

## **Review summary**

### **Recommendation: Minor Revision**

Firstly, I would like to welcome the revised manuscript. The reviewers have overall addressed many of the reviewer's comments. The structure is much improved and, in particular, the premise is much better. There are, however, 3 outstanding issues remaining that need to be addressed prior to publication. There are further minor issues I think the author should address.

### **Major comments**

#### **A) Chronology**

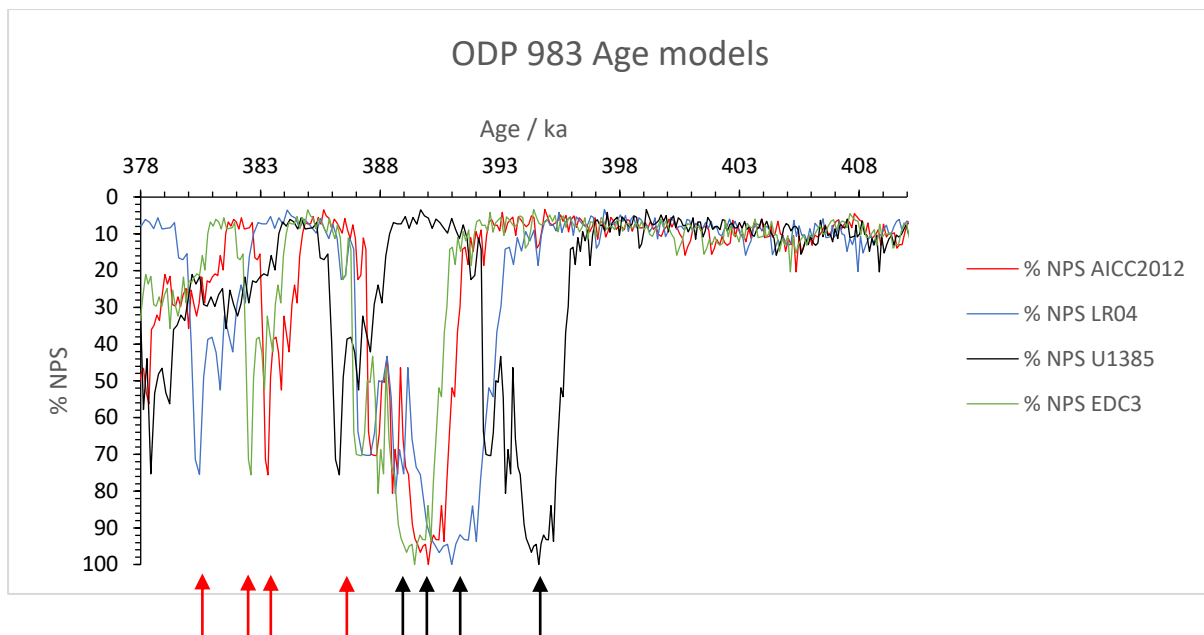
In the initial round of reviews, the chronologies of cores used for data comparison was raised. The authors replied to this in the comments stating which age model they used for ODP 983 (AICC 2012). The reply comments did not sufficiently address this, and the handling editor requested this be included as a discussion point. This does not invalidate the data of the study so there is certainly scope for addressing this; however, it is important to some of the arguments made in the discussion (in particular 5.2).

There are numerous age models in the literature used for ODP 983 (LR04, Lisecki and Raymo 2004; adjusted LR04 Wolf et al., 2011; AICC 2012 / EDC 3 (Barker et al., various); and U1385 (Barker et al., 2021). The authors have chosen AICC 2012, placing the 1<sup>st</sup> event in ODP 983 between 391 and 387 ka. There is no increase in ODP 983 NPS % during the timing of the authors first event in core 610 in their 983 age model (AICC 2012). This is similar for the EDC3 age model (391 – 386). LR04; however, places the onset earlier event in ODP 983 at ~ 393.5 – 386 and the 2021 U1385-based model for 983 puts an event at 396 – 391. I acknowledge that this is more problematic when it comes to the second event at 983, which is much later in LR04 than all of the models (though, notably much earlier in the U1385 based model).

There are significant difficulties in comparing ocean cores due to the uncertainties associated with age models during MIS 11c (quoted +/- 4ka, as the authors acknowledge). As a result, the authors are not wrong to choose AICC 2012 as an age model; however, given they are using LR04 ties for their site (610) I'm curious to why they haven't chosen the LR04 age model for ODP 983; particularly, as this fits with (at least the first, and longest event in) their data much better than the chosen model. Again, this is not to say the authors are wrong but the choice of age model could result in different arguments being made and must be mentioned.

This is important because the absence of their 1<sup>st</sup> event at ODP 983 is an important part of their hypothesis for north Atlantic dynamics at the time. I have included a quick graph on the differences in the choice of age model make to the '1<sup>st</sup>' (black arrows) and '2<sup>nd</sup>' (red arrows) event in ODP 983. **The authors should include either / both of the following:**

- 1. A justification for the age model chosen over others for comparison data (e.g. EDC3, LR04, U1385) with a brief section addressing that there may be other interpretations to be made with different age models.**
- 2. A dual discussion point, proposing theories for each age model outcome (e.g. ocean dynamics in the circumstances of a 1<sup>st</sup> event / without a 1st event at ODP 983).**



Addressing this in text is essential to the manuscript being published. I want to re-iterate this is not a criticism of the author's choice of age model but stating that a justification for that choice is currently absent from the text. At present, I would argue that the first event in ODP 983 has far more in common with the first event in your data (610) and that the 2 events actually line up well with those in ODP 983 (indeed, the IRD peaks would also line up quite well), but that is difficult to say without younger data to see what occurs after your two events (see **C**).

## B) Study premise

Overall, the authors have well addressed the initial paper's issue where it seemed to be discussing abrupt climate events during interglacials as a new phenomenon. I thank the authors for clarifying this in their revised manuscript. I also agree with the authors that the 1<sup>st</sup> of these events is occurring during low-cryosphere climate and that itself is important. That said, I still would not agree that the event is occurring within the main interglacial phase (despite GHG emissions / low cryosphere climate) but is happening as part of the start of the transition into glacial inception. **As such, there are a couple of areas where I think the authors should clarify the wording:**

1. Line 70-74: The statement of “similar to today” may be problematic. This is in the glacial inception/post main interglacial phase. This is at a period of ice sheet growth rather than ice sheet reduction. You do address this later on; however, so this may be something to address structurally rather than changing the wording (see ‘minor comments’ for more).
2. Line 430-431: You say previously it was only observed in glacial boundary conditions, but these are glacial/interglacial boundary conditions (albeit low ice volume). I am unsure if you are referring to a direction of travel (e.g. previously only seen in glacial – interglacial rather than in this case, interglacial to glacial). If so, this needs clarifying.

### **C) 2<sup>nd</sup> Abrupt event**

At present, the authors do not have data after ~ 389 ka BP. As such, how confident are the authors that that event has ‘finished’? The authors are making assertions about leads and lags between SSTs and deep ocean circulation, which are unaffected by this, but the duration of recovery may be. The recovery seems clear and complete in the raw NPS % data but less clear in the MAT and WTO data. Doing low resolution sampling on younger samples in the core might help this and also help clarify whether there is an absence of a 1<sup>st</sup> event in ODP 983 or whether it is a function of the age model chosen. **The authors should include either:**

1. **More data on younger samples to fully characterise the end of their 2<sup>nd</sup> event**
2. **Should this not be possible, a brief point explaining that younger data may impact the absolute recover times, particularly in the WTO dataset.**

## Minor comments

1. I think the abstract could better show off your data as a summary. At present it doesn't mention the site nor the identification of 2 events in the data (the latter of which I think would be quite eye catching for the casual reader scrolling through!)
2. Line 58: Forcing has a strikethrough – is this intentional?
3. The structure of the introduction could be improved. Line 70-74: The statement of “similar to today” may be problematic. This is in the glacial inception/post main interglacial phase. This is at a period of ice sheet growth rather than ice sheet reduction. You go to discuss this later on – I suggest the authors re-arrange this to be prior to your research aim. Presently, as a reader I think about why you have said this and then that doesn't immediately get clearly answered and what is there is much later on. I would put your research justification for MIS 11 vs MIS 1 prior to the aim of your study. I'm not confident I agree with the 'similar to today' comment and would suggest changing it to “during low cryosphere climate states” but I think it would be more fine for the reader to see you go from (1) high vs low magnitude variability (2) need to study low cryosphere climate states to investigate this (3) justifying MIS 11c glacial inception as an important area of study (**and crucially, the time slice you have chosen**) (4) therefore the aim of your study is to investigate site 94-610, which will flow well into your methods.
4. It is good you have clarified the conditions of Rockall Trough and how your site is not impacted by any issues associated with this. As a suggestion but not necessarily mandatory - I wonder if there is data to show in other interglacials (e.g. Holocene, MIS 5e) that sites of similar proximity / depth have also not experienced these issues that you could reference?
5. Line 424: you reference figure 6 and refer to a number of sites in preceding lines. One of these is 1063, but 1063 is not in your figure 6. I see figure 5 has the C13 data

in a longer timespan but it is focussed on your time slice in figure 6. Did you mean to add 1063 to figure 6 or are you wanting to refer to figure 5 instead?