

Dear authors,

Thank you for submitting a revised version of your manuscript and for addressing all the points raised during the previous round of reviews. You have responded to all the reviewers' technical queries, the readability of the figures has been improved, and additional results demonstrating that key findings do not depend on model resolution are included in the supplementary material (although note that the caption to figure S15 needs updating).

I have read the revised version of the manuscript and a small number of points require clarification:

1. **MIS11 and MIS11c:** these terms appear to be used somewhat interchangeably, please clarify whether there is a difference between them, and if there is, then check that terms are used consistently throughout the text.
2. **Timing of the highstand:** it seems to be assumed that the MIS11c sea level highstand was at 405 ka. If this is the case, please state this clearly somewhere, referring to supporting evidence. If the highstand is not independently constrained to be at 405 ka, then do your results perhaps suggest that the lower bound for the AIS contribution to the highstand is 4 m – based on the Vostok scenario (fig. 6), where the sea level contribution is ~4 m at 410 ka?
3. **CCSM3 cold bias:** the statement on lines 304-305 requires further clarification. If I have understood the reviewer correctly, the CCSM3 LGM climate is too cold (even after correcting for the lapse rate), but the CCSM3 PI climate is relatively accurate. Due to the approach used to create the GI, this therefore leads to a cold bias for positive GI values and a warm bias for negative GI values (i.e. during an interglacial) – as shown in figure S14. Is this correct?

Additional minor suggested corrections are listed below. Once these, and the issues raised above, are addressed, I would be happy to accept this article for publication in The Cryosphere.

Pippa Whitehouse
Associate Editor

Minor points

Line 8: 'contributed 3.2-8.2 m to...'

Line 10: delete 'further'

Lines 12-13: it is not clear how the climate signal is linked to global sea level, or what it means to 'match the recorded global sea level highstand'. Text could be tightened and perhaps linked more closely to information in the final sentence

Line 18: delete 'ka'

Line 51: make it clear that the Dry Valley moraines are interpreted to indicate local ice advance

Line 68: 'apeak' – space missing

Line 79: To improve the structure of this paragraph, I suggest first stating that model results depend on forcings, boundary conditions, model parameters etc. You can then summarise which areas have previously been studied, before highlighting which aspects you will focus on.

Line 84: 'could help guiding' – check grammar

Line 85: Awkward sentence, suggest, "We evaluate the impact of the following on AIS volume and extent during MIS11c: the choice of..."

Figure 1: I do not see any red diamonds indicating sediment cores

Line 96: this looks like eq. 9 in Bernales et al. (2017a), not eq. 1. Should u_{sta} be multiplied by w ?

Line 127: your approach does not include all aspects of glacial isostatic adjustment (an important component is the spatial variation in sea surface height), suggest replacing with 'bed deformation'

Line 142: 'When analysing the results, we ignore...'

Line 144: Clarification needed because the EDC record was not used to force *all* the ensemble runs, e.g., it was not used to force all the CFEN experiments

Line 157: '...assess the impact of similarities and differences...'

Table 2 caption: clarify that 'Age (ka)' relates to *LGM* reference values

Line 187: delete 'Mean'. Also, given that your approach does not account for local gravitational perturbations to sea surface height, I suggest adding a sentence: "We approximate the sea level forcing applied at the boundaries of the ice sheet using global mean sea level reconstructions."

Lines 214-221: this text describes the initial ice sheet configurations (gmt1-gmt3) for the EDC case (shown in figure 3). Does it also hold for the cases when DF and Vostok forcing are used?

Lines 218 and 219: '...than the control...'

Table 4: reference to δX_{Hol} is perhaps left over from an earlier version of the manuscript?

Line 263: you state above (line 258) that using the LR04 average values gives a 3.4% smaller ice sheet at 402 ka, and here you state that using the EDC average value gives a 2.3% larger ice sheet at 402 ka. However, in figure 4b, the orange solid/dashed lines are much closer to each other at 402 ka than the black solid/dashed lines - please check calculations

Line 271: 'It directly reflects their effect' – references to 'it' and 'their' are ambiguous

Line 284: 'different initial geometries'

Line 285: 'The latter two...' – check, I think it is Totten and Dibble that are thicker, with Cook thinner

Line 318: 'the former two' – not clear what this refers to

Line 323: 'the different ice-sheet configurations' – make it clear that you are talking about model runs forced by the same ice core record, but with different initial ice sheet configurations

Line 340: clarify that the values relate to ocean temperatures

Line 356: 'WAIS collapse was triggered' – more caution needed in the language used, it is not proven that WAIS collapsed during the LIG

Line 370: 'ested' -> 'tested'

Figure 10 caption: (b,e) -> (b,f)

Line 391: 'when comparing their results' – check the logic in this sentence

Lines 397 and 399: suggest 'interval' -> 'range'

Line 398: 'ice core experiments'

line 444: the tense of the final sentence is odd, suggest "We found that this threshold needed to be sustained for at least 4 kyr for strong WAIS ice retreat to be triggered."