General comments

'Simulated dynamic regrounding during marine ice sheet retreat' is a 2D flowline model study, which makes use of the finite element model Elmer/Ice to solve the full Stokes equations of ice flow dynamics in order to compare ice sheet retreat on a retrograde bedrock slope following buttressing reduction for two different friction laws, i.e. the friction law proposed by Schoof (Schoof, 2005) - which is called "cavitation sliding relation" in the manuscript - and the traditional Weertman friction law (Weertman, 1957). The authors find out that depending on the magnitude of the perturbation (i.e. the amount of buttressing reduction relatively to the pre-retreat state) it is possible to obtain temporary "dynamic regrounding" when the Schoof friction law is used whereas the Weertman sliding law produces no "dynamic regrounding" at all for all the tested perturbations. There are also some reflexions about the different surface ice sheet profiles induced by the two different laws and the fact that these different profiles could be compared to observations in order to discriminate one of the two laws as the most plausible.

Overall, the results highlighted in this study are rather interesting, especially regarding the "dynamic regrounding" behavior and this paper ought to be published, yet I have three major criticisms to formulate.

First of all, the influence of the chosen friction law on the grounding line dynamics for a 1HD ice sheet resting on the bedrock shape designed for the MISMIP intercomparison exercise Pattyn and others (2012) has been investigated in great details by Brondex and others (2017). Therefore, I do think that this paper should at least be cited in yours. In particular, Brondex and others (2017) obtain thoroughly different GL dynamics with, respectively, the "cavitation sliding relation" (Schoof friction law) and Weertman friction laws following a loss of buttressing starting from identical steady states so that observed differences can only be due to differences in the friction law formulation; in addition, they also draw some conclusions about the effect of different friction laws on surface ice sheet profiles which turn out to be in line with the findings of Tsai and others (2015) and Gladstone and others (2017).

This lead me to the second major criticism: by construction, the two initial states which are used as the starting points of the perturbation experiments that you lead are not equivalent; one is obtained with the "cavitation sliding relation" and leads to a steady GL located at $x_G \sim 1400$ km (from Fig. 1a, but it would be better if this value was given in the text) whereas the other is obtained with the Weertman law and leads to a steady GL located at $x_G \sim 1430$ km (from Fig. 3a, but here too it would be better if this value was directly given in the text).

On page 4 line 26, there is an explanation on how the "Weertman sliding friction coefficient" (by the way, I think this terminology is confusing: it is either a "sliding coefficient" or a "friction coefficient" and if you refer to the parameter A_S that you use in equation (5) as I think you do, then it is a friction coefficient as τ_b increases when A_S increases): it is said that a spatially uniform value of this coefficient is chosen "to match the value of basal shear stress observed in the cavitation sliding relation experiments at maximum values of ice velocity and height". This sentence is not very clear to me but I can say for sure that with this procedure the steady basal shear stress that you get with the Weertman law at the end of the spinup time differs from the one you get with the law of Schoof (only a spatially varying Weertman friction coefficient can reproduce, with a Weertman law, a reference basal shear stress field initially obtained with a Schoof law, see Brondex and others (2017)).

As a consequence, it is difficult to attribute the differences that you get with the Weertman and Schoof friction laws to differences in the friction law formulations rather than to differences on the initial steady states used as starting points for the perturbation experiments. Therefore, I would suggest to emphasize, in the discussion section, on the fact that the differences observed between the results produced with the two laws could be due not only to different dependencies of τ_b on N and u_b (depending on the chosen friction law) but also to differences on the two initial states built with the two laws.

Finally, the "dynamic regrounding" that you observe with the Schoof friction law seems mostly due to the fact that with this law the bottom ice shelf profile appears to be rather linear whereas the one obtained with the Weertman law exhibits a concave shape. Looking at appendix B, it appears that the value you have chosen for the parameter C, i.e. $C = 0.1$, is rather low (see for example Pimentel and others (2010), Pimentel and Flowers (2011), Hewitt (2013), Leguy and others (2014), Asay-Davis and others (2016), Brondex and others (2017)). I would suggest you to test the sensitivity of your results to the value of this parameter as I would expect that, for higher values of C, the bottom ice shelf geometry becomes closer to the concave profile obtained with the Weertman law, and maybe to such an extent that you do not get dynamic regrounding with the Schoof law anymore. Indeed, I do think that the geometry that you get with the Schoof law is highly sensitive to the value attributed to the C parameter in equation (2): if you think in terms of asymptotic behaviors with the Schoof law, you can see that far from the GL (where N is very high) the Schoof law is almost perfectly equivalent to a Weertman law with $\tau_b \rightarrow A_S u_b^m$ (by the way, the way you use A_S in equation (2) is not consistent with the way you use it in equation (5), see specific comments); in contrast, in a narrow region located right upstream the GL (where N is very low) the Schoof law is almost perfectly equivalent to a Coulomb law with $\tau_b \rightarrow CN$; the horizontal extension of this region depends on the value attributed to C with lower values leading to wider extensions and presumably to stronger differences in ice shelf profiles relative to the ones produced by a Weertman law (the consequences of having a Coulomb friction regime at the GL on the surface ice sheet profile were investigated by Tsai and others (2015) but I don't think there is any conclusions about the ice shelf profiles).

Beside these major points, I have also noted several inconsistencies, especially regarding the formulation of the two friction laws. These inconsistencies are summarized in the "specific comments" section.

Specific comments

From P1 L21 to P2 L1: I don't understand this sentence, is there a problem with the end of it: "and when focusing on retreating glaciers such as Pine Island"

P2 L4: Brondex and others (2017) have also shown that tuning the spatial distribution of the Weertman friction coefficient to match the basal shear stress produced by a Schoof law could also lead, under certain circumstances, to steady GL positions located on the retrograde slope without having to add any lateral stress.

P2 L6-7: I have the feeling that the two sentences about the need for a very fine mesh resolution at the GL are redundant.

P2 L12: "do not cause such strong mesh resolution dependency and do not suffer from the same mesh resolution issues as Weertman type relations" $\rightarrow$ here too I think that the information is redundant. In addition I would suggest to cite the work of Leguy and others (2014).

P2 L22: "even leading to a partial recovery from MISI" $\rightarrow$ it is not clear what is meant by "partial recovery" here, i.e. is it the grounding line stabilizing on the retrograde slope ? Or advancing back to its initial position ?

P3 L11: I think there is a mistake in Eq. (3): it should be a n instead of the m for the exponent of the parameter C. Otherwise, you need to define the parameter m for this equation (but it is defined for Eq. (5) as $m = 1/n$, whereas it should be n for Eq. (3)).

P3 L11 and L19: the way you use the parameter A_S in Eq. (3) is not consistent with the way you use it in Eq. (5). Indeed, a dimensional analysis of Eq. (3) reveals that in this equation A_S should be given in $m\text{yr}^{-1}\text{Pa}^{-n}$ (which does not correspond to the unit you give in Table 1 of Appendix B) while it should be given in $\text{Pa}m^{-1/n}\text{yr}^{1/n}$ in Eq. (5).

The mistake probably comes from the fact that your Eq. (5) is not equivalent to Eq. (13) of Gagliardini and others (2007): they have $u_b = A_S \tau_b^n$ (in this case A_S ought to be called a sliding parameter as an increase of A_S is associated to an increase of u_b) while you have $\tau_b = A_S u_b^{1/n}$ (in this case, A_S should be called a friction coefficient as an increase of A_S induces an increase of τ_b). I would suggest that you read again Gagliardini and others (2007) and rewrite properly all these equations. It could also be worth it to justify the fact that you decided to take $q = 1$ in Eq. (4), or at least to explain what is the role of this parameter. You also need to be clearer on the terminology "sliding parameter/coefficient" and "friction parameter/coefficient": most of the time the notation A_S is used for a sliding coefficient while in Eq. (5) you need a friction coefficient. More broadly speaking, I would rather speak about "friction law" instead of "sliding law" (or "sliding relation") when τ_b is given as a function of other parameters.

P3 L24: once again, the expression that you give for K does not correspond to the original one of Gagliardini and others (2010). In particular, I don't really understand why ρ_i appears in your expression. In addition, in the current state of your manuscript it is impossible for the reader to understand how this parameterisation of lateral drag is included in the global stress balance, which is not straightfoward as we are considering a 1HD ice sheet.

P4 L16: "resulting in a total spinup time of 25000 years" $\rightarrow$ It seems to me that this is not consistent with the description of the spinup procedure given in the previous lines: 16600 years of spinup with 1 m yr^{-1} of top surface accumulation followed by, at least, 10000 years of spinup with 0.3 m yr^{-1} of top surface accumulation as you say that: "We determine that the spinup has finished and the ice sheet has reached a steady state when there has been no change in the grounding line position, and the mesh velocity, determining the change in the top and bottom free surfaces, remains less than 0.001m yr^{-1} over 10000 years"

P4 L19: "an" → typo

P4 L26: as already said, the way you choose the Weertman law friction (or sliding ?) parameter is not clear to me.

P4 L28: you have also run simulations for $W = 400$ km and $W = 500$ km based on Fig. 3.

P5 L11-18: in my opinion, it would make more sense to have this part in the discussion section. In addition, I think that the point discussed here could be better illustrated by a plot of the thickness rates of change $\partial H/\partial t$ as a function of x at different times following buttressing release.

P5 L23-26: as already said, this result might be highly sensitive to the value attributed to the C parameter in Eq. (2). Therefore, I would suggest to run similar simulations with higher values of this parameter.

P6 Fig1: I think you never refer to Fig. 1c in the text, therefore I wonder if it is really relevant to have it here.

P6 L5: "Fig. 3a" → I think you mean Fig. 4a

P7 L6 and P8 L1: "but we cannot rule out the possibility that such behaviour could stabilise a retreating ice sheet for certain geometries." → This is a too strong statement considering the results that are presented in your manuscript for which the transient regrounding obtained with the Schoof law for $W = 350$ km and $W = 375$ km are far from preventing the GL to retreat over the retrograde slope.

P8 L9: "ice shelf ice shelf" → typo

P8 L13: "force balance" → In my opinion, "stress distribution" or "stress state" would be more appropriate

P8 L20: "transition zone" → I don't agree with the use you make of the term "transition zone" in this case. In line with Pattyn and others (2006), I understand the "transition zone" as being the narrow region right upstream the GL over which τ_b progressively vanishes. By construction, the Weertman law with a uniform friction coefficient leads to a discontinuity of τ_b at the GL which is equivalent to say that the length of the transition zone is reduced to 0. There cannot be any "transition zone" within the ice shelf - as you seem to suggest - as $\tau_b = 0$ wherever ice is floating.

P9 L5: this result was already highlighted in Tsai and others (2015), Gladstone and others (2017) and Brondex and others (2017) (the Budd law being investigated in the two latter), and therefore a citation of these studies would be welcome here.

P10 L5-6: "in which regrounding of an ice shelf after retreat has stabilised may occur through bedrock uplift after ice unloading" → I don't understand the meaning of this sentence, is there a problem with it ?

P10 L7: to me, it is not very clear to what timescale you refer. Is it the duration of the regrounding ?

P11 L13: "For each channel width" → This formulation is misleading as the cases $W = 250$ km, $W = 375$ km and $W = 450$ km do not seem to be tested in your sensitivity analysis.

P11 L14: "We conclude that the effects shown here are not dependent on the mesh resolution" → here too the formulation is misleading: if I am correct (based on Fig. 6), the 2 km mesh spacing case shows no regrounding at all even for $W = 350$ km so it is not correct to state that your results are not dependent on the mesh resolution.

P12 L3: "Previous studies into the mesh resolution" → I am not a native english speaker but this formulation sounds odd to me

P12 L4: "eg. (Durand et al., 2009)" → (eg. Durand et al., 2009)

P13: you need to correct the friction law parameters after having rewritten the equations of P3. In addition, m should be 1/3 and not 3.

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