

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 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 CO<sub>2</sub> 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. CO<sub>2</sub>) 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 CO<sub>2</sub> 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×CO<sub>2</sub> 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  $\alpha$ ) 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 CO<sub>2</sub> 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 CO<sub>2</sub> 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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