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The Cryosphere
Discussions

Interactive comment on "Insights into ice stream dynamics through modeling their response to tidal forcing" *by* S. H. R. Rosier et al.

Anonymous Referee #2

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In this paper, the authors demonstrate that significant results derived from simpler models of ice-stream tidal response still hold with a full-Stokes model for the (separate) cases of three-dimensionality (including lateral boundaries) and grounding-line motion. They demonstrate that a nonlinear sliding law can lead multiple short-period forcings to result in a long-period response. Also, they derive useful period-dependent stress coupling length scales for upstream propagation of tidal effects.

I found this paper to be quite good. I thus have only relatively minor questions and requests for clarification.

(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.

(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.

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

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

(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.

(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.

(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.)

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

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

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

(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?

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

(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?

(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.

(674, 18) "velocity of amplitude" should be "amplitude of velocity".

Interactive comment on The Cryosphere Discuss., 8, 659, 2014.