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The Cryosphere
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3, S124–S138, 2009

Interactive
Comment

Interactive comment on "The Gregoriev Ice Cap evolution according to the 2-D ice flowline model for various climatic scenarios in the future" *by* Y. V. Konovalov and O. V. Nagornov

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Response to the referees

We would like to acknowledge the reviewers for the interest to the paper and detailed review of our paper. Below we have cited remarks of the referees and given our answers.

G. Leysinger Vieli: *"...The paper is too detailed in the model description, as the model has been published before,...The introduction wants to persuade us that the chosen model (full-system) is the one model to use - but I wonder: is it that clear? As discussed below the mass-balance might be of much larger influence than the made model assumptions. Despite of the partly slippery bed a shallow ice approximation*

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Interactive Discussion
Discussion Paper

model (SIA) might be sufficient as length changes for spatially uniform perturbations in mass-balance forcing are well reproduced with the SIA model. The goal of this paper, as I understand, is not to compare any models nor to develop a new model but to use an existing model based on Pattyn (2000) and apply it on the Gregoriev Ice Cap to investigate its evolution for different climate scenarios and investigate an inverse model approach to reconstruct the air temperature from observed length changes. Nevertheless a major part of the paper is taken up by the model description, which is very detailed (approximately 8 pages out of 17 pages text). As this is not a new model such a detailed model discussion is not needed. . . I also believe that such an exact model is not needed for the goal of the study as length changes for a shift in ELA are essentially model-independent (e.g. Leysinger Vieli and Gudmundsson, 2004). Even more so as the inverse model used for air temperature is a very simple model."

Authors: We applied the SIA model but the model is not valid for the Gregoriev Ice Cap. Depth averaged horizontal velocities in the SIA model differ from the ones in the full model at about 2..5 times along the flowline. The account of longitudinal stress components is important for this glacier (Nagornov et. al., 2006).

The basic equations (mathematical statement of the problem) of the 2D model were taken from (Pattyn, 2000) and (MacAyeal, 1989, 1992), i.e. Eq. (1), Eq. (5), Eq. (9), Eq (12). But the ice surface boundary condition approximation (Eq. 7) was developed by authors of this paper and has been published in detail for the first time. Why is it needed? We face with the problem of the numerical solution instability which stems from the dynamic boundary conditions approximation in the finite-difference models. The dynamic boundary conditions are the first order differential equations. If the dynamic boundary conditions prevail over the kinematic conditions, then the approximation of the first order differential equations along the boundary of arbitrary domain can lead to the numerical solution instability. To avoid the instability we develop the approach which consists in the replacement the first order differential equations with the second order ones. For the replacement we use the main equation which is valid inside the flowline (ice) domain. In principle, such approach is known but we didn't set the additional (fictitious) nodes.

We also successfully applied the approach in the ice stream model (MacAyeal, 1989).

The approach is more effective for a problems such us a crevasses appearance due to seasonal changes of ice viscosity and stresses in a subsurface layer (Nagornov et. al., 2006).

In this study the dynamic boundary conditions don't prevail over the kinematic conditions along the full domain boundary. And first order approximation by the Eq. (6) can be applied successfully too. The "complex" approach provides only second order of the approximation and it allows to use sparse grids in vertical direction. But, for example, in the case of ice and sea water interaction boundary the approach provides the solution stability in the full 2D flowline model.

Finally, we can say that the approach provides numerical stability of the solutions in full 2D (3D) finite-difference models in some cases when the solutions unstable for the basal approach (such as by Eq. (6) in the 2D model). So, the approximation (7) expands the area of 2D (3D) full finite-difference models applications. Furthermore, low spread of results for the full Stokes models (Pattyn et.al., 2008) proves that the approach provides second order (order of vanishing or smallness) of the boundary condition numerical approximation (taking into account that ξ_x' and ξ_{xx}'' are small values). The approximation (7) needs for the detailed description and it is one of the goals of the study.

We also have described in detail why did we introduce the basal sliding and how did we realize it. The full (common) linear friction law (10) in 2D flowline model have been written without any references because we didn't see the Eq. (10), for example, in (Pattyn, 2000). If anybody used the friction law in the form of Eq. (10) and published it, of course, we'll apologise and give the reference... And at this point "The Cryosphere Discuss." has obvious advantage.

We can't say what described model is a new, because, the basic equations were formulated in (Pattyn, 2000). Our model is the developed model and some details need for the careful description.

The model can be simplified and we have written about it (Lines 9-15 at the page 91 and Fig. 14). But we wanted to test the complex model. Furthermore, the authors are not glaciologists per se. Their field of experience is mathematical modeling in different physical applications, and they are not interested in repetition the existing models without any changes (modifications) of the models. We suppose that the model can be used in other applications – the quasi-steady flow of a viscous fluid ($Re >> 1$) – when the "complex" way of the approximation can be more significant.

The approach is complex, but the finite-elements method is complex too. On the other hand the finite-difference method allows to solve a problem at a grids with sufficiently different steps in vertical and horizontal directions. Usually the ratio $\frac{\Delta z}{\Delta x}$ is a small in the ice flow models ($\frac{\Delta z}{\Delta x} << 1$).

In principle, the detailed description in the 4.1 and 4.2 can be missed in the final paper if they are presented in "The Cryosphere Discuss." (in the discussion version). The authors going to refer to the approach description in future studies.

Referee #1: "*It is never stated whether the model successfully passed the ISMIP-HOM tests. In that paper it is shown that another model by the same author (a Full Stokes 3D model) participated in experiment A and E. These experiments are without basal sliding. The presented flowline model in this submitted manuscript does however include basal sliding, but it is not stated whether other ISMIP-HOM tests were done.*"

Authors: The model successfully passed the ISMIP-HOM test in the experiment E without sliding.

G. Leysinger Vieli: "*Equation 5: Last term on right hand side different than in Pattyn. Is it correct?"*

TCD

3, S124–S138, 2009

Authors: Yes it's correct. We can write the deformational heating term for incompressible (!) substance as $\sigma'_{ik}\dot{\varepsilon}_{ik}$, where σ'_{ik} are stress deviator components and $\dot{\varepsilon}_{ik}$ are strain-rate tensor components (instead of $\sigma_{ik}\dot{\varepsilon}_{ik}$). Then we can rewrite it in terms of second invariants: $\sigma'_{ik}\dot{\varepsilon}_{ik} = 2\,\sigma'\,\dot{\varepsilon}$. Finally, taking into account the Glen law ($\dot{\varepsilon} = A\,(\sigma')^n$) we obtain $\sigma'_{ik}\dot{\varepsilon}_{ik} = 2A^{-\frac{1}{n}}\,\dot{\varepsilon}^{\,\frac{1+n}{n}}$. So, we avoid additional calculations of σ' for account of the deformational heating at each time step. We haven't written a new equation, but we have only rewritten the last term for incompressible ice (it is written in the common form in (Pattyn, 2000)). So, we believe that the reference is correct.

On the other hand, we haven't referenced to (Pattyn, 2000) for prognostic equation (3) because we have added the new term in the equation. It leads to a new negative effect (dissipation), but it provides the numerical stability.

G. Leysinger Vieli: *"Equation 1: second line - term on right hand side; should be a dependency on 'z' (e.g.$h_s - z$ or is$h_s = s - z$?)"*

Authors: Variables x and z are independent variables, so $\frac{\partial\,(h_s - z)}{\partial\,x} = \frac{\partial\,h_s}{\partial\,x}$. The h_s is ice surface elevation, the h_b is bottom elevation (are missed in the Table 1, thanks).

G. Leysinger Vieli: *"Lines 4-7: Clarify sentence; 'one mechanical equilibrium equation' makes no sense. Do you mean 'one dimensional'?..."*

Authors: No, we don't. The mechanical equilibrium equation in terms of stress deviator components is two dimensional in 2D model (one horizontal direction and vertical direction). But the initial system of diagnostic equations in 2D model includes two mechanical equilibrium equations (two dimensional equations: $\frac{\partial\,\sigma_{ik}}{\partial\,x_k} + \rho\,g_i = 0$). In principle we can consider the initial system of equations (continuity equation, mechanical equilibrium equations, relations between strain rates and deviatoric stresses). The advantages of the 2D flowline model (Pattyn, 2000) are (i) minimum equations (Eq. 2) and (ii) account of ice flow convergence/divergence. Only one equation from the basic equations of the 2D flowline model is one dimensional. It's prognostic equation (3) – one horizontal direction.

The index “i” at x in line 10 (page 80) should be deleted. Thanks.

G. Leysinger Vieli: *“Line 15: Why two integro-differential equations? Only the second line of Equation 2 is one. Equation 2: top line - why showing the same equation as shown in Equation 1 again? Do not rewrite the same equations again it happens throughout model description. . . ”*

Authors: We mean the system of integro-differential equations that we solved to obtain two unknown values u and w. First equation is differential equation (continuity equation without any changes), second equation is integro-differential equation (mechanical equilibrium equation after manipulations). So the system will be system of integro-differential equations. If we mean a system of equations, we should write all equations of the system to avoid uncertainty in mathematical statement of the problem. Similar comments we can give about the system (7).

We’ll insert the words “system of” in line 15 at page 80. Thanks.

The equations of the system (2) are mentioned in (Pattyn, 2000) in a words. So, the reference to (Pattyn, 2000) will be correct.

G. Leysinger Vieli: *“Is equation 14 used in equation 15? In this case the air temperature amplitude (ΔT_a) nor T_{a0} (reference air temperature?) is needed ($M_s(t) = M_{s0} + \Delta M_s \sin(2\pi t/t_p)$). Is T_a not time dependent? Subscripts not explained.”*

Authors: The substitution of T_a defined by Eq. (14) into Eq. (15) obviously leads to $M_s(t) = M_{s0} + \Delta M_s \sin(2\pi t/t_p)$. The Eq. (15) was applied also in the case of superposition of N harmonics with equal temperature amplitudes (Fig. 19). But the Eq. (15) requires a generalization in cases of non-equal amplitudes. More general form of the Eq. (15) can be written as $M_s(t) = M_{s0} + \sum_{i=1}^{N} \Delta M_{s\,i} \frac{T_{a\,i}}{\Delta T_{a\,i}}$, where $T_{a\,i}$ are harmonics of air temperature.

The T_{a0} is reference temperature. The reference mass balance M_{s0} corresponds to

TCD

3, S124–S138, 2009

Interactive Comment

the T_{a0}.

Referee #1: *"Eq.(3): The authors use a transport equation for the solution of continuity. Such an advection equation is generally used for fast glacier motion, such as ice streams or ice shelves. However, in case of glaciers it is more common to use a diffusivity equation, which avoids to add artificial viscosity to stabilize the solution."*

Authors: Both the common form ($div\left(\mu\vec{\nabla}H\right)$) or the specific form ($\nu\frac{\partial^2 H}{\partial x^2}$) of the diffusivity term negatively affect to the solution of the prognostic equation at the point of mass conservation. So, there are no any preferences in the choice of the diffusivity terms because they are absent in the prognostic equation per se. In the long-term prognostic experiments it's important to minimize losses of the ice mass which appear due to the dissipation. The results (Fig. 7) show that the problem is successfully decided in the model – two consistent minimum positions are at about the same levels.

Referee #1: *"A geothermal heat flux of 0 W/mˆ2 is extremely low, and hard to imagine plausible in mountainous areas. Even for central Asia, geothermal heating is of the order of 40-80 mW/mˆ2. It is not very clear why it was necessary to evolve the temperature field, as only sliding occurred near the front of the glacier, which is quite interesting, since the ice here is the thinnest."*

Authors: The annual air temperature (the ice surface temperature) decreases with elevation according to the Eq. (8). So, more cold ice flows downstream along the flowline. Moreover the ice surface temperature changes in time, although the temperature amplitudes are relatively small in the considered experiments. The boundary condition at the vertical segment of the flowline domain can be properly formulated only at the summit ($x = 0$), where heat transfer becomes one-dimensional. And we took the solution of the one-dimensional problem as the boundary condition at the vertical segment $x = 0$. We assume that taking into account the small temperature amplitudes the boundary (vertical segment) can be placed somewhere inside the flowline domain ($x > 0$). But the main part of the computing time is employed for the solution of the diagnostic system and the benefit will be inessential.

Unfortunately the full temperature profile down to the bottom wasn't obtained and it's difficult to determine the geothermal heat flux. But the ice temperature below the seasonal temperature perturbations is practically unchanged (Arkhipov et. al., 2004; Nagornov et. al., 2006). On the other hand the steady state temperature profiles have the visible trends at the same depths below depth of the seasonal temperature perturbations.

So, we can assume that the geothermal heat flux can be less than 0.04 W/mˆ2.

Referee #1: *"The basal sliding law used is a linear (viscous) one. However, basal sliding is supposed to follow a non-linear relationship as a function of basal shear stress. A proper sliding law (Weertman type for instance) should be implemented. Furthermore, it is not clear what the amount of basal sliding compared to the deformational velocity is. Since sliding is limited at the front, I suspect that only a limited amount of basal sliding is present."*

Authors: Taking into account relatively small flow velocity and ice thickness in comparing with the ones of the Shirase Glacier we suppose that the linear friction law is well-complied with the basal sliding. A generalization of the Eq. (10) can be written for a non-linear law. The generalization for the Weertman-type basal sliding is

$$\begin{cases} u_b + \frac{d\,h_b}{d\,x}\,w_b = \frac{A_b}{N} \times \left| -\frac{d\,h_b}{d\,x}\,\left(2\,\sigma'_{xx} + \sigma'_{yy}\right) + \sigma'_{xz}\left(1 - \left(\frac{d\,h_b}{d\,x}\right)^2\right)\right|^{n-1} \times \\ \quad \times \left(-\frac{d\,h_b}{d\,x}\,\left(2\,\sigma'_{xx} + \sigma'_{yy}\right) + \sigma'_{xz}\left(1 - \left(\frac{d\,h_b}{d\,x}\right)^2\right)\right) \times \left(1 - \left(\frac{d\,h_b}{dx}\right)^2\right)^{\frac{1}{2}-n}; \\ -u_b\,\frac{d\,h_b}{d\,x} + w_b = 0. \end{cases} \tag{10.1}$$

G. Leysinger Vieli: *". . . The introduction gives the needed overview with the exception that the reader doesn't know where the flow line is situated. The location of the glacier is mentioned in the text but the figures corresponding to the text are not clear enough* *(show position of flow-line and width of modelled glacier)... The reader does not know if the modelled glacier changes in width and if the glacier width is time dependent. For such a glacier the overall mass-balance is affected and the length extend for such a glacier will differ to a glacier with constant width..."*

Authors: We investigated the central part of the glacier (The Gregoriev Ice Cap) which covers one of the plane-tops of the Terskey Ala-Tau South slope. The flowline crosses the ice cap from the summit down to the ice front and lies at the tops of the transverse ice cap profiles. Considered ice cap width corresponds to the length extends of the transverse profiles. It increases from about 1.3 km at the summit to about 2.8 km at the front and so we accounted the widening of the ice cap.

We didn't take into account time dependence of the ice cap width. We think that the ice cap width variations with time can be properly obtained only by 3D ice cap modeling (taking into account 3D ice cap surface and bottom topography).

G. Leysinger Vieli: *"Checking for the enhancement factor and shifting the mass-balance parallel to the reference mass-balance are both fine, nevertheless this section has some problems: The authors are investigating the flow law rate factor but omit to discuss the effect of the chosen mass-balance distribution. The effect of the uncertainty in rate factor (which has been addressed here with the enhancement factor) and especially the uncertainty in the mass-balance is much larger than the uncertainties introduced by the model assumptions (full-system or SIA) (e.g. Leysinger Vieli and Gudmundsson, 2004). Therefore the discussion about how well the steady-states are reproducing the observed surface (p. 88, lines 16-25) should also include the fact that the modelled steady-states are highly dependent on the chosen mass-balance distribution and also on the (changing?) width of the glacier."*

Authors: The surface mass-balance measurements at the Gregoriev Ice Cap were carried out in 1987 and 1988 only (Mikhalenko, 1989). It is reasonable to take the average values as the reference mass-balance. The average values are close to the linear distribution. We suppose that the steady-state experiment shows that the glacier length and the ice thickness distribution are in good agreement with the experimental ones for the flow law rate factor from (Paterson, 1994) without any fitting multipliers (Fig. 4).

On the other hand, we can vary the reference mass-balance to improve the compliance between modeled and experimental data. At the mathematical point it means that we should solve the inverse problem, i.e. we should derive the steady mass-balance from the ice thickness distribution by means of minimization of the discrepancy between modeled and observed ice thickness distributions. But, it's well known that the inverse problems solutions are sensitive to the input data perturbations. As we understand the ice thickness measurements are not absolute and have errors too.

Furthermore, we shouldn't forget that, in fact, the ice flow is not 2D but it's 3D. The 2D flowline model is only the approximation of the realistic ice flow.

Finally, the glacier length is defined by overall mass-balance, i.e. by integral $\int_0^L b\,(M_s - M_b)\,dx$ and, if we change the mass-balance (at $x < 1.7\,\text{km}$) so as the integral will be practically unchanged, then such mass-balance variations can be neglected in the non-steady experiments because the minimum of the glacier length is about 2.2 km (Fig. 7).

The glacier width changes can be more important. But both the continuity equation and the prognostic equation depend on the derivative $\frac{\partial\,\ln(b)}{\partial\,x}$. So, if the width changes close to proportional ratio $b\,(x,t) \approx k_0\,(t)\;b_0\,(x)$ and flowline position is practically unchanged, then the changes will insignificantly influence to the glacier length and to the ice thickness distribution obtained by the 2D flowline modeling.

We underline that the ice cap width variations with time can be properly obtained only by 3D ice cap modeling. It will be interesting to compare the results of 2D flowline and 3D ice cap modeling and we'll plane to do it in the future manuscripts.

TCD

3, S124–S138, 2009

Interactive Comment

Thanks for the remark.

Referee #1: "*Experiments are done for m=0.15 and m=0.3. Both values are rather low, which means that the ice has a very low viscosity or is eventually colder than modeled. Could it be that the cold ice is a relict of a larger geometry in the past (when colder temperatures were prevalent), as is sometimes the case with polythermal glaciers in the Arctic?*"

Authors: There are data of the ice surface velocity measurements which were carried out in 1962 (only at the one ice surface point close to the ice front). The values are in the range 2..4 m/y (Vinogradov, 1962). If it's supposed that the ice surface velocity close to the ice front is in the range 2..4 m/y, then the enhancement factor should be in the range 0.15..0.3. But we suppose that the experimental values could be obtained for relatively small ice thickness at the point due to local bedrock undulations.

The modeled glacier length along the flowline profile corresponds to the observed one for the flow law rate factor from (Paterson, 1994) without any fitting multipliers (i.e. $m = 1$, Fig. 4).

G. Leysinger Vieli: "...*It is not clear how α, β and γ are obtained. Nor is it clear if (i), (ii) and (iii) (page 92, lines 6-7) are related to the three lines in equation 17. Furthermore what is not clear to me is the following: In the previous section some assumptions of temperature have been made to obtain the history in length changes - now through this history the temperatures are derived. But this is a circular argument. After reading those sections again I realised that a numerical inversion approach to reconstruct climate (air temperature) from length changes is investigated. The modeled length changes act in this section as synthetic data to investigate the performance of air temperature reconstruction of the inverse model.*"

Authors: The α, β and γ are obtained as the solution of the system of linear algebraic equations (17). The equations (17) stem from the conditions of the discrepancy minimum $\frac{\partial \Phi}{\partial \alpha} = 0$, $\frac{\partial \Phi}{\partial \beta} = 0$ and $\frac{\partial \Phi}{\partial \gamma} = 0$, where Φ is the discrepancy between left and right

TCD

3, S124–S138, 2009

Interactive Comment

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Interactive Discussion

Discussion Paper

members of the Eq. (16), i.e.

$$\Phi\,(\alpha,\beta,\gamma)=\int_{t_1}^{t_2}\left\{T_a(t)\;-\;\alpha\;-\;\beta\,L\;-\;\gamma\,\frac{d\,L}{d\,t}\right\}^2 dt.$$

If $T_a(t)$ and $L(t)$ are experimental data, then the solution of the Eq. (17) gives the parameters in the relation (16) for the experimental data.

We used the modeled length changes as the synthetic data. That's right. We don't see any circular argument in that, because the model doesn't “know” a priori about any relations between air temperature and glacier length. Of course, the values of α, β and γ depends on the model parameters and assumptions about relations between the mass-balance and the air temperature, and we have presented some dependences (Fig. 17, Fig. 18). Furthermore we can give a bulk of such dependences by using mathematical modeling.

Such way also is well known as the test for inverse problem. We can give the example - the air temperature paleoreconstruction from borehole measurements. First the direct problem for given air temperature is solved. Then the solution of the direct problem (temperature profile) is used as the synthetic data (instead of borehole temperature) and the derived ice surface temperature is compared with given ice surface (air) temperature. And then the inverse problem model parameters are chosen by comparing of given and derived ice surface temperatures.

See for example

a) MacAyeal D.R., J. Firestone and E. Waddington, 1991, Paleothermometry by control methods. Journal of Glaciology, V. 37, 127, p. 326-338.

b) Nagornov O.V., Yu.V. Konovalov, V. Tchijov. Temperature reconstruction for Arctic glaciers. Palaeogeography, Palaeoclimatology, Palaeoecology. V 236, 125-134, 2006.

Thanks for the remark.

G. Leysinger Vieli: *"Not to sure what to look for in Figure 7 and 12 to see the 'transitions'. Do you mean the kinks in the curve of Fig. 12. Not too sure what the 'above mentioned instability in the diagnostic system solution' is. This subsection about sliding needs more discussion/explanation."*

Authors: The no-slip/slip transitions in the glacier tongue base occur in the following way (in the described model). When the basal shear stresses achieve the limit – the critical value σ_{cr} - the glacier tongue begins to slip (or to drag). The transitions are implemented in the model by switch on (off) the basal sliding if $\max\limits_{X_{tr}<x<L} (\sigma_{xz})_b \geq \sigma_{cr}\ (\leq \sigma_{cr})$.

The ice flow velocity at $x = L$ changes abruptly at the points of the transitions and the derivative $\frac{dL}{dt}$ has the break points (or points of discontinuity) at these moments. So, the curves $L(t)$ have the kinks (or pikes) at these points.

In the cases of relatively large mass-balance amplitudes the ice thickness growing can be cause of significant ice flow velocity increase. But the no-slip basal boundary condition coupled with the continuity equation for incompressible substance in the downstream hamper the upstream ice flow increase. Because the ice flow velocity at the front point ($x = L$) is equal to zero in the case of the no-slip condition. Such internal contradiction of the model leads in the long run to the "above mentioned instability in the diagnostic system solution". If downstream ice flow velocity increases due to the basal sliding in concordance with upstream velocity growing then the diagnostic system solution remains stable.

G. Leysinger Vieli: *". . . only after rereading it several times I started to understand. . . change 'Fig. 17' to 'Fig. 19' . . . clarify. . . Line 11: Do you mean Fig. 14 instead of 13"*

Authors: After rereading it several times we started to understand too that in principle Fig. 19 can be placed before Fig. 17. Furthermore in Line 11 at the page 91 'Fig. 14' should be instead 'Fig. 13'. Thanks.

TCD

3, S124–S138, 2009

G. Leysinger Vieli: *"The larger part of the references covers the modelling aspect (10 references) and some are specific for the Gregoriev Ice Cap (5 references). I was astonished to see only 1 reference to cover the aspect of the reconstruction of annual air temperature and also that it dated as far back as 1994. Oerlemans himself did more recent work on this topic (e.g. 2005) which could be used here. I'm not an expert in this field at all and I wonder - there must surely be some more recent and relevant publications on this subject?"*

Authors: We agree. All missed references should be mentioned in the final manuscript.

G. Leysinger Vieli: "*The language shows many errors (mainly misuse in grammar) which makes the paper difficult to read as some phrases are wrong and the meaning difficult to understand. . . "*

Authors: We agree. The paper should be corrected carefully. As we understand the referee has everyday practice in English. We trust and acknowledge the referee for English corrections of our study.

The corrections are

1. Page 78, Line 5: 'the' instead of 'one'
2. Page 78, Line 6; Page 84, Line 1: 'coupled' instead of 'in couple'
3. Page 78, Line 26:'for the' between 'retreated' and 'last' is missed
4. Page 79, Line 17: 'ratios' instead of 'rations'
5. Page 79, Line 18: 'the' between 'with' and 'ones' is missed
6. Page 79, Line 20: 'in this paper' instead of 'at this paper'
7. Page 79, Line 26: 'second' instead of 'secondary'
8. Page 79, Line 27-28: 'equation obtained earlier by Oerlemans' instead of 'equation early obtained by Oerlemans'
9. Page 81, Line 11: 'vanishes' instead of 'vanish'
10. Page 81, Line 14: 'as small as possible' instead of 'as small as it possible'

TCD

3, S124–S138, 2009

Interactive Comment

11. Page 82, Line 10: ’incompressibility’ instead of ’incompressible’
12. Page 82, Line 11: ’at the’ instead of ’including free’
13. Page 82, Line 13: ’the’ between ’at’ and ’ice’ is missed
14. Page 83, Line 14: ’(Arkhipov et. al., 2004)’ instead of ’(Arkhipov, 2004)’
15. Page 83, Line 17: ’practically’ instead of ’particularly’
16. Page 83, Line 21: ’steepen’ instead of ’steep’
17. Page 84, Line 1: ’incompressibility condition’ instead of ’incompressible equation’
18. Page 84, Line 2: ’ice flow’ between ’upstream’ and ’increase’ is missed
19. Page 84, Line 19: ’components tangential to the bedrock’ instead of ’tangential to the bedrock components’
20. Page 84, Line 24: ’In this paper it’s assumed’ instead of ’At this paper is assumed’
21. Page 87, Line 5: ’first’ instead of ’fist’
22. Page 88, Line 4: ’of’ between ’comparison’ and ’the’ is missed
23. Page 88, Line 5: ’The’ before ’Authors’ is missed
24. Page 88, Line 9: ’high variability in velocity’ instead of ’high velocity variability’
25. Page 88, Line 14-15; Page 89, Line 4; Page 90, Line 20:’in Fig.’ instead of ’at Fig.’
26. Page 90, Line 4: ’in the curves’ instead of ’at the curves’
27. Page 90, Line 9: ’as possible’ instead of ’as it possible’
28. Page 92, Line 16: ’the’ between ’point’ and ’optimal’ is missed

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Interactive comment on The Cryosphere Discuss., 3, 77, 2009.

TCD

3, S124–S138, 2009

Interactive Comment