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Interactive comment on "Mechanics and dynamics of pinning points on the Shirase Coast, West Antarctica" *by* Holly Still and Christina Hulbe

Anonymous Referee #1

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This paper provides a detailed modelling study of the effect that the Shirase Coast Ice Rumples have on the flow of the Ross Ice Shelf. The work looks technically sound and well carried out. The main issue I have with this paper is that it's a bit hard to see the forest for the trees. Specifically, there results are shown several ways and numerous figures are given that show various aspects of the with and without the rumples, but the discussion is fairly thin its not clear that it makes the case that ice rumples matter. Other than the fact the that the rumples will affect the velocity and the stress distributions in somewhat different ways with different assumptions in model parameters, it's not really clear to me whether they are essential features contributing to the overall stability of the ice shelf. Yes, the details of the shelf will differ depending on whether they are present, but does that really matter. I would think this manuscript would be much improved if

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some kind of big picture statements could be made about the influence of the rumples on long-term stability (even if turns out the ice shelf behaves roughly the same, albeit with some changes in the details). This seems to be what Table 3 indicates (mass flux differences of only a few percent, which may still reflect transients from the abrupt and somewhat unphysical removal (actual ungrounding might be followed by regrounding due to the increased flux).

It might be good to remove a figure or table or 2 and put less emphasis on the all of the details, many of which have to do with the response to the abrupt removal of a bedrock feature, which not going to happen in reality. It's still a good thought experiment, so more discussion about the effect of the rumples on overall ice stability. How would the RIS be different in a parallel universe where the bathymetric features that give rise to the rumple did not form? Along these lines, is 150 years a long enough period to get rid of all transients associated with the step-removal of the topo. As noted above, the flux changes are relatively small, especially compared with everything else going on these ice streams over 150 year, so why do we care if our bottom line is sea level rise estimation. Especially if the this the difference between all or nothing. Maybe a good-enough pinning point representation is fine. I am not saying this is the case, rather I want to see the case made based on the overall system response, not just in the details presented here.

I think with a bit for refocussing and wordsmithing this could be an excellent paper.

Specific points Line 27 "and THE resulting. . ."

Line 59. Given we are getting much more frequent time series, it would be good soften the limited snap-show view statement, even if it may apply here. (At least go with snap-show viewS plural since we have very detailed time series for some areas).

Line 66. Hyphenate ice-sheet and ice-shelf, and separate with En dash (I think the En dash is currently there).

Equation 3 – Its disappointing to see this sliding law still being used. Why not use at least Weertman if not Coulomb plastic. At least add some discussion on the implications and validity of this this choice.

Line 102, Equation 4. Are you actually updating the model with this height above flotation effective pressure? It's not clear that this is appropriate for Siple Coast (or maybe anywhere) since most boreholes indicate effective pressures near zero.

Lines 118-125, please change all instances of resolution to posting – most of these data sets have nothing like this resolution (especially the bed products).

Paragraph beginning at line 131. It's unclear without looking at the supplement whether you are inverting for B on the floating ice and alpha on the grounded, or B on both. The text seems to indicate the latter, but Figure 2 seems to indicate the former since no value of B is shown on the grounded ice. Please clarify in main text. Along those lines, inversion for both B and alpha can be problematic. Yes, you will always get less model-data mismatch, but that's not necessarily a better fit since you have introduced an extra degree of freedom.

Line 149. A 2-year time step with speeds that potentially move ice 750 m (Fig. 1) through elements with 500-m dimension seems a bit dangerous with respect to CFL. Please justify.

Line 174: As a point that extends beyond this sentence, nothing is mentioned about whether any regularization has been applied in the inversions. Even if none has, that should be stated. Specific to this sentence, regularization might have smoothed out the friction coeff, so no manual adjustment would have been required.

Figure 3. With all of the map view figures in the main manuscript, nobody should have to skip to the supplement to see the locations of profiles. Please show in on or more figures in the main text.

Line 186. Remove the word "very" the numbers are similar, but given the uncertainties,

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some of this may be due to chance, so just keep it at “similar”.

Section 3.3 Another sentence or two here of introduction and why these equations are being given would be appropiate.

Table 1. How is their basal shear stress, however small, when the ice rumple is removed (where is the traction coming from). Maybe this is just an artifact of the force budget computation, but some kind of explanation is required (zero to within errors?).

Line 192. Minor point, but since 10b is referenced before 10a, they should be swapped, especially since there is no other compelling reason for the current ordering.

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