

Interactive comment on “Influence of ablation-related processes in the built-up of simulated Northern Hemisphere ice sheets during the last glacial cycle” by S. Charbit et al.

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Having heavily relied on PDD/refreezing (TP02) for my work, I find this an interesting study, though with a few puzzling results detailed below. Anyone involved in modelling past/present/future ice sheets will be well aware of the poorly constrained processes that need to be parametrically represented (for want of quantum computers...). This study raises clear attention to one set of such processes and demonstrates the need for a better constrained set of parametrizations for refreezing and representation of sub-monthly temperature variations (ie via σ in the submission). It is the first study, to my knowledge, to consider these issues over a whole glacial cycle. It also seems to in-

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dicate that parametrizations "tuned" (to varying extents) for present-day Greenland can have quite different relative responses when applied to the whole Northern Hemisphere during paleo conditions. As such, I see this an appropriate submission for Cryosphere and would recommend acceptance after the comments below are addressed.

#####moderate comments

In, Reijmer et al. (2012) the Janssens and Huybrechts (2000) refreezing model (same as TP02), consistently predicted less refreezing than all but one other model including either of the coupled RCMs and snow models and the Pmax=0.6 (Reeh 1991, ie RH91) model. This doesn't square with RH91 refreezing being < TPO2 in Fig3 and makes one question whether: 1) there is an error somewhere (either study)? 2) the different climate forcing and topographic regions explain the difference? 3) the weak refreezing in FST09 is compensating for weak ablation? 4) ??? Any which way, this Reijmer et al. (2012) result need to be discussed and if possible, an explanation is needed as to the source of the difference in results between that and this study.

Fig 2,3 120 ka apparent inconsistency: # TPO2 has a much higher value of refreeze-ablation than the other two # in Fig 3, and so should have significantly higher ice volume or # at least rate of ice growth in Fig 2, which it doesn't. # This logic doesn't necessarily hold if there is a large difference in surface # elevations and therefore total accumulation rates. But if the latter is the # case, then it would be much elucidating to have Fig3 show results for the same # ice surface so that there is no confounding by different accumulation rates. # Furthermore, in this latter case, it's unclear how FST09 originally gained # more ice area given that the relative refreeze and ablation values in fig3 120ka.

Please add a plot, table, or just text summary (depending on actual results) for the results of the application of the various models using present-day (PD) observed (eg reanalysis) climate and topography. Given the critical impact of FST09 altitudinal sensitivity of σ , I'm wondering whether it would lead to present-day accumulation that

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is contrary to observations. And please do the same for FST09 with PD climate from CLIMBER and with PD topography. This will help assess whether the current results are heavily biased by biases in CLIMBER.

minor comments

#abstracts and conclusions: it would really help the quick reader if the actual impacts # of varying the 3 components (DDFs, sigma, refreezing) were explicitly quantified # say using the FST09 as a base case

I take your most important conclusion as that given the # sensitivities, we really need a high res ISM/RCM/energy balance snow # model study to generate a better constrained PDD/refreezing surface # mass-balance model. Would be worth adding this to the abstract.

p4901 In this formulation, the total amount of positive degree-days represents the sum over one year of all probabilities for having positive temperatures. Therefore, this number can be considered as a melt potential and is expressed as a normal distribution given by: # incorrect as stated. PDD for a single day is the expectation of truncated positive # temperatures, generally based on a normal distribution.

equation #1 is incorrect (missing factor T in integrand)

eq 6 line 2: insert "-" in front of "d" (you've copied the typo from Tarasov and Peltier, 2002) # also, what value did you set "d" equal to?

490815 module is designed to only -> model is designed to only

490913-4 Our objective here is, as far as possible, to avoid the use numerous parameterizations that are not well constrained against reliable data. # assuming you are talking about the climate model, I find this # statement problematic. If you are avoiding parameterizations, then # you are not representing the associated processes. How does this # improve your analysis?

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repeated error: floatation -> flotation

4909 Ice velocity is therefore determined with the same set of equations used for ice streams but with a basal drag coefficient set to zero. # are any pinning points assumed following Pollard's approach? If so, this # does introduce some drag

4910 the free atmospheric lapse rate. #lapse rate from model or assumed? if latter, state value

#figure 1: colour scheme is confusing as bathymetric colours overlap with dry land elevations # Also, add a clear ice margin outline (hard to discern for the 116ka plots).

491213 Moreover, despite -> Except for # avoid the overstated spin

4912 simulated spatial distributions of LGM ice sheets favorably compare with available reconstructions (Peltier, 2004; Lambeck et al., 2006). # Should add Tarasov et al, EPSL 2012 for North America (which I submit is much # more highly constrained than the cited reconstructions given that only Tarasov # et al has uncertainty estimates and glaciological self-consistency). # Also, by spatial distributions, do you just mean areal coverage or actual # ice thickness distributions? If the latter (which is how it comes across), # then "favorably compare" is either a fluffly meaningless statement or an overstatement.

4909 It also simulates the dynamics of ice shelves and fast ice flow occurring in ice stream zones. The width of ice streams being not greater than a few kilometers, individual ice streams are not explicitly resolved since the model resolution is 40km×40km. Instead, the large-scale 15 characteristics of fast flowing regions are represented with the shallow-shelf approximation (MacAyeal, 1989) using criteria based upon effective pressure (i.e. balance between ice and water pressure) and hydraulic load. 4912 Moreover, in the present version of GRISLI, the impact of water-saturated sediments on basal sliding that favour the ice retreat has not been taken into account. These missing mechanisms may explain why the last glacial termination is not simulated sat-

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isfactorily in our experiments# I'm surprised that there is little evidence of LGM ice streams, # I'm surprised that there is little evidence of LGM ice streams, # especially the Hudson Strait ice stream in figs 1 and 4. From past # experience, this will definitely affect deglaciation.

4912 to which extent -> extent to which

4915 FST09 and TP02 ice volumes follow almost identical evolutions with ablation/refreezing values ranging from $2.9/1.0 \times 10^{11}$ m³ yr⁻¹ (FST09) to $4.8/5.1 \times 10^{11}$ m³ yr⁻¹ (TP02). In the TP02 experiment, refreezing counterbalances ablation, while in the FST09 one, ice volume mainly results from the expansion of the ice as shown by the evolution of the ice-covered area (Fig. 2). # took me a while to understand the 2nd sentence. Might be clearer if you stated something like: # "Unlike that of TP02, the FST09 case has an excess of ablation relative to refreezing # but this is offset by enhanced accumulation from expanded ice area", # if not rephrasing, then at least: expansion -> horizontal expansion

4918 full coupled -> fully coupled

Fig 2. Why does TPO2 start with larger ice area than the other runs at 120 ka?

Fig 3 caption: by "net ablation", do you mean total ablation? I would interpret "net ablation"= ablation - refreeze.

fig 5, colour scheme mixup again. Bathymetry colour need to be distinct from those in colour legend (or just white).

fig 6 caption needs to better explain what each of the runs are (pain to jump back and forth to tables)

fig 1, 4, and 5: I take it that the black contours are surface elevation. If so, please state so in the captions. If not, then please replace with surface elevation contours.

Interactive comment on The Cryosphere Discuss., 6, 4897, 2012.

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