Review of Hodell and Channell CPD

Hodell & Channell present substantial new stable isotope datasets measured on benthic foraminifera and on bulk carbonate from North Atlantic IODP Site U1308. They use these data, together with physical property measurements and previously published U1308 magnetics data, to shed new light on the evolution of Quaternary North Atlantic climate on orbital to suborbital timescales. They propose that orbital- and millennial-scale variability centred on the North Atlantic 'co-evolved' during the Quaternary and link this evolution to a series of mode transitions in climate at ~2.7, ~1.5, ~0.9 and ~0.65 Ma. In presenting this work, the authors seemingly bring to fruition one important goal of Expedition 303/306: to document the evolution of Quaternary millennial-scale climate variability recorded at U1308, the reoccupation of DSDP Site 609. Studies of 609 set the agenda for our understanding of abrupt and rapid North Atlantic climate change during the last glacial and it is fitting that its reoccupation is proving to be just as important for advancing our understanding of these issues. It is certainly nice to review a paper in which I cannot find any real problems. For me its publication in CP is a formality following minor revision. The attached pdf contains a series of minor comments that I would like to see addressed to help improve their contribution further still.

Best Wishes, Ian Bailey

Line by line comments

Line 35: For those less familiar with DSDP/(I)ODP best to spell these acronyms out in full here.

Line 37: might help to add a time in Ma in parentheses after 'latest Pliocene'.

Line 113: Was this modification made by Channell et al. (2016) or was it this study? There seems to be a consistent 9 cm depth offset between the depths assigned to the ages (so for depths greater than ~100 m) presented in Tables 1 of this Ms and of Channell et al. (2016). I apologise if I've got this wrong, but if I've read the tables correctly, does this represent an a slight modification of the Channell et al. (2016) age model in this Ms?

Section 2.3: Do all the benthic ∂^{18}O data come from Hodell et al. (2008) and Channell et al. (2016)? Is what you present here for the first time the associated ∂^{13}C data for the older than ~1.5 Ma interval? If so, you could save text by simply saying you utilise previously published stratigraphies based on benthic ∂^{18}O and present a new benthic ∂^{13}C record from the >1.5 Ma samples analysed by Channell et al. (2016) that extends the previously published ∂^{13}C from Hodell et al. (2008) back to ~3 Ma.

Your comparison of 607-U1308 stable isotope data in Section 3.1 Ma would benefit from using ∂^{18}O/∂^{13}C splices for 607/U1313 (so using the Bolton et al. (2010)/Lang et al. (2014) data for >2.4 Ma). The U1313 stable isotope records for ~3.3-2.4 Ma are twice the resolution of the 607 record for this time, and using these data will modify some statements you make in this section. Both datasets can be found on Pangaea, but please feel free to email me and I can provide you with a copy.

Lines 189–190: Is U1308 ∂^{13}C really typically that much more negative than that from U1313 during MIS G6? It's hard to see the detail in your Figure 3, but it looks as though the much more U1308 negative signal can be attributed to two data points. Instead it seems that most of the time U1308 ∂^{13}C is only ~0.2-3‰ lighter than at U1313 during G6. This difference may point towards some fundamental difference in source/aging of ∂^{13}C at the deeper U1308 relative to 607. During G6 the ∂^{13}C gradient between U1313 and records of end-member NCW ∂^{13}C (e.g. potentially assessed from Site 982) is still relatively large (Lang et al., 2016). If there is significant SCW at U1313 during MIS G6 in the deep (3.4 km) western North Atlantic, then the lower ∂^{13}C values at the deeper (3.8 km), albeit more northerly eastern basin Site U1308 may reflect that there is a stronger SCW influence at U1308 than at U1313 during MIS G6 (is that really likely?). Alternatively, the waters bathing U1308 may be dense overflow waters from the north (Bell et al., 2015), although the similar ∂^{18}O values at U1313 and U1308 during this glacial would suggest otherwise.

Line 223–225: Do LGM iceberg drift models of Grant Biggs and Ros D'Eath support the notion that British Chalk/Scandinavian rocks might be a notable source of IRD to U1308? Don't they show it is unlikely that many Scandinavian icebergs/IRD would reach south of Iceland.

Line 235: ∂^{18}O (benthic – bulk) increase during MIS 82 is consistent with the fact that this glacial may be characterised by the first late Pleistocene-magnitude sea-level fall Rohling et al. (2014).

Line 245: the sentence here reads as though you are saying that there is Ca/Sr data in Figure 4.

Line 323: Bailey et al. (2012) is a good reference for North Atlantic IRD sources during MIS 100, but the key reference for evidence of a dominantly Archaean provenance for North Atlantic IRD prior to MIS 100 should be Bailey et al. (2013) where that observation was published for the first time.

Line 330: Raymo et al. (1992) interpret a divergence in Site 607 ∂^{13}C towards values more negative than that of Site 552 during MIS 100 as the first evidence for decreased NCW in the deep North Atlantic Ocean during iNHG. That view has been updated recently in Lang et al. (2016), since it seems that based on U1313 ∂^{13}C and fish debris ε_{Nd} that MIS G6 is the first glacial associated with significant (and potentially LGM magnitude) SCW incursion into the deep North Atlantic Ocean.

Line 341: Perhaps cite Rohling et al. (2014) here for magnitude of MIS 82 glaciation.

Lines 342–344: We didn't find sand IRD in U1308 sediments for MIS G6 when studying at a 30 cm sampling resolution (Bailey et al., 2010). Bolton et al. (2010) and Lang et al. (2014) have shown through higher resolution analyses (every 5 cm) that sand IRD is similarly absent in sediments deposited at U1313 until MIS G4, but that values of ~40 grains gram (comparable to the LG scenario at U1313 outside of H-events; Lang et al., 2016) do not occur at this site until MIS 100. These more recent studies have updated the view of when significant icebergs arrived at 40°N based on DSDP studies (Raymo et al., 1986; Kleiven et al., 2002) and support what your data show, i.e. that widespread iceberg rafting and IRD deposition across the North Atlantic Ocean did not occur until MIS 100…reflecting the true large magnitude of that NH glaciation relative to previous cold stages (as potentially also confirmed by, e.g. Balco and Rovey, 2010; Brigham-Grette et al., 2013).

Line 345: MIS 94-52 broadly coincides with inference on increased AMOC strength by Bell et al. (2015). Do your eastern basin U1308 ∂^{13}C data support the Bell et al. interpretation, or suggest an alternative origin of the Walvis Ridge 'overflow' signal they report?

Line 352: the low benthic ∂^{13}C values you report for U1308 in the early Pleistocene should be discussed in the context of the ideas of Bell et al. (2015). You may end up dismissing this suggestion (if you haven't already), but I think this is worth considering because Site 607 doesn't record significant evidence for major shoaling of NCW between 1.5-2 Ma (Lang et al., 2016), and you think it would do if FIS meltwater was impacting significantly on NADW production at this time. The NCW cell can shoal and AMOC can remain relatively strong, but models suggest that if AMOC is reduced then the NCW cell has to shoal. Can we rule out productivity aging of benthic ∂^{13}C at U1308?

Line 368: A obvious question here is "based on the records we've got so far, does it look as though the magnitude and spatial fingerprint of suborbital climate change observed for MIS 3 replicated at any other time during the past ~3 Ma?" The short answer is probably yes, with evidence for DO events as far south as 30°N since ~0.9 Ma (Ferretti et al., 2010; Weirauch et al., 2008). Prior to this time strong evidence exists for DO-like events during MIS 40 and 38 (~1.3 Ma) at 37°N in the northeastern equatorial Atlantic (Birner et al., 2016), but seemingly not at 30°N in the northwestern North Atlantic (Weirauch et al., 2008). No convincing evidence exists anywhere yet for DO–magnitude change during any earliest Pleistocene glacials, e.g. during MIS 100 (one of the more well studied cold stages for this time), but instead muted suborbital change in planktic ∂^{18}O and SST ~40-60°N (Bartoli et al., 2006; Becker et al., 2006; Bolton et al., 2010; Friedrich et al., 2013).

Benthic ∂^{18}O records suggest our planet's climate system has been crossing this +3.5‰ threshold during glacials ever since ~2.7-2.5 Ma. If we assume that this benthic ∂^{18}O value corresponds to a relatively narrow range of NH ice-sheet growth then the available evidence suggests that the spatial fingerprint of DO-like change over the past 3 Ma is not consistent with the climate system responding in a repeatable (pseudo predictable) manner to it sitting in an intermediate ice-volume window. If it did, we should expect to see the same spatial pattern more or less emerging for amplification of suborbital climate change during all big benthic ∂^{18}O glacials (>+3.5‰) from ~2.5 Ma. The occurrence of DO-like change is clearly linked to NH ice sheet size, but records of the 41-kyr world suggest to me it is too simplistic to think of it as a straight forward ice-volume feedback (or our understanding of NH ice sheet volume during the 41-kyr world needs revision). If as yet undiscovered DO-like magnitude change really is restricted to the highest latitudes during the earliest Pleistocene, then that suborbital change (and the mechanisms responsible for it) do not seem to be analogous to events during the LG.

Line 375–278: McIntye et al. (2001) present strong evidence for millennial-scale changes in iceberg rafting to Site 983 in the early Pleistocene (~1.93-1.75 Ma). You also know we found the same thing at U1313 and U1308 during the much older MIS 100 (Bolton et al., 2010; Bailey et al., 2010) and at U1308 during MIS G4 (Bailey et al., 2010), but none of these earliest Pleistocene events are yet found to be associated with large amplitude swings in SST/∂^{18}O (Becker et al., 2006; Bartoli et al., 2006; Bolton et al., 2010; Friedrich et al., 2013). The point I am trying to make here, and one you obviously appreciate, is that millennial-scale pulses of IRD deposition do not necessarily imply large magnitude swings in climate on such timescales, just that there are likely millennial-scale swings in climate driving the mass balance of ice-sheets/glacier at the coast at those times.

Marshall and Koutnik (2006) show that millennial-scale episodes of iceberg rafting can still be anticipated with muted suborbital climatic variability, but that such pulses might be set against a steadier background of IRD inputs, making them less distinct in the sediment record. If suborbital change during the earliest Pleistocene was muted relative to the late Pleistocene, we may therefore find that overall IRD inputs during earliest Pleistocene glacials were higher, but that suborbital-scale IRD pulses superimposed on this signal were muted, relative to IRD inputs during e.g. MIS 3 at U1308. Maybe it is best to look for this at a site further north where the iceberg/IRD survivability issue less strongly influences IRD inputs, but maybe worth thinking along these lines here since your record is the only suborbital proxy IRD record we have that spans the entire Quaternary.

Line 384/436/561: is Figure 5 the correct figure to cite here? Don't you mean Fig. 6 evol. power spec?

Line 391: please place a horizontal line at the benthic ∂^{18}O value of +4 ‰ (~MIS 4) and +3.5 ‰ (McManus) to guide the reader's eye when they examine Figure 4.

Line 397: 'ice volume was about twice as great in North America compared to Eurasia'. I don't disagree that your datasets suggest that the deposition of HS-sourced material increased from 1.6 Ma (seems consistent with U1313 data from Naafs et al., 2013), but how do you then extend that to what seems like a relatively precise quantification of relative differences in ice volume?

Line 410: Good to plot an indicator of IRD in Figure 9 to help the reader see more easily the relationship between iceberg rafting to U1308 and U1313 and the SST gradient evolution.

Lines 412–426: again, see the recent findings of Lang et al. (2016) for new context on the pioneering observations of Raymo et al. (1990; 1992) and those made subsequently by e.g. Lisiecki (2014).

Lines 446–462: Have you compared your bulk ∂^{18}O record and/or ∂^{18}O (benthic – bulk) to Steve Barker's synthetic Greenland DO record? Is it may be worth showing a plot of this, if only in the supplementary guide. How does the variability in your record(s) for MIS 41-37 compare to those from your work in Birner et al. (2016)? Do we see evidence for the same number of ice-rafting events at U1308 as reported by Raymo et al. (1998) further north at ODP Site 983 during MIS 40 that you've tied convincingly to the DO-like variability seen in *G. bulloides* ∂18O from the Iberian Margin?

Lines 485–486: what's the Site 982 bulk ∂^{18}O data source? Are these data produced for this study? If so, please mention these analyses in your methods text. If not, the data source needs including.

Figures

Figure 1. Nice map. Perhaps state what the yellow/green triangles mean in your key too.

Figure 2. I think it would still help to have a key on the figure so it is easier for the reader to work out which record is the LR04 vs U1308 $\partial^{18}O_b$ (like you do in Figure 10).

Figure 3. A key showing which records are from 607 versus U1308 would aid the reader. I suggest labelling the horizontal lines with '21', '41' and '100' kyr. Ditto Figures 4, 6 and 7.

Figure 4. Please add the horizontal lines for MIS 5b, 4 and 2 onto the benthic ∂^{18}O data (as it is on Figure 3). Please also label the key bulk carbonate ∂^{18}O values referred to the in text, e.g. the -4 ‰ value characteristic of H-layers and the -2 ‰ value characteristic of DO-type ice-rafting events.

Figure 6. Given the density increase with depth, to make the suborbital events even clearer, it might be helpful to detrend the density data plotted in Fig. 6 by subtracting the linear best fit from it.

Maybe combine Figures 8 and 9 to help the reader see clearly how the 982-U1313 SST gradient evolves alongside changes in IRD inputs to these two sites.

Figure 13. Please label site names on dust records. Could also do with labelling key MIS on the LR04 or including vertical guide lines. It would also help to label all HS H-layers on the relevant figures to help tell apart HS-sourced H-layers and non-H-event (DO) IRD deposition in your bulk ∂^{18}O record.

Figure 12: Data sources for U1304 NGR and benthic ∂^{18}O not given in caption, or is it all presented in Xuan et al. (submitted)? If so a quick revision of the caption text is needed.

Additional references cited:

Bailey, I., Hole, G.M., Foster, G.L., Wilson, P.A., Storey, C.D., Trueman, C.N., Raymo, M.E. (2013). An alternative suggestion for the Pliocene onset of major northern hemisphere glaciation based on the geochemical provenance of North Atlantic Ocean ice-rafted debris. Quaternary Science Reviews 75, 181–194, http://dx.doi.org/10.1016/j.quascirev.2013.06.004.

Becker, J., Lourens, L.J., Raymo, M.E. (2006). High-frequency climate linkages between the North Atlantic and the Mediterranean during marine oxygen isotope stage 100 (MIS100). Paleoceanography 21, PA3002, doi:10.1029/2005PA001168.

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Mudelsee, M., Raymo, M.E. (2005). Slow dynamics of the Northern Hemisphere glaciation. Paleoceanography 20, PA4022. http://dx.doi.org/10.1029/2005PA001153.

Naafs, B.D.A., Hefter, J., Stein, R. (2014). Dansgaard-Oeschger forcing of sea surface temperature variability in the midlatitude North Atlantic between 500 and 400 ka (MIS 12). Paleoceanography 29, 1024–1030. doi:10.1002/2014PA002697.

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