Replies to Interactive comments on "Influence of ablation-related processes in the built-up of simulated Northern Hemisphere ice sheets during the last glacial cycle" from Lev Tarasov (Referee 1)

by S. Charbit et al.

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 modeling 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 submonthly temperature variations (ie via sigma in the submission). It is the first study, to my knowledge, to consider these issues over a whole glacial cycle. It also seems to indicate 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

Comment 1: *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.*

Reply to Comment 1: As precised by Lev Tarasov, the refreezing model proposed by Janssens and Huybrechts (2000) (hereafter JH00) is similar to the TP02 refreezing scheme except that in this latter study the thickness of the thermally active layer (d) is set to 1 meter, while in the former one it is considered as variable and equivalent to the annual snow accumulation.

During the preparation of the revised manuscript, we scrupulously checked all the steps of our ablation / refreezing calculations. This has been done to verify that was no apparent error in our study and to try to identify the potential sources of discrepancies between our results and those of Reijmer et al. (2012).

We first performed additional experiments by running the GRISLI model with the JH00 refreezing scheme (with d= snow accumulation). The experimental setup of these new simulations is similar to that of the GRISLI equilibrium simulations presented in section 4.2 (section 4.2.2 in the revised manuscript). These experiments have been carried out for the three previous periods (120, 112 and 21 ka), but also for present-day conditions (with atmospheric CO2 concentration set to 360 and 390 ppm) since the period under study in Reijmer et al. (2012) spans from 1958 to 2008. In each case, we examined the amount of simulated refreezing for both the Greenland ice sheet (120, 112, 21 ka and present-day) and for the Northern hemisphere (NH; past periods only). It appears that the relative difference between the different amounts of refreezing predicted by each model is critically dependent on the climatic forcing. As an example, at 120 ka, RH91 and JH00 refreezings differ by 36 % for the Northern Hemisphere, and by 47 % for the Greenland ice sheet (GIS). Under present-day conditions, these differences decrease to 11 % for GIS. The differences between RH91 and TP02 refreezings are larger: 44 % and 52 % for NH and GIS respectively at 120 ka and 30 % under present-day conditions (GIS only). As expected, this confirms that the relative differences between simulated amounts of refreezing clearly depend on the climate forcing and to a lesser extent on the geographical location (and topography). By the way, the time series (Figure 6) in Reijmer et al. (2012) shows that the RH91-JH00 refreezing is larger when both models are forced by the outputs from MAR (rather than by RACMO2). However, in our study, the JH00 model always predicts more refreezing than RH91 in contradiction with Reijmer et al. (2012). Nevertheless, it is worth noting that these reported amounts come from GRISLI equilibrium simulations and are not computed for the same ice-sheet surface. As outlined by Lev Tarasov, this makes difficult a direct comparison (see Comment 2).

Therefore, we have also computed the amounts of ablation and refreezing for the same ice-sheet surface (same extent and altitude: see reply to Comment 2). These new calculations lead to the following results: JH00 and TP02 only differ by a few per cents (suggesting that, at least in our study, the difference in the thickness of the thermally active layer is not as critical as suggested by Reijmer et al.) and still predict larger refreezing than RH91.

As a consequence, we did not manage to find a clear explanation of the source of differences between results from Reijmer et al. (2012) and ours. As a result, we simply added a comment in the revised manuscript specifying the main points of the conclusion drawn from the investigation obtained above (see section 4.2.2).

Comment 2: *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.*

Reply to Comment 2: As confirmed by this remark, the conclusions coming from the analysis of Figures 2 and 3 were not clear enough. The amounts of ablation and refreezing predicted by each PDD model did not correspond to the same ice-sheet surface. In the original version of the paper bar plots displayed in Figure 3 were related to ablation / refreezing amounts obtained after 1000 model years (out of 50 000). Our objective was to compare these different amounts at a given time when the ice sheets were not so much different from the initial state. Since, the simulated geometry of ice sheets was different from one PDD model to the other, even after 1000 model years, this approach was not appropriate, and we were obliged to account for the difference of ice-covered areas. We acknowledge that this made confusing the discussion.

Therefore, as explained in detail in the new section 4.2.2, we computed off-line the amounts of ablation and refreezings for the same ice-sheet surface. The new results are displayed in Figure 3. Differences between each PDD model are more pronounced than previously, but these results are now consistent with those displayed in Figure 2. Moreover, the right panels in the previous Figure 2 have been removed, since they represented a zoom of the first 1000 model years (this period is no longer considered in the revised presentation of the results).

Comment 3: *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 sigma, I'm wondering whether it would lead to present-day accumulation that 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.*

Reply to Comment 3: As suggested, we made additional GRISLI simulations with the three PDD models forced by present-day conditions given by CLIMBER outputs and by the climatic variables coming from the ERA-Interim database. The simulations are listed in Table2a and are explained and discussed in a new section 4.2.1. We also added a plot to illustrate the simulated ice volumes and ice-covered areas obtained from the CLIMBER and the ERA-Interim climatic forcing.

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

For clarity reasons, we choose not to quantify the impacts of the different parameters in the abstract. It appears difficult to summarize in a concise manner all the results. However, this quantification now appears in the conclusions and in some places of section 5.1

 # 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.

We agree with the reviewer. This conclusion has been added in 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.

We agree that the PDD method was not properly explained. The first paragraph of section 2.1 has been changed accordingly.

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

Corrected

 # 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?

Corrected. This error only appeared in the manuscript (and fortunately not in the code).

4908l5 module is designed to only -> model is designed to only

Corrected

4909l3-4 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 parametrizations, then # you are not representing the associated processes. How does this # improve your analysis?

We fully agree with Lev Tarasov and we acknowledge that parameterizations allow the representation of processes that cannot be described explicitly. Such approaches are particularly justified and widely used in low- or intermediate- complexity climate models where many processes are not represented. In the present study, we did not choose to remove the parameterization of the impact of dust deposition (and this constitutes a difference with the previous study of Bonelli et al. (2009) based on earlier versions of climate and ice-sheet models) to improve our analysis. Our objective was rather to make easier the interpretation of our results. Actually, the impact of dust deposition is likely to be different at any phase of the last glacial cycle. Thus, it could have been hard to discriminate the dust effect with the effect of each PDD parameter. In the revised version of the manuscript, we replaced the previous problematic statement with these latter remarks.

repeated error: floatation -> flotation

Corrected

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

Indeed, we omitted to discuss the case of pinning points where basal drags (20 times lower than in ice streams regions) are introduced; this has been mentioned in the revised manuscript.

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

The lapse rate is computed by the model. This precision is now added.

#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).

Color scales,surface iso-contours and contours of the continents have been changed to make the figures clearer

4912l3 Moreover, despite -> Except for # avoid the overstated spin

Corrected

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.

We added the reference suggested by Lev Tarasov. Moreover, “spatial distributions” mean “areal covereage”. This has been specified in the revised manuscript.

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 satisfactorily 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.

The sediments are not implemented in the version of GRISLI used in the present study. As a result, fast ice flow is not very efficient. This is why there is so little evidence of LGM ice streams. Plotting the ice flow velocities reveals a large ice stream in the Northwestern Canadian Territories, but the ice stream in the Hudson Bay is almost absent. However, in a later work we have implemented the sediments in GRISLI. This considerably improves the representation of fast ice flow and, as expected, favours the deglaciation.

4912 to which extent -> extent to which

Corrected

4915 FST09 and TP02 ice volumes follow almost identical evolutions with ablation/refreezing values ranging from 2.9/1.0_1011 m3 yr□1 (FST09) to 4.8/5.1_1011 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

Corrected

4918 full coupled -> fully coupled

Corrected

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.

All ice-covered areas start from the same initial state. The confusion stems from the fact that there is an adjustement during the very first time steps of the simulation.
Moreover “net ablation” takes into account the refreezing contribution. Nevertheless, we did not use this terminology in the revised manuscript.

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

Corrected; Figure 5 has become Fig. 6

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

Corrected. Figure 6 has become Figure 7

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.

Figure captions have been clarified