Response to reviewer comments

1. Response to short comments by Rupert Gladstone

Many models for marine ice sheets, such as in the MISMIP experiments, use a sliding relation in which no dependency on basal water pressure is included. Such a dependency is likely to occur in real systems, and sliding relations exist which incorporate the dependency (the main difficulty lies in computing the basal water pressure itself).
The current study uses one such relation (Schoof 2005), and calculates basal water pressure using the ocean pressure as a function of depth. A tunable parameter is introduced to scale the basal water pressure between depth dependent water pressure and zero. This basal drag law is shown to relax the resolution requirements for simulating grounding line migration.
It is good to see work done in this area, as pressure dependence for basal sliding has a strong physical motivation and ways to parameterize it need to be considered.

We appreciate that you have taken the time to informally review our work and have found it useful.

However, what is presented as a new parameterization is really just a scaling parameter in an existing basal sliding law. The main point of the paper, that resolution requirements are relaxed by pressure dependence in the sliding law, is not a result of the new scaling parameter, but is inherent in the existing basal drag law.

We agree that the main point of the paper is that resolution requirements are relaxed by pressure dependence in the existing basal drag law. We think it is fair, however, to call our effective-pressure parameterization (Eq. 15) a new one (although the limits p = 0 and, to a lesser extent, p = 1, have been explored in previous work). In the revised manuscript we have tried to make it clear that while the effective-pressure parameterization is new, the basal friction law of Schoof (2005) is not (see, e.g., the fourth sentence of the abstract).

No physical justification for the choice of ‘p’ is given.

As explained in Section 2.2, p can take values between 0 and 1, with these values representing the range of physically plausible behavior. It is likely that different values of p would be appropriate in different locations, depending on the detailed local hydrology and topography. The revised text (Section 2.2 and the new Appendix A) includes a physical interpretation of p: the basal water pressure is attenuated to a fraction p of the full ocean pressure on the inland side of the transition zone.

Still, the point is worth making, and I'd be happy to see this work published.

The point is made that this approach only affects a region (20km is suggested) near the grounding line, associated with the assumption that H increases steeply inland of the grounding line. Is this really true? What about the Siple coast ice streams, with their gentle profiles, or the Pine Island Glacier, which is pretty deep under the main trunk? Surely the approach taken here will have some impact on sliding far inland?

First, we have clarified (last paragraph of Section 2.2) that although the basal friction is directly affected only near the grounding line, the indirect effects extend far upstream. Second, we have emphasized (e.g., the last few paragraphs of Section 2.2 and the next to last paragraph of Section 4) that our results depend on the particular parameter values chosen. In follow-up work we will explore the dependence on other parameters, including bed slope.

Page 371. Why must the transition occur over a finite length scale. I am pretty sure that you are right, but what is the argument for this? How can you be so sure that there isn't a step change in basal drag in the real system?

We have removed this sentence, since it is not the case that basal friction under real ice sheets *must* be continuous over length scales that can feasibly be resolved in models. In the revised manuscript, we merely state that effective pressure (and therefore basal drag) could vary smoothly where the subglacial hydrology system is connected to the ocean.

Change domain of fig 1 b,c,d to 0.9km – 1.4km? 0.7-0.9 is not really interesting.

We tried this but felt that the original domain was clearest. Little clarity is gained by reducing the domain size by 200 km, and we think it is useful to include the point where the topography passes through sea level.

And perhaps units should be 10ˆ3 km instead of km?

Yes, we have corrected this error.

Presumably the results being analyzed are for steady state geometry? Can you state this clearly?

This is correct. In several places we have clarified that the results are for steady-state geometry (e.g., last sentence of the abstract, beginning of Sect. 3, first paragraph of Sect. 5).

Key point: this is a change to the physics. Glossed over in the conclusions. Not only is this approach easier for models, it is likely closer to the real world.

We agree. The revised Sect. 3 (second paragraph) and Sect. 4 (first

paragraph) emphasize that we have changed the model physics (in a physically plausible way), not just the numerics.

In discussing the resolution requirements, it may be worth citing the Gladstone 2012 Annals of Glaciology paper showing that buttressing can relax the resolution requirements.

Yes, this is a good point. We have cited this paper in Sect. 1 (fifth paragraph) and Sect. 4 (third paragraph).

Like buttressing, the suggestion here of pressure dependence in the sliding relation is a change to the physical problem that makes it easier for current ice dynamic models to solve. Both studies (Gladstone 2012 and the current study) make the point that the real system may be less harsh on models than the original MISMIP experiments!

We agree.

Don't get hung up on saying that 1km is the appropriate resolution. Presumably the required resolution is a function of slope of bedrock near the grounding line? Also (see Gladstone 2012) it is likely a function of buttressing. You can say that 1km was appropriate in this set of experiments, but in different real world situations it may vary.

We agree that the appropriate grid resolution depends on model physics and parameter values. In the revised text (last sentence of abstract, next to last paragraph of Sect. 4, first two paragraphs of Sect. 5) we have clarified that these results are specific to the MISMIP experiments and to the parameters chosen.

Conclusions section is very long. Suggest you separate out into discussion and a much shorter conclusions section.

We divided this long section into Sect. 4 (Discussion) and a short Sect. 5 (Conclusions).

2. Response to reviewer comments by Frank Pattyn

General appreciation:

This novel contribution explores new and physically-based parametrizations for representing grounding-line dynamics in large-scale ice sheet models. While the results are presented for a flowline case, the authors demonstrate that the same functions can easily be transferred to three dimensions. The concept is twofold.

First a new sliding law across the grounding line is presented, taking into account both the effect of basal hydrology and its connection with the ocean. Both aspects guarantee a smooth transition of basal drag across the grounding line. A similar mechanism was presented by Pattyn et al. (2006), but the advantage of the new approach is that it is physically consistent and not an ad hoc parametrization (i.e. one does not need to specify the width of the transition zone). Second, a grounding-line parametrization through linear interpolation is introduced, similar to the work by Gladstone. The combination of both factors greatly improves the reversibility of grounding line response on linearly sloping beds under steady state conditions.

The modeling presented here in sufficient detail is novel and may have a big impact on future modeling initiatives, especially with respect to improving large-scale ice sheet models to cope with grounding line migration.

We appreciate that you find our work useful.

Furthermore, it is very well written and leads the reader through the model and experiments in a clear way.

Thank you.

The conclusions, however, are way too long and should be cut up in a section 'discussion' and a short section 'conclusions', taking up the major findings of the study.

We agree. We divided the old conclusions section into Section 4 (Discussion) and a shorter Section 5 (Conclusions).

The analysis does present a couple of flaws that should be rectified, or at least put in the right context. The new parametrization as a function of p introduces faster flow at the grounding line with larger values of p. This results in different sizes of ice sheets.
For the linear bed slopes, this does not introduce a bias, but it is important in comparing results for the nonlinear beds. Part of the different response across nonlinear bed slopes may be due to the fact that the ice sheet size is influenced by both the value of A and the choice of p.

We agree that the different ice-sheet extents complicate the analysis of the polynomial-bed experiments. We struggled with this issue in the previous version of the manuscript. The rapid variation in bed slope at the grounding line and the strong influence this slope can have on the numerical error (as shown in Gladstone 2012) make it impractical to compare results with significantly different values of x_g.

In the revised manuscript, we have used approximately three times as many values of A over the advance-and-retreat cycle to better sample a variety of

grounding-line positions for each value of p. We no longer attempt to compare errors at common values of A, but rather at common values of x_g. Also, we emphasize as the primary results of these experiments the reversibility (or lack thereof) as a function of p, resolution and the absence or presence of a GLP, as opposed to errors at specific parameter values or locations within an experiment.

The reversibility results are also better for large values of p, but in that case it is not possible to compare the model directly to the boundary layer solution due to Schoof. This comparison is only possible for p = 0.

Yes, as we say in Sect. 3 (second paragraph), comparison with the boundary layer solution is not possible for p > 0, and we have therefore computed a very-high-accuracy benchmark solution using Chebyshev polynomials, which we have validated against the boundary layer solution in the case p = 0.

The reason why the reversibility (even for larger grid sizes) is better for p = 1 is given by the fact that the transition zone at the grounding line is better.

We agree.

It has also been shown by several authors (e.g. Pattyn et al., 2006) that larger transition zones show a better reversibility, but this is because the underlying physical model is different than the physical model for p = 0, as used by Schoof.

We agree that the underlying physical model is different. The revised Sect. 3 (second paragraph) and Sect. 4 (first paragraph) emphasize that we have changed the model physics (in a physically plausible way), not just the numerics.

While in Nature, the smoother change in basal conditions across the grounding line is probably more common than a sharp transition, it is not possible to directly address the result to a novel parametrization that gives a better reversibility.

As discussed above in response to Rupert Gladstone's comments, we have removed the text suggesting that the transition in basal friction cannot be sharp in reality.

It is a different physical model that in any case will assure a better reversibility because of the faster flow (hence response times) at the grounding line. It does not show that the numerical problem is solved at a higher spatial resolution.

We have clarified in the revised manuscript that values of p>0 represent different model physics, as opposed to a numerical improvement (like Gladstone's grounding-line parameterizations).

It is not clear to us that the better reversibility is a result of faster flow or

response time. Because we integrate to steady state at each stage of a given experiment, the model has as long as it needs to respond to changes in the ice softness. In some experiments that we are currently performing for a follow-up paper, we do not see any obvious correlation between faster flow and better reversibility at a given value of p. Instead, we think that improved reversibility is achieved primarily because of better resolution of the transition in basal friction at the grounding line.

In the previous version of the manuscript, we had suggested in the discussion section that values of p ~ 1 could be used throughout the ice sheet as a regularization, allowing coarser resolution everywhere. On further reflection, it is not clear that this approach would work, given that our results show that changing p can lead to an ice sheet with a very different flow rate and geometry, including a different grounding-line location. We have modified the discussion section (third from last paragraph) to cast doubt on the idea that we could simply set *p* to a large value everywhere without the loss of accuracy or realism.

If MISMIP would have run with a higher sliding parameter, the reversibility for the ensemble of models would intrinsically be better.

It is likely true that reducing the value of C in the basal stress would lead to better reversibility for all values of p. Effectively, this is what our parameterization does near the grounding line for p > 0. In a follow-up paper we are investigating the effects of co-varying p and other parameters including C. However, we found that the value of C used in the MISMIP experiments lies near the center of the distribution of C values produced by inversion within a 3D model (BISICLES), suggesting that values that are significantly smaller would not be consistent with observations.

I would suggest that the authors try experiments with a significant higher spatial resolution, comparable to the one that is used by other authors (e.g. Cornford et al. who descend to 200m) for the different physical models, including p = 0, which would prove this point. It would also put the results in a broader perspective, i.e. spatial resolution is an important factor, and lesser constraints on this could be achieved by using a different physical model of sliding at the grounding line (however, with the consequence that the reproduced ice sheets are different in size). Given the fact that it is a vertically integrated flowline model taking up little computational cost, this shouldn't be a problem.

In the revised manuscript (Sect. 3) we present results down to 50 m resolution. These results show that 150 m resolution is needed to obtain reversibility when p=0 without a GLP, and that 120 m resolution is needed to reduce the error when p=0 to ~5%.

At the end of Section 2.2 we have pointed out that the effects of p on the ice sheet geometry extend hundreds of km upstream, with large p resulting in a smaller ice sheet.

In essence, the results of the experiments should be independent of the numerics, i.e., spatial resolution, so that the effect of the two parametrizations can be clearly identified.

We think that our very high resolution, high accuracy Chebyshev solver provides a benchmark solution that is independent of numerics and spatial resolution. The benchmark solutions on their own show how the grounding-line position changes with p for given values of A, C and other parameters. We have tried to clarify this point in Sect. 2.3, where we introduce the Chebyshev benchmark code. Specifically, we added the following text at the end of the 5th paragraph in that section: "We verified the numerical convergence of the Chebyshev benchmark by comparing grounding-line positions with those computed using 2049 modes at various values of *p* and *A*. We found that results changed by at most 50 cm by doubling the resolution, suggesting that numerical errors in the Chebyshev grounding-line position are negligible compared to those from the fixed-grid model."

The new, higher resolution fixed-grid simulations described in the revised text converge to the benchmark solution approximately linearly with resolution, further validating our assertion that the benchmark is an accurate representation of the resolution-independent solution.

So instead of focusing on numerical errors in reversibility on the linear sloping bed, the authors would do a better job in a priori defining an error margin and then repeating the experiments with increasing resolution until the result is within the defined error bars. The output would then be 'spatial resolution' as a function of p and with/without GLP.

This is a good suggestion. We have added plots (Figures 7 and 9) showing the maximum error (for the linear bed experiment) and the root-mean square error (for the polynomial bed experiment) as functions of p and resolution. We have included a curve in these plots showing the resolution below which the error is less than 30 km (representing an error of ~5% of the total change in x_g from most retreated to most advanced states).

Another factor is that the paper is only looking at steady states. Since the authors have a moving grid model at hand that gives a good match with the (steady state) boundary layer theory of Schoof, why not looking at the transients as a function of p and GLP compared to the moving grid model? Or is this for another paper. I think that this would be very interesting, and probably more important to look at IPCC time scales.
Subsequent research efforts in model intercomparison, such as MISMIP3d, focus on such time scales. So the paper does not clarify how good the model performs on such small time scales, especially since very large perturbation (in A) are used, which are physically unrealistic on shorter time spans.

We agree that time-dependent simulations are an interesting avenue for follow-up research. The benchmark code we used for this paper is only capable of computing steady states. However, we have developed a time-dependent (but much slower) benchmark code for use in follow-up studies. We have added relevant text in the last paragraph of Section 4.

Minor remarks:

p376, Line 24-27: to ’within millimeters’. Is this really necessary as a measure. It conflicts with a statement later in the manuscript on p379 where it is stated that ’The grounding-line position in our Chebyshev simulation differs from that of the boundary-layer solution by less than 1.2 km’, which is not millimeter.

We have tried to clarify these remarks in Sect. 2.3 (sixth and seventh paragraphs). It is correct that our benchmark agrees with the Schoof boundary-layer solution to within millimeters when we make the same assumptions: neglecting longitudinal stress in the outer problem and removing the effective pressure N from the basal stress. When we include longitudinal stress and the effective pressure in the basal stress, this introduces differences of ~1 km, but we attribute these differences to approximations in the boundary-layer solution, not to errors in the benchmark solution.

p378, line 26-27: rephrase this without the terms within brackets.

This sentence has been rephrased as follows: “However, we found (not shown) that the behavior of both metrics is qualitatively similar: larger errors in grounding-line position correspond to larger errors in volume above flotation.”

p382, line 5-6: the bias is still a function of spatial resolution, bed slope is an aspect, but resolution issues are still dominant.

You are correct. The bias is a strong function of spatial resolution, going to zero with very fine resolution. We merely meant to imply that the errors at a given resolution appear to be systematic rather than random. Since we are accustomed to “tuning” our models to compensate for systematic biases, it might be tempting to do so in these circumstances. But even if that were possible (and we suggest it would not be on a bed with variable slope), the bias correction would have to be a strong function of resolution as well, yet another reason not to attempt such a correction. In the revised manuscript we decided to avoid this discussion of bias correction in favor of focusing on experiments at higher resolution, as suggested in your general comments.

p385, top para: this is an over-interpretation of the results: reducing p increases the transition zone, hence makes the ice sheet smaller and faster, and will lead to different results because of a different model.

We have removed much of this paragraph from the revised manuscript. However, we think it is worth noting that the model follows the benchmark hysteresis curve with increasing fidelity as both *p* and resolution increase. Although the extent of the ice sheet is different for different values of *p* at the same value of *A*, we have made a point of varying *A* in such a way that all experiments pass through the unstable region during both advance and retreat phases. Therefore, we feel that it is reasonable to compare the ability of the model to capture hysteresis between different value of *p*. The text now states, "Figure 10 shows the error in grounding-line position as a function of the benchmark grounding-line position during the retreat phase. The figure makes clear that the error increases as the grounding line approaches the unstable region. These results suggest that the fixed-grid model can capture hysteresis with increasing fidelity as *p* increases and (to a lesser extent) as resolution increases, and that errors nearly always decrease at a given value of x_g as *p* increases."

p386, line 10-14: p and GLP are two different things; the first one alludes to a different basal slippery model, the second one to a numerical interpolation technique. A high value of p does not mean that you have a small error. Its numerical behavior remains a function of spatial resolution. At high values of p you can obtain reversibility at coarser resolution. That's all. GLP is a numerical aid that produces reversibility in steady state (but may hamper the transient response - the latter has not been investigated in this study).

We agree that this paragraph in the previous revision implied that p was a knob to be adjusted by the modeler in order to attain smaller numerical error. We did not properly address the fact that changing p implies a change in physics. In the revised manuscript, this discussion has been rephrased as follows (first paragraph of Sect. 4): " Instead, with the use of our basal-friction parameterization and assuming good connectivity to the ocean (p~1), we find that a fixed-grid model can yield accurate results at relatively coarse resolution. These improvements do not require modified numerical techniques, such as a GLP, but arise from physically plausible changes in model physics."

p387, line 10-15: similar remark - see also general appreciation.

Again, we agree and have rewritten this paragraph (Sect. 4 paragraph 2) as follows : "Our results suggest that it may be possible to simulate marine ice sheets at much lower computational expense than would be required with traditional friction laws. Models with adaptive and unstructured grids (Goldberg et al. (2009), Favier et.al (2012), Perego et al. (2012), Cornford et al. (2013)) could be made more computationally efficient by reducing the need for very fine resolution near grounding lines. Also, our parameterization might allow uniform-grid models to simulate whole ice sheets, since ~1 km resolution throughout the ice sheet is feasible (though expensive). However, this could require setting p~1 everywhere in the ice sheet, which might not

be physically realistic for some regions."

Figure 6: use 'without GLP' instead of 'No GLP'; explain the dashed line in the figure caption

We have made the requested changes. We now explain the dashed line in the caption as follows: "The dashed line (at 50 km) in each panel shows the location of a transition in scale of the *y*-axis, which allows the same figure to present both very large and relatively small errors."

Figure 5 and 7: a higher resolution experiment would be better, because reversibility holds. If it doesn't, the model is not ok.

We now include our highest-resolution results (50 m) on these figures (now Figs. 5 and 8), showing that the fixed-grid model is reversible with sufficient resolution.

Figure 8: explain dashed line

This is now explained in the figure caption, as above.

1. Does the paper address relevant scientific questions within the scope of TC? yes
2. Does the paper present novel concepts, ideas, tools, or data? yes
3. Are substantial conclusions reached? yes
4. Are the scientific methods and assumptions valid and clearly outlined? yes
5. Are the results sufficient to support the interpretations and conclusions? **not entirely**

We have tried to address the reviewer's concerns in the revised manuscript and hope that the results now sufficiently support our conclusions.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? yes
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? yes
8. Does the title clearly reflect the contents of the paper? yes
9. Does the abstract provide a concise and complete summary? yes
10. Is the overall presentation well structured and clear? yes
11. Is the language fluent and precise? yes
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? yes
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? yes
14. Are the number and quality of references appropriate? yes
15. Is the amount and quality of supplementary material appropriate? yes

3. Response to comments by referee #2

This paper presents a more physically-based approach to regularizing the discontinuity in basal friction across the grounding line than was previously done in Pattyn et al (2006), and shows that a continuous basal shear stress allows convergent results to be computed at lower cost than a discontinuous one. This is a very useful contribution.

We are glad you find our work useful.

There are a few things that I was not particularly keen on in the general description of how the present work fits into the literature. The notion of a "transition zone" is mentioned in multiple places. I would argue that the main "transition" around the grounding line, in terms of large-scale ice sheet dynamics, must be the transition from an extensional-stress dominated flow in the ice shelf to a vertical-shear dominated flow in the ice sheet. That transition occurs whether we prescribe some continuous change in basal friction parameters away from the grounding, as is the case here through the dependence of τ_b on N (and therefore its assumed dependence on H), or whether we keep basal friction parameters constant, for instance by putting $\tau_b = C|u|^{m-1}u$ as in the MISMIP experiments.

(Let me say right away that I understand the confusion that typically arises here. "Vertical-shear dominated" does not have to mean that the velocity field inland is dominated by vertical shear, nor does it even have to mean that vertical shear stress is much larger than extensional stress. What I mean is that vertical shear stress dominates force balance, so terms like $\partial\tau_{xz}/\partial z$ are much larger than $\partial\tau_{xx}/\partial x$, which is ultimately the basis of "shallow-ice" type models.)

The relevant "transition length scale" can be identified as the extent of the boundary layer in Schoof (2007b). No other transition zone is *necessary* though it is clearly possible to invent additional physics that leads to more transition length scales, for instance by having a sliding law that has different asymptotic behaviours for small u/N^n and large u/N^n, as is done here. Those transition zones (here, the transition from a low, Coulomb-like shear stress to a high power-law like one) are presumably secondary to the one transition that remains unavoidable, namely the extensional- to vertical-shear-stress transition. This should probably be reflected in the text.

We rewrote the text to define the primary "transition zone" as the transition from vertical-shear-dominated flow to extensional-stress-dominated flow (first sentence of abstract, second paragraph of Sect. 1). Later (Sect. 2.2) we define the "friction transition zone" for our problem as the region where $0 \le N(p)^n < ku$. The friction transition zone is closely related to the transition zone defined above,

because the vertical-shear-to-extensional transition should take place where the basal friction drops from a large to a small value. We hope the revised nomenclature reduces the confusion.

I'll briefly go further along this path. On page 374, line 6, the paper states that "this simplified friction law leads to a set of equations with an accurate semi-analytic approximation (Schoof, 2007a), whereas the more complex friction law in Eq. (15) does not, to the best of our knowledge, lend itself to a similar semi-analytic solution."

The boundary layer formulation in Schoof (2007b) can actually be rewritten for the present friction law rather straightforwardly, illustrating how the change in friction law introduces additional parameter dependences into the the formula linking discharge Q through the grounding line and ice thickness H_f at that location (which is then no longer computable analytically)
The relevant boundary layer problem would be
$(UH)_x = 0$,
$4(H|U_x|^{1/n-1}U_x)_x - (1+\frac{\upsilon|U|}{H^n(1-H_f/H)^{np}})^{-1/n} |U|^{1/n-1}U - HH_x = 0$,

with
$4H|U_x|^{1/n-1}U_x = 1/2(1-r)H_f^2$ at $X = 0$
$H = H_F$ at $X = 0$
$UH \to 0$ as $X \to -\infty$
$U \to 0$ as $X \to -\infty$
which is identical with the original boundary layer model in Schoof (2007b) except for the term $(1+\frac{\upsilon|U|}{H^n(1-H_f/H)^{np}})^{-1/n}$ appearing in the friction law — which however tends to unity as H -> infinity in the matching region with the rest of the ice sheet, and therefore does not affect the solvability of the boundary layer problem, which could presumably be attacked with the same method as in e.g. Schoof (2012, JFM, appendix), except that the arguments for Q having a power-law dependence on H_f no longer applies.

We are not sure which reference is intended by "Schoof (2012, JFM, appendix)", Schoof was a coauthor on four JFM papers in 2012, but we were not able to find an appendix in any of the four which clearly related to the above derivation (with $\nu = 0$).

However, we fully agree that the boundary-layer theory of Schoof (2007b) could be revised as given above to include our friction law. Presumably the boundary-layer solution could be computed numerically for a given set of parameters, and this may be something that we explore in follow-up work.

We added the following text to Sect. 2.2 (seventh paragraph): "Many models define the basal-friction law throughout the ice sheet to have the form of Eq. (16) as in Schoof (2007a) and the MISMIP experiments. This simplified friction law leads to a set of equations with an accurate semi-analytic approximation (Schoof 2007a,b), whereas the more complex

friction law in Eq. (15) does not lend itself to a similar semi-analytic solution (see Appendix B)." We have added Appendix B, which shows the comparison to the boundary-layer model outlined above.

The main difference with the original boundary layer problem is the appearance of the parameter υ = k[U]/($\rho_i g[H]$), where [U] and [H] are the scales for velocity and ice thickness in the boundary layer identified in Schoof (2007b). This parameter, as much as p, should dictate discrepancies between the formula for Q in Schoof (2007a,b) and the results used here. You would need large ν to have a boundary layer whose length significantly exceeds the boundary layer scale [X] estimated in Schoof (2007b); otherwise the boundary layer size (which I would maintain is the most sensible transition zone extent) will remain the same as that in Schoof (2007b).
This is relevant because the paper focuses on p as the main control parameter in regularizing basal friction, when k is at least as important.

We agree that the nondimensional number $\nu = [m_{max}/(\lambda_{max}A_b)][U]/(\rho_i g[H])^n$ also plays an important role in determining the impact of the friction law on the solution. We added text in Sect. 2.2 (next to last paragraph) and Sect. 4 (next to last paragraph) stating that the grounding-line dynamics is sensitive to κ in Eq. (15).

In this paper we chose to focus on N(p) because it has a physical interpretation in terms of connectivity with the ocean, and we think it likely that this connectivity is found in real systems near the grounding line. It is less clear that κ would vary systematically near grounding lines. We chose the values of m_{max}, λ_{max} and A_b given in Pimentel et al. (2010), giving a non-dimensional $\nu = 1.1 * 10^{-3}$. This value is, indeed, large enough that the modified friction law plays a role in the boundary layer solution.

We plan to analyze the dependence of solutions on κ, among other parameters, in a follow-up paper. However, we added some preliminary analysis (Sect. 2.2, next to last paragraph) that show similar results when κ is varied as we see with different values of p: "The size of the friction transition zone depends on $\kappa \equiv m_{max}/(\lambda_{max} A_b)$ as well as *p*. For this study we chose the values of m_{max}, λ_{max}, and A_b as in Pimentel et al. (2010) and given in Table 2. Since the focus of this paper is on the impact of our effective-pressure parameterization near the grounding line, we defer a full analysis on how variation of κ affect our results at different values of *p* and *A* for a follow-up study. Here we simply summarize what we observed for a specific ice softness $A = 4.6416 \times 10^{-25} Pa^{-3}s^{-1}$. Increasing κ by an order of magnitude introduces a finite friction transition zone of ~1 km when p = 0 and triples the size of the friction transition zone to ~28 km when p = 1. Although the friction transition zone becomes finite when p = 0, the basal friction remains discontinuous across the grounding line. Even so, a larger

value of κ could decrease the model resolution required for small values of p. Decreasing κ by an order of magnitude has no impact on the friction transition zone when p = 0, but halves the friction transition zone to ~ 5 km when p = 1. More generally, as κ goes to zero the basal friction law will asymptote to Eq. (16), regardless of p.”

With regard to the statement “You would need large ν to have a boundary layer whose length significantly exceeds the boundary layer scale [X] estimated in Schoof (2007b)”: Our understanding is that the condition for the boundary layer to have a greater length than [X] is $\kappa u \gg N^n$, which is consistent with what we say in the text. This condition can be achieved either with large κ (or ν) or with small N. From Pimentel et al. (2010) we have $\nu < 1$. It is unclear that ν would systematically increase near grounding lines; and if it did, it is unclear how it would vary by more than one or two orders of magnitude.

The second point that struck me was that the description of the regularization of basal friction was repeatedly described as being rooted in physics. This is true, but only to a very limited extent. I don’t actually think that what is going on here is a particularly good description of water pressure and therefore effective pressure near the grounding line — the main thing that the model in the paper does is to ensure that τ_b goes continuously to zero, while it approaches a more canonical form $\tau_b = C|u|^{1/n-1}u$ inland. This is done by making the "effective" sliding coefficient C_e that gives τ_b through $\tau_b = C_e|u|^{1/n-1}u$ depend on ice thickness through

$$C_e = C\left(1 + \frac{k|u|}{\rho_i g H\left(1-\frac{H_f}{H}\right)^{pn}}\right)^{-1/n}$$

The particular formulation for how C_e goes to zero has H_f is approached is almost neither here nor there, because the dependence of N on thickness is simply made up.
In that vein, I’d be happier if the "parameterization" of effective pressure was simply recast as a "regularization" of the transition in basal friction.

We agree that we may have overstated the extent to which the effective-pressure formulation, Eq. (14) is rooted in physics. In the revised text we have tried to clarify the limitations of the formulation, emphasizing that it is not based on a detailed physical model, but rather is a simple function chosen for its limiting values (e.g., Sect. 2.2, paragraphs 3 and 4, and Sect. 5, last paragraph). We now point out that although the limits of the function (e.g., p = 0, p = 1, $x = x_g$, x far from x_g) are physically motivated, the form of the function is ad hoc.

We agree that Eq. (14) can be regarded as a mathematical regularization, but we think it is also reasonable to call it a parameterization. These two characterizations are not mutually exclusive. A parameterization, as we use the term, is the replacement of “processes that are too small-scale or complex to be physically represented in the model by a simplified process.” (This definition is from Wikipedia.) In our case, we have replaced detailed

hydrologic processes with the idea that there is some degree of connectivity between the subglacial hydrology and the ocean.

In support of the view that (14) is not “merely” a regularization, we note that the choice of p affects the steady-state geometry hundreds of km upstream, as pointed out in the last paragraph of Sect. 2.2.

I could come up with many other simple models that look different, without even involving drainage or thermal physics. Why not for instance assume that water pressure is always equal to that in the ocean at the same elevation as the bed, which would give N = $\rho_i g H - \rho_w g b$, b being downward-positive in the (slightly silly) sign convention of Schoof (2007a). This would have the same property of N = 0 at the grounding line. It would lack the arbitrary control parameter p, which would then be replaced by k.

The expression N = $\rho_i g H - \rho_w g b$ corresponds to (14) with p = 1. We prefer to keep the full expression (14) with parameter p, because this allows us to sample the full range of behavior between p = 0 and p = 1. We think the full range is physically plausible, since real hydrological systems probably have different degrees of connectivity with the ocean. As stated above, we cannot see why κ would vary systematically near the grounding line in the way effective pressure would. Also, we are unsure how to choose appropriate upper and lower limits for κ.

This is not to say that the version of N above is better than the one in the paper and should be implemented (though it seems more obvious) but the verbiage about things being physics based is perhaps a bit too strong.

The version of N above is, in fact, included among the range of p studied in the paper. (It corresponds to p = 1.) We think there is a physical motivation for varying p between 0 and 1, but we have toned down the language about (14) being physics-based. For example, we now call it an “effective-pressure parameterization” instead of a “basal-hydrology” parameterization because it is not a detailed model of basal hydrology.

Detailed points, apologies for any repetitions of the above:

abstract: in my view, the "transition zone" is best viewed not so much where ice lifts off the bed - that would be the grounding line, which in a depth-integrated model is not a zone but generally a curve, and "resolving" that curve is probably not what is meant by "Adequate resolution...". The transition zone is probably more sensibly defined as the region where an extensional-stress dominated flow (the ice shelf) transitions into a vertical-shear dominated flow (the sheet); this is the boundary layer defined in e.g. Chugunov and Wilchinsky (1996), Schoof (2007b). I don’t see any other sensible definition of transition zone if it is a "zone" not a "line" (in which case it may as well be called the grounding line). The Pattyn et al (2006) "transition zone" is a red herring – it is a made-up length scale over which a transition from effectively no slip to free slip is regularized.

We agree, and we have changed the language in the abstract as suggested.

p 364 line 25: The phrase "Full-Stokes" should be struck from the glaciological dictionary. "Stokes flow" refers precisely to the equations Durand et al solve; anything else is an *approximation* of Stokes flow. Also "gold standard" is a weird concept — we're not building cars or houses here. "The Stokes equations contain the fewest approximations of all the widely used ice flow models" would be better (make no mistake; they are still an approximation.)

We have made both changes, as requested. The revised text refers to Stokes models as "the most accurate of the widely used ice-flow models."

page 365 line 6: Bueler and Brown (2009) is actually not a shallow ice model but a hybrid between shallow ice and shallow shelf. I think it is misleading to refer to shallow ice here because - at least as far as I can tell - it is now well-established that a shallow ice model on its own (without some representation of what happens in terms of coupling with the shelf, for instance as per the work of Pollard and DeConto) does not give a sensible representation of grounding line migration.

We agree that a shallow-ice model on its own does not give a sensible representation of grounding-line migration or shelf flow and have revised the text accordingly. We deleted the reference to Bueler and Brown (2009) under shallow-ice, and we added references to Bueler and Brown (2009) and Pollard and DeConto (2012) under the category of hybrid models.

page 365 line 16: "The physically based parameterization...results in further reduction of this error, with the added advantage that the width of the resulting transition zone is essentially independent of model resolution." That did not make any sense to me, for two reasons. a) Define "parameterization". You've talked a lot about different models that differ through the degree of approximation in various stress components and the extent to which they can be depth-integrated and therefore be made computationally cheaper. Your "parameterization" is nothing to do with this. In fact, it is not so much a "parameterization" as a *regularization* of the transition from free slip to frictional slip that happens at the grounding line; it is that transition that leads to the extensional- to shear-stress-dominated flow transition referred to above

We agree that this sentence is confusing, and we removed it.

As stated above, we think that Eq. 14 can reasonably be called a parameterization (in the sense defined in Sect. 2.2, fourth paragraph), as well as a regularization of the transition from free slip to frictional slip.

b) "the resulting transition zone is essentially independent of model resolution" — if you have a convergent numerical scheme for a well-posed partial differential equation model, your solution needs to become independent of model resolution when that resolution is high enough. You seem to be suggesting that this is not the case without your regularization, implying that all previous models are based either on an ill-posed set of partial differential equations, or employ numerical methods that are not convergent. Presumably that is not what you mean to say, so please re-write this part.

Yes, this is not what we meant to say. We removed this sentence. What we meant to say is better expressed in the discussion section (Section 4, end of paragraph 1): “ Instead, with the use of our basal-friction parameterization and assuming good connectivity to the ocean (p~1), we find that a~fixed-grid model can yield accurate results at relatively coarse resolution.”

p 365 line 23: " Both models have the drawback that the accuracy of the grounding-line dynamics strongly depends on grid resolution" As per the previous comment, this may be misleading. "The models have the drawback that very high grid resolution is required for convergence."?

We made the suggested change.

p 365 line 25 ". A tolerance of a few kilometers in the grounding-line location requires a resolution on the order of tens to hundreds of meters" You should probably make it clear that this refers primarily to fixed grid models.

We made the suggested change.

page 366 line 9 "...with the goal of reaching neutral equilibrium..." I am not sure what neutral equilibrium has to do with this; to my understanding there is no indication that marine ice sheets exhibit "neutral equilibrium" in the sense of Hindmarsh (implying a locally non-unique steady state grounding line position)

We changed the text to “with the goal of reaching steady state.”

page 367 line 6 "Another type of basal water channel forms through pressure-induced melt. Channels of this type that form within 50 to 100km of the grounding line are also likely to connect to the ocean (Cuffey and Paterson, 2010)." I’m not sure what you are talking about here (a page reference to Cuffey and Paterson may help). Pressure- induced melt for a start is a thorny subject; all that pressure does is to change the melting point. The melting has to come out of assorted heat fluxes or heat sources — geothermal and frictional heating being the obvious candidates.

In fact, I would avoid talking at all about hydrology to the extent that the paper currently does, as doing so suggests you will actually be modeling the processes that control effective pressure (which would be untrue). As far as I can see, the main argument here is that you expect effective pressure N to be continuous up to the grounding line, where it is zero. This will ensure that the basal shear stress goes continuously to zero as the grounding line is approached, which makes the numerically difficult discontinuity in τ_b go away. Furthermore, this can be done in a way that the sliding law inland agrees with the sliding law used for instance in the MISMIP experiments. That argument could be stated in a single sentence (or maybe two) without getting tied up in extraneous physical processes and observations that actually raise more questions about the model in this paper (for instance, lake drainage is clearly non-steady and there is not necessarily a persistent hydrological syste; if there is channelized drainage, its effects on effective pressure at the bed could be quite localized).

If you do wish to persist with the present list of reasons, beware any spurious arguments about tides making your formulation more appropriate — if you want tides, you might have to start resolving the migration of the grounding line over the tidal cycle and its time-integrated effect on the evolution of mean grounding line position, which presumably starts to involve a rather complicated viscoelastic formulation. I'd want to stay well away from that.

We agree that this discussion of detailed hydrology processes could mislead the reader. We substantially cut this paragraph, including the references to pressure-based melting and tides. The remaining text is intended to suggest the possibility of connections between subglacial drainage systems and the grounding line, so as to motivate our effective-pressure parameterization without suggesting it represents other detailed processes.

page 367 line 28: "Based on this parameterization we give a new definition of the transition zone." - see comments at the start of the review regarding the meaning of a "transition zone"

We removed this sentence. In Section 2.2 (third from last paragraph) we now define what we call the "friction transition zone," which is distinct from (but related to) "the transition zone" as defined above.

page 368: I'm not a fan of the $\tau_l + \tau_b + \tau_d$ notation. Even though τ_l has units of stress, it is actually the divergence of a depth-integrated stress. Equation (2) also obscures the fact that we are looking at a second-order elliptic problem for u.

Also, the sign convention for τ_b is odd, as it requires the sliding law to be stated with a minus sign throughout the rest of the paper. I'd suggest switching τ_b to - τ_b everywhere to agree with standard glaciological usage.

We agree that although τ_b and τ_d are stresses, τ_l is actually the divergence of a depth-integrated stress. We changed the notation accordingly. We replaced τ_l with $(H\tau_l)_x$, where in the new expression, τ_l is the vertically averaged longitudinal stress. We also switched the sign of τ_b as suggested.

pages 372/373: "When Hf/H << 1 and the bedrock is below sea level (b > 0), the fraction of the bed with water-pressure support approaches p" — how do you define "the fraction of the bed with water pressure support"? And why is it clear that p is equal to that fraction? Or is it simply convenient to interpret p as a fraction if it lies between 0 and 1? This is reiterated later (p 353 l 15), but if you want to stick with that characterization, you really need to explain it better.

We agree that a better explanation is needed. We added a short mathematical Appendix A to show that in the stated limit, the basal water pressure is attenuated to a fraction p of the ocean pressure at the depth of the bed.

p 373 line 16: . "Our model represents only the portion of water- pressure support related to the ocean; basal water pressure in the model falls to zero when the bedrock reaches sea level (b = 0). More sophisticated models of basal till find that the basal water pressure remains a significant fraction of the overburden pressure in much of the ice-sheet interior (Tulaczyk et al., 2000b; van der Wel et al., 2013). A more complex model might include a network of channels as well as water-laden till at the base of ice streams. This hydrological network would influence the basal friction through water-pressure support outside the transition zone."

This kind of misses the point. You are *not* modeling any of the processes that could conceivably control effective pressure, but simply imposing a form of N that ensures
N approaches zero continuously as the grounding line is approached, and therefore basal shear stress goes to zero continuously. To pretend that the paper does anything more than that is disingenuous. Just state what you do, what you don’t do, and leave it at that. Start mentioning papers like Tulaczyk et al etc and readers may legitimately ask questions about things like surges, the thermal state of the bed etc.

It is true that we are not modeling the processes that control effective pressure. Rather, we have put forth a simple parameterization that can be physically interpreted in terms of connectivity to the ocean. We refer to these more complicated models in order to be clear about what our parameterization does *not* do, but might be done in a more sophisticated model. In the revised text we have tried to be more clear about what processes are and are not parameterized (see the fifth paragraph of Section 2.2).

page 373 line 26 "This formulation does not require the introduction of an arbitrary length scale of basal transition, as in the parameterization proposed by Pattyn et al.(2006)" — Again, a bit of a fixation on the Pattyn et al concept of "transition zone", see comments at the start of the review.

We think it is fair to state that our effective-pressure parameterization (14), combined with the basal-friction law (15), does not introduce an arbitrary length scale. This is an important difference between our paper and Pattyn et al. (2006).

page 374, line 6, "This simplified friction law leads to a set of equations with an accurate semi-analytic approximation (Schoof, 2007a), whereas the more complex friction law in Eq. (15) does not, to the best of our knowledge, lend itself to a similar semi-analytic solution." — see comments at the start of the review.

We added Appendix B to explain briefly why the more complex friction law cannot be solved with a boundary-layer solution like that of Schoof (2007a,b).

page 376 line 21 "The Chebyshev code produces grounding-line positions that match the semi-analytic solution of Schoof (2007a) to within millimeters (when the appropriate terms are neglected)." — you are about to repeat this in the next section, I would discuss the matter there. See immediately below.
page 378 line 6 "We configured our benchmark code with the same simplifying assumptions, and found that we were able to reproduce grounding-line positions from Model A to within fractions of a millimeter (the error tolerance of the Chebyshev solver). When we include the full longitudinal stress in the Chebyshev model, we found that differences with the Model A grounding-line position increased to 1km." Two points:
i) The second result is actually more important than the first; you are demonstrating that model A works remarkably well for an asymptotic model in reproducing steady states of the depth-integrated marine ice sheet model it is meant to approximate. This is perhaps worth pointing out because some of the previous literature is rather confusing on this matter: Durand et al (2009) take a rather more negative view of the performance of model A based on its discrepancies with grounding line positions computed from a Stokes flow solver. What the result reported here shows is that that discrepancy is not primarily the result of the asymptotics done in Schoof (2007b), but because of the difference between a Stokes flow model and the depth-integrated approximation that is the basis for Schoof (2007b) as well as for the present paper. This is actually relevant for the interpretation of the MISMIP results in Pattyn et al (2012, 2013), where the sources of discrepancies between numerical solutions are perhaps not identified as clearly as they could — some are due to numerical error (which is avoidable) while others are due to different model formulations (which is unfortunate but cannot be improved on by better numerics)

We agree that the comparison between the benchmark model and Schoof Model A warrants more emphasis, that Model A performs remarkably well against the benchmark code, and that the differences between Model A and the Stokes flow model likely arise because of the differences in stress approximation, not because of the asymptotics. We rewrote this discussion (see the last several paragraphs of Section 2.3) to clarify the reasons for these differences and avoid redundancy.

ii) It is not clear how the code is reconfigured "to the same assumptions" as in Schoof (2007b) — in that paper, there is no magic line at which extensional stresses suddenly go to zero; the whole point of an asymptotic solution is that extensional stresses naturally tend to zero when moving inland from the grounding line, and that a length scale for the associated decay can be identified. There is no "assumption" that they *are* zero inland however — just that they are sufficiently small to be neglected at a suitable order of approximation, which can be identified in terms of the relevant small parameter (in the notation of Schoof (2007b)

We modified the text (near the end of Sect. 2.3) as follows: “In order to give us further confidence that the benchmark solutions are accurate, we compared the Chebyshev results with the semi-analytic boundary-layer model from Schoof (2007a) known as *Model A*. We can reproduce the grounding-line position in *Model A* to within fractions of a millimeter if we neglect longitudinal stresses, use the friction law from Eq. (16) and apply boundary conditions given by Eqs. (9) and (13). (This approach can be used to reproduce the grounding-line position from *Model A* but not the velocity and thickness solutions.)” We hope that this clarifies how we configured our model to reproduce the grounding-line position of Schoof Model A.

page 385, line 9 :"The largest errors occur near the local maximum in bed elevation at around $x = 1.25 *10^3$ km" — you might want to state here that this is completely predictable if you believe the Weertman (1974) argument about stability. A steady state grounding line near that maximum effectively signals that the system is near a bifurcation, at which the steady state near that maximum will be extinguished if A is decreased further (a kind of saddle-node bifurcation); in the vicinity of a bifurcation, solutions will always be more sensitive to changes in parameter values (in fact, infinitely sensitive at the bifurcation).

Thank you for clarifying this point. We modified the text (last paragraph of Section 3.2) as follows: “The largest errors occur near the local maximum in bed elevation at around $x = 1.25*10^3$ km, and decrease sharply as the bedrock steepens further into region 3. This behavior is to be expected as the grounding line approaches a bifurcation point. No stable steady-state solution will exist near that maximum if A is decreased further; small changes in A will lead to large changes in grounding-line position.”

page 366 line 12: "we have shown several advantages of a novel, physically based basal-hydrology parameterization together with an appropriate basal-friction law." — there is a certain point of view here that it’s worth being clear about. The "advantage" corresponds to *assuming* different physics. It is true that the physics in all ice sheet models is presumably wrong, but to hail the approach here as an "advantage" is actually not to say "we have found a better way of solving a problem that has presented problems in the past" but to say "we have found a different problem that kind of looks the same but is easier to solve". Which is fine, but it’s important to be clear about the difference. This also applies to the last sentence on page 386, where suddenly numerical method (applicable regardless of details in the formulation) are put on the same footing as actually changing the problem that is being solved.

This permeates most of the rest of the conclusions. At no point is there any mention of the need to figure out how basal hydrology actually works and how rapidly N changes away from the grounding line. Without any real knowledge of that, the present formulation has a semblance of better physics, but really (in my view) amounts to little more than a regularization of the discontinuous jump in basal traction — which is very similar to what Pattyn et al (2006) did, although with a bit more physics, and the advantage that sliding does not go to zero inland but rather that a canonical sliding law $\tau_b = C|u|^{m-1}u$ is eventually "reached".

We modified the text to clarify the differences between the GLP (a modified numerical method) and our parameterization (a change in physics). See, e.g., the first paragraph of Sect. 4: "Instead, with the use of our basal-friction parameterization and assuming good connectivity to the ocean (p~1), we find that a~fixed-grid model can yield accurate results at relatively coarse resolution. These improvements do not require modified numerical techniques, such as a GLP, but arise from physically plausible changes in model physics."

We also agree that it is important to better understand how basal hydrology behaves in the vicinity of the grounding line. The last paragraph of Sect. 5 now emphasizes the limits of our model and the benefits that might be derived from a more detailed physical model.