General comments

The paper 'Hysteretic evolution of ice rises and ice rumples in response to variations in sea level' by Clara Henry, Reinhard Drews, Clemens Schannwell and Vjeran Višnjević is a modeling study making use of the Finite Element code Elmer/Ice in order to investigate, from synthetic three-dimensional scenarios, the stability of ice rises and ice rumples, as well as the dynamical transition from one flow regime to the other depending on the amount of friction at the ice/bed interface. To this end, starting from an initial steady state corresponding to an ice rise situation, perturbation experiments consisting in cycles of sea level rise and decrease are run solving the full-Stokes set of equations. Obtained initial steady surface velocities on the grounded part are compared to their Shallow Ice Approximation (SIA) counterparts in order to quantify the importance of longitudinal stresses transmitted from the surrounding ice shelf to the grounded ice. Unsurprisingly deviations are significant when basal friction is low, whereas they become negligible in the high basal friction scenario. The transient simulations show that an increase of sea-level induces a transition from an ice rise flow regime to an ice rumple regime in all friction scenarios. However, in the high friction scenario, much higher sea level increase is required than in the other scenarios to switch from the ice rise to the ice rumple regime, and the latter is unstable (i.e. complete ungrounding rapidly occurs). Interestingly, the sea level decrease experiments bring to light a hysteretic response of grounded ice, with the grounded area and induced buttressing effect being systematically lower than in the sea level increase phases when sea level is decreased back to its initial level. Conclusions are then drawn regarding the initialisation of ice flow models as well as the inversion of basal friction parameters.

Overall, the paper is well-written, the proposed methodology is rigorous, the experiments are well-designed, the figures are mostly clear and relevant, the supplementary video is very illustrative, and the conclusions regarding the stability of ice rises, as well as the highlighted hysteretic behavior in response to sea-level changes and associated irreversibility are significant for improvement of the accuracy of sea level rise projections. Therefore, I think the paper ought to be published and I have only a few minor modifications/comments to propose.

My main point regards the logical link that is made between the hysteretic response of the ice rise to sea level rise and the requirement for careful model initialisation (e.g. l.18 or l338-339). I am not completely sure that this association really holds. Don't get me wrong, I totally agree that careful initialisation of models is of prime importance when running transient simulations of the future evolution of ice sheets/shelves. I also agree that *"the dynamics and buttressing effect of ice rises and ice rumples are dependent on the initial geometry prescribed, which is typically unknown" (l.319-320)*. I see clearly the link between the hysteretic behavior and some form of irreversibility: if the system is forced with a given perturbation from a given initial steady state, it does not come back to the same steady state when the perturbation is removed. However, it does not necessarily mean it will behave dramatically differently if you start from a slightly different initial state, as long as the perturbation pattern is similar (i.e. in your case, sea level increase OR sea level decrease). A good illustration of this point is the second cycle of perturbation that you impose for the *low friction* scenario: the initial steady state from which the system starts for this second cycle is different from the one of the first cycle, and yet the dynamical evolution of the grounded area and buttressing effect become relatively rapidly similar to that of the first cycle (dotted lines are 'rapidly' superimposed to solid lines in Figs. 8a-c). Once again, I have the feeling that it is more the history of the perturbation (are we in a sea level increase or decrease phase ?) that is of importance rather than the initial state.

Another point regards the presentation of the SIA model (Sect. 2.3 and 2.4). It seems to me that it is largely inspired from Greve and Blatter (2009), and some notations become inconsistent with the ones that were used to introduce the full-Stokes model in Sect 2.1. See specifics comments.

Finally, there are a few points that, in my opinion, lack of clarity. First, I would write straight away in the abstract that you are running synthetic experiments and not dealing with real-world applications. Second, the fact that the comparison between the full-Stokes and SIA surface velocities is done for the initial steady states only would benefit to be stated more clearly in the text. Finally, although it is never clearly mentioned in the text (unless I missed something), it seems from Figs.3-8-9-10 that the sea level decrease experiments are continued after the initial 0 m level has been recovered. If this is true, this would deserve some explanation in the text.

Below, I list some specific comments.

Specific comments

P2 L30: 'control' → Is that not too strong ? What about 'influence' or 'affect' ?

P2 L46-47: 'simpler ice-flow approximations' → shouldn't it be singular ?

P4 L62: I think there should be a minus sign in front of g as the vertical unit vector is pointing upward.

P4 L64: The 0 should not be bold as the divergence of a vector is a scalar.

P4 L76: Here, the vector $\mathbf{u}$ represents the velocities at the surface/base, which should be precised. There is an additional constraint that you did not mention and that is of importance for the grounded part, i.e. $b(x, y) \leq z_b(x, y, t) \leq z_s(x, y, t)$ where b represents the bed elevation.

P5 L82: How do you tune this 'tuning parameter' ?

P5 L82: 'the closest' → what do you mean by 'the closest' ?

P5 L85: 'no friction' → I find more natural to speak about 'free-slip', but it is a matter of choice.

P5 L91: Why did you choose the 'First Floating' implementation ? You are dealing with Weertman friction law with constant friction coefficients, which induces a discontinuity of friction at the GL on the mathematical level. The proper way to represent this discontinuity in the FEM numerical framework of Elmer is to use the 'Discontinuous' implementation. Using the 'First Floating' implementation, you artificially impose a linear decrease of friction over all the last grounded elements due to the interpolation of friction parameters at the integration points from their nodal values through the FEM basis functions. This choice could be justified by the fact that, on the physical level, one would expect a smooth transition of friction from its value on the grounded part to zero at the GL. If that was your reasoning when choosing the FF implementation, then it would be worth to state it clearly in the text.

P6 L103: According to Gagliardini and others (2016), with such a resolution of 350 m, the GL behavior keeps being sensitive to the numerical treatment of friction at the GL (i.e. First Floating, Last Grounded or Discontinuous implementations). This is an additional reason to give a clear justification for your choice of the FF implementation.

P6 L105: It would be worth precising the order of magnitude of considered time scales here, even if it becomes clearer later in the text.

P6 Eq10: It is strange that you switch from z_s to h and from z_b to b to denote the surface/base elevations. It feels like you have taken Eq. (5.84) of Greve and Blatter (2009) without adapting the notation to your own work.

P6 Eq11: Same remark as above, plus I don't understand the purpose of the subscript h for the gradient operator. First, it does not appear in Eq. (10). Second, h being the surface elevation (your z_s), it does obviously not depend on z so that the gradient operator does only consist in the x and y components. In addition, in the first occurence of the gradient operator in Eq. (11), the subscript is not at the proper position.

P6 Eq12: Here again, the formulation of Greve and Blatter (2009) would require some adaptations to your own case. More precisely, this expression is not directly consistent with the way the basal shear stress is defined in Eq. (8). In Greve and Blatter (2009), the effect of the normal stress N_b on the basal shear stress is explicited in the formulation of the friction law (5.35), while most of the time this effect is hidden in the friction parameter C of the Weertman law. I know that Eq. (13) is intended to establish the consistency between the two formulations, but in that case I think there should be a minus sign in Eq. (13) (or in Eq. (8), but you have to choose), and the exponents p and q that appear in Eq. (12) would need to be defined somewhere. In your case $q = 1$ and $p = 1/m$. I think it would be cleaner to have Eq. (12) formulated directly consistently with Eq. (8).

P6 Eq13: See comment above.

P6 Eq17: Here, the expression of h_0 does not correspond to the one in Eq. (5.117) of Greve and Blatter (2009). Could you double-check ?

P8 Fig. 3: The Fig. is not consistent with the main text of Sect. 2.5: the decrease of sea level seems to continue further below the initial 0 m level (green and red lines), while the text says that "Sea level is then decreased at a rate of 0.02 m a^{-1} back to the initial level followed by a second phase of constant sea level for 2000 years."

P8 L148: "with ice being frozen to the bed" → "mimicking ice frozen to the ground" ?

P9 L162: "characteristic timescale" → I have a hard time to figure out what does this quantity represents physically. Maybe, it requires more explanation or maybe you can simply drop it as it is not used anywhere else later on.

P9 L189-190:"the upstream ice shelf velocities" → it is not very clear what velocities exactly you are referring to, and how you can get a single value of velocity. You give a brief explanation in the caption of Fig. 9, but some precisions would be required here too.

P10 Fig. 4: You should precise that the velocities are the full-Stokes ones (are they ?).

P11 Fig. 5: What is represented exactly ? Is is $|v_{stokes} - v_{SIA}|/|v_{stokes}|$? If yes, wouldn't it be more interesting to represent $|(v_{stokes}| - |v_{SIA}|)/|v_{stokes}|$, so that we could tell when the full-Stokes surface velocities are higher than the SIA ones and conversely ?

P12 L215: "at sea-level displacements of" → this term is confusing as we are in the sea-level decrease phase. What about something like "when sea level is back to 21 and 19m above initial level".

P13 L223: "and independent of the initial conditions" → I am not sure that this formulation is appropriate as you have actually shown that, depending on the initial state, the system might or might not go back to its initial state. I would rather say something like "The hysteresis cycles are now closed with the final steady state corresponding to the inital one".

P13 L226: It is not completely clear that the state obtained after the sea level is back to the initial 0 m level is a "viable state" (you mean a steady state, right ?), as it seems that you are keeping decreasing sea level below this initial reference.

P14 Fig.8: "The crosses represent the results of steady state branches of the transient simulations at corresponding sea levels." → This is not very clear to me. How can you be sure that these are steady states while you keep increasing/decreasing sea-level ?

P14 L253-254: "the ad hoc grounding line positions of the full Stokes model" → I don't quite like this expression as it sounds a bit like if this GL position was fixed arbitrarily, while it is actually the most rigorous way to define it. Indeed, in the hydrostatic approximation there is the assumption that the stress in a horizontal plane is purely vertical and equals the weight of the overlying ice column (i.e. shear stresses t_{xz} and t_{yz} are neglected), which allows to deduce the GL position from the flotation criterion. In contrast, in full-stokes such assumption is not made, and a contact problem must be solved in which the normal stress at the ocean/ice interface (which is not necessarily purely hydrostatic this time) is compared to the sea water pressure.

P16 Fig. 11: 'blue lines' → Do you mean 'solid lines' ?

P16 Fig. 11: 'after an equal increase and decrease in sea level.' → 'after a full cycle of sea level increase and decrease' ?

P17 Fig. 17: Panel (b) is the intermediate friction scenario and not the high friction scenario, right ?

P18 L280-282: I have a hard time to understand your point. Could you explain ?

P18 L303-304: I don't understand this point. Could you reformulate to make it clearer ?

P19 L330: 'full Stokes velocities with SIA velocities' → 'full Stokes steady velocities with SIA steady velocities' ?

P19 L331: 'due to a greater influence of stresses from the surrounding ice shelf' → 'due to stronger mechanical coupling to surrounding ice shelf' ?

References

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Greve R and Blatter H (2009) Dynamics of ice sheets and glaciers. Springer Science & Business Media