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Climate of the Past Discussions

Interactive comment on "Climatic interpretation of the length fluctuations of Glaciar Frías, North Patagonia, Argentina" *by* P. W. Leclercq et al.

Anonymous Referee #3

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General comments:

The manuscript provides an important contribution for understanding the past climate of the North Patagonian Andes. By combining a surface energy-balance model and glacier model the authors compared the length fluctuations and corresponding glacier mass balance to independent climate reconstructions. As stated by the Anonymous Referee #2, these combination is also to my knowledge the first attempt for the Southern Andes. The approach is fair and clear, in particular, as they rely on the longest and most detailed glacier fluctuation record for the Southern Andes. The results provide interesting information about the dominating factors which causes length fluctuations.

I am not an expert in surface energy-balance modeling as well as in climatic interpretation, therefore it was sometimes hard to follow. As the other referees (R. Neukom and

an Anonymous Referee #2) have already reviewed the paper and made many suggestions (that I have read, and I agree with most of them), in particular on the climatological interpretation, I will try to avoid repeating suggestions, except in a couple of cases in which I wish to emphasize that the change should be done or I wish to provide some further comment on the same subject.

As I am more familiar with glacier dynamics (in contradiction to R. Neukom), I focused on that subject.

From this perspective the authors should take care about the usage of the shallow ice approximation (SIA). SIA is not capable for numerical modeling the flow dynamics of valley glaciers (or small glaciers) like Glaciar Frias. However, I figured out that you are using the approximation which is adequately for modeling the dynamics of a valley glaciers (Both equations are looking almost similar; otherwise I would not trust the modeled ice dynamics). Additionally, the authors should address some more details and/or discussion about the long-term ice dynamics. I don't think that the glacier dynamic only depends on the glacier geometry in the entire investigated time period. As its a manuscript for CP and you don't want to overload the paper with glacier-dynamical issues it would be at least helpful to bring some statements about your assumed simplifications (more details in my comments below). These simplifications and related uncertainties have to be carefully discussed and probably you can refer to them in a short outlook.

In general, the paper is well written and well structured. However, the manuscript lacked in consistency (namings, spellings, symbol usage etc). After adaption and correction of my comments and the comments of the other reviewers, the paper is worth to be published in CP.

I will state my comments/suggestions by order of appearance (not in order of importance), and will include at the end a list of technical comments.

Specific comments:

P3653 title

include the temporal coverage

P3658 line 1 and line 10

You give a reference to Fig.8 before Figures 2-7 are referenced, in particular Fig. 8 showing results which are explained later in the text. However, you cannot always account for this in a paper. Especially in this case you can reference Fig. 1b (and Table 1).

P3658 line 9

→ see comment of the second Anonymous Referee#2.

P3659 line 1 – line 9

Please clarify the height reference you use. On P3665 line 14 you provide a height reference as a orthometric height (m a.s.l.). All other heights in the manuscript are just given in m. Are they ellipsoidal heights?

P3659 line 26

write ELA out (first appearance of ELA)

P3660 line 3

I don't found the Volcano Villarica on Fig. 1. Either you drop the Figure reference or include the location of the Volcano.

P3664 line 22

The parameter T_a is not introduced. The parameter c appears in Table 2 as c_1.

P3663 line 12

Here B is introduced as annual surface mass balance at a certain point. On P3666 line 18/20 B is named as specific mass balance. Also, you should be especially careful

about this, since the Glossary of Glacier Mass Balance has been announced
http://unesdoc.unesco.org/images/0019/001925/192525E.pdf

The authors provide a convention for lower-case and upper-case usage (Sect 3.2.3): 'lower-case symbols refer to quantities at a point on the glacier surface or to the column beneath such a point, and upper-case symbols refer to glacier-wide quantities.'

P3663 line 18

You introduce the altitude dependent lapse rate p and make a reference to Table 2. However, in the table the parameter is named 'precipitation vertical gradient'. I suggest to use consistent namings. Check the whole manuscript if the namings in the table are the same as in the text or vice versa.

P3666 line 2/4

If you introduced the reference profile B_ref(z) you can also use the Symbol in the following text (for instance on P3668 line 9, P3668 line 21, P3669 line 14/15). Compare my comment for 'P3669 line 14 – line 17'.

P3666 line 22

w0 is not introduced

P3667 line 1

Is H the thickness at the flowline?

P3667 line 6

I am rather sure that you are not using SIA. SIA is valid for the big ice sheets, where the aspect ratio (typical thickness/typical length) is small and ice flow is only determined from tau_xz and tau_yz. The approximation you use, is the so called 'cross-section-flow' where tau_xy and tau_xz remains (cf. Budd and Jenssen 1975, Paterson 1994).

May you take a quick look in Greve and Blatter (2009) and compare the SIA and valley glacier equations. For instance, the authors state on page 153 (sect. 7.3): “For these reasons, the shallow ice approximation is no longer applicable for small glaciers”. However, the equations are looking almost similar, but I would strongly recommend to delete ’SIA’ in the text to avoid any misunderstandings.

P3667 line 7

I don’t found Budd et al. (1979) in the References.

P3667 line 10

Please explain the values f_d and f_s (similar as in Stroeven et al., 1989). I was surprised about the value of f_d, which looks similar to the commonly used Arrhenius factor/flow parameter in ice modeling (cf. Paterson 1994), which indicates a very cold glacier (Paterson 1994, Hooke, 1981). In this regard you have to explain the thermal state of the glacier (I don’t found a statement in the manuscript). I am rather sure you treat the glacier as entirely temperate due to the prevailing maritime climate etc. Than you can also say that basal sliding (second term on the right hand side of Eq. 11) is allowed/expected everywhere.

P3668 line 6 – line 15

To reproduce the bedrock topography from the ’inverse modeling’ is a solid approach. However, I think you have to address the statement ’The bed profile that reproduces the present-day (referred to 2009?) surface best . . . ’ with some error values. I am wondering why your provided bed-profile is the best: The resulting glacier length is 6025m vs. an observed length of 5550m. Therefore, you would receive very high mismatches for $x>5550$, because there is no ice observed. Are you able to keep the ice thickness equal to zero at x=0m and x=5550m during the ’inverse modeling’? I would expect (just a guess) that a glacier geometry with a modeled length closer to the 2009 length will give a better result with respect to a defined error indicator, in particular as your surface altitudes are from 2000-2009. I suggest to include a difference plot of

the modeled and observed surface vs. distance in Fig. 2a.

P3668 line 8

You have to define an equilibrium criterion.

Sect. 4.1 Steady state

I suggest to include a figure which shows basal velocity, deformational velocity etc. (similar as Fig. 8 in Oerlemans (1997a)) to show that your modeled ice velocities are in a reasonable range. Obviously, ice velocities out of range would highly affect your thickness evolution (Eq. 13). I tried to calculate some by estimating the slope and the thickness from Fig. 2a, some make sense to me some not.

P3669 line 14 – line 17

It is not surprising, that your modeled glacier length is in good agreement with the observed glacier length and the calculated mass balance is fairly accurate.

To summarize your procedure:

1) ’inverse modeling’ (Sect. 3.2): You are using B_ref (Fig.4) together with the glacier model to generate iteratively the bedrock topography until the equilibrium state match the observed width and surface.
2) Definition of steady-state model runs (inferred from P3668 line 20): Running the glacier model with time-independent boundary conditions (→ B_ref) until an equilibrium state is reached. Again, you have define the equilibrium criterion! You don’t dropping the time-dependent terms in Eq. 13?

3) Steady state (Sect. 4.1): Now, you are using the generated geometry from 1) as input , running the glacier model with B_ref (Fig. 4) as forcing until an equilibrium state is reached.

So, you have in advance adjusted the bedrock topography using B_ref that it fits roughly to the observed glacier geometry. I don’t expect then any sensitivity of the glacier model forced by B_ref.

Your 'inverse modeling' procedure is responsible for your good accuracy to the observed glacier length and calculated mass balance.

It is just an idea, a sensitivity analysis would be

1) to estimate upper and lower bounds of the calculated mass balance profile and run the model with these three scenarios.

2) to vary the bedrock topography. The time period from 1980 to 2009 is characterized with a rapid retreat of Glaciar Frias. Therefore, you can generate three different bedrock topographies with respect to three different glacier lengths (1980, 2009, mean; the latter corresponds roughly to the scenario you have shown). (If you are able to keep the length of the glacier constant in the 'inverse modeling' runs.)

Otherwise, if I got it all wrong (steps 1,2, and 3), I got lost, for instance in your multiple definitions:

climatological mass balance (Fig. 4) on P3668 line 9;
present-day climatological mass balance profile (Fig. 4) on P3668 line 21

calculated climatological mass balance of the period 1980-2009 on P3669 line 14/15

P3670 line 9

→ see comment of R. Neukom regarding the response time.

P3670 line 5

Please reference an Eq. where you added the temperature perturbation.

P3671 line 12

I think the unit should be m w.e. aˆ-1 instead of m w.e. aˆ-2

P3673 line 17 – line 26

How do you vary the sliding and deformation constant f_s and f_d?

1) Do you vary them in time? In any way related to climate, so that a warmer climate intuitively induces higher deformational velocities and probably changing sliding velocities (Taking some response time into account until the warmer temperatures are transferred to the glacier. I don't have an citation on hand regarding this issue, but I would assume that the glacier reacts more or less instantaneous)? However, if you bring a statement such as 'the glacier is assumed as to be temperate over entire time-period' the motivation for a time-independent deformational constant is roughly justified. To make a statement about the thermal state of the glacier would be also interesting, as you conclude that the length fluctuations of Glaciar Frias are temperature-driven (P3678 line 27).
Changing sliding velocities are more difficult to discuss as its regarded as one of the key problems of glacier flow (dependent on various parameters). To pick up the argument of the Anonymous Referee#2, surface ice velocities could also show inter-annual variability probably due to varying sliding velocities which are coupled to the available surface meltwater (drained through crevasses to the base) .

2) Do you just vary them for each individual model run?

Please clarify and add a (short) discussion about changing ice dynamics over the observed time period.

→ Additionally, see comment of the Anonymous Referee#2 for P3673/3674.

P3674 line 4

write SE out (first appearance of SE)

P3686 Table 2

Some parameters of the ice-dynamical model are missing (density, the acceleration due to gravity).

P3687 Fig. 1

In the legend the accumulation area is colored in black. Probably a problem with my printout. Take care of this within the proof-read stage. Additionally, contour lines a hard to identify. May you can improve it by making them thicker and/or choose another color. I also missing the coordinate axes.

P3688 Fig. 2

Please rephrase the figure caption. I think the first sentence is misleading. The surface altitude is taken from the DEM. The bedrock topography is derived from your 'inverse modeling'.

P3689 Fig. 3

temperature label: unit is ˚C

P3690 Fig. 4

As I understood this is the B_ref(z) profile? → use B_ref(z) in the caption.

What means the abbreviation NB?

P3693 Fig. 7

Are the model runs with ΔT=+1K around 150a in equilibrium? In the caption and the manuscript please use ΔT= $value.

The value t=25a is not explained in the text.

Technical comments:

Either avoid present-day or define it at first appearance on P3659 line 8.

Use equal namings/spellings for parameters both for multiple appearance in text and appearance in text and tables.

→ see Technical comment of Anonymous Referee#2

Although it is clear in most cases which model do you run, but please avoid to use only 'model' in the text (for instance P3668 line 8). Choose a consistent naming for both models in the whole manuscript.

References:

Budd, W.F. and Jenssen, D. (1975), Numerical modelling of glacier systems. IAHS 104, 257-291.

Greve, R. and Blatter, H. (2009), Dynamics of Ice Sheet and Glaciers. in Advances in Geophysical and Environmental Mechanics and Mathematics. Springer

Hooke, R. LeB. (1981), Flow law for polycristalline ice in glaciers comparison of theoretical predictions, laboratory data and field measurements. Reviews of Geophysics and Space Physics 19 (4-81),

664-672.

Paterson, W.S.B. (1981), The physics of glaciers, 3nd ed., Pergamon Press, Oxford

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