Response to Mikhail Verbitsky comment (CC3)

Dear Mikhail Verbitsky,

Thanks again for your comments. We hereafter respond point by point:

Dear Sergio Pérez-Montero, Dear Colleagues,

Thank you for considering my comments.

1. Unfortunately, your equation CC1.1 is also problematic because it implies that ice sheets that do not have a boundary with an ocean ($L_{\mathrm{ocn}} = 0$) have ice discharge $q \to \infty$.

We disagree. We are not trying to simulate any ice sheet with equation CC1.1. We are aiming at estimating, in a spatially adimensional manner, the ice discharge of past Northern Hemisphere ice sheets, whose potential contact with the ocean is determined by their geographical distribution. Perhaps, L_{ocn} should rather be called "potential" or "maximum ocean boundary". And, the way of interpreting L_{ocn} is the following: according to equation CC1.1 and a given positive surface mass balance: an ice sheet with a low L_{ocn} would need a high ice discharge in order to be in equilibrium ($dH/dt = 0$). Conversely, an ice sheet with a greater contact with the ocean (a high L_{ocn}), would require (and show) a smaller ice discharge to be in equilibrium for that same surface mass balance. In any case, the relevant consideration regarding equation CC1.1 is whether the ice discharge q, scales well with $v \cdot H$ or some other relationship is better. We will show that indeed $q = k_1 \cdot v \cdot H$ is a good approximation, k_1 simply being $1/L_{\mathrm{ocn}}$. Therefore fixing L_{ocn} to a non-zero value is entirely justified (see below).

Let us return to the basics. The total mass balance of an ice sheet can be written as

$$\frac{\partial(HS)}{\partial t} = \dot{m}S - \oint_{\gamma} \int_0^{h_g} v_g dzdy$$

Here S is the area of an ice sheet, γ is the grounding line, h_g, v_g are ice thickness and normal velocity at the grounding line, correspondingly. Indeed, the parameterization of the term $\oint_{\gamma} \int_0^{h} {}_g v_g dzdy$ is a difficult task because at the grounding line ice thickness $h \to 0$, but ice slope $\partial h/dx \to \infty$.

The ice thickness at the grounding line does not tend to 0. By definition the ice thickness must not be 0 there, otherwise it would not be a grounding line. Furthermore, the ice thickness at the grounding line (the mean value in Antarctica is about 220 m) is generally very similar to its surroundings, particularly in ice streams, because of the quite flat surface, and can sometimes be even higher than at its upstream vicinity. Also, the ice slope at the grounding line tends to be infinite only for the cases in which there is not downstream floating ice at all, which is a very rare case in Antarctica and unusual in Greenland. We believe this misinterpretation ($h \to 0$ and the slope to infinite at the grounding line) is inherited from a rigid view of how the spatial profile of an ice sheet must be, and it frames your concerns regarding our chosen approach for the ice dynamics.

You are not the first ones who face this challenge. For example, in 1980, as a PhD student, I had to present my work to G. I. Barenblatt, and I was honored to be schooled by him about this term. I am not going to advocate here necessarily for the parameterization that was adopted as the result of this conversation (we can always discuss it off-line if needed), but if you want to continue to scale the above mass balance the same way as you did before, then your mass discharge q should be proportional to

$$q \sim \frac{L_{\text{ocn}}}{L^2} vH \qquad \textit{CC3.1}$$

Your proposed equation has, in turn, a really problematic feature: that the ice discharge will dramatically increase when H is small. Because $L = c \cdot H^2$, your suggested equation for q will go as v/H^3, which we believe is not appropriate. Nevertheless, we have tested both the validity of our former equation and the proposed one for q by running a Northern Hemisphere glacial cycle with a state-of-the-art 3D thermomechanical ice sheet model, Yelmo (Robinson et al., 2020). The cycle is driven by a glacial index (e.g. Zweck and Huybrechts, 2005; Banderas et al., 2018) that weights the temperature and precipitation anomalies given by the PMIP3 climate models. Our simulation is shown in Figure CC3.1 in terms of the relative sea level elevation (RSLE) from 120 kyr BP to present day and four snapshots of ice velocities and ice thickness. This simulation was intended to be representative of the last glacial cycle (but not particularly tuned) and uses the same physics that Moreno-Parada et al. (2023), which shows good results in reproducing the last glacial maximum.

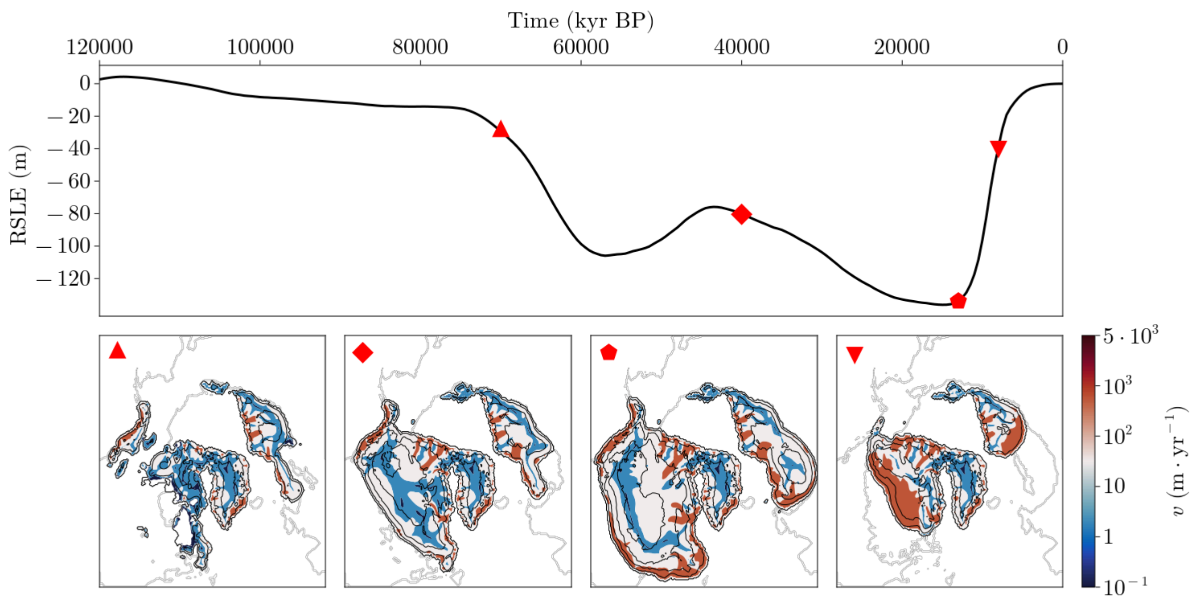

Figure CC3.1. Last glacial cycle simulated using Yelmo. Here we represent the relative to present day sea level elevation (RSLE) and different snapshots marked with their respective symbols of ice velocity (coloured) and ice elevation (the lines represent elevation isolines every 1000 m).

Using the ice velocities and thickness simulated by Yelmo we can diagnose the ice discharge using the two methods explained above (the current PACCO approach and your suggested equation (Eq. CC3.1)), and compare them to the simulated ice discharge by Yelmo, q_{Yelmo}, (Figure CC3.2). In the calculation we set the constants k_1 and k_2 so the diagnosed q are in the best possible agreement with the simulated ice discharge q_{Yelmo}. As can be seen in Figure CC3.2, our approach shows a diagnosed q, which is much closer to q_{Yelmo}. Using the suggested Eq. CC3.1 implies an underestimation of the ice discharge for half of the cycle. Note that, even setting a minimum value of H to 700 m (to avoid an explosion of q when H is small), your suggested equation clearly overestimates q at the beginning and the end of the cycle and underestimates it from the inception to the deglaciation. In conclusion, our approach is appropriate to simulate the mean state of the ice discharge of the ice sheet.

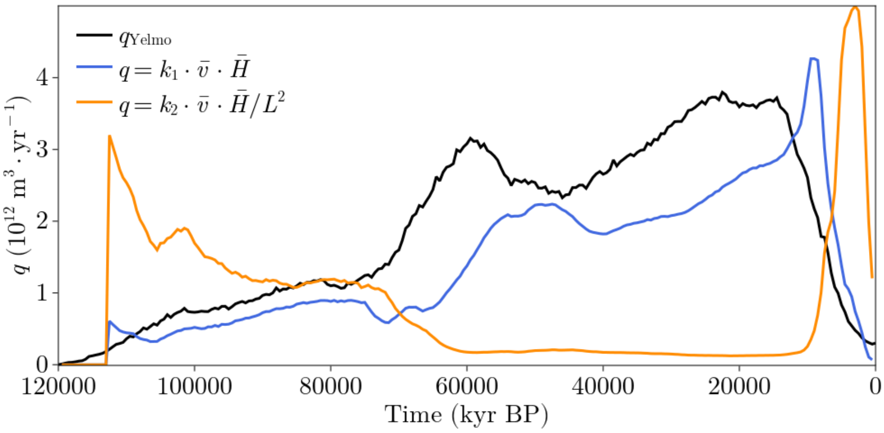

Figure CC3.2. (black) ice discharge calculated in Yelmo compared with the diagnosed ice discharge using (blue) PACCO formulation and (orange) the proposed formulation based on the ice sheet profile.

"2. I am afraid my advocacy of the vertical temperature advection has been misunderstood. Within the basal boundary layer the vertical temperature advection is indeed negligible. What I meant is that regional air temperature T, that goes into calculations of your h_{cond} is not instant $T(t)$ but it is temperature "from the past", i.e., $T = T(t-\tau)$ where $\tau = H/\dot{s}$ is the vertical-temperature-advection timescale, i.e. time of the temperature "delivery" from the ice-sheet free surface to the upper boundary of the basal boundary layer.

Respectfully,

Mikhail Verbitsky"

Thanks for your clarification. We think that the delay is somewhat captured by both the Péclet number and τ_{kin}. On one hand, we now let the advective regime determine the basal boundary layer thickness following your suggestion $H_b = H \cdot Pe^{-1/2}$. On the other hand, our sensitivity analysis (shown in the current version of the manuscript) shows that we explore the enhancement of basal sliding across a large range of kinematic wave times, τ_{kin}. And we find that these timescales represent the delay you propose. The explored phase space would not change substantially given that τ_{kin} already captures the relaxation effect in the "temperature delivery". Ultimately, playing with thermodynamics aims to capture the phenomenon by which the base becomes temperate, and explore the impact of it being higher, lower, faster or slower, in terms of its effects on ice sliding activation. In short, we aim at showing how a thermodynamically activated basal sliding affects deglaciations. And that is already largely explored in the current version of the manuscript and will be even more robust thanks to your comments.

This process has helped us to refine the manuscript and gain further confidence in our formulations, which are of course, just one of many possibilities. We believe PACCO represents a useful model for exploring glacial cycles and hopefully this is clear through its presentation.

Respectfully,

Sergio Pérez-Montero et al.

<u>References</u>

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