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Comment on cp-2021-97

Ian Candy (Referee)

Referee comment on "Reorganization of Atlantic Waters at sub-polar latitudes linked to deep-water overflow in both glacial and interglacial climate states" by Dakota E. Holmes et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-97-RC1>, 2021

Review by Daniel Parkes and Ian Candy (Royal Holloway, University of London) of Reorganization of Atlantic waters at sub-polar latitudes linked to deep water overflow in both glacial and interglacial climate states. Holmes et al., 2021

Paper summary

The stated aim of the paper is to investigate abrupt climate events during interglacials/warm climates, with the authors highlighting that these are "*generally attributed as a characteristics of glacial climate states*". In order to do this the paper looks at changes in both the surface and deep ocean at site DSDP 610, the southern extremity of the Rockall Trough, during the transition from MIS 11c – MIS 11b. The paper is based on a multi-proxy approach, including; 1) Benthic stable isotope data of ^{18}O and ^{13}C across 82 samples at an 8cm resolution, 2) Lithic abundance (IRD) at 0.5, 1, and 5cm resolution, 3) Faunal counts on forams at 0.5, 2.5, and 5cm resolution for SST reconstruction using MAT and 4) Grain size analysis for bottom flow strength. The papers main conclusions are that at least 2 abrupt climate events (the 1st lasting 7000 yrs and the 2nd last 2000 yrs) occurred during the transition from MIS 11c to MIS 11b and that this challenges the idea of abrupt events being exclusive to glacial periods. Furthermore, the authors argue that the offset between proxies indicates that there is a lag between the first slowdown of Wyville Thomson Ridge Overflow Waters (WTOW) and the subsequent cooling of SST of ca 320 years and ca 710 years for the first and second event. In both cases WTOW recover slower than SSTs.

Summary of decision

Whilst the paper is data rich and represents an interesting new record we have a number of significant issues that we feel need to be resolved prior to the paper is accepted for publication. We have flagged this as major revision as we feel that it needs a re-thinking

of some of the underlying concepts but it may be that the changes required can be dealt with more rapidly than this and the changes may be relatively minor. Firstly, we feel that the way that the paper is set up does not really give a true reflection of the papers findings. The paper claims that abrupt climate events are; 1) a characteristic of glacial climate states, and 2) the results of this study (showing abrupt events occurred at the transition between MIS 11c and 11b) question this and our ideas about our concept of warm climate stability. Neither of these claims are strictly true. It has long been known that abrupt climate events occur during interglacials – the 8.2 ka event (along with the pre-boreal oscillation, 4.2 ka event and 2.8 ka event) is a good example of this. Whilst it is likely that none of these were of a magnitude or duration comparable to the Lateglacial Interstadial (i.e. the Younger Dryas) they had transformative effects on ecosystems, surface processes and societies so are still significant. The concept of “warm” climate stability was surely abandoned a long time ago. If the authors are distinguishing between high magnitude (glacial) and low magnitude (interglacial) abrupt events then they need to do so more clearly and, ideally, in a quantified way. Regardless of how abrupt events are defined, the study presented here doesn’t move this argument forward. The events discussed here occurred on the climatic downturn into MIS 11b and, consequently, long after fully interglacial conditions had ceased. They are actually more true of abrupt events under a glacial, or transitional, state in that, as shown in Figure 6, a significant fall in sea level had already been experienced prior to their occurrence. A number of authors have shown that abrupt events occur under fully interglacial conditions during MIS 11c (i.e. Barker et al., 2015; Kandiano et al., 2017). The events described here are more similar to those that occurred during the transition from MIS 5e to 5d that are discussed in the introduction. The occurrence of such events, at interglacial/glacial transitions, are relatively well-known, particularly from the North Atlantic, which slightly detracts from the originality of this study. *The paper needs to consider the rationale and significance of this work in much greater detail, in terms of how it is discussed in the abstract, introduction and conclusion.*

Secondly, the Rockall trough, as a system, is a hydrographically unique area with cyclonic re-circulation of sediments. Consequently, changes in sediment characteristics within DSDP 610B could reflect variations in the strength of flow within Rockall Trough and not just actual changes in the strength of WTOW. The authors may have considered this but at the moment the manuscript reads as though the complexity of this location is being ignored and overlooked. *The paper needs to show a much greater consideration of the complexities of the oceanographic processes that operate at DSDP 610B and explain why the proxies record the role of WTOW and not more local processes.*

Thirdly, we are not convinced that the cooling that occurred between 397 and 390 ka should be classified as an “abrupt event”. Not only does the event last for some 7,000 years but it is characterised by relatively slow and protracted cooling. This is relatively clearly seen in the SST data presented here where a decline of some 6°C occurs progressively over ca 5 ka. The second event that is described is, as the authors acknowledge, is much more typical of an abrupt event (a decline of >6°C in ca 0.5 ka) though this is confidently outside the main interglacial phase and thus does not support their conclusions of high magnitude events during interglacial periods. The change in grain size data for the first event is more dramatic and has much in common with the second event, however, this elicits a very different response in SST values and this is not really acknowledged or addressed. It is also quite important for the authors to discuss the discrepancy in the SST data of late MIS 11c between DSDP 610 and M23414. From 403 ka to 398 ka there is an offset of upto 6°C between the two sites, significantly greater than the modern temperature gradient between these two localities. Again this isn’t discussed

but is fundamental to an acceptance of the data and ideas presented here. *The difference between the two events need to be discussed and explored in much greater detail, whilst the validity of the SST estimates for DSDP 610 need to be discussed in more detail, particularly with reference to the record from M23414.*

Further points to be considered are:

- Line 328 – 334 – It seems odd and out of place to make this Holocene comparison without any accompanying graphs or quoted data to support this.
- Line 396 – 400 – authors argue that C13 data for 980 and U1308 reduce at 398.4 and 399ka respectively. This isn't that convincing – at 980 values increase during the event / at the onset of the event. U1308 also reflects a gradual reduction. The resolution seems too low to claim that the data depicts WTOW depleting during this event. The authors also do not discuss other causes for this.
- Line 426 – This begins with wider palaeoceanographic context, but this has already been stated in the previous 2 paragraphs. Perhaps the whole discussion needs segmenting better? M23414 has been discussed (indeed in the figures) whilst other sites are being discussed (U1308, 980, MD99-2277) in the previous.

Furthermore there are series of more **minor points** that need to be considered:

- **Figures:** Some general points on uniformity of font sizes, writing (610B – this study) or (610B) or (this study) rather than a combination of the 3
 - Figure 1
 - Indicating which branch of ISOW is WTOW would be helpful for the reader, particularly as this is the focus of your study. It may also be helpful to include other labels (DSOW, other ISOW branches, NADW etc but not necessary).
 - The grey site label names on a grey background are something I'd advise to change for legibility
 - Figure 2
 - In some figures you have labelled the data for this study and in figure 2 you have not. I'm assuming those not labelled relate to DSDP 610 in this study?
 - You've plotted N.incompta + N.deutertrei % together from Kandiano et al 2007 but you've listed this as sub-polar (following Kucera et al 2007); but Deutertrei is a sub-tropical species. You also don't talk about this in the text so wondered why it is plotted?
 - The two shades of each colour may be difficult for colour
 - Colour blind people so symbols may help with this.
 - The graph feels busy – it might benefit from extending horizontally
 - Figure 5
 - IRD for 983 is in grey in the axis and appears black in the graph
 - The range of font sizes looks untidy
 - Figure 6
 - IRD for 983 is in grey in the axis and appears black in the graph
 - The NPS % from barker et al 2015 was updated in the barker et al 2019 paper. There aren't any major changes in %, though some minor peaks are smaller in

the data you have used (e.g., ~ 394ka)

- Is the age model for ODP 983 tied to DSDP 610B in any particular way or just placed on a timescale with 2 different models (+ associated uncertainties)? I ask as the LR04 age model for ODP 983 places the increase in NPS % at ~ 395 ka – 389 ka and a second 387-385 ka which is much closer to the authors claims for these events (in both duration and timing). The authors also seem to have tied their core to LR04 so I wonder why (it seems) they have not tied ODP 983 to this.
- **Text:** Some general points line by line through the text. The piece in general would benefit from some sub-headings to organise the flow as presently sections seem to overlap considerably.
 - Line 74-75 – would cite Barker et al 2015 “icebergs not the trigger for NA cooling events” (they reference the paper later but they do not cite it here)
 - Line 80-84 – quantifying importance of NADW to AMOC and quoting overall contribution from WTOW would be good
 - Line 90 – Global average temperature difference between MIS 11 and MIS 1 would be good
 - Line 109 – add space between reference and ‘today’.
 - Line 85 – 115. This seems muddled. It seems to be a descriptive piece setting out the conditions during MIS 11 but starts and ends with talk about orbital similarities as a justification for looking at MIS 11. I would have set out the orbital similarities prior to then describing MIS 11.
 - Line 347 – 350 – the authors state that the offset is 9 samples (4.5cm), 320 years. Firstly, from the graph, it doesn’t look like all 9 of these samples have been run so this is confusing wording
 - Line 404 – random ‘o’ in the sentence
 - A good paper to cite on surface waters in the Nordic seas being unusually cold and fresh in MIS 11
<https://www.frontiersin.org/articles/10.3389/fmars.2018.00251/full#h7> which is absent in the bibliography
 - The reference list needs to be checked there are a number of typos throughout and some repetition (i.e. McManus et al., 1999 is included twice).