into'?

P155 line 18 (Fig 7): The observation of a breakdown of the seesaw model for AIM 2 and 18 is very interesting and must ultimately be related to the severe glacial conditions prevalent during these times. This would be a good opportunity for the authors to include some discussion of their records in a wider context e.g. with respect to marine records, which clearly demonstrate the peculiar conditions during MIS 2 and 4.

Minor comments:

P151 line 12: Suggest change to 'Here we present some of the possible mechanisms...' P152 line 16: maximum (not maxima). Table 2: GS durations given using GICC05 age scale to 50ka (should be 60ka?) Figure 2: Axis labels are confusing – better to have consistency between each plot. Also GIS 21 is labelled GIS 20 and GIS 23 is labelled GIS 21.

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