We would like to thank all the reviewers for excellent reviews!

We found number of issues raised by the reviewers very helpful and the paper has benefitted from number of very constructive comments.

A detailed point-by-point reply to all comments raised is given below.

**Referee: H. Seroussi**

**The manuscript “The stability of grounding lines on retrograde slopes” presents an example of a stable grounding line for which a section rests on a retrograde bed slope using three-dimensional and vertically integrated two-dimensional models. One dimensional flow-line models showed that grounding lines are unconditionally unstable on retrograde bed slopes. Here, with two and three-dimensional models, the authors show that this statement does not hold as some configurations of stable grounding lines do exist on retrograde slope.**

**This manuscript could lead the community to reevaluate the West Antarctic Ice Sheet Instability that was based on the assumption that the grounding lines located on retrograde bed slopes were always unstable. While this statement is generally true, there might be some locations where the grounding line might actually be stable. The manuscript is generally clear and the methods well described, the figures and references appropriate. The methods and results are stated clearly in a well-written text. I therefore recommend this manuscript for publication after addressing the few changes described below.**

We thank Helene Seroussi for her kind words.

**In the Numerical models section, you mention several purely numerical aspects, such as using linear, quadratic or cubic elements for Úa or the importance of mesh resolution. However, these aspects are never discussed in the Results or Discussion sections. I would have liked to see a paragraph in which you discuss the numerical aspects of the model and answer questions like: What level of mesh resolution was required to avoid mesh dependency? How long does it take to reach the steady-state? What is the impact of element type (linear, quadratic, cubic) in Úa? What is the influence of the initial conditions?**

It is true that we did not discuss in detail the effects of various numerical details of individual models on the results. In some way we feel we have addressed these issues by showing that two different numerical models produce almost identical results. In particular, both models give steady grounding lines on retrograde slope and this is the key result of the paper. We furthermore point out that both models participated in the recent model intercomparions MISMIP3D.

With Ua, using 59 669 elements with 120 100 nodes, with median, maximum and minimum element sizes of 3 125 m, 20 080 m, and 1 452.6 m, respectively, instead of 228 537 elements with 457 340 nodes, with median, maximum and minimum element sizes of 569 m, 26 862 m and 86.7 m, respectively, caused a shift in the position for the grounding line along the medial line from 1092 to 1083 km.  In other words, changing the minimum element size by about a factor of 17 caused a about 10 km shift in the position of the grounding line.

The initial condition has no effect on the solution. We run towards a steady state and we never encountered an example where the model would converge to different steady state depending on initial condition.

**In your simulations, as you mention in the text, only a section of the grounding line is located on retrograde slopes. Is it possible to have the entire grounding line on retrograde slope ? Do you think this stable grounding line on retrograde slope is something unusual due to the particular configuration with a deep trench in the middle of the bedrock and much higher bedrock on the sides ? Or do you think it could be something pretty common that was not noticed earlier as models were mainly relying on flow-line models ?**

These are interesting questions that we are not really in a position to answer fully. In the paper we show specific examples of grounding lines located on retrograde slopes, and that at least one such example CAN BE constructed is pretty much what the paper is all about. We do not try to give an overview over all possible such situations and we expect that doing so might be very difficult.

**A last point I am a little bit concerned about is the grounding line break up shown in Fig. 2. This pattern seems surprising and you mention that it is not a model or figure artefact. Do both models (Elmer and Úa) lead to this kind of break up ? Do you have the same pattern for other channel widths ? It seems to be caused by the very deep channel and the sudden variation in bedrock topography. Could you elaborate on this point.**

This is not an artefact, although at the same time the exact pattern will depend somewhat on grid resolution. At these locations the ice is either grounded or very close to be grounded over a region that is a few km wide and about 10km or so long. Consequently, the area breaks up in small regions of grounded patches. This is perfectly understandable given the flow pattern in this region where the ice move approximately tangential to the grounding line with the result that ice from the interior region of the ice shelf is advected sideways across this region. As the thicker ice in the centre region of the ice shelf is advected towards the margins and across the higher lying parts of the ocean floor, it hits the ground at a number of locations, giving rise to these isolated patches of grounded ice.

Helene Seroussi also made a number of technical comments and we made all changes as suggested. As suggested we also added a few lines on the automated remeshing algorithm used in Ua.

**Anonymous Referre #2**

**My major concern is the use of the word “stable” instead of “steady state”. The grounding line would be stable if a steady state grounding had been perturbed by a change in accumulation, sea-level etc and reached a new steady state or equilibrium configuration. The paper has successfully shown that steady state configurations on retrograde slopes are possible, but not that they are stable.**

We are a bit puzzled by this comment. By the very definition of stability it is impossible that a solution will converge with time towards an unstable solution. However, rather than arguing this point we have now added one figure that shows that if the steady-state solution is perturbed by changing the slipperiness and then changing it back to its original value, the grounding line position converges back to its previous position. This will hopefully convince Referee #2 that the solutions are stable with respect to perturbations in this model parameter as well. As we repeatedly state in the paper the solutions are clearly stable with respect to perturbations in thickness, because we start with a different thickness distribution, and the solutions then slowly converge towards the steady state solutions presented.

**The paper would be stronger with a few more sentences on how the findings differ with previous work, and a few more sentences on your actual results. For example, the only steady state of Goldberg et al. (in their Fig 12) which is obtained with parameters similar to Dupont and Alley, has a width which is similar to some of your experiments, and bed slope of 0.3. Thus, what is the bottom slope of your steady states? The other experiments of Goldberg et al. where no steady states on a retrograde slopes where obtained had a smaller bed slope and larger width than Dupont and Alley: : : Do you have a feeling whether it is the bed slope that is more important compared to the width? What is the shape of your grounding lines for all of your steady states? A simple map-plane view of your grounding lines for the different half-width could be placed in your figure 5.**

The bed slope along the grounding line varies from positive (reverse slope) to negative, and it is not clear how one can quantitatively state that bed slope is more important, or less important, than width.

We have added a sentence making it clear that Goldberg has already presented results with a steady-state grounding line located on a retrograde slope.

**P2600, L2-4: “It is unclear what three-dimensional geometrical configuration, if any, used by Dupont and Alley (2005) in their example.”. I would remove this sentence as it does not add anything to your text, especially since Goldberg et al have managed to reproduce a similar configuration (their figure 12).**

We disagree with reviewer #2 on this point (more about this later) and have kept the sentence. It is not clear what geometry would give rise the prescribed side drag and ice-front pressure prescribed in the Dupont and Alley paper and there is no guaranty that such a geometry exists.

Reviewer #2 makes a number of other technical comments that we have taken into consideration. We have however not changed `stable' to `steady-state'. As mentioned above our steady-state solutions are stable. If they were not, our numerical models would clearly not have converged with time towards those solutions.

**Comments by R. Walker**

R. Walker finds our comment that "It is unclear what three-dimensional geometrical configuration, if any, gives rise to the type of prescribed side drag used by Dupont and Alley (2005) in their example" puzzling and unduly negative. He points out that Dupont and Alley used, in addition to the prescribed side drag, a prescribed buttressing along the ice front.

Answer: We still find it unclear what geometry will give rise to the side drag/front resistance in flow-line models of this type, and we stick to our original statement that there is no guaranty that any such geometry exists. The fact that Dupont and Alley not only used side-drag parameterisation but also prescribed the buttressing at the ice front only confounds the problem.

Using a flow-line model to study the effects of transverse variations on stability is always going to be problematic. Even more so when the effect studied does not exist in a strict flow-line setting. The results then become entirely dependent on the parameterisation used.

One should only use a flow-line model to address 1HD flow problems, or when deviations from strict 1HD setting are not expected to affect the results significantly.  Using a flow-line model to study process that does not exist in 1HD is simply not a very good idea.

The side drag parameterisation assumes that the effects of transverse geometry can be accounted for by using a given channel width along the profile, for both the grounded and the floating sections. But what is this width? In our model the width of the channel does not change with distance, but the width of the ice shelf does.  So what is the width to be used when parameterizing the side drag?

And if one were to use a fixed width in the side-drag parametrisation as done in Dupont and Alley, what kind of transverse geometry would give to that type of side drag?  Clearly constant channel width does not, as our example shows. Does such geometry exist? We think asking this question is entirely legitimate. We therefore stick to our original wording.

**Interactive comment by G. Jouvet:**

**This study addresses the problem of the existence of a steady Grounding Line (GL) on retrograde slopes in two horizontal dimensions (2HD). In the 1HD case, GLs on an upward-sloping bed are well-known to be unstable to small perturbations, this statement being supported by theoretical arguments. However, in the 2HD case, the same argumentation does not hold anymore because of possible buttressing effects, leaving open the existence of such stable GL. This paper provides an example of stable GL that is partly lying over a retrograde bedrock. To build such an example, the authors consider a channelized bedrock. On the sides, the GL – localized on an upward-sloping bed – is stable and sufficiently well-affixed to sustain the GL over central part (the channel) stable even if the bed is downward-sloping in this area. The authors use two different models (including the most accurate one, Stokes) to verify that the solution is not dependent on the model. This is an interesting and well-written paper which presents original results. I recommend to accept this paper for publication after addressing or answering the listed points below.**

Answer:
We thank G. Jouvet for his assessment of the paper.

**The chosen channelized geometry shows very sharp transitions of the bedrock along the y-axis. Taking a smaller fc would change the channel into a V-shaped valley and smooth the bed in y. In that case, I expect less buttressing and then a narrower range (or maybe empty) of wc (like on Fig. 5) that allows for a stable GL that is partly lying on a retrograde slope. It would be interesting to add few runs to study the effects of parameters fc and dc since there are directly related to the "level of ice-shelf buttressing at the GL" (line 276).**

We agree that further parameter studies would be of interest. However our intension here is simply to show that there ARE possible steady state solutions on retrograde slopes. We do provide such an example, and that is really what the paper is about.

**I believe that a part of the model is missing: nowhere it is written that the Weertman sliding law (Eq. (9)) applies under the grounded part only, and that a perfect sliding applies under the floating part (Eq. (9) in Pattyn and al, 2012). Did I miss something ?**

For the floating part the basal resistance term is dropped in the vertically integrated model.

**Since both models have been already published (lines 157 - 158), I wonder if they need to be re-described with this level of accuracy. Number of equations could be efficiently replaced by words and references. Also, it would be clearer to decouple/separate the full Stokes and the vertically integrated models by describing them successively, and not in mixed way.**

It is always difficult to find the right balance when it comes to describing a numerical model that has already been described in other papers. One of the other reviewers wanted a more detailed description. We have not taken out any part of the model description as it is presumably better to give too much information than to little.

**Lines 134 - 136 : I would have expect the vertically integrated ice flow model to be naturally coupled to the vertically averaged mass conservation equation dh/dt+ div (uh)= a and not to the local mass conservation equation (kinematic boundary condition, Eq. (13) in Pattyn and al, 2012). Was this choice made for the sake of the comparison between both models ? Do you have a reference for such coupling ?**

You are of course right that the vertically integrated model used a vertically integrated version of the mass balance. This should presumably be clear to most readers. For the sake of brevity we did not list the full set of model equations and boundary conditions.

**Lines 205 - 206 : The model comparison would be even more convincing if Elmer and Úa's runs would have been performed independently. Indeed, starting one model from another one might influence the results and skew the comparison. Moreover, multiple steady state states are not excluded since no theory exists in 2HD. Do models agree even if both were run by starting with an initial constant ice thickness?**

The Elmer/Ice runs took a very long time and to start the runs with a solution far away from the expected steady-state solution would only have made the runs even larger.

**Lines 207 - 209 : The profiles across the central section agree well, but do the whole GLs and velocity fields also agree ? Additional data would be valuable for comparison.**

One could of course look at other parameters as well. However, it is not our intention to do a full model-model intercomparision here. Only to show that grounding line positions obtained with these two very different models agree.

**Lines 222 - 226 : Is the GL "protuberance" the consequence of the very abrupt channel walls, does it vanish when smoothing the bedrock in y ?**

We expect that you might be right here, but we have not done any numerical experiments to investigate this.

**Line 230 : Why is the ice thickness forced to be slightly positive and not positive ? Otherwise Eq. (5) degenerates ? (or for Stokes, you need to restrict the domain of computation only where the thickness is positive ?). Could you, please, give details the "numerical reasons" ?**

We could in principle have forced the thickness only to be positive instead of slightly positive. This would not have affected our results but the constrained minimisation problem that we need to solve at each time step would have taken longer to solve. If the thickness is allowed to become zero, then one can potentially end up with small patches of ice that are detached from each other and the system becomes singular.

- **Lines 231 - 257 : This paragraph (even if of interest) looks beside**

**the point or at the wrong place. Indeed, a substantial part concerns the model description. I encourage the authors either to restore the first part (line 231 - 240) into the dedicated part "Numerical model" or to postpone the whole paragraph in appendix since it is not essential for the paper.**

We do find that it is presumably better to keep this paragraph in this place within the paper (in the result section rather than in the model-description section) because it is only relevant to the paper because of some of the aspects of the solutions themselves.

**Lines 256 - 258 : Regarding to the example of steady ice sheet you have built, it would be of great interest if you could give rise to "longitudinal stresses decreasing with respect to the ice thickness" in a figure, if this is possible.**

This is an interesting point, and in fact the subject of a follow-up paper that has now been submitted to TCD.