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Interactive comment on "Dynamic influence of pinning points on marine ice-sheet stability: a numerical study in Dronning Maud Land, East Antarctica" *by* L. Favier et al.

Anonymous Referee #2

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1 General statement

The manuscript "Dynamic influence of pinning points on marine ice-sheets stability: a numerical study of Dronning Maud Land, East Antarctica" studies the impact of the presence of pinning points on outlet glaciers stability and their contribution to sea level rise. It shows that pinning points provide additional buttressing and therefore decrease the ice discharge by about 10%, but also that their presence strongly affects the ice shelf rheology inferred from data assimilation and therefore the model initial conditions. The authors suggest that including or omitting these pinning points impacts grounding line retreat and collapse of promontory on the timescale of several hundreds of years.

The manuscript is well written and the figures appropriate. The main point missing in the paper is that the authors do not show that their results are not resolution dependent. This can be easily done by rerunning a couple experiments with a higher mesh resolution and must be done in order to be confident that the results presented in this paper are robust. The interpretation of the impact of the melting is also quite ambiguous, as it differs in the discussion and conclusions. The section below describes in more details these two points and a few other specific comments.

2 Specific comments

p.1 l.16: "collapse" does not seem to be an appropriate word to describe a rather natural phenomenon that happens during simple grounding line retreat. This is also quite different from what is presented in the results.

p.2 l.11: The statement of ice rises not being detected by satellite observations is surprising: measurements of velocity and grounding line have a resolution of a few hundred meters while observations from altimetry also have an along track resolution a few hundred meters and the tracks are spaced by a few kilometers. Only bedrock topography does not have the required resolution, but sounding radars are operated on airborne and not satellite. So satellite observations, and in particular grounding line mapping should have the capability to resolve small ice rises and pinning points.

p.2 l.34: The L-curve analysis is described in more details in Jay-Allemand et al. (2011) and not Gillet-Chaulet et al. (2012).

p.3 l.13: The eastern ice shelf of Thwaites Glacier experienced a complex behavior during the past coupled decades, with successive periods of acceleration and deceleration (Mouginot et al., 2014) that are not coherent with the gradual grounding line retreat and unpinning of the eastern shelf ice rise (Rignot et al., 2014). Entrainment of the Eastern ice shelf by the main ice shelf and changes in the region between the two parts of the ice shelf in this zone of intense shear is the preferred scenario to explain the complex changes observed and not a simple acceleration of this ice shelf (Mouginot et al., 2014).

p.4 l.25-30: How sensitive are the results to these two additional excavations?

p.7 l.7: Why limit the resolution to 1km? BISICLES does not have problems using improved resolution and the domain simulated is small enough that increasing the resolution to 500 or 250 m should not too much or a problem. This is especially surprising at this manuscript focuses on the impact of small ice rises and pinning points. Authors would need to show that their results are not resolution dependent by performing a couple of the simulations that experience large changes with a grid resolution divided by two (500 m or less at the grounding line).

p.7 l.16: How are the inversions of C and ϕ performed? Are they done simultaneously or one after the other? Are they done over the same region of different parts of the domain? This is not clear form the text and is an important question as changes in friction and stiffening factor can have a similar impact on ice flow.

p.8. l.10: How large is the melt rate in this region? It is surprising to see that adding just 1 m/yr drastically change the grounding line evolution from rapid retreat to small advance. This suggests that the model is very sensitive to this parameter.

p.12 l.17-18: This statement contradicts what is shown on Fig.7. The type of melt rate applied seems to play a significant role in at least the rate of grounding line retreat as the three upper plots with melting type M_{b1} all have a similar grounding line evolution, while the three lower ones with melting type M_{b2} also have a similar behavior, distinct from the previous one. And this is actually quite different from what is summarized in the conclusions.

3 Technical comments

p.2 l.6: “to more investigated” → “to be more investigated”

p.2 l.32: “overmatching” → “over fitting”

p.3 l.30: Which field measurements?

p.4 l.2: How do the first decades of the simulations compare with the observations that we have of the past couple decades?

p.4 l.3: How were these melt rates chosen? How do they compare with observations?

p.4 l.32: Authors should quickly summarize the model or observations that were used to derive this surface mass balance, and the year that is reproduces.

Fig. 2 caption: Notations should be consistent: B/S or B_e/S

p.7 l.24-25: Not clear. Simply say that you use the improved velocity and the standard bedrock topography maps.

p.8 l.19: What are the values from Rignot et al. (2013) and Depoorter et al. (2013) and what are the values used in the simulation for the different ice shelves? A table with the melt observed and used for each ice shelf could help make this comparison.

p.9 l.8: “mismatch” → “difference”

p.9 Fig. 3: This figure would be much clearer with colors.

p.11 Fig.5: Same as Fig.3, would be better with colors.

p.11 Fig.5 caption: “Velocity absolute” → “Absolute velocity”

p.12 l.7: There are many other very relevant references for the collapse of the WAIS.

p.12 l.13: “In total” → “Overall”

p.16 l.9: Weertman, capital missing

4 References

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