(p 662, line 16) "long period Msf response" – For the convenience of readers, please give the description (lunar, solar, etc.) and period of tidal constituents when you first mention them. I believe this also occurs a few times later in the paper.

-More details on the tidal constituents mentioned in the paper will be added to the revision along with a general description of tides around Antarctica.

(662, 26) "Due to this nonlinearity…" – Maybe I'm just reading this wrong, but this looks backwards to me. Shouldn't more shear stress slow the ice flow? Or are you talking about driving stress, not basal drag? Please clarify this.

-Increase in basal shear stress (τ_b) does indeed mean an increase in the basal motion as shown in the sliding law.

(663, 12) "using the same dataset" – It's ambiguous whether you mean the Murray et al. or Heinert and Reidel data set.

-We are referring here to the Murray et al. dataset and will make this clearer in the revision.

(663, 21) Regarding the role of currents, you may also be interested in Kelly Brunt's 2008 thesis from the University of Chicago.

-We could not locate a copy of Kelly Brunt's thesis online and have contacted her to try and obtain one.

(664, 11) Walker et al. (2012) uses a GPS-derived kinematic boundary condition at the downstream end, with no ice shelf, so the question of flexural stresses shouldn't matter. It is depth-integrated, however, which might cause differences. Also, I'm pretty sure there are some significant geological differences between the settings of Rutford and Bindschadler, so I wouldn't discount that the results are different because the beds themselves are different.

-Certainly the Bindschadler and Rutford ice streams are different and it is likely that these differences would necessitate a different modelling approach.

-While we choose not to try and match our results to one particular ice stream our model setup is based around the Rutford and we cannot discount that a different geological setting would require a different stress exponent (however still >1).

(665, equations 1-3) Are you using the comma notation for partial derivatives and the summation convention? Probably best to explicitly state this. Also, I think you don't want the comma in the subscript of sigma in line 7.

-The reviewer is correct regarding the notation and this will be made clear in the text, the comma in line 7 is a mistake and will be removed in the revision.

(666, 2) These parameter values are different from those used by Gudmundsson (2011) to emulate Burger's rheology. Could you explain why you switched to these values? Also, please clarify where Poisson's ratio enters the equations. (Through G, I think, but please make it explicit.)

-The range of modelling parameters was the same as those used by Gudmundsson (2011). As in that previous study, we found that the choice of Poisson's ratio and Young's modulus did not have much effect on results and used the same values that he sets out in his paper (stated in Gudmundsson, 2011 - fig. 5).

-The equation for G will be added to the revision.

(667, 9) "numerical chattering" – I like this phrase, but could you be a little more specific about what you mean?

-In the contact problem where two bodies are touching along a very shallow slope, the vertical displacement needed for one body to be considered to have left contact with another is very small and as a result there may be segments near the grounding line which flicker between the two contact states at every time step. This flickering can in turn affect displacements and the resulting artificial numerical effect can be quite large. Adding a very small separation stress can help to avoid this problem.

-We will add an explanation of what is meant by this phrase in the revision.

(667, 24) "there is still debate about its value" – It would be nice to cite a paper or two here.

-Citations will be added to the revision.

(668, 6-9) It would probably be clearer if you waited to mention which constituents you used until you got to the experiment descriptions.

-We will move this to a later section for clarity.

(668, 9) Regarding the elastic spring boundary condition, are you discussing how equation 9 corresponds to something in the commercial FE package? Or are you just explaining how this boundary condition can be viewed as a spring?

-This is discussed in the context of an elastic spring because this is how the boundary condition must be implemented in the commercial FE package. This will be made clear in the revision.

(670, 23) "Mf frequency" – Same comment as for Msf earlier.

-Same response as earlier.

(671, 14 and following) It would be interesting if you were to discuss the size of the stress resulting from changing basal drag relative to the sizes of the other stresses, and also their relative timing. It's a complicated situation with stresses acting opposite one another, so I'd like a more detailed description.

-This is an interesting idea, we will look in more detail at the stresses and possibly add a figure to the revisions or at least add some comments regarding our findings in the text.

(672, 3) Are you discussing how the Msf constituent propagates when it's present in the tidal forcing, how it's generated from M2 and S2, or the sum of these effects?

-We're not sure what the reviewer is referring to here, in this section the model is forced with a number of different constituents including M2 and S2 but not Msf. The t_tide package is then used to analyze the resulting Msf modulation in velocity upstream and how this varies in time and space.

(672, 8 and following) You may want to consider moving the description of the linearized experiments to later in the paper, so it immediately follows the analytic solutions as a check. On first reading, it was hard to figure out what you were doing here.

-We will try and change the structure and how we introduce this section to make it clearer to the reader what we are doing.

We would like to thank the reviewer for their insightful comments which will help to greatly improve the quality of this manuscript.