**2nd Review of Saini et al. (egusphere-2025-1990)**

The authors Saini et al. have provided an updated version of the manuscript based on the referees' remarks. Many of those have been nicely taken into account which led to a clear improvement of the manuscript. However, since the manuscript is quite comprehensive and presents a lot of details, the large number of minor issues still needs to be clarified/corrected before final publication. This should help to avoid that the reader will get lost in the details and will help that the key messages of the manuscript on the climate response to MIS3 boundary conditions and their relative role can be clearly conveyed.

*Please note that the referee's comments (page/line numbers) refer to the revised manuscript version. Where reference to the first submission (FS) version is necessary, it will be referred to accordingly.*

**Remarks with respect to the previous revision**

1. The authors modified the abstract by adding more details on the simulated responses, which I appreciate. However, you should include one sentence making the main approach explicitly clear, e.g. near l. 4: "...around 49,000 years ago and study the relative role of the MIS3 boundary conditions…. Sentence 2 in the Abstract is a bit misleading by explicitly mentioning the ice sheets only.
2. Thank you for providing figure R1 in "Reply to Referee 2" on global ocean temperature and AMOC. The trends you mention in your reply only refer to the final 100 years of 49ka-full, but caution is required with the trends in 49ka-co and 49ka-alb.
3. I appreciate Figs. R2 and R3, although I find it still a bit puzzling to center everything at 49 ka and then use 52 ka forcings for vegetation and ice sheets. R2 and R3 figures are not part of the manuscript, therefore you should at least mention 52 ka in Tab. 2 where required. In the current version of Tab. 2, one gets the impression that everything is set to 49 ka boundary conditions. I would also adjust the caption and titles in your new Fig. 1 to something like
   "49 ka" ice sheet
   "49 ka" vegetation and ice mask
   Many people only look at figures and captions nowadays, so some hint to your special "52 instead of 49" forcings should be there. Please be precise whether the height values in ll. 105-106 are from Gowan et al. for 49 or 52 ka? Please be precise also in l. 119/120: "topography at 49 ka" is confusing.
   Please also adjust the caption of Fig. 1 into "represented by the ice sheet at 52.5 ka".
4. In my previous remarks, I was asking about the link between insolation / high obliquity and the Hadley cell strength (FS p. 8 l. 184). I don't see a big change in the revised manuscript (l. 215). Some more details about how high obliquity affects the ITCZ would be helpful (or a reference to a previous study that investigated this).
5. Table A1 is very helpful. Please double-check the headings in Tab. A1 with "49ka-ice" and "49ka-alb", while you stated earlier that you have renamed 49ka-ice into 49ka-alb. Please be precise with the sign of the anomalies in brackets. Shouldn't the anomalies from PI show a cooling (negative numbers)?
6. The authors replied to my previous comment on the temperature gradient between hemispheres (FS p. 10, l. 190) that they quantified this and added the information to Table A1. But in the revised manuscript, I don't see numbers for the meridional contrast which would help the reader.
7. Fig. 2: Even with the adjustment of the sea ice isolines, it is very hard to see and distinguish the isolines. I understand the authors' argument of sticking with the projection, but you should at least increase the size of the figures a bit, e.g. by having 2 global plots next to each other and moving the third one (Fig. 2c) to a 2nd row.
8. Fig. A3 caption still does not mention the barotropic streamfunction in contrast to the authors' reply. Why did you choose a different lat-lon coverage for the three rows of the Figure? The reader should at least be pointed to this in the caption.

**Some new remarks (referring to the revised manuscript)**

9. Overall, I would recommend a detailed double-checking of the text. For instance, “the” is missing here and there (l. 57, 73, Fig. 2 caption “at the 95% confidence level”…), l. 163 (“in”=> “over”),...
10. A modified title of the manuscript such as “The Influence of Marine Isotope Stage 3 boundary conditions on Atmospheric Circulation” would better reflect the content of the manuscript, since the study is not only investigating the ice sheets’ impact (also relevant for l. 2, 65 and 69 where only the ice sheet forcing is emphasized).
11. Abstract l. 2: “North Atlantic” => “North Atlantic region”
12. Abstract l. 14/15: This sentence mixes forcing and responses. => “… non-linear effects of Marine Isotope Stage 3 forcings on large-scale atmospheric circulation and precipitation patterns.”
13. The authors specify the paleo timeslices e.g. by 49 ka. I would suggest to include “before present (BP)” at some early point in the manuscript.
14. l. 35: “on understanding the impact” => “on the impact”
15. l. 40: Suggestion for shortening: “An exception from this list is the work of Löfverström et al. (2014), who investigated… Their study found...” => Löfverström et al. (2014) investigated both the LGM as well as … and found that the…”
16. l. 46: “of varying LIS height on the atmospheric-oceanic system” => “of different LIS heights on both the atmosphere and the ocean.”
17. l. 49/50: Please double-check the journal’s guidelines on lists of references (alphabetically, by publication year…?).
18. l. 64: “how ice sheets influenced” => “how Marine Isotope Stage 3 boundary conditions influenced”
19. l. 83/84: I am not a WOMBAT expert, but would rearranging the sentence be more appropriate? => “The ocean carbon cycle is modeled using the World Ocean…. (WOMBAT) model for ocean biogeochemistry which also includes a nutrient… (NPZD) scheme…”
20. l. 86-88: please double-check for typos in the brackets
21. Tab. 1: There are no units given for the GHG concentrations.
22. The numbering of subsections needs correction. 2.1.1 => 2.2
    In order to also take into account the ITCZ part at the end of section 2, the title of section 2.2 could be “Experimental setup and diagnostics”.
23. l. 90: I would rephrase into “As a reference for our paloeclimate simulations, we use the 1000-year preindustrial control simulation (piControl) based on the year 1850 CE by Ziehn….” Since the control simulation is the less prominent part of your study, you could move this sentence towards l. 126 and start section 2.2 directly with “To investigate the climate around 49 ka…”
24. l. 97: “For instance…” is not clearly connecting these sentences. I would rephrase into “Therefore, in a second step (49ka-alb), we modify the land surface properties by incorporating only the ice-sheet extent (no topography yet)….”
25. l. 113+114: => “broadleaf deciduous type” (2x)
26. l. 124: => “averages”
27. l. 129/130: I would move the psi symbol to the next line, since according to the formula, psi denotes the streamfunction, not the strength based on the max. Please also double check the units of the streamfunction (Fig. 8). For instance, in a paper by Pikovnik et al. (https://wcd.copernicus.org/articles/3/625/2022/), the absolute values are at about $10^{11}$ kg/s (their Fig. 1) which would correspond to about $10^5$ Sv when assuming a representative air density, wouldn’t it?
28. l. 140: The annual mean numbers of 4.9°C and 2°C are not given/visible in Tab. A1 nor in Fig. 2a.
29. l. 149+151: Please refer to the individual panels of Fig. A3.
30. l. 146: Wouldn’t higher SST lead to a stabilization of the water column and weaken deep-water formation?
31. Section 3.2 title: I would delete “changes”.
32. l. 170-171: Since 850 hPa is not the jetstream level, I would just write “tilt of the wind field” and “...both at the surface and at the jetstream level in the upper troposphere”.

33. l. 174: “southward shift and zonal jet”=> “southward shift and more zonal orientation of the jet”
34. l. 175: “more zonal configuration”. How about “more southerly pathway”?
35. l. 178: I would delete “the persistence of”.
36. l. 179: “remaining more zonal than during PI” – I don’t clearly see this.
37. l. 181: I guess you mean “moisture transport convergence”.
38. l. 182: “the westerly band” => What do you mean by this?
39. l. 183: “mild temperature conditions”. But this still shows cooling. Do you mean “minor cooling” then?
40. l. 188: Please delete “the influx of”.
41. l. 190: I would make the structure of the section even more clear by inserting a sentence here, e.g. such as “The role of individual boundary conditions (only orbit and GHG forcing) becomes evident from the 49ka-co experiment.”
42. l. 191: I wonder whether this shift is really significant. How about just writing “enhanced and hardly shift southward (Fig. 4a, 9a)”.
43. l. 196: => “by 8°C”
44. l. 198/199: I am not so sure about this being consistent and directly coupled since GH and precipitation responses seem to be on different spatial scales here.
45. l. 201: => “temperature and hardly any significant precipitation changes”
46. l. 205: Instead of starting the paragraph with a list of Figures, you could write “In summary, only the full implementation….” and move the list of Figure refs. towards the end of the paragraph in brackets.
47. l. 206: “southward-shifted” => “southward-shifted and intensified”
48. l. 216: shift by 0.6° - Maybe you could mention that regionally shifts may be larger (if this is the case).
49. l. 213: I find it still hard to see the 20 Sv. Maybe you could at least refer back to your diagnostics part in section 2 and make clear that the 20 Sv are the anomaly between the two experiments’ max values between 400 and 600 hPa.
50. l. 224: Fig. 3h does not really show the globally averaged ITCZ.
51. l. 228/229: Which numbers in Tab. A1 do you refer to here? The annual or seasonal means? Please try to help readers find the information directly.
52. l. 233: “in 49ka-co but weaken further in 49ka-alb” => “in 49ka-co and 49ka-alb”
53. l. 236: “strongest weakening” => “most pronounced weakening”
54. l. 240/241: I would move the reference to the figures to the end of the sentence in brackets and start the sentence with “The implementation of 49 ka…”
55. l. 242/243: “broader” is very unspecific. Furthermore, you investigate all boundary conditions in your study.
56. l. 247: “the NH ice sheet forces”, but then you refer to Fig. 8a which points to the response to all boundary conditions in 49ka-full.
57. l. 257: moisture transported from ? into the region
58. l. 259: “part off Australia” or “part of Australia”?
59. l. 279: “consistent with PMIP4” => “similar to PMIP4”
60. l. 282-285: For the sake of completeness, you should specify whether the given numbers denote global and annual (seasonal?) means.
61. l. 293: => “in our 49ka-full simulation”
62. l. 295/296: Where does the model have its deep-water formation regions in the North Atlantic? So the transport of saline waters may be more important than the warm waters (see my comment #30)?
63. l. 321: => “fully”
64. l. 343: “westerly intensification” => “westerly intensification in 49ka-full”
65. l. 348: => “under different combinations of boundary conditions”

66. l. 355-357: Are you always referring to annual values here? It seems that the 2nd sentence would rather be valid for the seasonal response, wouldn't it?
67. l. 357: "during high obliquity" => "in response to high obliquity"
68. l. 361: I am wondering whether you should discuss an insignificant response. "somewhat consistent" (l. 365) does not sound very scientific.
69. l. 362: remove "seen in"
70. l. 364: => "a response that is not reproduced in our simulation"
71. Section 3.4: Why do many changes around/over Australia happen in the coastal areas and less over the continent (Figs. 3h, 7i)?
72. Section 5 does not fully capture the main message of your study. There should be an explicit mentioning that your study used a stepwise approach to investigate the role of individual MIS3 boundary conditions and what is your main conclusion from their relative importance. The manuscript title and the Conclusions section give the impression that you only investigated the response to ice sheets. You might want to use something from the first paragraph of Section 4 for Section 5.
73. Fig. 2 caption: I guess the last sentence refers to the proxy asterisks. Please make this more clear.
74. Fig. 3: I cannot see any significance stippling in Figs. 3a and b. Also, the white areas in Fig 3c-f should be described in the caption. Is it the ice mask at the surface level or at the 850 hPa level? This also holds for other Figs, e.g. Fig. 5 where the white areas seem to be the same for PI and 49ka-full.
75. Fig. 4: I guess this is based on 850 hPa as well. Please include into the caption.
76. Please double-check in all wind vector plots captions whether it should read "overlaid with 850 hPa wind vectors" or "overlaid with 850 hPa wind vector anomalies" (or 200 hPa in Figs. A4 and A5).
77. Fig. 8 caption still contains 49ka-ice. Please correct.
78. Fig. 9: I don't fully understand how you determine the latitudinal shift in westerlies over the North Atlantic when averaging also over the latitudes (both in a and b)? Regarding the last sentence in the caption about the latitudinal shift, wouldn't this only hold for the northern hemisphere?
79. Fig. A1 caption:
    - "These experiments" => "The 49ka-alb+ experiment is not analysed"
    - "based on the prescribed ice sheet" => to account for the increase of continental ice mass"
    - "the time-averaged periods" => "the periods used for the analysis of time averages from each exp."
    - "...the zero line" => "the zero line for each diagnostic shown"
80. Fig. A2: => "Shortwave downward radiation"