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Geoscientific Model Development Discussions

Interactive comment on "Adding a dynamical cryosphere into *i*LOVECLIM (version 1.0) – Part 1: Coupling with the GRISLI ice-sheet model" *by* D. M. Roche et al.

Anonymous Referee #1

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Roche et al. present a model description of their initial inclusion of a dynamical ice sheet model into an EMIC (iLOVECLIM). Implementation of ice sheet models into EMICs is a worthwhile enterprise, given the ability of these models to simulate long time-periods. As such, I believe this work is well-motivated scientifically. However, I do believe very major revisions are required for this manuscript before it is released from the Discussion stage of GMD. Very broadly, these revisions focus on:

1. description and justification of coupling procedures

2. robustness of model evaluation

3. omissions of important diagnostics of model performance

4. unexplained inconsistencies in model results

5. overly confident assertions of model ability to perform science simulations

English grammar and conciseness also needs a lot of work, but I would like to see the more important concerns I have addressed before worrying about grammar.

I have indicated pages and line numbers for particular points, and prefaced general points with 'General:' The comments are in order of appearance, and I have highlighted comments which I consider most important with a '***'

**********Comments**********

P5216,L8: 'for the Northern Hemisphere ice sheet (Greenland' -> 'Greenland'

P5216,L13: 'reasonable' is a subjective word. I think the authors get a 'reasonable' comparison, only because the simulated GrIS is unable to expand any further, because of the surrounding ocean in the model.

***P5216L17: first sentence of manuscript discusses glacial/interglacial cycles, but no glacial/interglacial simulations are carried out here, only constant-climate equilibrium simulations. Suggest rewording this first sentence, so reader is not left with impression that the following work involves transient simulations, or simulations of past climates.

P5216L26: 'In this framework': not clear to reader what 'framework' this refers to

P5217: 'direct evolution': 'direct descendant'

***General: Please include more references to earlier efforts to couple ice sheets to climate models (other than LoveClim), and explore how the author's approach differs from these approaches. A few examples: -Ridley et al, 2005/2009 (10.1007/s00382-009-0646-0) -Charbit et al., 2008 (10.1029/2008GL033472) -Fyke et al., 2011 (10.5194/gmd-4-117-2011) -Vizcaino et al., 2008 (10.1007/s00382-008-0369-7) I also think the authors could refer to the body of literature which describes the role of climate forcing to influence ice sheet evolution. A few recent examples: -Quiquet et al, 2012

(10.5194/tc-6-999-2012) -Yoshimore et al., 2012 (10.1175/2011JCLI4011.1)

***P5219L1: The precipitation adjustment over the ocean occurs right around where Greenland is. Can the authors link the over-precipitation they see over GrIS to this broader bias (which is apparently important enough for oceanic processes to need to be fixed with a virtual precipitation pipe to the Pacific)?

P5219,L4: How are the coupled ice-sheet/climate models LOVECLIM1.2-AGISM, and iLoveClim/GRISLI) different?

P5219,L4: elaborate on how the model decides where to put ice streams. A map of where these occur over the simulated GrIS would be useful, or at least a verbal description.

***P5219,L6: How does this calving scheme work for GrIS? What does it generate? the authors say 'no significant ice-shelf areas are expected', but, what does the model actually do?

P5221,L2: I appreciate that the authors are adopting a non-bias-corrected approach, despite the difficulties it introduces in reproducing reality.

P5221,L8: Perhaps clarify what the authors mean by the 'seasonal cycle perturbation bias' argument. Would this go away if the authors used a monthly bias correction?

***P5221,L14: the authors use a PDD-based scheme in the coupled model. Can you quantify how much this scheme disregards conservation of mass/energy, where it is employed?

***P5221,L14: How is snow simulated elsewhere in iLoveClim? Is this default snow scheme operational over the GrIS, and if so, what happens when this snow melts over the GrIS in this scheme? Particularly, if ice is exposed, can the bare-ice surface warm above 0C? Does this exposure occur consistently with what occurs in the PDD scheme, as far as melt rate coefficients are concerned?

P5222,L15: "bilinear interpolation considering a GRISLI grid point and the 15 surrounding corresponding ECBilt center grid points": it is not clear why the authors need 15 points to do bilinear interpolation? Why not just 4?

***General: It seems like a basic figure of the SMB generated by the model is a very important missing piece of this manuscript. Please include a figure of the SMB field over the GrIS, as generated by the model, and compare this field to available observations or other simulations. Also, does GRISLI also receive surface temperatures as a boundary condition from the climate model?

P5222,L15: I suggest a re-write of the interpolation description, to first summarize the entire process of moving T/P from the ECBilt grid to the GRISLI grid, then describing in detail the separate downscaling processes. An additional flow-chart of the downscaling process would be a great addition.

P5223,L23: Could the authors provide a schematic figure, that shows an example of the vertical downscaling of temperature?

P5223,L4: what is 'along-slope'? Vertical gradient?

Figure 4: Could a clearer figure be a simple regression of non-downscaled vs. downscaled, temperatures? Perhaps this could be a second panel, of this figure.

***Figure 4: Annual mean temperature is much less important than summertime temperature, for GrIS SMB. What does this plot look like for JJA temperature, for example (sort of like Figure 11, where the authors show July temperatures)? And/or, a spatial map of the difference between the non-lapse-rated vs. lapse-rated temperatures, would help show readers how this scheme affects downscaling.

***P5223,L12: I would expect the difference in downscaled temperatures to be also a function of the difference between ECBilt and GRISLI elevations? Also, what are the range of lapse rates, and the spatial pattern of lapse rates that the scheme generates? Does it produce negative lapse rates (inversions)?

***P5224,L5: Is it either entirely snowfall, or entirely rainfall, for an entire month, if the monthly temperature threshold is below 2C (so that even if it is warmer than 2C, it is still snowing, for example)? Or, more preferably, can the model switch between snow and rain, on a sub-monthly basis (e.g. on the atmospheric model timestep)?

P5225,L12: Does ECBilt atmospheric circulation actually respond dynamically to changes in GIS topography, during the course of a simulation?

***P5225,L16: How is albedo, and albedo changes over the ice sheet, represented, given the PDD nature of the SMB scheme, the coarse nature of the model. This is important to describe, since the authors explicitly claim 'albedo is exchanged' in the Conclusions.

***General: Are ablation zones simulated in the model? What is the spatial pattern of SMB on the GRISLI grid? How is the snowpack simulated? Again, this is very important information, which is not presented at all in the current manuscript.

P5225,L20: Are isolated 'glaciers' actually simulated by GRISLI during the coupled simulation? This contradicts the statement: 'the ice reaches the sea all around Greenland'.

***General: I think the manuscript need much more justification and motivation for focussing on the sensitivity of an equilibrated ice sheet to the form of precipitation downscaling. Do the authors consider this the most sensitive aspect of the model design, or the aspect of the model that is least-constrained? As it stands, it seems like this sensitivity test is very arbitrary and not well motivated, given the wide range of other aspects of the model that could use sensitivity testing. I'm not sure the simulations presented here really explores model performance, especially since precipitation downscaling has very little effect on model results.

***P5226,L9: this section describes 1 control simulation, but Table 1 lists 2 control simulations. It is very unclear how to reconcile this, and also, which control simulation

is described in the text.

***P5226,L15: For 'SNOW' simulation, the authors say that ECBILT provides the snow accumulation. But earlier, the authors state "To this end, ECBilt does not provide snow only but the total precipitation (liquid plus snow) to the coupler." What does ECBilt provide in terms of snow/rain, and what are the authors considering snowfall, in the SNOW simulation? Do the authors assume snowfall=total precipitation? This is very confusing, and hinders any further analysis by the reader.

P5226,L18: the authors should comment on the potential impact of a transient spin-up (i.e. through the last glaciation), on results. At the very least, this is because in the very first sentence of the manuscript is: "The most prominent feature of the Quaternary era is the alternation of glaciated and less glaciated periods" yet after this, little mention is made of the transient nature of glacial evolution, or how the model would perform in a transient, or at least a paleoclimate snapshot context.

Figure 6: 'Grounding line': do the authors mean, 'ice margin'?

Figure 6/7: Combine these figures to show both absolute thicknesses and differences in an easily viewed manner. Currently, Figure 6 is not very useful, given the small ratio of the difference to the absolute thickness.

Figure 7: Difference both SNOW and PRECIP against 'observed', to make it easier to interpret the performance of the PRECIP experiment.

***P5227,L5: I don't believe a 33% overestimate of volume, and ice reaching the coast everywhere around Greenland, is actually consistent with the final statement of the manuscript: "Results of a 14 000 yr integration under pre-industrial yields a reasonnable ice-sheet distribution on the overall which brings us confidence for the use of this coupled model for long term climate change applications." It rather seems to me, like the model is simply growing the maximum-sized ice sheet possible, and is only being limited from growing further by the presence of a coastline. Is there actually an

ablation zone in the model? What is the fraction of ice lost to calving, versus surface melt? Generally, and importantly, it is not clear the current result adequately validate the model for future science, in any substantial way.

***General: A much stronger test of the model would be the difference in ice sheet geometries that results from snapshot simulations of multiple climate states (for example, LGM, Eemian, and present-day). Also, a very good indicator of model performance would be at what CO2 level the GriS deglaciates completely (for example, is the GrIS still stable at unrealistically high Cretaceous CO2 concentrations?).

P5227,L16: "where observations are giving ice-free conditions" -> "where no ice currently exists."

P5228,L13: Here is the first suggestion that there is actually more than one CTRL simulation.

P5228,L13: "Analysing the CTRL runs reveals that the results over Greenland are very similar " . . . Similar to what?

***Figure 8: It is not clearly explained: - what information the CTRL simulation(s?) give, in the context of the PRECIP/SNOW simulations -why the off-GrIS differences in accumulation are so large (according to the color bar changing the downscaling scheme over the GrIS results in >150% changes in accumulation over Canada, Baffin Bay, for example) -why Figure 8b has a large greyed-out region in the Southeast of the plot.

P5229,L3: Please clarify the description of the observational dataset used to compare the model against. For example, does Arctic precipitation over land come from Serreze and Hurst, or New et al?

Figure 9: what is 'TS1'?

Figure 9: It seems like a large signal of temperature difference between observations (TS1??) and the model over the ocean comes from differences in sea ice extent?

***General: Have the authors simulated the recent historical period with the model, and if so, how does accumulation pattern change? For example, one would expect recent historical accumulation to be higher than preindustrial accumulation due to a warmer climate state.

P5229,L24: "somehow in between these two extreme cases" is not a clear statement.

***P5229,L28: As the authors note, mean annual temperatures are not as important as summertime temperatures, in determining SMB and eventual ice sheet geometry. Yet, the discussion of temperatures is dominated by discussion of the annual temperature bias (3 paragraphs, versus 1 paragraph for summer temperatures).

P5229,L28: Now the authors mention only one CTRL simulation, not two.

***General: Can the authors carry out a recent historical simulation, to actually generate directly comparable temperature fields instead of indirectly comparing preindustrial simulated temperatures to modern reanalysis?

P5230,L20: As the authors note, it is not clear that the improved annual temperature in the SNOW simulation is a valid result, given that they indicate it results from cancelling errors.

Figure 10: Does a warm (red) color mean it is warmer or colder in the model, relative to reanalysis? I am having trouble reconciling the text description of these figures, with these figures themselves. For example, there is a warm (red) bias in southern Greenland in figures 10c/d, yet in the text the authors say: "there is a common pattern of cooler conditions . . .. in central and southern Greenland".

***P5230,L27: The large overestimate of summertime temperature is very confusing, since this should drive much excessive melting, which should in turn lower the ice sheet. It is not clear at all to me why the ice sheet is able to grow under the presence of such a high positive summer temperature bias, even given the positive accumulation bias. In addition, it is not clear that the author's explanation that ice height growth alone

is remotely capable of countering this large bias (by up to 13C), as the authors suggest. Using a conservative July lapse rate of 5C/km, this implies a vertical change of over 2 km, which is not the amount the modelled ice sheet grows. Furthermore, it is unclear how modelled temperatures of (apparently) 5-10C above zero can occur in July over the ice sheet (e.g., Figure 11a plus Figure 11b), given that excess energy should go to melting ice, and not raising surface and air temperatures. Is the bias in Figure 11b is due to a lack of back-coupling between the SMB model and the climate model, in terms of energy?

***General: I think the consistency, or lack thereof, between summertime temperature biases, precipitation biases, and resulting ice sheet evolution, needs to be much more clearly explored, definitely as more than one short paragraph.

***P5231,L24: I think that the fact that the simulated ice sheet reaches the coast is not so much due to the lack of higher-order ice dynamics, but rather large climate-side biases. It is hard to judge this, however, without an actual map of SMB on the GRISLI grid, which clearly shows the integrated effect of precipitation and temperature biases. SMB is also the basic boundary condition that the climate model generates for the ice sheet model, but it is not shown at all in the manuscript. Lack of an SMB figure is a critical missing component of this study.

***P5232,L10: This point, that the heat fluxes to the ice sheet PDD model are not reflected in equal but opposite fluxes to the atmosphere model, during run-time, should be made clearly earlier in the manuscript, to clearly describe the level of 'coupling' that has been achieved with the model. This is important, so that subsequent science with the model can be interpreted in the context of level-of-coupling. As one specific example, described above, it is hard to interpret the near-surface temperature biases (Figure 11b) and their impact on SMB, since it is not clear whether surface temperatures are actually pinned to near the freezing point during the summer.

P5232,L14: . . ."Seem to be in better agreement with pre-industrial temperature

reconstructions. . ." - this comparison was not actually shown at all in the paper; the authors only compared against reanalysis temperatures.

P5232,L9: "...relatively reasonnable (sp) distribution of ice in Greenland": what are the author's criteria for "relatively reasonable"? This is a very subjective statement, given the excessive area and volume the model produces.

P5232,L22: I cannot understand why "cumulated differences (???) in these regions" shows that "the model is able to reproduce regional effects."

P5233,L9: What is the weak dependence of ice sheet thickness to temperature fields in central Greenland? Where was this shown in the text? Do the authors mean the their argument that reduction of surface temperature biases comes from growth of the ice sheet?

***P5233,L16: How will a scheme to redistribute moisture to avoid excessive accumulation in central Greenland work? The problem here is a regional climate bias, not a problem with the sub-gridscale distribution of precipitation, I believe.

P5233,L14: 'slight preference of direct accumulation of snow computed on the ECBilt grid.' Do the authors mean, omitting the dependence of precipitation type on temperature produces a better result? This does not seem like a useful result, because it just indicates that consciously neglecting one physical process within the model fortuitously cancels the errors of another poorly resolved process or regional bias.

***P5233,L22: It is simply not clear to me how this one set of equilibrium preindustrial simulations that result in quite over-estimated ice sheet area and volumes 'brings us confidence for the use of this coupled model for long term climate change applications'.

***General: The conclusion is somewhat confusing, and difficult to tie with the various results presented earlier.

***General: Extensive work is required to improve the grammar and flow of this manuscript.

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