

Dear Editor, dear reviewers,

We are grateful for the detailed feedback by the reviewers and their very useful suggestions. We have worked on the manuscript since the first review in October 2025 on the following important points:

- 1) We have clarified the use of our coupled framework, focusing on climatic steady states, where all components reach a quasi-stationary state, included the ones with a slow response such as ice sheets and vegetation. Consequently, we have changed Abstract, Introduction and Conclusions to better explain how our coupled framework can be used to identify climatic steady states and their basin boundaries, where the climate becomes unstable and shifts from one attractor to another.
- 2) We have updated the pre-industrial simulation (*run1*) using a slightly different setup, which solved the issue of the energy imbalance at the top of the atmosphere, giving overall results that are similar in performance to the original ones with new tuning parameters. As in the old manuscript, *run1* is used to test our new coupled framework by simulating the pre-industrial ice sheet starting from the bedrock elevation and to compare the results with those of two CMIP models with dynamical vegetation.
- 3) We ran additional simulations:
 - a deep-time simulation (Permian-Triassic cold state based on Ragon et al. *Scient. Rep.* 2024) to show the ability of our coupled setup of dealing with ice sheet formation in different continental configurations and climate conditions.
 - a forced simulation from 280 ppm to 360 ppm (new *run2*). In this case the ice sheet is kept constant to the extent and volume obtained at 280 ppm. This simulation is used to assess our results against reanalyses and observations in non-stationary conditions.

With these updates in mind, we believe that our manuscript can be strongly improved, following the reviewers' remarks.

Please find below a detailed point-by-point response to the comments of the reviewers.

Sincerely,

On behalf of all coauthors,

Laure Moinat and Maura Brunetti

Response to reviewer 1 for: Moinat et al. – biogeodyn-MITgcmIS (v1): a biogeodynamical tool for exploratory climate modelling.

Summary

Moinat et al. present a modelling tool that consists of the MITgcm augmented with additional offline components for vegetation, runoff and ice sheets. The model is run at intermediate resolution and mainly intended for deep time steady state simulations. Simulations under pre-industrial and present-day forcing are compared to other models and observations.

General comments

I have struggled quite some time to understand what this modelling tool is supposed to be used for. It is not until page 6 that this is clearly spelled out ("Since our purpose is to investigate climatic steady states in which ice sheets are in balance with the ocean, atmosphere, and biosphere"). If this is true, the statement should already be present in the abstract and the introduction should be geared towards that purpose. However, it is important to understand that in the real world, an ice sheet is never really in balance with the prevailing climate. While there is also mention of transient simulations, I believe in practice the model can only be used for time slice simulations, and only for those where assuming ice sheets in steady state with that climate is an acceptable assumption. The premise of the presented modelling (operating with climate and ice sheet steady states) excludes the application of the tool to interactions between climate and ice sheets that happen on timescales shorter than the relaxation time of the slowest component (the ice sheets), which is 40-100 kyr (1289). This may e.g. be illustrated by the Greenland ice sheet losing large amounts of its volume in run2 as it is relaxed to a steady state in the present climate, rather than attaining a transient state.

Answer: We sincerely thank the reviewer for the detailed report and for the constructive criticisms, which are very useful for improving our manuscript (see also our response on October 17, 2025). We agree with the reviewer that we need to clarify the applications of our model setup. The main application is to find, for a given climate and CO₂ content, the corresponding ice sheets and vegetation cover over multimillennial timescales. This is particularly useful in deep time, where the ice sheet extent and volume are poorly constrained, as well as the vegetation cover. Our model setup can also be applied in the case where the ice sheet is fixed to find the corresponding climate and vegetation under a given forcing or under the effect of a forcing that changes on a time scale smaller than the ice sheet response (for example, by changing the CO₂ content on centennial time scales).

In the first version of the manuscript, we focused exclusively on steady-state examples, in which the ice sheet evolves toward equilibrium with the corresponding climate and vegetation. This approach is particularly suitable for investigating deep-time climates, such as those of the Early Triassic, where paleogeographic reconstructions provided by PANALEISIS or paleoMap do not include ice sheets or account for isostatic adjustments. Nevertheless, the climatic conditions in the Early Triassic, for example, could be sufficiently cold to allow ice sheet formation, as demonstrated in previous numerical experiments conducted within our group (Ragon et al., 2024). The advantage of our ice sheet model is being at global scale, allowing for the description of climates where the ice sheets are not confined to high latitudes, like during glacial periods, waterbelt or snowball states.

However, before applying our coupled setup to deep-time climate configurations, it is essential to verify whether the model can reproduce the preindustrial ice sheet starting from the bedrock topography (*run1*: preindustrial (PI) state).

Regarding *run2* in the first version of the manuscript, which represents the evolution of the ice sheet under increasing CO₂ forcing until a steady state is reached, we agree with the reviewer that this experiment cannot be directly compared to observations, as the latter reflect transient, forced conditions. This is why we have run a new *run2* simulation, as detailed below.

I can see two options for the manuscript to continue, both involving considerable work:

1. There may be value in this tool being applied to simulating deep time climate states (as hinted at e.g. in the title and model name) with steady state ice sheet configurations. In that case, the manuscript has to be strongly refocussed, aspects of transient simulations removed, comparison focussed on the preindustrial and a deep time use case added and discussed.

Answer: We fully agree with the reviewer that including a deep-time simulation is relevant to demonstrate the capability of our coupled setup to be applied to different paleogeographic configurations. We therefore now include the case of the Permian–Triassic cold state described in Ragon et al. (2024), in which we employed the ice sheet model to estimate the thickness and extent of the ice sheet that develops in the Northern polar region.

2. It may be possible to modify the model setup to allow for transient ice sheet simulations. It is common practice to run asynchronously coupled simulations between climate and ice sheet models where the climate forcing is accelerated. In that case the ice sheet component is the 'pacemaker' of the experiment and determines the physical time evolution. In that case more work would have to go into improving the modelling. Adding another example for a climate state different than the PI/PD would also be needed here.

Answer: We agree with the reviewer that setting the ice sheet component (pacemaker) and let the climate evolves consequently is the way we can run transient simulations. This is what we now do in *run2*: the ice sheet is kept fixed at its PI configuration, while the responses of the climate and vegetation are examined under a gradual increase in atmospheric CO₂ concentration from 280 ppm to 360 ppm. This forced simulation can be readily performed with our coupled setup and is compared to available reanalysis data. Such an approach is commonly employed in CMIP-style CO₂-forced experiments. Note that this procedure can be applied to any arbitrary initial ice sheet configuration. However, since this is a validation paper, we have applied this procedure to PI/PD.

The following comments are mostly independent of this choice.

What I miss in the introduction is a clarifying discussion of limiting factors in ESM modelling: spatial resolution, complexity and timescale. With limited HPC resources compromises are typically made on all three of them with different weight depending on the specific application of interest. What is the benefit of the specific choices made here? There is a relatively high resolution climate component compared to traditional EMICs but very low resolution of the ice sheet components. I don't understand the latter choice, since ice sheet models are typically the least expensive component and it would be feasible to run at a reasonable resolution without slowing down the system considerably. If the ice sheet resolution is not improved, it should at least be made clear that the current setup has a spatial resolution of the ice sheet models that is well below of what is considered appropriate to resolve ice sheet dynamics.

Answer: We have reformulated the discussion on the limiting factors (spatial resolution, complexity and timescale) in the Introduction. The main advantages of our setup are the following:

1. The ice sheet model is global, which is particularly important for deep-time simulations where data are scarce and the potential location, extent, and thickness of ice sheets under cold scenarios are not known a priori. The fact that is at the global scale allows our model setup to describe climates when the ice sheets are not confined to high latitudes, but spread to large regions, like the waterbelt and snowball states.
2. It includes the first-order, large-scale dynamics of ice sheets, allowing global-scale simulations on millennial time scales to be performed without the need for high spatial resolution and with relatively low computational costs.
3. It employs the same cubed-sphere grid as the underlying climate model (MITgcm), thereby avoiding interpolation errors.

Once the ice sheet extent and volume are estimated at the first order with our setup, one can always run high resolution ice sheet models to investigate other processes, but this is outside the scope of the present setup.

I think it should be demonstrated that the model can be set up for a considerably different climate.

For a tool that supposedly can run multi-millennial simulations with ice sheet components it would be reasonable to at least be able to show an LGM configuration. Ideally I would also like to see a warmer climate configuration in addition like the last interglacial or the Pliocene. The setup of the tool may be in principle the same for any other time slice, but as modellers we know that there are always more complications once it is actually done. As is, the pre-industrial and present-day climate are too similar to serve as good validation experiments alone.

Answer: We have added a deep time simulation, the Permian-Triassic cold state in Ragon et al. 2024, to demonstrate the ability of our coupled setup of simulating very different continental configurations and climate conditions.

The analysis is heavily focussed on comparison to other CMIP models and observations, (where the latter is questionable due to the steady state nature of the modelling). Since I imagine that the uncoupled MITgcm has been validated extensively before, I believe there should be more focus in 3.2.1 and 3.2.2 on comparison to a pre-industrial state of the reference MITgcm without the additional components. This would show how the presented developments change the model behaviour.

Answer: In our *run1* simulation, the present-day ice sheets are obtained starting from bedrock conditions. This is now compared with MITgcm present-day simulations starting from etopo2 (which includes ice sheets), using the same parameters as in *run1*. Another MITgcm simulation is in Brunetti & V erard 2018 but with different parameters.

Specific comments

Title and model name "a biogeodynamical tool"

I am a bit confused about the term "geodynamical". According to the Wikipedia definition (<https://en.wikipedia.org/wiki/Geodynamics>), this should involve mantle convection and plate tectonics. If a deep time application with different continent distribution and climate could be shown, this would make a bit more sense.

Answer: The Permian-Triassic example is now added; therefore, the title and name of the model is more adapted.

l2 "climate tipping elements"

The mention of climate tipping elements early in the abstract and in the introduction is confusing. Studying

climate tipping points most people are concerned about requires full physical coupling between different climate components, which involves specific time scales, not steady states.

Answer: The part on the tipping elements was removed in the Introduction, which is now refocused on steady-state simulations and its application to deep time.

17 "a complexity level intermediate between EMICs and CMIP-class models"

Consider another description of the complexity level of your model.

EMIC already contains the term intermediate. I would say your model is by definition an EMIC, albeit maybe of relatively high resolution of the climate component compared to many other EMICs (add references).

Answer: This part in the abstract was reformulated. The following references were added in the introduction: Holden et al. 2016, Willeit et al. 2022.

19 "offline couplings"

Depending on where the modelling goes, I would suggest to use the term "asynchronous coupling" instead. "Offline" may be read as one-way coupling without feedback on the core climate model.

Answer: It is effectively asynchronous, as there is feedback to the core climate model. This is corrected in the revised manuscript.

110 "(MITgcmIS)"

The naming is a bit confusing. If MITgcm in itself is the climate model, MITgcmIS sounds like it could be the coupled climate-ice sheet model.

Answer: The motivation for this name is that the ice sheet model is explicitly designed for the cubed sphere that is used in MITgcm.

110 "The latter is implemented on the same cubed-sphere grid"

This should be motivated at some point. It also has to be noted somewhere that this resolution is well below of what is considered appropriate to resolve ice sheet dynamics.

Answer: The motivation about using the same cubed-sphere grid for global-scale ice sheet reconstructions has been added in the Introduction, and more in detail in Section 2.4.

112 "the new ice sheet model and the coupling procedure"

Focus is on the ice sheet component, which is not represented in the title. Consider modifying the title accordingly.

Answer: We have changed the title to:

biogeodyn-MITgcmIS (v1): a biogeodynamical tool for exploring climate steady states with a new global-scale ice sheet model

##112 "We evaluate biogeodyn-MITgcmIS" and 114 "biogeodyn-MITgcmIS successfully reproduces the large-scale climate and its major components"

Offline coupling of the new components (18) would suggest that the base climate state of biogeodyn-MITgcmIS is identical to the core MITgcm. I suppose MITgcm has been validated before. Clarify what is evaluated here specifically and why another response is expected.

Answer: We have specified that the assessment is performed starting from bedrock topography in pre-industrial climate conditions until our coupled setup gives rise to the ice-sheet formation. We then compare the results of our simulation (*run1*) with the control runs of CMIP models in terms of the overall climate,

vegetation cover, and ice sheet extent and volume, assuming that the pre-industrial climate is at steady state. A second experiment (*run2*) explores the forcing conditions in the 1979-2009 period, and the results of our simulation are compared to reanalyses and observations. In this second experiment, the ice sheet is kept constant to its pre-industrial extent and volume.

113 "pre-industrial period and the 1979-2009 period"

Difficult to imagine how to evaluate climate-ice sheet feedbacks for these rather steady periods. How does the model perform when the ice sheets are changing considerably?

Answer: To tackle this question and the concern about the model application to deep-time climates, we added a simulation for the Permian-Triassic paleogeography based on Ragon et al. (2024), starting from the cold-state conditions. In this case, an ice sheet develops in the Northern Hemisphere, with a volume of 8.5 million km³, corresponding to around 20 m of sea level change. This is in the range of possible values in the Early Triassic (Simmons et al. 2020). A new section has been added in the revised manuscript with the following figure:

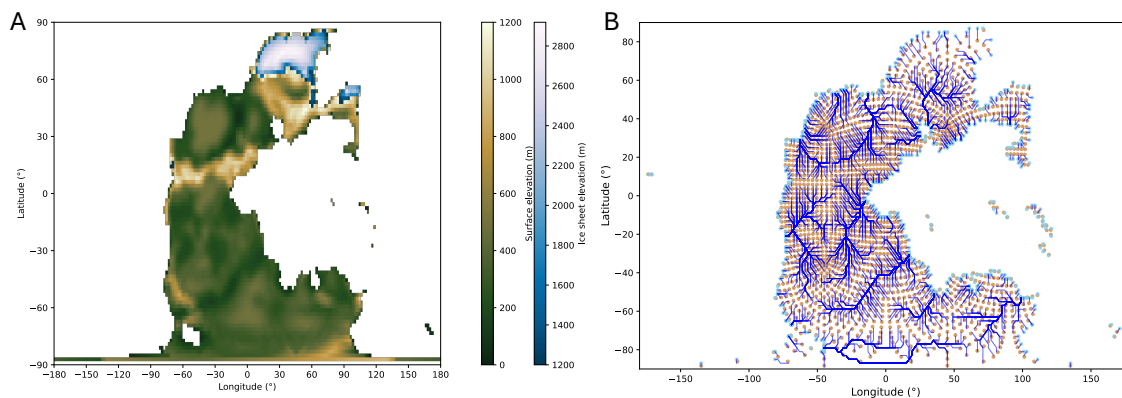


Figure 14. *A Permian-Triassic topography with ice sheet elevation obtained using biogeodyn-MITgcmIS (the bedrock elevation used to start the simulation can be found in Ragon et al., 2024); B. Runoff routing map for the corresponding topography.*

118 "CO₂ concentration raises under the present-day climate crisis"

Strange combination in this sentence of future perspective and "present-day". Reformulate.

Answer: Reformulated in the revised manuscript.

132 "simulations cannot reach stationarity in slow deep-ocean and ice sheet dynamics"

Focus only on stationarity seems like an insufficient or even problematic goal when interest is in tipping, feedbacks and dynamics.

Answer: We have now better defined stationarity and tipping behaviour in the Introduction. Simulations reaching a steady state are used to understand the dynamical structure of the Earth climate by identifying the different climate steady states (attractors) under external forcing (e.g. CO₂), their limits of existence (basins of attraction), and the associated boundaries, which may correspond to global tipping points (that is, forcing values beyond which the climate system shifts towards a new steady state). Reaching stationarity is an important property for characterizing climate attractors (Hawkins et al. 2011, Drótos et al. 2017, Lucarini and Bódai 2017, Brunetti et al. 2019, Brunetti and Ragon 2023, Moinat et al. 2024).

135 "the nonlinear interaction among climatic components"

In your setup, this could only be addressed with high enough frequency of the asynchronous coupling and transient ice sheets.

Answer: We meant that in EMICs, some simplified parameterizations may have an impact on the nonlinear interaction among climate components. In our setup, the numerical representation is improved with respect to EMICs (in terms of spatial resolution, ocean dynamics, asynchronous coupling with ice sheets and vegetation). However, as we mentioned above in the response to the general comments, the ice sheet is too coarse to run transient simulations on centennial time scales, since km-scale processes are not included. We have improved the Introduction following the reviewer's remarks.

146 Add reference to Smith et al. 2021 (UKESM)

Answer: Added in the revised manuscript.

156 "complexity that is in between EMICs and CMIP-like models"

See comment 17

Answer: This has been reformulated in the revised manuscript.

159 "We will provide a complete description of all the components"

A complete description of all the components of a coupled ESM does not fit in this paper. Maybe you meant a brief description of the climate components and a full description of the ice sheet component?

Answer: This is indeed what we meant. This part was reformulated in the revised manuscript, where we mention that we will provide a short description of the dynamical core (MITgcm), the vegetation model (BIOME4) and the hydrology model (pysheds), as they have been already detailed elsewhere, while we will extensively describe the ice sheet model (MITgcmIS).

174 "SPEEDY is in the intermediate complexity class"

See comment 17

Answer: This has been reformulated in the revised version of the manuscript, where we explicitly state that SPEEDY is an intermediate complexity model for the atmosphere.

180 "planetary boundary layer"

Is this level used to provide boundary conditions for the ice sheet model? Usually PDD models ingest 2m air temperature. Discuss this as caveat of the coupling.

Answer: The temperature for the ice sheet model is indeed taken at 2m. The lapse rate is calculated using the temperature in the atmosphere levels. This information is included in the revised manuscript.

190 "runoff map"

Is this runoff mapping, like a runoff routing scheme? It prescribed the pathway but not the amplitude? Clarify.

Answer: Indeed, the runoff map that we need to provide in input is a routing scheme and not an amplitude scheme. This has been clarified in the revised manuscript. We added two new figures showing the runoff routing maps for *run1* and the Permian-Triassic case.

190 "salinity and sea temperature for all the ocean levels"

Sounds like salinity and temperature are prescribed rather than dynamically calculated. Is that the case? Or is this just for the initial state? Clarify.

Answer: The input salinity and temperature files are used just as initial conditions, then these variables are dynamically calculated. This information is now included in the revised manuscript.

190 "Orbital parameters can be set by specifying the obliquity, the duration of the day and the radiative influx from the sun"

Orbital parameters are typically obliquity, precession and eccentricity. I don't think "duration of the day" and "the radiative influx from the sun" are strictly speaking orbital parameters. Is it maybe possible to say that the effect of the orbital configuration can be set by specifying ...?

Answer: This sentence is indeed ambiguous. The orbital parameters can be explicitly set by changing obliquity, precession and eccentricity. In addition to this, it is possible to change the duration of the day and the radiative influx from the sun. This is clarified in the revised manuscript.

191 "200 years per CPU day using 25 cores."

Does this mean you can run 200 years in 24h or 5000 years? Please present two numbers: 1) how fast is the model practically, i.e. how many years per day do you run with your typical configuration, 2) what is the efficiency of the model, e.g. how many CPU hours does a 100 year run consume.

Answer: The model in our setup can run 200 years per day using 25 cores, which is equivalent to 300 CPU hours for 100 yr (25 CPU x 12 hours). These two numbers are added in the revised manuscript.

193 "as well as deep-time climates"

Since the focus of the section is not different climates but different palaeogeographies, I would reformulate here.

Answer: This has been reformulated in the revised manuscript.

1107 BIOME4 is described here as a purely diagnostic component forced by output of MITgcm. Is there any aspect of the vegetation model that would feed back on climate? I am mainly thinking about albedo, but also runoff, since you mention water holding capacity.

Answer: The albedo and vegetation cover are given back to the coupled MITgcm. This is added in the revised version of the manuscript. The water holding capacity can also change depending on the vegetation cover and is used in the MITgcm land module. However, we kept it constant in our simulations, as specified now in the revised manuscript.

1120 "28 including land ice"

What does that imply for the interaction between BIOME4 and the land ice component? You do not mention ice sheet extent as input to the vegetation model. What happens when the ice sheet retreats or expands? Can vegetation grow on land that has newly become ice free? What if vegetation is overrun by expanding ice sheets?

Answer: BIOME4 is the last part of the coupling, therefore the vegetation can grow on land that has become ice free. If expanding ice sheets overrun the vegetation, the albedo (0.75) and the vegetation fraction (0%) of the ice sheet is given. This has been clarified in the revised manuscript.

1130 "interested in horizontal resolutions of the order of 2° or coarser"

A bit strange to state that you are "interested" in such low resolutions. I think you should aim for a setup that resolves the processes you are interested to represent.

Answer: Corrected in the revised manuscript.

1130 "we can neglect basal melting and other fine-scale processes, as calving and ice streams"

I would rather say that you neglect those processes because they cannot be represented at the coarse resolution you are modelling. I still do not understand why you chose such a low resolution for the ice sheet components.

1132 "STREAMICE"

Could you explain why you are not using this module?

Answer: Our wording at L130 of the original manuscript is revised. The reasons for our low-resolution approach to ice-sheet dynamics are as follows. We are primarily interested in the first-order effects that ice sheets play in setting the global climate state i.e. their orographic and albedo forcing, as well as large impacts on global mean sea level. As such, fluctuations in marginal geometry e.g. rapid speedup of marine ice streams or tidewater glaciers due to calving or ice-ocean interactions are not our primary focus.

While a higher-order ice-sheet model such as STREAMICE could potentially be used, there is little purpose in doing so. Running STREAMICE at the same resolution as other components would require extensive computation for higher-order stresses that are not relevant at these scales, and as STREAMICE uses an explicit time stepping scheme, would limit the time step. At the same time, running STREAMICE at kilometer-scale resolution, as it is generally used, would preclude long-timescale simulation due to the computational cost of the associated nonlinear elliptic equation. Moreover, it likely would not improve the physical representation of the slow-moving interiors of ice sheets, and there would not be a reliable means of representing ice-ocean interactions at the margins with such a coarse ocean model. In general, as well, using STREAMICE would require complicated interpolation between a cube-sphere grid, and the cartesian space within which STREAMICE runs.

We do concede we are unable with our current framework to study large-scale drawdown mechanisms triggered by ocean melt of ice shelf or tidewater calving (e.g. Marcott et al, 2011). Still, we feel our approach is sufficient to represent the aspects of ice sheets in which we are interested at low computational expense.

1135 "improved Positive Degree Day"

Is your method improved compared to Braithwaite (1977) or compared to other more recent PDD implementations?

Answer: It is improved compared to Braithwaite (1977), as shown in the original paper by Tsai and Ruan (2018).

1135 "as we also want to apply our coupled framework to paleoclimates."

Not sure to understand the reasoning. Is this an argument for using PDD instead of prescribing a given SMB?

Answer: This sentence has been removed in the revised manuscript. This point was indeed discussed in the next part of the paragraph, where the choice of the PDD against the Surface Energy Balance (SEB) method was motivated.

Also here, I would like to see how the PDD results would look like for a considerably different climate.

Answer: This is shown in the new section on the Permian-Triassic cold climate.

1137 "We choose an improved PDD approach"

Same question as 1135

Answer: It is reformulated in the revised manuscript where we explicitly mention the paper by Tsai and Ruan (2018).

1139 "This is an important limiting factor to consider that makes it difficult to apply SEB to paleoclimate simulations"

I am not convinced about the reasoning here. Similar limitations (too coarse resolution, missing important elements of the surface energy budget) certainly also apply to the PDD method, except that it is not as obvious/explicit as for an SEB method.

Answer: We agree that both methods are affected by resolution. We therefore deleted this sentence in the revised manuscript and instead focused on the fact that we consider the PDD method to be the best choice when atmospheric and snow processes are represented at the global scale on a coarse grid and treated in a simplified way, as in the SPEEDY and land modules of our setup.

1163 "where $\tau_d = \rho_i g H \alpha$, $\alpha = -\nabla_z S$ "

I don't see the purpose of defining alpha here. It is also easily confused with "a" defined in 1160

Answer: We agree, alpha is removed in the revised manuscript.

1173 "Since our purpose is to investigate climatic steady states in which ice sheets are in balance with the ocean, atmosphere, and biosphere"

This should be declared much earlier in the manuscript!

I am trying to make sense of the implications of this statement: an ice sheet fully adjusts to new climate conditions on a 100 kyr time scale. In the real world, an ice sheet is therefore never really in balance with the prevailing climate. I suppose this means that you are not interested in the coupled interaction between ice sheets and climate, which involves specific time scales?

Answer: We are interested in climatic steady states, and more generally in how a given climate is compatible with the presence of ice sheets. Therefore, the feedback of ice sheets on climate needs to be evaluated iteratively until convergence is reached, since we run our ice-sheet model starting from bedrock configurations. As a consequence, the model is not intended to perform *transient* simulations, for which time-scale issues would arise. Moreover, our ice-sheet model cannot resolve the kilometre-scale processes involved in ice-sheet transitions, as discussed above. In the revised version, we have clarified from the Introduction that our focus is on climatic steady states.

1174 "we choose not to represent these higher-order processes"

In ice sheet terms, "higher-order processes" has a specific meaning. Please use another description.

Answer: This is reformulated as 'km-scale processes' in the revised manuscript.

1192 "ODE"

Introduce abbreviation.

Answer: We removed the acronym and write it explicitly as 'ordinary differential equations'.

1198 "The required input is the air surface temperature"

See comment 180

Answer: The MITgcm output of the surface air temperature is provided at 2 meters. This is specified in the revised manuscript.

1199 "significant improvement in capturing early-season melting"

After reading 1173 and in the light of all the other simplifications in the model this seems like a quite unimportant refinement to me. But I guess it is good to use recent developments. I would be concerned if the tuning of PDD model parameters presumably done at much higher resolution by Tsai and Ruan and e.g. with a geometry that resolves the ablation areas is translating to your setup. Have you considered retuning the PDD model?

Answer: We have adapted some of the PDD parameters to our setup to correctly reproduce the volume and the orography of Antarctica. In particular, we set $k/h = 40 \text{ W m}^{-2} \text{ }^\circ\text{C}^{-1}$, $H_p = 10 \text{ m}$, $h = 2 \text{ m}$ in our setup, and we tuned the Glen's law parameter.

Maybe more importantly for the deep-time application you envision, the assumption that the PDD with present day parameters translates to other orbital configurations is highly problematic

Answer: We mentioned this potential limitation of PDD in Section 2.4, citing the paper by Plach et al. 2018.

1209 "In summary,"

This summary concerns both 2.4.2 and 2.4.3 but appears in 2.4.3. I would suggest to describe ablation and accumulation in a joint section.

Answer: These two subsections have been merged in the new 'Surface mass balance' section.

1212 "accumulation rates"

"accumulation" and "accumulation rate" are used interchangeably. Please be consistent in terminology.

Answer: Corrected in the revised manuscript.

1215 "Taking into account the isostatic correction"

Why "correction"? Maybe "isostatic adjustment"?

Answer: Corrected in the revised manuscript.

1216 "where a time delay is included"

Why not use instantaneous adjustment in line with your interest in steady states? This does not seem consistent with the approach in other components.

Answer: The time delay is used during the growth of the ice sheet in the ice-sheet model that is run for 40000 yrs. In contrast, during the run of the core climate model, the topography does not change.

1232

Define T_{new} and T_{pickup}

Answer: The term T_{pickup} is replaced by T_{old} as pickup refers to the output files of the coupled MITgcm. T_{new} is the temperature after the lapse-rate correction. These new definitions are added in the revised manuscript.

1233 "lapse rate is computed at each ice-sheet grid point using the MITgcm output"

Can you explain how this is done?

Answer: A temperature value is extracted from each pressure level in the atmosphere, and then, based on the altitude of each pressure level, a linear regression is made between the temperature and the altitude. The slope from this linear regression is the lapse rate. Finally, zonal average is performed.

l235 "freshwater flux to or from the ocean is computed and included at restart at the ocean boundary of the ice sheet"

I am confused about how that works. Does the freshwater flux enter the ocean instantaneously? What determines the time scale of release?

l236 "To guarantee the conservation of salt, a compensation is performed at the global scale"

How does that work precisely? Is the model operating with physical freshwater fluxes or with (negative) salt fluxes?

Answer: The model operates with virtual salt fluxes. We adjust the salinity content in the global ocean considering the volume of water that is stored in the ice sheets.

l283 "the other with daily frequency (for MITgcmIS)"

It seems counterintuitive that the ice sheet component needs a higher frequency than the vegetation model. In some PDD implementations the seasonal cycle is parameterised to reduce the input to monthly or even annual. This should be mentioned and you should consider it to improve the performance of your model.

Answer: The computation in the ice sheet model has been improved; therefore, it is no longer a limitation. To improve the performance of the ice sheet model, the following was done: 1) for the first 10 yr, the model computes the surface mass balance for every land grid point; 2) afterwards, it identifies the land points where the surface mass balance was systematically negative since the first iteration; 3) it withdraws these points from the computation of the surface mass balance. Applying this procedure to the pre-industrial run, the number of land points reduced from 1772 points to 166, giving a reduction of the computation time by a factor of 4.

l285 "if the sea ice thickness"

Do you mean "ice sheet thickness"?

Answer: It is the sea ice thickness indeed. We are describing here how the ice sheet can extend towards a shallow ocean covered by sea ice.

l289 "MITgcmIS is run for an equivalent of 40-100 thousand years"

Why "equivalent"? Isn't it run for an actual 40-100 kyr?

Answer: It is run for an actual 40-100 kyr. It has been clarified in the revised manuscript.

l290 "isostatic, lapse-rate and sea level corrections (Sec. 2.4.4) are included at this stage"

Are these corrections applied after the ice sheet model has run in MITgcm? Clarify. If so, how are feedbacks between isostasy and ice sheet SMB incorporated in this way?

Answer: This was not clearly explained in the manuscript. The isostatic adjustment is calculated during the ice-sheet model runs, using the lapse rate estimated from the previous convergence step. Then, the ice sheet volume is determined and is converted to a sea level change. The mask, topography and bathymetry files are changed in consequence. This explanation is now included in the revised manuscript.

L291 "pysheds is applied using these files"

What are "these files"?

Answer: pysheds needs the mask and the topography files. This is now explicitly mentioned in the revised manuscript.

l299 "the pickup files"

What is that?

Answer: We reformulated this part as follows: `... salinity and sea temperature at all ocean levels are averaged over the last 30 yr to generate new input files for the coupled MITgcm setup. Then, these files, along with the new vegetation fraction, albedo, topography, bathymetry and mask files, are given back to the MITgcm to run the whole coupling process at least twice...`.

1303 "describes in a consistent way the evolution of all the climatic variables"

I do not I agree with your notion of "consistent" evolution. The resulting evolution is not physically consistent in that the time scales of various processes are modified or removed. In the real world, e.g. the ice sheets do not have 40-100 kyr time to adjust to a new climate condition.

Answer: The term `consistent` has been replaced in the new sentence: `... which thus describes the climatic steady state, including those components with a slow response...`.

1304 Figure 1 "Transient simulations: repeat the same procedure by changing the forcing each N years"
See point 1303. This setup is not made to run transient simulations with time steps shorter than 40-100 kyr.

Answer: This part was removed from the figure, and the figure was updated in the revised manuscript.

1317 "with the addition of an isostatic correction"

Could you give a short summary of how that works? I am mostly interested if this is in line with the LLRA model you are using.

Answer: The ice-free isostatic topography is estimated in Paxman et al. (2022). We directly took the map from this paper and adapted its resolution to the cubed-sphere grid used in our setup.

1321 "The resulting isostatic map"

What does "isostatic map" mean? Is this the bedrock adjusted for unloading of the ice sheets? If so, it would be interesting to see the difference to the reference bedrock in addition.

Answer: It is the bedrock adjusted from the unloading of ice sheets. The isostatic adjustment to the reference bedrock (from BedMachine) is provided in Paxman et al. 2022.

1324 "PANALEISIS or other reconstructions directly provide bedrock elevations"

Not sure about the ice sheet loading in that case. Is that always included?

Answer: In all the reconstructions (PANALEISIS, PaleoMap) the ice sheet loading is not included. There are indeed huge uncertainties in ice sheet extent and volume at deep time, this is why we need a global-scale ice sheet model able to provide a first-order estimate of the ice sheets.

1325 "pre-industrial conditions at 280 ppm"

I am confused about combining present-day ice sheet reconstructions with pre-industrial climate. Is that the idea here?

Answer: We compare the present-day ice sheet to the results of *run1* since there are no observational data available for the pre-industrial period. Note that for models without dynamical ice sheets, the present-day ice sheets are used in *piControl* runs (Eyring et al. 2016).

1331 "set to $a = 1.2 \cdot 10^{-15} \text{ Pa}^{-3} \text{ s}^{-1}$ "

What was the tuning procedure/target to find this value?

Answer: The tuning procedure, which was detailed in section 3.2.4, is now moved to Section 3.1.1 (Validation procedure) in the revised manuscript. The target is to obtain an Antarctic ice sheet volume within 10% of the observed values, while maintaining a similar surface elevation profile. The surface mass

balance produced for Antarctica is also estimated to be of the same order of magnitude of that obtained in an ensemble of Regional Climate Models.

1333 Figure 2. I find this projection unusual and not very intuitive to interpret. Could you give additional figures with traditional lat-lon projection for global views and polar stereographic projections for the ice sheets?

Answer: We now show the topography in the lat-lon projection in Figure 2, and stereographic projections for the ice sheets in Figure 10 of the revised manuscript.

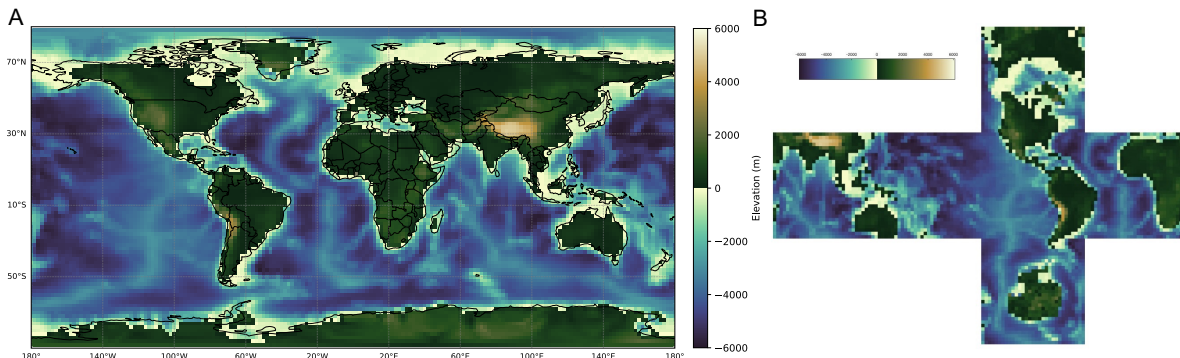


Figure 2. Bedrock topography (with isostatic adjustment) and bathymetry used as initial boundary conditions in run1 on the latitude-longitude grid (A) and on the cubed-sphere grid (B).

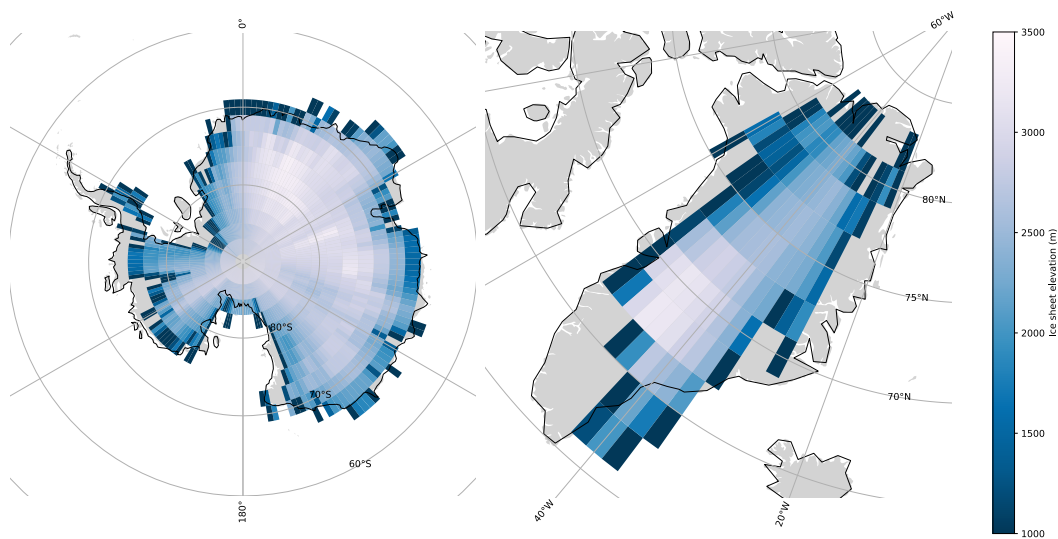


Figure 10. Map of the region where ice sheets form with biogeodyn-MITgcmIS, showing the ESPG:3031 and ESPG:3413 projections for Antarctica and Greenland.

1335 "The second simulation (run2)"

Can you give more details on how this run is set up? What is exactly the difference to run1 other than another CO2 concentration? Is this also a steady state run.

Answer: In the original version of the manuscript, run2 was performed until the climate reaches a steady state at 360 ppm. As discussed earlier, these stationary conditions cannot be compared with the forced conditions of reanalyses. Therefore, following the reviewer's suggestion, we replaced this run2 by a new run that is started from the 280ppm steady state (run1, pre-industrial simulation). Then, we applied a CO2

increase until it reaches the 1979-2009 CO₂ average value (360 ppm). This description is now added in the revised manuscript.

1336 "This run will be evaluated against the reanalysis and observational data"

Are you assuming that the real climate is in equilibrium with a 360 ppm CO₂ forcing over this period? Are the ice sheets? How is it conceptionally possible to compare the steady state model against transient observations?

Answer: We agree with the reviewer, and we have now replaced *run2* with a forced simulation, therefore this is not an issue anymore.

1352 "5 hours of CPU time due to the daily stepping of the ODEs"

That seems very expensive for such a low resolution ice sheet model. Consider optimizing the SMB calculations.

Answer: The ice-sheet model has been optimized, as explained earlier (see comment at 1283).

1352 "for all land points"

Maybe you can exclude some points from the daily calculations when no ice is present?

Answer: It has been done in a similar way (see comment at 1283).

1351 "one week to reach equilibrium"

Could you describe further how many years have been run for the different components?

Answer: This sentence refers to the coupled MITgcm, which is run for thousands of years. We have reformulated as follows: 'The MITgcm took approximately 1500 simulated years per week to reach equilibrium using 25 processors for each iteration. BIOME4 and pysheds run in less than 5 minutes on a desktop computer. MITgcmIS needs around 1 hour of CPU time due to the daily stepping of eqs. (6)-(7) for all land points.'

1353 Table 2.

Specify meaning of variables in first column in the caption.

Answer: Table 2 was modified accordingly in the revised manuscript.

1356 Figure 4

Add latitude tick labels for NorESM2-LM

Answer: corrected in the revised manuscript.

1361 "Excluding the ice sheet formation"

It would be interesting to see an analysis how the inclusion of the new components (vegetation, ice sheets) changes the model behaviour compared to the reference MITgcm.

Answer: The coupled MITgcm setup (including ocean-atmosphere-land-sea ice) has been compared to observations in Brunetti & V erard 2018 but starting from ETOPO2 topography (including present-day ice sheets). Here, we start from the bedrock topography and perform a detailed comparison with CMIP models and reanalyses, obtaining ice sheets and vegetation cover in good agreement with these products. Thus, the asynchronous coupling of the ice sheet and vegetation models allows us to obtain a climate in good agreement with observations. We added comparisons with a simulation which starts from ETOPO2 (like in Brunetti & V erard 2018 but with all parameters as in *run1*) in the revised manuscript.

1369 "which underestimates the elevation of Antarctica and Greenland"

Maybe this should be verified with a biogeodyn-MITgcmIS run where the ice sheet elevations are prescribed. Having that option in the model would anyhow be a good development.

Answer: This option is already available. As the topography is prescribed in the coupled MITgcm, one can always start from a topography that includes ice sheets, as for example ETOPO2. This corresponds to the simulation performed in Brunetti & V  rard 2018 and the new *run2*.

1371 "the model capability to correctly reproduce the Hadley cells"

In line with comment 1361, I seem to understand that MITgcm has been validated extensively. If true, this suggests strongly to focus the present analysis on how including the new components changes the model behaviour compared to the uncoupled reference in addition to comparison with other models.

Answer: MITgcm in its ocean-only version and the SPEEDY atmosphere have been distinctly validated. As said in the comment at 1361, Brunetti & V  rard 2018 already described a coupled MITgcm simulation starting from ETOPO2 (including present-day ice sheets).

1398 "the energy used for ice sheet growth is not included in this diagnostics"

Could it be? Shouldn't it be? Does that mean energy is not conserved? What is the result when you include it?

Answer: The TOA budget problem was linked to a parameter that was inactive in previous versions of MITgcm and became active in the c68s version used here. We realised this issue with the help of Dr Jean-Michel Campin at MIT (his help is acknowledged in the revised version). Therefore, all simulations were rerun, and the problem is now solved. The results of the new simulations (with new tuning parameters) are very similar to the previous ones, except for the TOA budget that is now in the range of CMIP models.

1412 "due to higher surface temperatures"

Could this be related to equilibrium climate vs transient climate. That's an important shortcoming of this comparison that should be discussed.

Answer: This was indeed a problem of the previous version. Now that we compare reanalyses with a forced simulation (new *run2*), this issue is no more to be discussed.

1438 "3.2.3 Vegetation"

I seem to understand that the offline vegetation model BIOME4 has been extensively validated. In addition you state "Offline coupling between the coupled MITgcm atmosphere-ocean-sea ice-land setup and BIOME4 has been already successfully applied in Ragon et al. (2024, 2025)". I am wondering about the added value of section 3.2.3 in that context. Instead, it would be interesting to learn if including the ice sheet and runoff components changes the model behaviour. Is there an interaction between ice sheet change and vegetation? What vegetation grows in a much warmer climates where the ice sheets retreat?

Answer: The study in Ragon et al. (2024, 2025) is on the Permian-Triassic climates (without ice sheets), hence the validation of the coupled setup is needed for the present-day geography.

1465 Why is there no evaluation of the runoff component here? That should be added.

Answer: We added two figures showing the runoff routing maps obtained for the PI and Permian-Triassic simulations.

1466 "3.2.4 Ice sheet"

I find it very difficult to justify the presented detailed comparison with observations of ice sheet models at such low resolution. This is particularly true for Greenland, which consist of only 25 grid points in the PI

simulation.

Answer: To assess our model, we compared the volume and the elevation distribution with the observations. This allows us to evaluate the global scale features of the ice sheet. As the ice sheet mainly interacts with the coupled MITgcm model through the surface albedo and the topography, it is important to evaluate these two quantities with respect to observations.

1469 "evaluating the SMB produced for Antarctica is a prerequisite to calibrate the Glen's law parameter"
The text until line 484 is not well placed in the Results section. I think this should be described earlier in the manuscript.

Answer: This part is moved to sec. 3.1.1 in the revised manuscript.

1471 Figure 10

Very unusual projection to display ice sheet results. Please consult some recent publications for inspiration. The standard are polar stereographic projections EPSG:3413 for Greenland and EPSG:3031 for Antarctica.

Answer: We now use these projections in the new Figure 10.

1486 "the Greenland ice sheet decreases strongly in height and extent"

This may be explained by the ice sheet being relaxed to a steady state in your model, while it is in a transient state in the real world. This just shows once more that the model is not meant to simulate transients with time scales of interest lower than ~50 kyr. In addition, I am concerned if the remaining 18 grid cells do a great job in simulating the dynamics of the ice sheet correctly.

Answer: *run2* ice sheet was removed, as *run2* is now a forced simulation with a fixed ice sheet (obtained in *run1* at 280 ppm). Thus, this comment does not apply anymore.

1489 "after smoothing to the same spatial resolution"

How does smoothing change the total volume?

Answer: Interpolation to a different, coarser grid has an impact on the ice sheet extent and volume.

1514 "further improvements are possible"

There are quite a few that should be discussed here.

Answer: Further developments are discussed in Section 3.3. We have reformulated this sentence.

1527 "In future iterations of MITgcmIS, sliding and basal heat balance could easily be implemented"

In my view, the most important improvements are to increase the spatial resolution of the ice sheet models and make the SMB model more efficient.

1528 "this would allow study of nonlinear processes"

Before any of that, your model setup has to be modified to be able to run transient ice sheet simulations and not only steady states.

Answer: As said above (see comment at 1283), the SMB computation has been improved and is now more efficient. Concerning increasing spatial resolution of the ice-sheet model, the reviewer can refer to our response at 1130.

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Response to reviewer 2 for: Moinat et al. – biogeodyn-MITgcmIS (v1): a biogeodynamical tool for exploratory climate modelling.

Summary

Moinat and colleagues present the “biogeodyn-MITgcmIS”, a global climate system model that is designed to encompass relatively low computational costs and yet couple extended components of the earth system – ocean, atmosphere, land surface, sea-ice, and ice-sheets. They do so by coarsening the grid of the MITgcm and, in regards to the land surface vegetation and ice-sheet models, by performing offline equilibration runs using the outputs of the MITgcm. This asynchronous running minimises computation costs that would otherwise be required in multimillennial simulations. Their procedure is to run the MITgcm, followed by offline runs of the ice-sheet model, water shed runoff model and vegetation model in that order.

The authors run two experiments: pre-industrial and a modern day simulations. They compare the preindustrial run to two CMIP6 models and they compare their modern day run against a suite of observational products. The model shows broad agreement against these datasets in this assessment.

The work is presented in a clear manner and the figures and writing are of good quality. The authors do not over-reach and the paper is appropriate for the journal.

Answer: We thank the reviewer for their positive feedback.

My only major comment is that the authors talk about climate tipping points in the introduction and conclusion, and yet, the authors and the experiments they have performed have not convinced me that this model is indeed able to simulate a climate tipping point where the system rapidly shifts to a new attractor. I challenge the authors to provide such an experiment and talk to this within a new section of their results. Given the low computational costs of their model, the authors could achieve this by performing a 4x CO₂ experiment, for instance, both with and without their ice-sheet model and then assessing the impact of including that model on the system. Or perhaps the authors have better ideas about this sort of feedback response could be achieved?

Answer: We thank the reviewer for raising this concern. The model is indeed not meant to describe transient simulations where all components (asynchronously coupled) are evolving. The aim of the model is to study climatic steady states (attractors) and to infer, from them, the dynamical structure of the climate system by studying how the climatic states depend on the external forcing (see for example Ferreira et al. 2011, Rose 2015, Brunetti et al. 2019, Ghil & Lucarini, 2020). This approach allows one to identify the range of existence of each climate attractor with respect to an external forcing (e.g. CO₂) and their basin boundaries. The latter correspond to unstable regions, where a small change in the forcing can trigger a shift from an attractor to another, thus giving rise to a so-called ‘global tipping point’ (a climate change at the global scale).

This approach has been used in the past with MITgcm in simplified configurations (such as in the aquaplanet configuration, that is an ocean-covered planet) (Ferreira et al. 2011, Rose 2015, Brunetti et al. 2019, Ragon et al. 2022, Moinat et al. 2024), and in the Permian-Triassic configuration (Ragon et al. 2024, 2025), a much warmer climate than today. However, we realized that for studying climatic steady states in the present-day continental configuration, an ice sheet model is necessary because of the presence of ice sheets in both the glacial and interglacial states.

We agree that the Introduction was not well written and could lead to confusion on what our model setup can achieve. We have now improved both the Abstract and the Introduction by focusing on climatic steady states and their relevance for deep-time climate studies and, in general, for identifying the dynamical structure of the climate system at any arbitrary continental configurations.

In the revised version, we discuss how our coupled setup simulates the pre-industrial climate at 280 ppm starting from the bedrock topography, as well as a Permian-Triassic cold state characterized by the presence of a small ice sheet in the Northern Hemisphere. We also include a new simulation (*run2*), which is initialized from the ice sheet obtained at 280 ppm and forced with increasing CO₂ concentrations until the mean value of the 1979-2009 period (360 ppm) is reached. In this forced experiment, the ice sheet is kept fixed, as our coarse ice sheet-model does not resolve km-scale processes that may become relevant on centennial time scales. This simulation is used to provide an indicative assessment of model performance against reanalysis and observational data, and replaces the previous *run2*, which was run toward the 360-ppm equilibrium state and is therefore not directly comparable with forced conditions. Concerning the 4×CO₂ simulation, applying an analogous experimental design (i.e., prescribing a fixed ice sheet) would not allow a meaningful comparison with observations; for this reason, we do not include it in the present validation-oriented analysis.

Our approach to obtain a ‘global tipping point’ would require following the stable branch of the pre-industrial steady state until it loses stability. This would involve a series of simulations run to equilibrium until the unstable boundary is reached (following the method described in Brunetti & Ragon 2023). Such an analysis is beyond the scope of the present study but will be addressed in future work.

Minor comments:

Line 33: “are fast reaching a stationary state” à “can reach a stationary state rapidly”

Answer: corrected in the revised manuscript.

Line 44: “such as the deep ocean”

Answer: corrected in the revised manuscript.

Line 47: “However, high computational costs makes these models appropriate for”

Answer: corrected in the revised manuscript.

Line 59: I think it’s best to remove all future tenses from the paper. Please make your writing either present tense or past tense.

Answer: corrected in the revised manuscript.

Line 85: “to capture mixing by mesoscale...”

Answer: corrected in the revised manuscript.

Line 101: “will be” à “is”

Answer: corrected in the revised manuscript.

Lines 100 – 106: Are all these features done automatically by the model or do you have to do these manually?

Answer: We use python or matlab scripts for these preliminary steps.

Line 114: Define what a PFT is. Also, make sure to correct your acronyms to “PFT”, not “PTF”.

Answer: We added the definition of the PFT and corrected the acronyms in the revised manuscript.

Line 133: How is this different to your new model? Do you use these models together in the same runs?

Answer: The STREAMICE package is meant to represent km-scale ice sheet processes and their interaction with the ocean. This package works on the latitude/longitude grid at regional scales. In contrast, MITgcmIS is used to simulate global-scale ice sheets on a coarse grid, thus excluding km-scale processes such as calving and ice streams. MITgcmIS is directly implemented on the same cubed-sphere grid of MITgcm at the global scale. These two models play two distinct roles, and they are used to answer different questions. In our runs, the STREAMICE package is not used because the resolution of the coupled setup is not suitable for the fine-scale processes addressed by STREAMICE. A clarification has been added in the revised version of the manuscript.

Line 135: Can you spell out SMB instead of adding a new acronym? You already have a lot of acronyms.

Answer: corrected in the revised manuscript.

Line 169: It’s interesting to me that you assign “a” as constant when making it vary with temperature or something else would actually provide your model with the kind of feedback that would create non-linear tipping points in the system.

Answer: While it would not be inordinately difficult to represent varying ice temperature by solving an advective-diffusive heat balance, it would (other than slowing our ice sheet component down) introduce uncertainties regarding boundary conditions, namely the magnitude of the geothermal heat flux. While this value is reasonably well constrained today, it likely varied greatly in the deep past in ways that are not quantifiable. Given this level of uncertainty, we feel it is more appropriate to represent this material parameter as constant.

Line 217: What is wss. Please define.

Answer: wss is the vertical displacement of the lithosphere that is due to the ice load. We have reformulated this sentence.

Line 267: How many models actually do this amongst the CMIP6 contingent?

Answer: CMIP6 models employ runoff grids and typically need to estimate runoff routine offline. We reformulated the sentence and added a reference to Hou et al (2013).

Line 284: “corrections”

Answer: corrected in the revised manuscript.

Line 292: “and giving a new runoff map as output”.

Answer: corrected in the revised manuscript.

Fig 1: Can you add numerals “1” and “5” to your steps in this figure? You have “2”, “3” and “4”.

Answer: Fig. 1 is corrected in the revised manuscript.

Line 316: “This map was...”

Answer: corrected in the revised manuscript.

Table 2: Please make the caption more informative. Tell me what all variables are.

Answer: The caption in the revised manuscript contains the definition of all acronyms in the table.

Line 372: "... reproduce Hadley cells (Figure 4). In run2 our..."

Answer: corrected in the revised manuscript.

Line 390: "model captures..." rather than "module captures..."?

Answer: The atmospheric module of the coupled MITgcm setup is based on the SPEEDY model. Since we are describing the output of the coupled MITgcm setup, we prefer to use 'module' in this case.

Line 398: Why don't you include this energy in your calculations? Shouldn't you? Would it be too difficult?

Answer: The problem of the imbalance at the top of the atmosphere was due to a parameter that was inactive in previous versions of MITgcm and became active in the c68s version used here. We realised this issue with the help of Dr Jean-Michel Campin at MIT (his help is acknowledged in the revised version). The issue was not due to the fact that 'the energy used for ice sheet growth is not included in this diagnostics', as we thought before. When the coupled MITgcm setup is running, the ice sheet is kept fixed and has not an impact on the TOA budget calculated by the coupled MITgcm setup.

Therefore, all simulations were rerun with the correct parameter, and the problem is now solved. A change in the tuning parameters (RHCL2 and a) guarantees that the results of the new simulations are very similar to the previous ones, except for the TOA budget that is now in the range of CMIP models in run1. The results of the simulations are updated in the revised version of the manuscript.

Line 416: Your ocean under modern day is incredibly fresh, and I am shocked that the ORA is that fresh. It can't be. Can you please recheck your calculations of ORA. It should be around 34.5 psu.

Answer: It was recalculated by doing an average on the first layers of the ocean instead of just the first one, which gives 34.36 psu.

Line 432: Please include a citation or citations for your statement that the behaviour is expected.

Answer: We have included the following reference in the revised manuscript:

Caesar, L., Rahmstorf, S., Robinson, A. *et al.* Observed fingerprint of a weakening Atlantic Ocean overturning circulation. *Nature* **556**, 191–196 (2018). <https://doi.org/10.1038/s41586-018-0006-5>.

Line 446: Please include a citation or citations for your statement that the behaviour is expected.

Line 446: Why is this linked to excess precipitation in SPEEDY? Seems the opposite of what I'd expect.

Answer: The precipitation in North Africa in *run2* is larger than in observations, as shown in Fig. A3. This gives rise to a shift from desert to shrubland. We have reformulated this sentence in the revised manuscript, as follows:

' It also displays the Amazon rainforest by returning the tropical evergreen forest biome, and the desert biome, although the latter is smaller than in observations. This is directly linked to the excess of precipitation produced by the SPEEDY module in North Africa (Fig. A3).'

We have included the following reference in the revised manuscript:

R.J. Norby, E.H. DeLucia, B. Gielen, C. Calfapietra, C.P. Giardina, J.S. King, J. Ledford, H.R. McCarthy, D.J.P. Moore, R. Ceulemans, P. De Angelis, A.C. Finzi, D.F. Karnosky, M.E. Kubiske, M. Lukac, K.S. Pregitzer, G.E. Scarascia-Mugnozza, W.H. Schlesinger, & R. Oren, Forest response to elevated CO₂ is conserved across a broad range of productivity, *Proc. Natl. Acad. Sci. U.S.A.* 102 (50) 18052-18056, <https://doi.org/10.1073/pnas.0509478102> (2005).

Line 462: Your correlations are not close to one. Please be more honest about this.

Answer: We specify now that the correlation coefficient r is close to 0.8 and the slope to 1.

Figure 10: Can you provide a new column of differences? The differences in Antarctica are hard to see.

Answer: The equilibrium simulation at 360 ppm has been removed and replaced by a forced simulation in which the CO₂ concentration is gradually increased until 360 ppm is reached, as suggested by reviewer 1. As a consequence, the *run2* ice sheet has been removed, and this issue no longer applies.

Line 493: Are these runs done with an IS model at equilibrium? If so, surely the response at 360 ppm of an IS model at equilibrium would be a good reason for the simulated flux of freshwater and ice sheet loss to be much greater than the observations? Please explain and make clearer here what was done.

Answer: Indeed, the equilibrium simulation at 360 ppm gave ice sheet loss greater than observations. However, in the revised version we have removed this simulation (see previous point), thus this part of the text has been removed as well.

Final paragraph before conclusions: I find it really interesting and actually a bit disappointing that you don't showcase how the model could simulate a climate tipping point to a new regime. This hasn't been shown even though you talk about it in the introduction.

Answer: As stated in our response to the major comment, our model setup is capable of identifying climatic attractors and their stable branches, as well as the associated 'global tipping points', as now clarified in the Introduction. Accordingly, the Conclusions have been reformulated in the revised manuscript.

Line 535: "agree broadly with..."

Answer: corrected in the revised manuscript.

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