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The Cryosphere Discussions

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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Summary:
In this paper the authors investigate the respond of the glacier length change for different climate scenarios. As a reference surface mass-balance the averaged mass-balance distribution measured for the years 1987 and 1988 is used. A 2-D full-system (solving the full Stokes equations) flow-line model has been employed to perform the modelling of steady state experiments, used to evaluate the best enhancement factor, and non-steady state experiments to investigate the effect of periodical climate variability on the extend of the ice cap. The modelled glacier length history is then used to

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reconstruct the annual air temperature over the same period.

General Comments:

The goals of this study, (1) to investigate the glacier response to climate variability, is relevant to the local community and, (2) to reconstruct air temperature (climate reconstruction) from glacier length changes, is interesting to a wider scientific community. Especially this latter part would fit well into the scope of 'The Cryosphere'. However the paper has major problems: (1) The aim of the paper is not very clear. From the title we get the impression that the future changes of the ice cap is investigated, but the authors are using a glacier (which one?) and do not discuss how this one glacier represents the Gregoriev Ice Cap. The authors are using past climate periodicities (short-term and long-term) to calculate the advance and retreat of the glacier. The future climate periodicities are assumed to be the same as in the past. The modelling calculations could as well represent a state from the past and there is no discussion about why this climate periodicity should represent the future. (2) 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. (3) The paper is too detailed in the model description, as the model has been published before, and not detailed enough in the actual discussion of the modelling results of the Gregoriev Ice Cap. (4) 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 (I have to add that English is not my first language either). (5) The topic of the air temperature reconstruction is interesting but only after rereading it several times I started to understand that the authors are using their previously modelled glacier length changes as a synthetic data set to investigate a numerical inversion approach to reconstruct the air temperature. This is not clearly stated in the text.

The core of the paper should be about presenting the modelling experiments, it's results and their discussion. As it is now the authors do describe in some extend what they

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did but the aim is unclear. The paper is especially towards the end not detailed enough and difficult or even impossible to follow. Furthermore the results of their modelling study is hardly discussed.

I recommend that the paper is rejected in it's current state and that it is only reconsidered once the modelling study is described clearly and discussed in depth and the manuscript checked for the English language. I believe that the study contains interesting results but unfortunately they are not highlighted nor discussed by the authors. My recommendation to the authors is to shorten the modelling bit and to expand on their experiments, especially on the inverse modelling of the air temperature. 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.

Specific Comments:

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 main objective of the paper is to explain the observed retreat of the Gregoriev Ice Cap by investigating what kind of climate is needed to have the glacier disappear entirely and how the glacier will develop under the current climate. A secondary (or rather 'second'?) objective was to correlate ice cap length changes and air temperature with a similar method as Oerlemans (1994) used on in-situ measurements. I believe the authors meant second objective as the larger part of the conclusions of the paper are mainly about this last objective. 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 model (SIA) might be sufficient as length changes for

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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.

The modelling part of the paper is discussed in three sections: (1) In the first section the authors are evaluating the enhancement factor for the flow law by comparing the modelled steady state for the reference surface mass-balance using different enhancement factors with the observed 1990 surface profile. The used reference surface mass-balance is a linear approximation of the two year average of the mass-balance measurements from 1987 and 1988 carried out at the Gregoriev Ice Cap flow line. Steady-states are then calculated for reduced surface mass-balances parallel to the reference surface mass-balance to evaluate the mass-balance needed for full deglaciation and the corresponding time span to reach full deglaciation.
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. The authors do not state why the 1990 surface profile has been chosen. It is not clear why the averaged mass-balance should be the appropriate

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mass-balance distribution for the 1990 surface. In my view the authors should not only test for the enhancement factor but also for the mass-balance distribution as the linear distribution will not reproduce the observed surface profile (see figure 3 and 4) and a small error in the measured mass-balance distribution has already a large effect on the model output.

(2) In the second section the glacier extend is modelled for a periodical varying climate forced by a prescribed annual air temperature variation. An empirical relationship between the annual air temperature and the ice surface mass-balance has been used. Calculations have been made for different mass balance amplitudes over a fixed climate period and for a fixed mass-balance amplitude and varying climate periods. This section is kept too short as not all steps taken are very clear and many new items are appearing in the text but are poorly introduced and discussed (8 Figures for 2.5 pages text). The captions of the Figures are not informative enough and the Figures are not properly discussed. Too much is covered in a short space and therefore not discussed in depth. Some of the saved space from the model description should go into this section to make it clearer. Unclear is e.g. how equation 14 and 15 are used. (see in Details). Furthermore the sliding subsection is not clear (e.g. p. 90 lines 4-11) nor is the snow accumulation section (p. 90 lines 24-5 (p. 91)). Adding to the difficulties is the occasional misuse of the English grammar.
The vertical grid size is mentioned but it is not mentioned which grid-size is used in the normal model by default (11?). A grid-size should be chosen so that the model results are grid-size independent, and this should be stated in the text.

(3) The third section is what has been called secondary objective in the introduction. However as this section is discussed in more details as the section above, which would be the main objective, I believe that the authors meant to call it 'second objective' instead. In this section the authors investigate and discuss the relationship between the glacier length and the annual air temperature. This section is again too short and rather complex. It is not clear how α, β and γ are obtained. Nor is it clear if (i), (ii) and (iii)

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(page 92, lines 6-7) are related to the three lines in equation 17. I believe that this section about air temperature reconstruction might even be the main piece of the paper. 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 modelled length changes act in this section as synthetic data to investigate the performance of air temperature reconstruction of the inverse model. This is all very unclear in the paper as it is poorly explained and not properly discussed.

The first part of the conclusions are fine but the second part of the conclusions seems to be a continued discussion of the previous section (air temperature reconstruction).

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? If not then it is even more important for this paper to address this topic in detail.

Technical Corrections:
Below are some questions, unclarities, typos and problems with the English language. I haven't picked all up (there are too many).

Page 78:
Lines 4-7: Clarify sentence; 'one mechanical equilibrium equation' makes no sense. Do you mean 'one dimensional'? 'coupled' instead of 'couple'.
Line 20: Is an 'ice cap' one glacier? Or rather it has many glaciers originating from the one ice cap?

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Line 20: 'in the Central Asia' do you mean the 'Plateau' or 'Central Asia'?
Lines 21-22: is this a general statement or just for the plane-top glaciers of this region?
Line 26 and everywhere in text: keep 'plane-top' glacier consistent in writing. It changes throughout the text.
Line 26: add 'in the' or 'for the' between 'retreat' and 'last'

Page 79:
Line 1: Can one say 'arise ahead of glaciological community'? Sounds wrong.
Line 2: Is it 'plane-top glacier' or 'plane-top ice cap'? Be consistent.
Line 4: 'or should the climate...' instead of 'or the climate should be...'.
Lines 9-11: Do you really need full-system?
Line 17: 'ratios' instead of 'rations'
Line 18: 'model results' instead of 'models results'; add 'the' between 'with' and 'ones'
Line 19: is 0.2 the highest aspect ratio? It's not clear.
Line 20 and everywhere in paper: replace 'at' with 'in', it should say 'in this paper' - misused throughout paper.
Line 26: Do you mean 'second objective'? Secondary means it's less important than the other objective.
Line 26-28: Sentence not clear. 'the' or 'to' between 'was' and 'search'. 'early' can not be used so. Do you mean 'equations obtained earlier by Oerlemans'?

Page 80:
Here and everywhere in paper: subscripts in equations are not always clear what they describe. They are not described in list of symbols (e.g. 'H' is there but not 'h_s' - do you mean 'H_s'?).
Equation 1: second line - term on right hand side; should be a dependency on 'z' (e.g. $h_s - z$ or is $h_s = s - z$?).
Lines 9-11: Not sure this sentence is correct (domain under the gravitation).

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 (e.g. p. 80 top line of equation 1 and top line of equation 2; Equation 6 and first section in equation 7, bottom line of equation 7 and middle line of equation 1).

Page 81:
Lines 9-10: not clear English.
Line 11: 'vanishes' instead 'vanish'
Line 14: replace with 'as small as possible'

Page 82:
Equation 5: Last term on right hand side different than in Pattyn. Is it correct?
Line 9: 'two flow velocity equations' not understood.
Lines 10-11: Sentence not clear (including free ice surface points)
Line 13: insert 'the' between 'at' and 'ice'

Page 83:
Equation 7: partial repetition of Equation 1 and Equation 6
Line 3: not clear - do you mean 'that the above described..'?
Lines 5-7: clarify sentence (provides second order of the boundary condition numerical approximation).
Line 17: 'in particular' instead of 'particularly'
Line 18: clarify '10 m and negative' what do you mean by negative? smaller?
Line 21: replace 'steep' by 'steepen'

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Page 84:
Line 1: Do you mean ice stream advance (increase of what?) or do you mean thickening?
Lines 1-4: Sentences not clear. Replace 'couple' with 'coupled'. Replace with 'vice versa'.
Line 19: move 'components' in front of 'tangential'
Line 24: insert 'it' between 'paper' and 'is'. Remember 'In this paper'.

Page 85:
Lines 1-4: not clear.

Page 87:
Line 5: replace 'fist' with 'first'
Lines 9-16: not clear.

Page 88:
Line 3: 'experiment'
Line 4: 'comparison of the ...'
Line 5: 'The authors took into account....'
Line 9: 'variability in high velocity'
Line 14: 'in Fig. 4' not 'at'.

Page 89:
Line 4: 'in Fig. 5'
Lines 10-20: Is equation 14 used in equation 15? In this case the air temperature amplitude (δT_a) nor $T_a 0$ (reference air temperature?) is needed ($M_s(t) = M_s 0 + \delta M_s sin(2\pi t/t_p)$). Is T_a not time dependent? Subscripts not ex-

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plained.

Page 90:
Line 4: 'transition in the curves...' not 'at'
Lines 4-11: 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.
Line 9: either '.. a maximum as possible..' or '.. a maximum as it is possible'; both have different meaning.
Lines 12-15: Clarify (should it say 'diagnostic nonlinearity problem'?)
Line 20: 'in Fig. 12'.
Lines 24-25: clarify. 'from the surrounding' not 'at'

Page 91:
Lines 1-5: Not clear, clarify.
Line 11: Do you mean Fig. 14 instead of 13
Line 18: Oerlemans (1994)? Year of publication missing.

Page 92:
Line 16: Figure 19 used before Figure 17 and 18 - renumber 19 to 17.
Line 16: Sentence not clear (point optimal...)

Page 93:
Line 6: change 'Fig. 17' to 'Fig. 19'
Lines 1-8: not clear how best fit of parameters is judged. And why which approach is best. Make it much clearer and go into some details.

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Figures:
In general the Figures need much more informative captions.
Figure 1: Scale for whole Figure (scale in square very small). Which on is the glacier you are looking at?What is the width of the glacier? Where is the flow-line?
Figure 2: Replace 'bottom' with 'bed' instead (do this for all 'bottoms' in all Figures). Put units in brackets (for all Figures).
Figure 4: Colour shading not necessary (not more informative).
Figure 5: Use colour shading also in legend - difficult to see the lines properly.
Figure 6: Mention 'deglaciation times' in caption would be helpful to understand figure without text. Discuss the kinks in the curves (mainly $\delta M_s = -0.6m/yr$). It must be related to the bed topography.
Figure 11: lines in legend not clear on figure - colour in legend would help.

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