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Interactive comment on "Sensitivity of the dynamics of Pine Island Glacier, West Antarctica, to climate forcing for the next 50 years" *by* H. Seroussi et al.

Anonymous Referee #2

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Authors, using the Ice Sheet System Model (ISSM), propose a sensitivity analysis of the dynamics of Pine Island Glacier following surface mass balance, melting and calving perturbations. This is a well written manuscript, with results apparently in line with most recent published work. This is original in the sense that this is the first time ISSM model is used with a mobile grounding line on an actual glacier and presented work may confirm or infirm precedent results. With that respect, this paper would clearly deserve publication as more studies on Pine Island Glacier must be done to better apprehend its forthcoming future. I however have one essential criticism detailed below that must be answered before publication could be envisaged.

As mentioned, this to is my knowledge the first time a dynamic grounding line is used

in ISSM on an actual drainage basin. However, very (too) few details are given to be fully confident that the grounding line dynamics is modeled with enough accuracy. And estimating the contribution of PIG to SLR in the future is essentially a question of grounding line dynamics. In particular, I fund astonishing to not see any figure showing grounding line positions during and at the end of at least one simulation! Regarding this issue, I think that the discussion on two specific points must be considered:

Are the results robust against the resolution (500 m) used in vicinity of the grounding line? A sensitivity analysis on the mesh size should be presented. In particular, it should be shown that a smaller grid size will not lead to a more extended retreat of the grounding line retreat and a larger ice discharge. Showing the region where mesh is refined sounds also important to me (i.e. show that the retreat is not limited because elements are becoming too coarse).

Authors mention a 10-km retreat. Please show it! And preferably with the elevation of the bedrock in background. How does this compare with recent results published by Favier et al. (2014) who claimed that no stable position could be found in the retrograde bed slope region? This also would require some discussion and positioning with/against Favier's results.

Some other technical or more minor comments.

p.1876, l. 24. Authors affect the bedrock elevation to ensure that the physics of the model is compatible with the geometry. I did not clearly understand whether the change has been done below the ocean or below the grounded part. They mentioned that it impacts the bedrock elevation over 10km upstream the grounding line. Isn't it downstream? So, to my understanding, they modified the bedrock below the grounded part. How much does it affects the bedrock elevation? 10m, 500m? We know that the bedrock slope is crucial for the dynamics of the grounding line in this region, details should be given on the procedure and the impact on the elevation. Furthermore, there is a striking agreement between the 10 km where the bedrock may have been modified

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and the extension of the grounding line retreat. I guess it is a coincidence? Or is is an artifact? This must be clarified.

p. 1877, l. 20. Authors have to relax the surface to avoid flux divergence anomalies. It is not clear to me whether the grounding line is allowed to move during this relaxation or not. If it is free to move, then does it move a lot? If the grounding line stays at its initial position during the relaxation, as the thickness is changing (figure 3), is there any problem when starting the projection (hydrostatic equilibrium not respected anymore)? This should be clarified.

p. 1880, l. 2. "the time series diverge. . . mass balance is affected both by changes in ice dynamics and enhanced basal melting". I agree because, as I understand, this is the change in volume that is plotted in figure 4. I would strongly suggest to not present the results in terms of total volume change but in terms of change in volume above floatation. This is the quantity which is meaningful in terms of sea-level rise equivalent (as the ice below the floatation surface will be replaced by water once melted or grounding line retreated). . . And in that case, only the dynamics or SMB over grounded ice shelf would affect the contribution.

p. 1883, l. 24. "We choose not to change the pattern of basal melting, as we do not know how changes in ice shelf cavity will impact oceanic circulation and basal melting rates, and our results are therefore conservative estimates of changes". Saying that the estimations are conservative make the implicit assumption that the melt will somehow increase and amplify the dynamic response of the glacier. If we do not know how change in the cavity will affect the melt rate, it assumes that we do not know the trend. Therefore pretending that it is a conservative estimate sounds as a strong hypothesis; melt may decrease. This should be reformulated.

p. 1884, l. 2. "Our results show that precise estimates of basal melting under floating ice are required and essential for constraining the evolution of the glacier dynamics". In the previous paragraph (p. 1883, l. 11-19) the authors say "Additional experiments

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(not shown here) show that introducing moderate melting rates under ungrounding ice does not affect our results". So, affecting the melt distribution seems to not have a strong impact. Why accurate estimation of melt rate should be done? This sounds very contradictory to me.

Interactive comment on The Cryosphere Discuss., 8, 1873, 2014.