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# *Interactive comment on* "Coupling the Glacial Systems Model (GSM) to LOVECLIM: description, sensitivities, and validation" *by* Taimaz Bahadory and Lev Tarasov

**Anonymous Referee #1**

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GENERAL COMMENTS:

The authors present a new, coupled climate - Northern Hemisphere ice sheet model consisting of LOVECLIM and GSM. The model description focuses on a set of novel, previously often neglected coupling features, and on the effect of these features on the glacial inception (120-106ka).

The conclusion that all of the four new features have significant impacts on the ice sheet evolution - which I assume is the ice sheet evolution during the last glacial inception - is not fully supported by the presented results. While the four new features mentioned in the abstract are 1) dynamic meltwater runoff routing, 2) advective precipitation, 3) a



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locally-varying dynamic vertical temperature gradient for lapse-rate corrections, and 4) a sub-shelf melting parameterization, sensitivity tests for the ice sheet evolution during the inception are shown for the inclusion of meltwater runoff (Figure 5), and at least as a snapshot for advective precipitation (Figure 3), but neither for the dynamic lapse-rate correction nor for sub-shelf melting. On the other hand, the sensitivity of the ice sheet evolution to different coupling timesteps and to the inclusion of a cloud parameterization are shown, but not mentioned in the abstract.

While I am a fan of the many sensitivity tests shown, I think that the manuscript would benefit from a more detailed analysis of what exactly causes the differences. For example, why does the changed river runoff lead to enhanced ice buildup during the inception, or how can a larger coupling-timestep cause faster melting?

In the second part of the manuscript, an ensemble of present-day coupled model runs is presented, aimed at "bracketing" the present-day climate (and at reducing the temperature bias of LOVECLIM / page 10 lines 4-5?) by varying ice and snow albedos, initial conditions, and other climate and coupling parameters. A subset of ensemble simulations is then selected based on the simulated 1700-1980 sea-level trend. Although this ensemble of simulations is impressive, it is unclear to me how these large-ensemble sensitivity results presented in Table 2 work together with the first part of the manuscript (sensitivity of simulated glacial inception to new coupling processes). Wouldn't it make more sense to first find a good set of ensemble parameters based on present-day observations, and then try to simulate the last glacial inception?

Since the coupled model is not publicly available, the results are only reproducible for co-operators of the authors. I do think that the description of the four new coupling features (or Section 3 in general) can still be useful for other modelling groups, who are developing or are planning to develop similar model setups (of course, making in particular the coupling-code immediately available would make reusage or development easier - maybe also for the authors, because the description of the coupling in the text might not have to be as detailed and could focus on why a certain method is applied rather than how exactly that is done). From just the text, it would be challenging if not impossible to reproduce the same coupling schemes (see, e.g., questions below about meltwater runoff).

SPECIFIC COMMENTS:

abstract / page 1, lines 3-4: "... interactions... that are often ignored" Please rephrase, e.g., "neglected" rather than ignored? For example river runoff changes were discussed frequently before, i.e., not ignored, but neglected. Can a "novel downscaling" really be "ignored"?

page 3, lines 5-6: I think it should be pointed out here that, if dust deposition and its surface albedo effect is to be included, then the PDD-approach will have to be replaced or at least modified to include insolation.

page 3, line 6-7: For clarity: The land-sea mask and bathymetry can be changed (see Roche et al., 2007, for LGM bathymetry), but dynamic land-sea mask or bathymetry has not been implemented.

page 3, lines 10-12: I think a list of the other available models, and of the reasons why they could not be used on what architecture would be beneficial (both for the model developers and potential users).

Table 1: Table 1 serves well to illustrate that the four mechanisms discussed here were neglected in many previous continental-scale ice sheet-climate simulations. However, adding "dust" to the table may suggest that this is a complete list of the important coupling mechanisms, which I do not agree with. I would suggest to only include the 4 new features, or to extend the list, including e.g.: stationary wave feedback, vegetation feedbacks, cloud feedbacks, ice sheets or also shelves, grounding line dynamics vs. prescribed ice mask, fixed/variable land-sea mask, and maybe also models used, simulated timeslice or period.

page 4, line 3: Which version of LOVECLIM is used?

page 4, lines 3-4: It should be noted that LOVECL*IM* already contains an ice sheet model (AGISM), which, I assume, is not available to the authors?

page 4, line 20: As mentioned above, changing bathymetry seems to be, in principle, possible (Roche et al., 2007), but interactive bathymetry has not been implemented. Or is it clear already that interactive bathymetry will never be possible with CLIO?

page 5, lines 11-12: Since the sub-glacial melting is one of the 4 key features discussed, the parameterization should be described in more detail here.

page 5, lines 12-13: Why is the sub-shelf melting also dependent on proximal subglacial meltwater discharge? I am curious to know what the mechanism behind this assumption is, or if there is observational evidence.

page 5 lines 19-20 / Figure 1: "Figure 1 displays all the fields the coupler transfers..." Figure 1 does not show all the coupling fields and processes. For example, winds, temperature, evaporation, temperature gradient, and ice mask changes are not indicated. Maybe, instead of Figure 1, a box-diagram that really does include all processes would be more accurate, maybe something like Fig. 4 of Roche et al. (2014)? I think such a box-diagram could work very well here, since Section 3 is already organized in "component-coupling-directions", so that arrows in the box-diagram could be named after the subsections in the text.

page 5, line 27: Why does the 100-year-coupling-timestep-run diverge from the others during the melting?

page 6, lines 2-3: What time period is used to compute the fields, only the last year, or for example the last 10 years before the coupling?

page 7, line 5: "The downscaled mean standard deviation..." Mean over several years? Are standard deviations computed from daily means? Or individual timesteps?

page 10, lines 9-11: sub-shelf melting Do I understand correctly that the temperature profiles at the 10 index-sites from Figure 4 are diagnosed at every coupling timestep,





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and then prescribed at the proximate shelves? What does "downstream" mean here? Downstream according to the 3D-ocean currents at each coupling timestep? And if so, what happens if the grounding-line / shelf ice moves inland with respect to the CLIO bathymetry? The locations of the index points seem arbitrary, how were they chosen?

page 10, lines 4-5: "We... do not apply a bias correction... and instead examine the extent to which an ensemble parameter sweep can reduce the bias." What is the result of the examination? Can the bias be reduced, and if so, how?

page 11, line 11: "... or preserving the valleys (w2=0), ..." If I understand correctly, the mean topographic height is preserved for w2=0, not the valleys.

page 11, line 17-18: "Any cell in the resulting grid with more than 30% ice coverage is then assumed to be ice covered." I think that this critical ice fraction would also be a good candidate for a sensitivity study. Please briefly discuss possible implications of this choice. I suspect that a critical fraction smaller than (the maybe more natural choice of) 50% facilitates the ice buildup during the inception?

page 11, lines 24-27 (freshwater discharge): Is the runoff flux calculated by the land model affected by the ice cover? What happens to the soil moisture in ice-covered grid cells? What happens in (according to the ECBilt-grid) partially ice-covered grid cells at ice margins? If a grid cell is covered by, say, 40% of ice and is as such defined as ice-covered, is all precipitation onto the land-grid-box blocked, although only 40% of the grid box "sees" precipitation? Are the "LOVECLIM discharge zones" the coloured areas in Figure 4? Shouldn't the coupler compute the freshwater produced in the respective catchment areas that correspond to the discharge zones, and then *discharge* that water in the discharge zones? Which mass exactly is conserved? Are the catchment basins and discharge areas adjusted to the changing LOVECLIM topography?

page 11, line 30: "... ice in the simulation including runoff grows faster as it gains more volume." This sounds like a very interesting feedback; why does the runoff enhance ice growth? I suspect a cooling due to a weaker meridional overturning?

page 18, line 10: Which orbital and GHG forcings are used?

Figure 8: The x-Axis label should probably be "ice volume change (m SLE / 280 years)" rather than ice volume.

page 22 line 10: replace "amounts of heat and freshwater" by "amounts of heat and seawater"?

page 22 line 13: "Our bounding reality criteria is not met by at least two AMOC features..." [replace "by" by "for"?]

TECHNICAL CORRECTIONS:

p2 l28: "(e.g., Roche et al., 2014)" [brackets, dots]

p2 l31: maybe better: "..., we are also working towards bracketing the effects of these feedbacks." [if not, then "strengths" with s?]

p4 l9: no space after "hours"

p4 l19: describe (no "s")

p7 l6: monthly mean [add -ly]

p11 l6: of all GSM grid cells [no "the"]

p22 l2: no dot after "ensemble."

p22 l4: full-stop missing

Fig 11 caption: Zonal average [no -ly]

Fig 13 caption: remove double brackets around "Balmaseda et al"

p28: please check reference Nikolova et al., 2013 [remove copyright statement]

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