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Interactive comment on "Drivers of Pine Island Glacier retreat from 1996 to 2016" *by* Jan De Rydt et al.

Anonymous Referee #1

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Overview This is a nice paper showing some well-constructed model experiments designed to understand the sensitivity of PIG to various parameter. Despite my many comments and criticisms, I would very much like to see this paper published and I think it makes a valuable contribution to the field.

General Comments

I would like to see more discussion on the quality of the data and how it affects the results. To me the quality of the 1996 data from radar altimetry are highly suspect – ERS DEMS can be off by 10s or 100s of meters in places. Figure 1c does nothing to improve my confidence in these data, particularly the strong gradient in the thickness change across the ice shelf, with nearly zero thinning on the shelf centerline. Thus, I expect some model discrepancies could be explained by errors in the data sets used

to constrain the model. Related to this issue, I would rather see results like Figure 4b expressed as changes in basal shear stress. Particularly in light of the noisy 1996 data set, I would expect inferred changes in basal shear stress to be largely due to topographic (driving stress) errors.

As mentioned in some of the comments below, I have concerns about the joint inversion for A and C. While some prior info seems to be used, it's not well documented in the appendix. For example, do the pˆ priors allow greater variation of A in the margins, while suppressing it elsewhere. I suspect not from the outputs. The inversions for A bear no resemblance to what my intuition would say the distribution should look like. There are numerous papers debating factors of 10 enhancement in shear margins, but this solution as irregular, patchy variations that vary by more than a factor of 100 (shouldn't the priors have not let A vary so much), sometimes with blobs of effectively very cold/stiff ice crossing the margins. I would really have liked the results better if the inversions for A had been restricted to the shelf where they don't make an already ill-posed problem even more so. This would have allowed for better discrimination of any role damage (A enhancement) on the ice shelf shear margins.

Other papers have presented similar results using a forward approach with somewhat better agreement with the data, though for a shorter time period (e.g., Gillet-Chaulet et al 2016 and Joughin et al 2019). There is some discussion about this work, but more comparisons could be drawn. For example, to get good agreement by assuming a reduction in basal traction near the grounding line driving by height-above flotation (effective pressure) variations. Could something like that be done here.

Figures. The colour maps are all in shades of red are hard to interpret. Maybe a different colour map or 25% contours would help.

Specific Comments, technical corrections, minor wordsmithing

Line 27: Would be fair to cite Seroussi et al 2014 (doi:10.5194/tc-8-1699-2014) and Joughin et al, 2010 (in refs) in this list. Alternatively, these might be better in line 34.

Line 70-75. Where does loss of traction due grounding line fit it, which is caused by thinning (this is probably a bigger factor than the original loss of buttressing that triggered it – e.g., Figure 4 Joughin 2019 ref, which thinning alone can actually slow ice shelf a GL velocities without the loss of traction from ungrounding). Payne et al 2004 also look at the shift of the GL, rather than just the loss of ice shelf thinning.

Line 84 deltaUA+deltaUC=0. Again is the loss of traction as the grounding line retreats being bookkept as a change in deltaUC or a change deltaUthin. This is also important for interpreting line 95 because the paper cited shows the thinning driven response, but that includes loss of traction as the ice approaches flotation and finally ungrounds. All I am asking here for is a sentence clearly stating where the loss of traction due to ungrounding is bookkept.

Line 139. "The resulting values for ΔH, linearly interpolated across the grounding line and in data sparse areas, are shown in Fig. 1c" I don't see how this is valid or maybe I am not understanding what is being done. You could have a case where the shelf thins by 10s or 100s of meters due to melting, but the grounded change in thickness is far less. Am I misinterpreting something.

Line 165. "linear, viscous or close-to plastic" It should be "linear viscous, non-linear viscous, or close to plastic" Line 167: "which caused small variations in τb between cases, " This has to do with the interplay between A and taub, not so much the exponent. If you had used a fixed A, then taub is in fact what you are using to achieve stress balance with its parameterization via the sliding law (any sliding law). So, I am not sure the following sentence is correct and probably should be removed. What is different about the two studies is that A is determined on the entire domain for this study vs only on the shelf. A such additional degrees of freedom are introduced, which will give a better fit to the data, but could result in model parameters that are far from the true physicl values (eg. To stiff A balanced by a too slippery bed). Given that the response is dependent on the flow and sliding law parameters, the accuracy of the fit is somewhat immaterial. There is a counter argument that many other studies fit for one parameter

or another using an assumed value for the other, so there could be errors in the assumed value. But I am not seeing a strong case for the improvement here. Line 172: I may be playing with semantics here but what is obtain is not a "best estimate" it's an estimate that produces the best fit, but as mentioned above, this by no means ensures that it is the best estimate. Line 176 "basal slipperiness were kept fixed" Please how the ungrounded region is treated – either here or above. While it may seem obvious, when various parameters are being held fixed, it's not always immediately clear that the traction is being zeroed when the ice goes afloat. Line 4 51: Why not initialize with an A estimated from temperature model; solutions are often better if you start nearer the answer. Also given that there are blobs of A in close proximity each other that can vary by a couple of orders of magnitude (a difference between ice at 0 and -30 deg) without any real correspondence to the flow pattern (outside of some shear margins) it would be worth some comment here about how physically realistic the solution is at least in a qualitative sense. Even for shear margins, the lower trunk on the west side where you would expect some enhancement, the ice actually looks like it's much stiffer than the reference -15 deg.

Line 216: Would be nice to have a brief introduction to this section between 3 and 3.1. Actually this would be a good place to describe the flux gates, which break up the flow of the text below. Line 221: "viscous, rate-strengthening bed rheology " Please add "non-linear" viscous here. Also please note that this description applies under the assumption of till at the ice bed interface. As conceived, the original Weertman law applies to sliding over a hard, non-deforming bed, with ice and rock separated by a thin water film. Inversions here and elsewhere suggest both types of bed may be present beneath PIG. To the extent that its applicable to till, its likely only at low sliding speeds (see Zoet reference). Line 224-225: 50% speedup. Be clear on where, it looks like this magnitude really applies only to the outer shelf. Might also be good to note that results are consistent with the Schmeltz et al 2002 reference that is cited in the text, where a similar experiment was performed (they only get a speedup of $\sim$40% right at the shelf front – mid shelf its more like 20%). Would be far more accurate to say "restricted to the OUTER ice shelf". A lack of speed from calving at the GL is consistent with a number of published results showing speed on PIG over the last decade, during which time there have been large calving events. Line 232-237.5: Please consider moving this flux gate description to intro paragraph before 3.1 as described above. Line 237: "This supports. . ." This what? How about something like "This moderate response supports. . ." Now please cite the earlier works that your work supports. Lines 239-243: This text could be wordsmithed – there are least 3 place saying something about the similarity to calving response. Could just say something like "The responses to calving and thinning are similar at the flux gates, but the calving induces a larger response on the outer shelf (50% vs < 25%). "Ice shelf thinning is generally accepted to be the main driver of ongoing mass loss of PIG, and patterns of ice shelf thinning elsewhere in Antarctica are strongly correlated to observed changes in grounding line flux (Reese et al., 2018; Gudmundsson et al., 2019)." This is a gross over-simplification of the conventional wisdom, which is that loss of buttressing can induce near GL thinning, which leads to retreat, which induces more loss. As AR5 notes, "Problems arise at the GL because, in addition to flotation, basal traction is dramatically reduced as the ice loses contact with the underlying bedrock (Pattyn et al., 2006). This is a topic of active research, and a combination of more complete modelling of the GL stress regime (Favier et al., 2012) and the use of high-resolution (subkilometre) models (Durand et al., 2009; Cornford et al., 2013) shows promise towards resolving these problems." Numerous studies have shown that the near GL speedup is a response to the loss of traction as the GL retreats, not the thinning itself, although that thinning believe to have triggered the whole process, likely through smaller speedups that thin the GL to flotation. Line 246: "in upstream flow, CONSISTENT WITH OTHER STUDIES THAT HAVE SHOWN SIMILAR RESULTS [e.g., Payne 2004, Joughin 2010, 2019, Seroussi, 2014, Schoof 2007. . ..]. Line 271 please add also "non-linear" before viscous. Also "non linear viscous bed rheology described by a Weertman sliding law". Weertman is not a viscous flow model even if the expressions are mathematically equivalent and generally is taken to mean an exponent between 2 and 3. Since your law applies to a broader

range of exponents and indeterminate bed conditions (probably both till and hard bed), how about just substituting here and above power-law sliding in place of Weertman sliding law. At some point where you used m=3 you can say "power-law sliding with m=3, which for hard beds corresponds to a Weertman sliding law" to credit Weertman (a reference to his work would be nice). Line 290 "Weertman sliding" change to "power law sliding" Line 300: Change "Weakening of the ice in these areas accounts for" to "Weakening of the ice in these areas is sufficient to account for". . . Its only one model on the knob, so it's a sufficient but not necessary condition. Line 302-303: I agree this change is physically improbable. But as noted above, so is the original A, which effectively has very warm ice in the margin just upstream of the margin where the ice very cold. See other comments about this. Line 368: The result is also consistent with the Schoof type sliding law used by Joughin et 2019, which produces Coulomb plastic like behavior at speeds > 300 m/yr and low-m (Weertman-like) behavior at slower speeds. The areas with plastic like behavior in Figure 6 transition to lower exponents in the area between the 600 and 100 m/yr contours shown in Fig 4, which would be worth commenting on. Line 387: "Compared to spatially uniform values of m. . ..". But this in effect what a sliding like that proposed by Schoof (and in another paper by Gagliardini et al) does – provides high-m behavior for fast flow and low-m behavior for slow flow. Line 399: Change "Based on the most comprehensive observations. . ." By what metric? Other data sets are arguably more comprehensive (you have only 2 snapshots in time). Whether they are or are not the most comprehensive has no bearing on the value of this paper. Instead, simply say "Based on a comprehensive. . ." and you are on firm ground. Line 402: Remove "unprecedented". The are some nice results in this paper, but they largely echo the results of earlier work (I mean no slight here this is true of most papers). Line 408: See general comments, but Joughin et al for a different time period got quite close agreement in a similar set of experiments by changes in basal shear stress proportional to the height above flotation in the region immediately above the grounding line. A compare and contrast sentence on this point would be good.

L410: we found that the recent increase in flow speed of Pine Island Glacier is only compatible with observed patterns of thinning if a heterogeneous, predominantly plastic bed underlies large parts of the central glacier and its upstream tributaries, CONSISTENT WITH EARLIER FINDING (there are several that are appropriate).

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