

Review on Himadri Saini et al.: „The Influence of Glacial Northern Hemisphere Ice Sheets on Atmospheric Circulation“

General comments

The current study is using the Earth system model ACCESS-ESM1.5 to analyze the impact of boundary conditions from 49 kiloyears (ka) before present on the simulated climate with a particular focus on the atmospheric circulation.

The major new aspect is the focus on 49 ka from a modelling perspective and on the role of individual 49 ka boundary conditions on climate. The methods used are standard basic climate model diagnostics. The authors present a solid piece of work with interesting results which warrant publication. However, the presentation of many detailed aspects of the study still requires the specific comments listed below to be addressed before.

Specific comments

Please note that the comments are not sorted by importance but largely follow the structure of the manuscript.

- In contrast to the authors' statement (1st sentence of abstract), there is already quite some literature published on various aspects of MIS3 climate which I would recommend to reference and to modify the Introduction accordingly. The focus of the Introduction should be more on MIS3 impacts rather than LGM. Malmierca-Vallet et al. (doi:10.5194/cp-19-915-2023), Brandefelt et al. (10.5194/cp-7-649-2011), Zhang et al. (doi:10.1002/2014GL060321), Merkel et al. (doi:10.1016/j.quascirev.2009.11.006) Zhang et al. 2023 (doi:10.1029/2023JD038521), Guo et al. (doi:10.5194/cp-15-1133-2019).
- Abstract l. 8 „Additionally...“: The abstract should be slightly modified to make the response to the various combinations of boundary conditions clear.
- The timeseries in Fig. A1 show surface variables only. How about the trends in deeper layers? My guess would be that the ocean temperature and AMOC might not have equilibrated yet which would clearly affect the discussion in section 3.1 (p. 6, ll. 116-130).
- Has this model been applied to other paleo timeslices, e.g. the classical PMIP timeslices? In particular, is there an LGM simulation available from the same model version? That could also provide some insights into the effects of ice sheets on the atmospheric circulation.
- Does this study use exactly the same model setup as for CMIP or are there any paleo adaptations (beyond the application of the paleo boundary conditions)?
- p. 1 l. 21 This needs a reference.
- p. 2: Already very early works by Manabe and Broccoli (1985) and Broccoli and Manabe (1987) provided evidence for a southward displacement of the jet.
- Introduction last sentence: Human migration and settlement patterns are not really tackled in the manuscript. You might rather put this or a similar sentence as an outlook/perspective at the very end of the manuscript.
- Has the stepwise experimental setup been used elsewhere? Then it should be referenced.
- Exp. setup: “PI derived from” - Does this mean that your PI run has been restarted from the Ziehn et al. 2020 simulation and ran for another 1000 years?
- Exp. setup p. 3 l. 83 and Tab. 2: Setting the ice to 52.5 ka is a bit inconsistent/confusing when centering everything around 49 ka. How different are the 52.5 and 49 ka ice sheets taken from Gowan et al. (2021)? A similar question would refer to vegetation (p. 4 l. 88).
- It would be nice to have either a figure and/or some sentence briefly describing the characteristics of the 49 ka ice sheet e.g. with respect to modern or LGM in terms of height/extent. Please state clearly in section 2.1.1. which ice sheets you implement (NH and SH), and how different the AIS is between 49 ka and

PI (height, extent, negligible?). The only information you provide comes at a late stage on p.15 l. 273.

- Section 2.1.1: You should explicitly state that the model has PFTs which remain constant throughout each individual experiment and that you modified the PFT distributions for some of your paleo simulations.
- Tab. 2 “Years analysed” - These are 51 or 101 years, respectively, in contrast to what the text on p. 4 says (last 50 or 100 years). Also, l. 98 mentions 292 years, but in Tab. 2 49ka-full seems to cover years 824-1555. Please double-check! What is the reason for averaging over different periods for the analysis (last 50 / last 100 years) Why did you not chose the last years of 49ka-ice for analysis if 49ka-ice runs up to year 824? The choice of 519-569 is unclear.
- I find the experiment name 49ka-ice a bit misleading. To me, it suggests that ice-sheet topography has been implemented in this exper., but you only use 49 ka albedo/vegetation. How about using “49ka-alb” instead?
- According to which criteria did you implement the additional boundary conditions at a particular model year? It seems to be a bit subjective.
- Section 3.1: How about providing a little summary table for the global mean / hemispheric diagnostics for all experiments and reference to that instead of to Fig. 2 which does not explicitly show these numbers?
- Fig. 2: Why is the significance testing only done for precipitation, and not for SAT/SST?
- Section 3.2: When discussing geopotential height (anomalies), I would also write this accordingly and not use the term “pressure” and/or insert “not shown” where required. Readers would look for corresponding (sea level) pressure figures which are not shown.
- Section 3.2: For the jet stream, I would analyze upper tropospheric wind patterns such as uwind at 200 hPa. Wouldn't the Rossby wave response you mention be more evident e.g. at 500 hPa? In contrast to the statement on p. 8 l 174/175, I cannot see the planetary wave structure too clearly.
- For the discussion in section 3.2 (p.7), Fig. A4 is quite clear and helpful. I would recommend to include it into the main part of the manuscript.
- Sections 3.2 to 3.4: I have some concerns regarding the section titles and the discussions in the respective sections. I think that the wording has to be very precise to clearly distinguish between 1) the response to all 49 ka boundary conditions (b.c.) in the 49ka-full experiment and 2) the response in those experiments where only some b.c. have been prescribed. In that sense the section titles “Impact of the ice sheet topography” are partly incorrect unless you explicitly discuss the difference between 49ka-full and 49ka-ice, and even this comparison does not allow to attribute the changes you see exclusively to the ice sheets since you also modify salinity and the land-sea mask etc. The sentence p7 l. 153/154 is an example where to my opinion several aspects are mixed (land-sea distribution, ice-sheet height changes,...). The title of section 3.4 also needs some modification into e.g. „Impact of 49 ka boundary conditions on...”
- Section 3.2: I find it a bit hard to stay on track when following all the details between different seasons, different experiments, and different variables. Maybe starting the paragraph on p. 8 l. 158 with “In the DJF season” could create a clear structure and nicely contrast it with the paragraph starting in l. 169. This also holds for section 3.3 - try to make the structure immediately obvious for the reader (You do it nicely in section 3.4.).
- Section 3.2 and p.15 l. 271/272: moisture (flux convergence), specific humidity: Have you actually looked into these model results? Then it would be good to add “not shown” where appropriate. Otherwise it might be a bit speculative.
- p. 8 l. 164: I would also mention the strong cooling which seems to stand out in response to the albedo change.
- Please be very precise with specifying where changes are happening, e.g. p. 8 l. 175/176 “larger temperature and precip. anomalies”, or p. 8 l. 179. “by altered thermal and pressure gradients” is very vague.
- p. 8 l 174: “topography and height” - Isn't height included in topography?
- p. 8 l. 182 and 183: I am confused by the 20 Sv and cannot find this in the shaded part of Fig. 7a (dark reddish colors between about 15S and 10 N). Isn't the anomaly much larger according to the shading? (similar for the 11 Sv anomaly)

- p. 8 l. 184 Can you please include a sentence or reference for the link/mechanism between insolation and the Hadley cell strength?
- When comparing the Hadley cell among the different experiments, it would be helpful to have it in the main text and not in the supplement to allow direct comparison for the reader. The authors want to emphasize the role of the different boundary conditions, so it would be helpful to combine figures 7 and A6.
- p. 10 l. 190: "temperature gradient between the two hemispheres" - How about quantifying this for all experiments and add it to the diagnostics table suggested above?
- p. 10/11 l. 197/198: I suggest to rephrase this sentence. => "Temperature contrasts between hemispheres are amplified due to the introduction of ice sheet topography which induces localized warming in Siberia and strong Antarctic cooling, but also due to the replacement of ocean grid cells by land." It might also make things clearer if you explicitly specify the direction of the Hadley cell shift in relation to the hemisphere temperature contrast (shift towards the warmer hemisphere?)
- p. 11: Since you already include the SH westerlies into Fig. 4, you could move the paragraph on SH westerlies to the end of section 3.2, discuss them there and keep a "tropical"-only focus in section 3.3.
- section 3.4 p. 12 ll. 208/209: I do not fully agree with this statement. Doesn't Fig. 5c (49ka-co) already show the increase in Australian rainfall? You mention the strong obliquity change at 49 ka, so you might need to take this into account as well and not attribute everything to the ice sheets. I would start the section with l. 209 (slightly modified) and carefully phrase the following part in the light of the different boundary conditions, not just ice sheets, especially when referring to the 49ka-full figures (3,7) in the subsequent paragraphs. You could add for instance "in response to the 49 ka boundary conditions" to the sentence in p. 13 l. 223.
- p. 14: AMOC strength during MIS3 seems to vary a lot among timeslices and models. A CCSM3 35 ka simulation initialized from LGM showed a weak AMOC (Merkel et al. doi:10.1016/j.quasci-rev.2009.11.006).
- p. 15 l. 275: What do you mean by "more dynamic"? The zonal response in Löffverström' study is also a dynamic one, isn't it?
- p. 15 l. 279/280: There are studies which demonstrate the impact of glacial boundary conditions beyond the N. Atlantic sector (see doi indications above, but also for instance DiNezio et al. doi:10.1126/sciadv.aat9658, Mohtadi et al. doi:10.1038/nature13196, Shi et al. 10.5194/cp-19-2157-2023).
- p. 15 l. 282: => "compared to our simulations"
- p. 15 vs. section 3: Some parts of the discussion on p. 15 have already been raised in section 3. Section 4 reads quite well, so you might consider to shorten section 3 and leave the interpretation for section 4.
- section 4 Fig. 8: This is a nice summary which might already be worth to refer to during section 3. When averaging over the North Atlantic, is this all ocean grid points in a latitude range or did you chose some lat-lon box?
- section 4: I would recommend to modify the titles in bold of this section. I don't think these subsections can be clearly separated, and as noted above, phrasing should be done very carefully and with less focus on the ice sheets in the titles due to the experimental setup. It is appropriate to discuss the important role of the ice sheets, but the titles should be more general.
- p. 17 l.309: You might want to be a little bit more specific about the term "water availability" (soil moisture, atmospheric moisture content,...?).
- p. 17 l. 313: Wouldn't it make sense to also refer to your Fig. 3g to support your argument related to the insolation changes?
- The Conclusions section is very short and does not mention MIS3 at all. It should be slightly rephrased in order to align better with the motivation and the main focus of the manuscript. Furthermore, the phrasing of the impacts of the ice sheets suggests that separate experiments have been conducted to isolate the respective impact of NH and SH ice sheets (LIS, AIS). Please make the wording more concise.
- Data availability: Will also the code, e.g. to calculate the atmospheric mass streamfunction, be made available? Which density has been used for the calculation in Sv?

- To my knowledge, in a student's t-test, you would call 95% the confidence level and 5% the significance level. Please correct the corresponding text and figure captions.
- Fig. A1 caption mentions „simulated ice sheet“, but according to the Methods section, ice sheets are prescribed as constant forcing in this study.
- Fig. A3 mentions „North Atlantic gyre strength“. Is it the barotropic streamfunction?

Technical corrections

- Please double-check all acronyms, not all of them have been explained or explained where they appear for the first time (e.g. CMIP6 p. 3 l. 63, CASA-CNP p. 3 l. 67)
- p. 1 l. 3 “simulate a glacial climate” => “simulate the glacial climate”
- p. 1 l. 14 “by their extent” => “by the ice sheet extent” (and also height?)
- p. 1 l. 19 “on the atmospheric”
- p. 2 l. 45 Menviel et al., 2020 => Menviel et al., 2020 and references therein
- Section 2.1 l. 67: “uses” => “consists in”
- p. 3 l. 81/82: => “with only the orbital parameters... and GHG concentrations ... being set to 49 ka values...”
- p. 4: Tab. 1: I would just put 49 ka instead of 49ka-full.
- p. 6 l. 1 => “divides the tropical precipitation amount”
- p. 6 l. 4 “within the same range” => „over the same vertical/pressure range“. Is there a reference for choosing 400-600 hPa?
- Tab. 2: caption: “in all” => “in the different 49 ka experiments”; “in the full time-series shown in” => “as shown in the timeseries of Fig. A1”
- Fig. 1 caption: a), b) missing; “per grid cell” => “of each grid cell”
- Fig. 2: Sea-ice edges are very hard to see in Fig. 2b. How about choosing polar stereographic projection instead?
- Fig. 2 caption: “ocean ice” => “sea ice”. “Contours are” => “Contours show”. I would also start the caption with “Annual mean anomalies of...”
- l. 125: => “by 5 to 6° latitude”
- l. 127: NADW => Do you miss “formation” here?
- p. 7 l. 137: I guess you are referring to the meridional temperature gradient here. Please add.
- p. 7 l. 138: => “shift by ~6°”
- Fig. 3 caption: I would start with the caption with “Anomalies between 49ka-full and PI for (a,b) SAT (°C)...”
- Fig. 3c-f: What happens at 0°E in the vector plots? Have you also looked at higher levels to avoid the plotting conflict with topography?
- p. 14 l. 269: => “landmasses”
- Fig. 8: Since you have a), b) in the Figure, you could adjust the caption accordingly. You could also make the lat-lon specifications in brackets consistent (hyphen, colon) and make the Figure and caption consistent (Europe/Eurasia).

- Fig. A1: The caption does not mention the lower panels and does not have references to "left panels", "middle panels" etc. You could also add a), b), labels. Please also mention what is shown, I guess it should read "Timeseries of anomalies of annual mean surface air temperature...".
- Fig. A2: What exactly is shown here? Have you taken the SW downward radiation at the top of the atmosphere/model?
- Fig. A3: Please mention in the caption that the white areas mark grid cells with continental ice, and that for the 49 ka simulation, continental outlines are shown based on the adjusted land-sea mask. I would also write "...the 49ka_full to PI difference".
- Fig. A5: caption incomplete: It is missing vector descriptions.