Response to reviewer #2 of “A new sub-grid surface mass balance and flux model for continental-scale ice sheet modelling: validation and last glacial cycle” by K. Le Morzadec et al.

July 28, 2015

The paper is about a revised version of the hypsometric approach by Marshall and Clarke (1999), which is thought to improve representation of topography in coarse resolution ice sheet models. For the longer time-scales of glacial cycles, modellers rely on coarse spatial resolution due to limitation in computational resources. Including hypsometric curves can better resolve accumulation of ice in higher mountainous region as well as melting of ice in lower valleys and, at the same time, preserving coarse resolution. A sub-grid scale (SG) model operating on these hypsometric levels is coupled with a coarse resolution ice sheet model in shallow ice approximation. While Marshall and Clarke used synthetic curves, the present paper uses a digital elevation model to create hypsometric curves for representative regions over North America. For parameterization of flux between hypsometric levels besides effective lengths, a slope parameter is used. Different parameterizations for the sub-grid scale flux are tested. The SG model is validated using a higher order ice sheet model of the Blatter-Pattyn type, although agreement of results between both of the models appears rather poor. Finally, the importance of the SG model for simulations of the last glacial cycle with the GSM (formerly MUNGSM) model is demonstrated.

"Validation" in the title was a problematic choice of word. We meant validation in the sense of testing and quantifying misfits not in the simplistic sense of proving the SG model to be a valid replacement for a high resolution model. We modified the word validation by testing in the title.

1 Major Points

1. In general, the description of the hypsometric parameterization needs more explanation, including more formula, a schematic figure and a flow diagram. Unfortunately, is the most known procedure – the PDD scheme – explained at great length, what is not necessary, because citation of previous work would have been sufficient. However, the hypsometric scheme, particularity your novelties, are not explain sufficiently. This is ever more important, because you do not make the code public.

As requested in the manuscript preparation guidelines, a new section called "Code availability" has been added: "The sub-grid code is available upon request from the first two authors". As detailed below and in the response to the other reviewer comments, we have added 2 figures and revised the text to better explain the hypsometric parametrization.

2. Page 3042, lines 10-11: “Then, the size of these bins is updated to avoid empty levels.” Is the size of the bins different for each region?
Yes, since as stated: "First, the region is divided into N bins of equal altitude range". We also clarify how the empty levels are adjusted: "Then, to avoid empty bins, the surface elevation range of each empty bin is expanded (consequently decreasing the elevation ranges of the higher and lower adjacent bins) until these three consecutive bins represent approximately the same surface area."

3. Section 2.1.1: It is unclear how you determine the effective length L and the slope. You wrote, “Specifically, for each hypsometric level, we compute the cube root of the mean of the cube of the magnitude of the slopes.” Which quantity do you compute? Could you write down a formula for this? How is slopek in Eq. (1) defined? Is this the surface slope length?
The quantity we compute is the hypsometric slope (slopek updated to S). This part have been clarified without the addition of a formula:"Specifically, for each hypsometric bin we compute the slope, S_k^0, as the cube root of the mean of the cube of the magnitude of the slopes from the GEBCO data."

You further wrote: “The effective width of each hypsometric level is set to the number of grid cells, multiplied by the spatial resolution, that are in contact with adjacent lower hypsometric levels grid cells.” What is the expression for the effective width? Is the effective width the same as the effective length? Could you please check the entire sections for error and rewrite it using some more formula in order to make the section more understandable. Could you please illustrate with a schematic figure the involved quantities?
Section 2.1.1 has been restructured to clarify that the effective width and length are different: "The flow line model requires an effective width, W, for the representation of flux between hypsometric bins. W of each hypsometric bin is set to the total contact length of the SG cells assigned to the bin with adjacent lower hypsometric bins grid cells as detailed in Fig.1."

4. Section 2.1.3 (Surface mass balance): This section can be shorted substantially as PDD parameterization is well know, described elsewhere and is not the topic of the paper.
This description has been significantly shortened to: "We use the positive degree day method described in Tarasov and Peltier (1999) to compute accumulation and ablation from monthly mean temperature and precipitation. A constant environmental lapse rate adjusts the temperature to the ice surface elevation. A parameterization of the elevation-desertification effect (Budd and Smith, 1981) reduces the precipitation by a factor of two for every kilometre increase in elevation. Snow is melted first and the remaining positive degree days are used to melt ice with allowance for the formation of superimposed ice. The supplement includes a more detailed description of the surface mass balance module.
The GSM and ISSM compute the surface mass balance using the same PDD method." The detailed description has been added in the supplement.

5. Sections 2.1.2 (Ice velocity) and 2.1.4 (Ice thickness evolution): Obviously, you use the isothermal shallow ice approximation (of order zero) to yield the ice velocity in the SG model. What is the rational to use the shallow ice approximation in the space of hypsometry, as the shallow ice approximation is formulated on the Earth’s surface? The scales and gradients on the Earth’s surface are quite different from those in the hypsometric space. Thus, immediately the question appears what are x (and

δ x) in Eq. (3), (6) and (7)? The coordinate x cannot be a length on the Earth's surface, because in your hypsometric model there is only sub-grid area, which is not a length. Marshall and Clarke (1999) were aware about this fact, see their Eqs. (15) and (16) wherein they clearly formulate flux in the hypsometric space. To be concrete: How do your Lk and slopek from Section 2.1.1 relate to your formulas in Sections 2.1.2 and 2.1.4? In particular, how does your flux – in your case possibly diffusivity – relate to your Lk and slopek ? The entire Sections 2.1.2 and 2.1.4 have to be completely revised incorporating my concerns and questions.

The scales and gradients on the Earth's surface are indeed quite different from those in the hypsometric space. That is why we define effective length and width that are a parameterization of the Earth's surface. $\frac{\delta h_d}{\delta x}$ in Eq.3 has been replaced by S, the surface slope. We also added after eq.5 that: "Δx is the effective length L and Δy is the effective width W defined in Sec.2.1.1."
The notations have been clarified so that $slopek$ (now S) from section 2.1.1 is present in Sections 2.1.3 formulas. L_k is used to updated S at every times step. The flux is proportional to S as seen in eq.4.
Sections 2.1.2 (Ice velocity) and 2.1.4 have been merged together and the velocity equation has been removed as it is already defined in the effective diffusivity term.

6. Page 3048, lines 19-21: "The GSM has been subject to a Bayesian calibration against a large set of paleo constraints for the deglaciation of North America, as detailed in Tarasov et al. (2012). We use a high-scoring sub-ensemble of 600 runs from this calibration." These sentences rather belong to Section 4.

The beginning of section 4 has been modified to included these sentences: "We present results of simulations over the last glacial cycle. The 39 "ensemble parameters" of the GSM (attempting to capture the largest uncertainties in climate forcing, ice calving, and ice dynamics) have been subject to a Bayesian calibration against a large set of paleo constraints for the deglaciation of North America, as detailed in Tarasov et al. (2012). We use a high-scoring sub-ensemble of 600 parameter vectors from this calibration to compare the GSM behaviour when the SG model is turned on and off. The primary supplement of Tarasov et al. (2012) includes a tabular description of the 39 ensemble parameters as well as input data sets. For the purposes of clarity and computational cost, we examined model sensitivity to different coupling and flux parameters using five parameter vectors (of the 600 members ensemble) that gave some of the best fits to the calibration constraints. As these five parameter vectors display similar behaviour we present sensitivity results using the parameter vectors for the two runs described in detail in Tarasov et al. (2012) (identified in that paper as runs nn9894 and nn9927)."

Do you use all 600 runs in section 4.1?

Yes, we use all 600 runs in section 4.1 unless specified otherwise for single examples. This is clarified with the modification done in comment 10. about Section 4.
"An ensemble of simulations" has also been replace by "The ensemble of simulations".

Corresponds the "sub-ensemble" with the five best fits?

The five best fits are five parameter vectors from the 600 parameter vectors ensemble. It is clarified in the beginning of section 4: "For the purposes of clarity and computational cost, we examined model sensitivity to different coupling and flux parameters using five parameter vectors (of the 600 members ensemble) that gave some of the best fits to the calibration constraints."

7. Section 3.1 (Comparison with ISSM): Could you clarify: Do you couple the SG model to the ISSM model? The SG model runs on one 30 km × 60 km rectangle. This rectangle is discretised in a

resolution of 1 km × 1 km for the ISSM model. Is that correct?
The SG model is not coupled to ISSM. The results from one model are compared to the results of the other one. ISSM is run over the 30 km × 60 km region. The SG model is run over the hypsometric curve generated using the 1 km × 1 km resolution DEM data for the same region.

Further, you write that no sliding is allowed is the ISSM model. Now, I lose understanding what you are modelling with ISSM. In mountainous regions, I would expect existence of glaciers that rapidly slide. Switching off sliding makes no sense then. Could you sharped/explain your motivation for using ISSM and switching off sliding, what implies that mainly shear stress plays a role.
We clarify in the text the reason for not including sliding in the ISSM and SG models at this stage of the project by adding in section 3.1: "To isolate the impact of using the SIA to represent fluxes in a mountainous region containing steep slopes in the hypsometric parameterization, our current experiments have no basal sliding. As glaciers can experience surging (via significant sliding) in this type of region, the next stage of this project will include sliding."

8. Again Section3.1: Why do you use only 2 kyr run time for ISSM? The application (a glacial cycle) which you are targeting operates on longer time scales.
If the ISSM could be run on glacial cycle time scales, that model would directly be coupled to the GSM for regions with rough topography. Unfortunately, the run time of the ISSM is too long for such an application. The ISSM took 2 to 5 hours using 10 cpus to generate the 2000 model years for each of the 30 by 60 km regions.

9. Section 3.2 (Test of alternative parameterizations): Why do you present to the reader parameterizations, which did not approve anything. These parameterizations would not help a user of your model.
Documentation of both what works and what doesn't work is of value to modellers to avoid future repetition of exploring dead ends.

10. Section 4: This section is incomplete, unclear and not too well organized. For example, it is unclear, whether you discus all 600 runs or only the 5 best fits in Section 4.1. Or do you discus the 5 best fits in the entire Section 4? For example, do you use all 600 runs or only the 5 best fits runs to determine the standard deviation shown in Fig. 8? In general, you should add a more detailed motivation, description and discussion of your experiments to Section 4. Partly, you can use sentences from your conclusions for Section 4 and erase these sentences from the conclusion section.
The structure of Sec.4 is now clarified in the introduction of this section: "We present results of simulations over the last glacial cycle. We compare the GSM behaviour when the SG model is turned on and off for the 600 members ensemble of simulations and for one of the best runs of this ensemble. The primary supplement of Tarasov et al.(2012) includes a tabular description of the 39 ensemble parameters as well as input data sets. We also examine model sensitivity to different coupling and flux parameters using five of the best fit to calibration constraints parameter vectors of the 600 members ensemble. Tarasov et al.(2012) presents in detail two of these run (identified in that paper as runs nn9894 and nn9927). For ease of interpretation, the ice volumes are presented as eustatic sea level (ESL) equivalent."
We also clarify in Sec.4.1 when we refer only to one parameter vector: "Fig.12 shows an example, for one of the parameter vectors of the ensemble of simulations, where. . . "

I recommend adding a new subsection to the beginning of Section 4, which includes a summary of the

model setup for the 600 ensemble runs (climate forcing, variated parameters, constraints) and which clearly says which subset of these runs you use further on in section 4.
We do not think that including a detailed summary of the model setup is appropriate given that it is detailed in the cited reference (what is the point of repeating the same tables from the cited reference?). We do now clarify that: "The primary supplement of Tarasov et al. (2012) includes a tabular description of the 39 ensemble parameters as well as input data sets."
We also clarify that the runs (nn9894 and nn9927) presented for sensitivity experiments are described in detail in the same paper : "Tarasov et al. (2012) presents in detail two of these run (identified in that paper as runs nn9894 and nn9927)."

The insets of Figs. 11, 12 and 13 indicate several sensitivity tests. However, in the main text belonging these figures you leave the reader somewhat alone and miss to explain sufficiently these sensitivity tests.
Modifications concerning fig.13 discussion: Description of the impact of turning on or off the fluxes between coarse grid cells when the SG model is activated (Fig.13, previously 11) has been modified to: "To better understand the range of responses to CG ice flow between grid cells that have SG activated, three case scenarios can be considered. Case 1: ice flows out of the lowest SG bins located above the ELA into the lowest SG bins located above the ELA of another CG cell. There is limited impact of not allowing ice to flow out of the CG cell as in both cases ice accumulates. Case 2: ice flows out of the lowest SG bins located above the ELA into the lowest SG bins located below the ELA of another CG cell. In that case, turning off the fluxes between CG cells tends to reduce the total melt. Case 3: ice flows out of the lowest SG bins located below the ELA into the lowest SG bins located below the ELA of another CG cell. Ice flowing into lower SG bins generates higher melting rates so permitting fluxes between CG cells will in this case tend to increase ice mass loss. In cases 2 and 3, the combination of ice flowing below the ELA from the adjacent CG cell and from the bins above the ELA can raise the surface elevation of lower bins above the ELA and reduce the melt. Depending on the proportion of each of these cases, not allowing ice fluxes out of coarse grid cells with SG activated generates higher or lower ice volumes (Fig.13). 50 ka is an example of a 60% increase of the total ice volume when the fluxes out of coarse grid cells (with SG activated) are not allowed. As a counterpose, 35 ka presents a case where turning off the fluxes out of (SG activated) coarse grid cells decreases the total ice volume."

Modifications about fig.15 discussion: "Fig.15 shows the results of the glacial cycle simulation when the SG model is turned off and when the minimum altitude variation SG activation threshold is set to 50, 150, 300 and 500 m. A non-linear dependence on the threshold can be observed. At 50 ka, for example, setting the threshold to 50 m generates the lowest total ice volume while a threshold of 150 m lead to the highest ice volume. The difference between these two runs is 34.5 mESL at 50 ka.Threshold of 300 and 500 m generate intermediate total ice volumes. Moreover, simulations using different parameter vectors (not shown) result in different behaviours. No conclusion could be drawn about the optimal threshold."
We are not sure what is meant by "leave the reader somewhat alone and miss to explain sufficiently these sensitivity tests". If it means that more description of the different setups for each test is needed in the main text, we believe the tests/comparisons are explained in adequate detail in the figure captions which we find to be more useful as a reader. If the intent is that more discussion of the implications of the results or of the results in need, then we do not see what. We feel we've conveyed the main points we wanted to from each plot.

Further on, you refer to Fig. S8 in the supplements. I would regard the comparison with previous

work as important enough to show the figure in the main paper.
Fig.S8 has been included in the main paper (as Fig.14).

11. Again Section 4: I find it interesting that there is such a strong sensitivity of ice volume to the SG parameters at about 60 to 50 kyr BP. Could you add further discussion and explanation about this?
We looked at glacial initiation prior to 50ka with and without the SG model and we could not identify a reason for that strong sensitivity of ice volume around 50 ka.
This has been clarified in the revised manuscript: "Looking at the simulation used in Fig.12, the differences in ice field distribution when the SG model is turned on and off at 60 ka are minimal. We could not identify a reason for the strong sensitivity of ice volume around 50 ka other than the inherent non-linearity of the GSM."

12. Conclusion: The conclusions are somewhat lengthy, in particular, when you address the glacial cycle simulations. Please, shorten and revise the conclusions.
The conclusions have been shortened.

2 Minor Points

13. Page 3038, lines 13-14: How do you know? Have you tried all possible parameterizations?
That sentence was changed to: "Results show that none of the alternative parameterizations explored were able to adequately capture SG surface mass balance and flux processes."

15. Page 3042, Eq. (2): What denote hd , k ? Please, explain that here either.
Then sentence before Eq. 1 was changed to: "At any time step, t, the surface slopes, S_k^t, for SG bin k, from 1 (highest) to N (lowest), are computed from the surface elevation $h_{d,k}^t$ and an effective length L_k:"

14. Page 3042, Eq. (1): What denote hb , k ? Please, explain that here.
h_b is the basal elevation. The k is the subscript representing the bins as explain in the previous comment.

16. Page 3042, Eq. (1) and Eq. (2) Could you eventually use for slopek a decent mathematical symbol sk ?
The $slopek$ symbol has been replaced by S.

17. Page 3048, line 24: “synoptic cell” I think this terminology is misleading, because the issues presented in this paper are not related with synoptic. Could you please use the terminology “coarse grid cell” instead here and for the other appearance of “synoptic cell” in the paper?
"Synoptic grid" has been changed to "coarse grid".

18. Section 3.1: Possibly, you can say a bit more explicit that your SG model is applied the 30 km × 60 km region.
The beginning of Sec.3.1 was changed to: "We compare 2 kyr ISSM and SG simulations, applying constant sea level temperature and precipitation over an inclined bed and 21 different test regions in the Canadian Rockies. These regions, for both the ISSM and SG simulations, have a dimension of 30 km by 60 km and we use a DEM of 1 km resolution."

19. Page 3056, lines 23, “setting the surface elevation”: do you mean “setting the surface elevation of the coarse resolution grid”?
Yes, this has been clarified. “setting the surface elevation” was replaced by "setting the CG surface elevation".

20. Page 3056, lines 26-27, “using the maximum of the two former methods”: What is the maximum of a method? To which physical quantity you applies the maximum? Please, be more precise.
It has been clarified that the physical quantity is the surface elevation. "using the maximum surface elevation generated by the two former methods)".

21. Page 3056, lines 25, “SC, method”: the comma should be erased.
SC was placed between parentheses.

22. Page 3056, lines 27, “MC, method”: the comma should be erased.
MC was placed between parentheses.

23. Page 3058, lines 3-26: Could you check what you wish to include in the itemized list and what not. Does the paragraph starting at line 20 belong to the itemized list too?
Yes it does belong to the itemized list and has been fixed.

24. Page 3059, lines 16-17, “…the installation of ISSM and helped including the new module in ISSM.”: Which module do you mean? As far as I understand the idea of Section 3.1, the ISSM model runs without the SG model and is used to assess the performance of the SG model.
Page 3047, line 3-5: In that short section describing ISSM it is stated: "For this study, a new surface mass balance module identical to the one present in the sub-grid model, and detailed in Sec. 2.1.2, has been incorporated into ISSM." This has been clarified in the Author contribution section. "… supported ISSM installation and helped build a new surface mass balance module for the ISSM."