KEY

Reviewer comments (green italic)

Response (black)

New or changed text (blue)

***Interactive comment on* "The contrasting response of outlet glaciers to interior and ocean forcing" *by* John Erich Christian et al.**

Martin Lüthi (Referee)

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Dear Colleagues

This is a very nice manuscript, well presented, with a good methodology and a thorough set of model experiments that comes to significant and interesting conclusions.

There are only a few minor things I would like to see changed, and which are indicated below.

Best regards,
Martin Lüthi

We thank Dr. Lüthi for the encouraging review, suggestions for clarification, and several very thought-provoking questions. Please find our responses and changes below.

General comments:

- clearly written

- flotation-based calving criterion -> what would change with other parametrizations?

Thanks for raising this question. The topic of assumptions at the grounding line (or calving front) is an important one. We are assuming the comment refers specifically to a case with no ice shelf (i.e., ice calves immediately at the grounding line) and thus no buttressing. In such a case the analytical form of Ω from Schoof (2007) would not be strictly valid. Many mechanisms might be implicated in ocean forcing in such cases, but given that reduced back-stress is often part of the picture (e.g., Nick et al., 2009; Joughin et al. 2012), we think forcing via the grounding line flux coefficient is likely to still be a good starting point for representing ocean forcing. As long as flux follows the general form of equation 1 ($Q_g = \Omega h_g^{\beta}$), we expect the main findings of the study to be applicable, at least in a qualitative sense.

Similar, but more detailed approaches to forcing have been considered previously. Nick et al. (2009) simulated several large outlet glaciers, perturbing the force balance and allowing the upstream glacier to evolve, but these simulations were limited to decadal timescales. We also discuss the similar experiments of Price et al. (2011) and Goldberg et al. (2015) in section 3.1

(committed change). These prior experiments have showed similar behavior to what we consider the "fast" dynamics, albeit with much more detail on short timescales. Extending this type of perturbation experiment to much longer timescales would be an interesting next step for investigating (a) how our results would change with more detailed dynamics at the glacier front, and (b) how their results extent to the eventual "slow" response.

We also discuss alternative grounding line flux rules in the discussion section, and reference the findings of Robel Roe and Haseloff ("RRH"; 2018), who explored additional nonlinearities associated with alternative solutions (see Hasleoff and Sergienko, 2018). The simplified flux condition and forcing scheme we used offered a very general approach for initial analyses, but we expect that considering alternative assumptions and dynamics (including calving) would yield further insights.

- call "interior forcing" something more like "mass balance forcing"

If the reviewers and editor find it acceptable, we would prefer to retain "interior forcing" throughout, we believe it helps highlight the geometric differences between interior mass balance and ocean forcing. That is, it may help remind the reader that it is distributed over the entire interior. (This of course is a simplifying assumption, and is addressed in the discussion.)

However, we agree that clarity in the language here is important. We have added a statement in section 2.3 (where the first forcing experiment is introduced) to establish that these terms will consistently refer to forcing described as such. Hopefully this makes usage of the term "interior forcing" clearer throughout.

In all model experiments throughout this study, "interior forcing" and "ocean forcing" will refer to perturbations applied in the following manner. Interior surface-mass-balance anomalies are assumed to be spatially uniform. We represent ocean forcing very simply by perturbing the grounding-line-flux coefficient, Ω …

In addition to this statement, we have made edits to ensure that usage is consistent throughout the manuscript. For example, there were some places where "terminus forcing" was used instead of "ocean forcing". Now, "interior forcing" and "ocean forcing" are used consistently when referring to our own experiments, or the parameters S and Ω are used as descriptors.

- The paper structure, even if it is not following the standard pattern, is useful and helps guiding the reader through the manuscript.

- The bibliography should be carefully revised. Capitalization of journal names is often wrong, and DOIs are missing. IPCC (in Stocker..) should be mentioned, etc..

The bibliography has been revised.

Style (to be adapted to journal standard everywhere):

We have tried to make the changes as suggested, but find some cases where colons and parentheses are necessary for clarity and/or sentence structure, and these cases seem to be consistent with usage in recent papers in *The Cryosphere*. However, we will defer to the editors on the preferred style.

- do not put variables in parentheses (as is occasionally done)

Changed throughout, except where variables are presented essentially as parenthetical statements (e.g., reminders).

- no colons before equations

We have removed most colons; however, there are some cases where it seems needed based on the sentence structure.

- write out "Equation" in the text, only abbreviate in parantheses using "Eq." oder "Sect." is not commonly used in manuscripts.

Fixed throughout.

- use real fractions in equation environments:

\frac{1}{2} etc

Fixed.

Specific comments:

*76 variable names should consistently *not* be enclosed in parentheses. Also indicated that this is just a statement of conversation of mass.*

Fixed. The conservation of mass has been made more explicit:

The evolution of local ice thickness h at a grid point reflects the balance of mass exchange at the surface and horizontal ice-flux divergence. Conservation of mass requires that

$$\frac{\partial h}{\partial t} = S - \frac{\partial(\bar{u}h)}{\partial x},$$

where…

86 "Glen-type coefficient": better say you use power-law rheology with A and n.

Fixed:

We assume a typical power-law rheology, with coefficient A and exponent n (e.g., Glen, 1955).

89 In an equation you should use real fractions: \frac{1}{2} also in Eq. (7) etc

\frac{\tau_b}{C}^{\frac{1}{m}} and so on, please change everywhere in the manuscript

Fixed.

105 so, here you allude that your model is defined on a grid?

The grid applies only to the flowline model, and not the two stage model. We have revised the text to clarify:

The PD12 model calculates grounding line flux based on a thickness h_g that is linearly interpolated from the height above flotation of the last-grounded and first-floating grid cells, to the point where flotation is reached. The corresponding sub-grid grounding-line position is shown for all output from the PD12 model in this study. Although the PD12 model is typically run on coarse grids (O ~1–10 km) for continent-scale simulations over many millennia, we use a grid of 100 m to better resolve the details of grounding line variations.

115 "Section" (not abbreviated in the text)

Fixed.

- Figure 1d: it would be interesting to also display the forcing (maybe as gray line with appropriate scaling).

Good suggestion – we have added the noisy forcing to Fig. 1d.

- Table 1: why is the sliding coefficient given with 5 significant digits? I think in this study this can be any arbitrary number of about that order of magnitude. Same for A.

This is a good point – the exact value of C and A are not important to that level of precision. A and C affect the equilibrium grounding line position identically for both models (via \Omega). The treatment of interior fluxes is different for the two models, but provided the two-stage model captures interior thickness reasonably, it should emulate the flowline model over a range of values for A and C.

These values were initially chosen for comparison with results from Robel et al., 2018 (and are "default" values in several other studies). We would elect to keep them defined to full precision in the table for the sake of clarity, but would defer to the Editors on this.

Scientific notation does not use a \cross, but a \cdot

We will defer to editors on this … recent papers in *The Cryosphere* appear to use a cross (\times).

Also consider two more columns, one for the symbol (should come first) and one for the units.

Good suggestion. Columns added here, and for Table 2 as well.

Table header should not be given in bold.

Fixed.

164 it is not clear what the "two stages" are. Some other designation might better describe the model.

This is a good point – it was not initially clear as described. We would prefer to keep this terminology following Robel et al. (2018) (where the model is derived), and to maintain a connection to other "stage" models (Roe and Baker, 2014). However, we agree that this designation should be made clearer. We shifted introduction of the term to a later paragraph, when the two equations are presented. We then refer the reader to the later section (2.6), where we discuss the stages in more detail.

Two coupled equations capture the transient adjustment of the two degrees of freedom, *H* and *L* as they relax towards a steady state that balances all three fluxes:

$$\frac{\partial H}{\partial t} = S - \frac{Q}{L} - \frac{H}{h_g L}\left(Q - Q_g\right)$$

$$\frac{\partial L}{\partial t} = \frac{1}{h_g}\left(Q - Q_g\right).$$

Because achieving steady state requires adjustment of both *H* and *L* we refer to this model as the ``two-stage model", following RRH. We discuss the operation of these stages further in section 2.6…

And In section 2.6, we also added a link to the two-stage terminology to make this concept clearer:

These modes can also be conceptualized as a two-stage low-pass filter on any forcing time series…

165 "static geometry" (as opposed to "dynamic") is probably what is assumed by all models. Do you mean "linear/sloped"?

We have re-worded here to clarify:

The geometry is further described by a bed topography with constant average slope b_x .

We also clarified description of the bed for the flowline model, noting that it is constant in time. We thought it worthwhile to clarify this, as the PD12 model can include an isostatic adjustment option, which has been used in previous studies.

The bed is constant in time, and has an elevation of −100 m at the divide and constant prograde slope b_x of -2×10^{-3}

167 "S\cross L" should be "S\cdot L" or just "SL". Do not put variable names in paren- theses in some places.

$S \times L$ has been replaced with $S \cdot L$ throughout.

167 "Q" : what is this, the flux into the grounding zone? Please make description clearer.

Yes – this has been clarified:

Ice thus enters the system via an accumulation flux $S \cdot L$, flows via an interior flux Q to the grounding zone, and leaves the system as a flux across the grounding line Q_g .

190 Repeat which variable is the "mass balance rate" (I think S)

Yes it is *S* – this has been clarified.

192 What is this "small reservoir"? Until now there was just one, described by S and L. Maybe this is the purple box in Figure 2. If so, this setup should be made more explicit from the beginning, and Figure 2 should be improved.

Yes, it is the grounding zone reservoir and the purple box in figure 2. We have clarified here that it refers to the grounding zone:

τ_F is controlled by h_g, and thus the volume of the system's small grounding-zone reservoir

However, the two reservoirs are already described above this, and the figure referenced: "The glacier can be conceptualized as a small grounding-zone reservoir with a length $l_{gz} \ll L$ and thickness h_g , coupled to a large interior reservoir with thickness H and length $L-l_{gz}$. … The model dynamics are derived by balancing ice fluxes through these linked reservoirs (Fig. 2a)."

200 It is not clear what the use of a linearized system is. It is certainly useful to find eigenvalues and eigenvectors at a certain state.

But having a "full" model with all nonlinearities, and then using a linearized "deviation model" makes no sense for the large changes investigated. Also, the "full" model is extremely cheap to calculate, as it is just a 2-variable ODE.

Especially given the comments in line 210 that the nonlinear ("full") ODE give the same results makes one wonder, why the linearized version is used at all (except for investi- gation of eigenvalues).

This is an important question, and we are glad it has been raised. We view the linear model primarily as a tool for analysis. As noted in this comment, it has eigenvalues (here, $1/\tau_s$ and $1/\tau_S$), which are key to understanding the transient response. In our view, the eigenvalues are just one benefit of the linear model. Linear frameworks are fundamental in timeseries analysis and statistics, and allow one to apply a broad and well-established toolkit to analyze the system in question. For example, approximating the outlet glacier as a linear system not only allowed Robel et al. (2018) to identify that there are two characteristic timescales, but also provided a framework that shows how they operate relative to each other (i.e., their relative contributions to the total system response). In the present study, we find this framework to be helpful for understanding how the same two response times can yield different behavior depending on how forcing is applied.

Perhaps most importantly, the linear response times reveal how physical parameters govern the transient response. Even if a particular linearization is only strictly valid for small changes, these leading controls have robust physical interpretations, and so are useful for understanding the system. These insights would not be as readily available for response times estimated empirically from more complex models or data.

Of course, there are many salient aspects of marine-terminating glacier dynamics that are fundamentally nonlinear, which can't be fully addressed with linearized models. However, as the *physical* controls identified by linearization often translate to some degree into the nonlinear cases, linear models can help point to questions to be tested in a more complex model. Our point is not that the linear model is superior, but it does enable understanding in a unique way.

An additional and more specific point raised in this comment is why the linear model should be used (especially for large changes) when the nonlinear version is available and computationally cheap. The limit of a linearization is an important consideration, which we discuss further below in response to a related comment. However, in this section, we think it is in fact useful to compare the linearized response to the flowline model, as it reveals just how nonlinear the response is. For the relatively large forcing imposed (20% perturbations in S and Ω), there are indeed some differences between linear and flowline sensitivities (and as noted in the text, the nonlinear two-stage and flowline models have identical *equilibrium* sensitivity). However, the key transient aspects stand out in light of these differences. We feel it is important to show the linear response here in order to back up the later experiments and interpretations that are based on the linear framework. However, we agree that this motivation was not clear enough in the original text. We have re-worked the following paragraph in order to clarify why the linear output is shown here, and also to motivate its use later in the paper:

Figure 2 shows output from both models in response to step and stochastic forcings. Fig. 2b shows the grounding line retreat following a 20% increase in Ω (blues) and 20% decrease in S (orange/brown). For clarity, only the linearized two-stage output is shown. Note that the nonlinear two-stage model is constrained to have the same equilibrium response as the flowline model by Eq. (1). The linear and nonlinear responses match almost exactly for the stochastic fluctuations, but disagree somewhat for the step changes. However, the disagreement is not severe, suggesting that we can reasonably use the linearized model and its response times as analytical tools. Importantly, the two-stage model captures the faster initial response to forcing at the grounding line, with a slightly more pronounced slope break in the first few hundred years.

250 Sn important additional argument in this discussion is also the spatial scale. Interior changes take a certain time, given by the ice flow speed, to affect the terminus, while processes at the terminus are immediately affecting glacier length. This is the same as on any glacier, but the effect of terminus dynamics in tidewater are much faster and bigger than those of terminus melt on a mountain glacier.

We agree that this is an important consideration. We believe the essence of this basic point is already brought up in the next paragraph:

"A change in surface mass balance has an immediate, spatially distributed tendency on interior thickness, which slowly alters driving stresses and thus fluxes throughout the interior. Anomalous ice flux arrives at the grounding zone … In contrast, a perturbation to Qg (here, via Ω) is highly localized and first creates a large disequilibrium between Qg and interior flux…"

A related issue is the spatial pattern of the mass balance forcing. In the discussion, we bring up this issue and describe the effects of localizing the surface perturbation near the terminus.

265 Here a link to kinematic wave speed would be very interesting. How fast is the terminus signal propagating upstream.

This is an interesting point and a useful connection to make. The signal propagating up-glacier is diffusive, so we must define some threshold to track propagation. A natural one in this case is the peak in interior fluxes, shown in Figure 3. This shows when the gradual drawdown of interior ice (a negative tendency on flux) overwhelms the initial increase allowed by the perturbation at the grounding line. We now discuss this in this paragraph on flux evolution. We would prefer to discuss its speed in terms of the fast timescale (rather than absolute numbers), since this paragraph is intended to be a general and qualitative comparison of the flux changes between the two models.

"… Both models capture this transfer, and the flowline model flux gates show that it gradually propagates up from the grounding zone. The transient peak in fluxes is similar to a kinematic wave propagating from the terminus, which reaches well into the interior within a few multiples of τ_F . The ensuing drawdown of the interior reservoir (the slow mode in the two-stage model) brings interior fluxes back down, and again, Q_g must follow via grounding line retreat.

Figure 3: The two blueish-greenish colors are difficult to distinguish, use a better color table.

Fixed. We have used higher-contrast colors and bolder lines. Example:

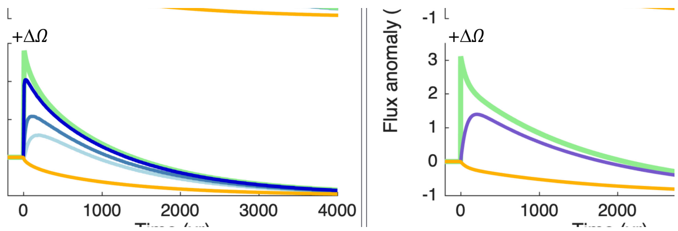
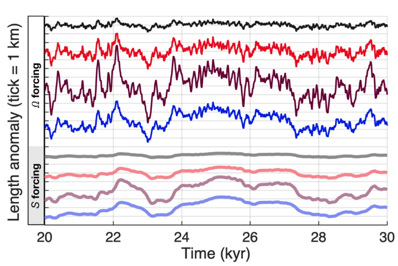


Figure 4: Use clearer colors, full red, blue etc. They might look less appealing, but are easier discernible.

We have made the reds and blues brighter. We would prefer to retrain the scheme where the lighter hues (and bolder lines) of the same color (black, red, maroon, blue) correspond to forcing with the same spectral characteristics, but applied as S anomalies. We tried other options to distinguish (like dashed lines), but found these to be less visually clear.

Example:



308 "white noise forcing" (also 311)

Fixed.

346 where do these exact numbers come from?

These are the linearized response times predicted by the two-stage model. Table 2, which is referenced shortly after, contains the other parameters for each glacier. We have added to the table 2 caption to clarify how these values are determined:

Table 2. Parameters varied between three idealized glaciers (top) and the resulting steady-state values (bottom). The steady state is calculated by the nonlinear two-stage model, and the linearized response times are given by Eqs. 13 and 14.

350 should the "as of 2020" follow "transient response"?

Yes – thank you for catching this. Clarified:

The most basic result is that, in all cases, the transient response as of 2020 is a small fraction (~ 1–23%) of the instantaneous equilibrium response.

Figure 5: caption: "Table" (upper case)

Fixed.

457 Here the question posed above gets more pressing: why is the linearized model used? Can it be used at all for such large changes? How wrong do the results get? Why is not the "full" simple model used here?

(see full response after next comment)

458 And then also: are the time scales always the same, even if the glacier geometry changes by a very large amount?

We will address the above two related comments together. These are good points and valid concerns for applying the linear model to the question of a long-term memory of past climates. We would agree that a linear model probably cannot accurately capture such large changes (i.e., Last Glacial Maximum to Holocene), and would not be the right choice for reconstructions.

The main reason we use the linear model here is that it is constrained to have the same equilibrium sensitivity to fractional anomalies in S and Omega. This allows us to more easily compare the transient responses side-by-side. Differences in the transient responses at any given time are thus due only to the different expression of fast and slow responses following each type of forcing.

One could use the nonlinear two-stage model (or the flowline model) here and normalize the results, potentially capturing some relevant nonlinearities. However, some of the nonlinearities depend on simplifications still present in the nonlinear models. Furthermore, their effects would depend on the magnitude of the forcing applied. For this highly idealized case (and one set of model parameters), we would be concerned to add model complexity that might overemphasize effects that vary widely between real settings. (For example, one nonlinearity inherent in these models is that glacier advance expands the surface area, increasing total accumulation flux. This would vary between real settings, and would be reversed in the presence of an ablation zone.)

Further study on various nonlinearities and how they vary between settings would of course be an interesting and very useful topic for future research.

We do not mean to argue here that the linear equations are a superior model. For this experiment, however, we believe it is more straightforward to (a) demonstrate a few simple points; and (b) to explain its assumptions and limitations.

We agree that the original manuscript should have been clearer, particularly on (b). We thus have tried to highlight the reasons for using the linear model, and, importantly, to shift emphasis to the smaller climate variations (e.g., LIA and late-Holocene cooling) for which the linear model is more valid. We think these variations are quite interesting in their own right, so we are glad that this comment gave us an opportunity to sharpen the focus of this section.

The revised part of this section is below, with new or modified statements in blue.

First, we consider a climate scenario with idealized representations of three events: a deglacial warming at 11 kya, a Little Ice Age (LIA) cool period from 1450 to 1850 CE, and an anthropogenic warming trend from 1880 to 2100 CE. The deglacial and LIA transitions are smoothed by error functions of 500 yr and 50 yr widths, respectively. The magnitudes of deglacial, LIA, and anthropogenic events have a ratio of 10:1:4, and the intervening Holocene climate is assumed constant. We scale forcings linearly to the climate anomalies, where warming corresponds to negative S' or positive Ω'. Obviously, different combinations of these forcings could be expected in reality. Rather than choosing a particular combination, we examine each in isolation and normalize the glacier responses. These experiments thus serve as limiting cases to illustrate the relative influences of ocean and interior forcing over different timescales. This scenario is designed to explore two practical points: (1) the glacier's memory of large, long-ago events compared to smaller, more recent events; and (2) the glacier's relative memory of past ocean vs. interior forcing.

We use the two-stage model, linearized with respect to the Holocene climate with parameters for glacier 1 (See Table 2; τ_F , τ_S ~ 76, 2000 yrs). The advantage of using the linear model here is that it has uniform sensitivity to fractional perturbations in S and Ω. This allows us to more clearly distinguish the signatures of fast and slow dynamics; the tradeoff is that it ignores nonlinearities that are surely a factor for very large climate changes. Accurate simulations over such transitions would depend not only on nonlinearities in ice dynamics, but also on spatial information (e.g., bed topography) that is simplified in reduced models. Nevertheless, the linear model is a straightforward tool for demonstrating consequences of having both fast and slow dynamics. Focus should be directed toward the fast and slow responses to the idealized LIA, for which the linearization is more valid. Including the deglaciation signal primarily serves to account for residual, albeit faint, disequilibrium implied by millennial response times.

Figure 7a shows the idealized climate (top) and two-stage model responses. Anomalies are shown relative to the mid- Holocene and normalized to the large deglacial transition. Figure 7b shows 1000 to 2000 CE in more detail, including a scenario with no anthropogenic warming (dashed). For reference, the length at which the glacier would be in equilibrium with the LIA climate is also plotted (gray). For both forcings, the grounding-line retreat due to the deglacial signal is nearly complete by the onset of the LIA. However, the LIA advance is ~ 2× greater for forcing in Ω, because it can engage the fast mode to a greater degree. Yet, even forcing in Ω yields a muted response: the transient response only reaches ~ 35% equilibration before the period ends. The slow mode, which takes up the majority of the response for both forcing types, barely feels our 400 yr LIA before it is reversed. This is worth bearing in mind whenever the duration of glacier excursions and climate anomalies are less than τ_S , because the system never achieves equilibrium. This would be an issue particularly if such events are used to tune glacier sensitivity in models.

The idealized LIA is a much more discrete "event" than is supported by paleoclimate records, and ignores other variations in the Holocene. Thus, we also consider the response to a more realistic forcing time series. Again, this is not a reconstruction of actual terminus changes, but it is useful to see how glacier memory integrates the continuum of variations found in paleoclimate records. We use a time series of temperatures for Disko Bay, Greenland, from the regional reconstruction of Buizert et al. (2018)

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467 "time series" (two words)

Corrected throughout.

References in responses

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