Review: Rosier & Gudmundsson; tc-2017-193

Dear colleagues,

This is an excellent paper on a very relevant topic. That being said, it could be easily extended to present some more model results, and to link it back to the problem of ice stream shear margins. Also, the problem of viscous vs. elastic stress should be carefully presented, especially in the theoretical discussion (Section 3).

My recommendation is to slightly extend the manuscript with the points mentioned below.

Sincerely, Martin Lüthi

General comments

The paper starts with a very well written and interesting introduction.

One point that needs attention is the intermingling of viscous and elastic stresses in the theoretical investigation (section 3). Elastic bending stresses are taken from beam theory, and then suddenly interpreted as viscous stresses. From the discussion it is not clear whether this is done because of the assumption of a Maxwell body, but then one would have to argue why elastic displacements are ignored.

It would be helpful to extend several figures, especially Figure 5 (adding τ_E) and Figure 6 (adding τ_E, extending it to all three experiments). Additionally, a figure showing time series of forcing, horizontal displacement, velocity, τ_E and strain heating in the shear zone would be most helpful.

One of the strong points of the paper is that no state change in the ice is required to produce the period doubling. However, there are three obvious mechanisms which should be discussed: grain size, fractural weakening and strain heating. All of these effects have been invoked to explain ice stream shear margins, so there is ample pertinent literature. It seems very likely that these processes are also active in a shelf shear margin, which is very similar to a fatigue experiment in material science. Certainly grain size will adapt to the continuous forcing, the material might suffer damaging, and strain heating (which is a model output) will warm and therefor soften the ice.

As a side-note, the authors seem to adopt (as in the recent glaciological literature) the term "full-Stokes" to mean Finite Element model, even if they don't solve the Stokes equation, but a visco-elastic extension thereof. There is no such thing as a "full-Stokes" equation, but "reduced-Stokes" solvers which ignore some terms of the Stokes equation. This is mentioned in many comments below, but should be consistently purged.

Specific comments

29 awkward end of sentence.

39 GL has not been defined (meaning grounding line).

40 I think this should be "independent"

62 To my understanding the effect should be greatest during periods of highest flexuring rates, i.e. during rising and falling tides. The reason is that viscous stresses are created by viscous deformation.

65 Consequently, also here should be "rising" and "falling" tide (not high and low).

69 here again the displacement is alluded to, but that is something elasticity is concerned about.

73 "equally as fast": check the usage of English

93 "Cauchy" upper case (it's a name)

98 There is no "Stokes flow" in these equations, that's just the force balance. (For Stokes flow you need the rheology).

123 "strain of the two components".

124 This equation is based on Glen's flow law, which was never introduced.

124 where does the factor 2 in the first term come from? Using standard Glen's flow law its just $\dot{\epsilon}_{xz} = A\tau_E^{n-1}\tau_{xy}$. Maybe you use non-standard definitions (as compared to glaciology textbooks), but general definitions should be given.

128 usually $\tau_E^2 = \frac{1}{2}\left(\tau_{xy}^2 + \ldots\right)$

133 How is M_{sf} absent? Tidal forcing is mainly vertical.

135 I don't understand how horizontal integration yields a vertical average.

Eq 10 explain that s and b refer to surface and bed. Also I think that units don't match since you integrate a stress divided by h (which the bar seems to indicate), to different powers.

Eq 17 in the overbrace of the third term: S_4 (currently no index)

140 and Eq. (10): somehow viscous deformation rates are obtained from elastic stresses, since λ and the τ's are derived from elastic constants (Eq. (6)). IIUC this should be obtained from the viscous analog stresses with $\mu = 0.5$ (for incompressibility).

175 It would be helpful mentioning why (integration, step from (16) to (17)).

184 Absolute speedup would also be interesting. IIUC, the relative speedup for small shelves is not due to n_{shift}, but due to an increase of u_0.

185 should this be "Table 1"?

191 "dependant" should be "dependent".

192 Again, viscous stresses are not the same elastic stresses. The cap on the value of E seems to be a hint in this direction: viscous stresses cannot be much higher for ice rheology since strain-rate softening limits the maximum sustainable stress.

210 Was the model surface geometry prescribed, or free to evolve to a consistent state?

213 Do you mean "Eulerian frame of reference" since total derivatives (Jaumann) appear below? These wouldn't be needed in a Lagrangian frame.

231 give a reference to the Equation how to calculate G in Eq. (22) from E and μ.

Eq 25 why is $\rho_w g w_a(t)$ not included here?

Fig 3 It might be important to highlight that the grounding line is not just upstream, but also sideways of the model.

237 Which physical process generates this buttressing stress?

239 and Eqs. (25) and (26): use consistently ρ_i, or drop the index "i" consistently.

240 I have no idea what an "elastic foundation" is. It looks like you just apply a normal stress on all faces in contact with the ocean, or at least this is what it should be. "Elastic" makes no sense here.

243 If this corresponds to the green "till" area in Figure 3, please say so.

246 How is this boundary condition implemented? Is a Dirichlet (i.e. velocity) condition prescribed, the magnitude of which is calculated from the basal stress of the last time step solution?

256 simply call this section "Discretization"

259 Leave away this sentence, we typically also use HEX20 or even better HEX27, so this is standard.

260 The model mesh appears extremely coarse for the task at hand. Especially the horizontal discretization in a shear zone should be considerably smaller to resolve the stress concentration there, and the element layer on the sides should be bigger than 2 elements. Or at least some model experiments with double mesh density should be used to show that the chosen resolution is sufficient to resolve the relevant spatial scales.

Why was a mesh with such a complicated structure chosen for a block, where a structured mesh, possibly refined in the areas of interest, would have sufficed?

260 Is the mesh moving? If not, how big are the errors in transient stresses?

267 ff.: better explain what n1, n3 means, and leave away the "denoted n3xyz" and similar.

268 space missing in x,y,z.

Fig 4 It is somewhat confusing to have the horizontal axis reversed as compared to Figures 1 and 3. Better mirror those.

Also indicate what is shown with these contours. Are these velocity amplitudes? Or vertical displacement amplitudes at the surface, or the base of the ice? Or horizontal displacements?

Additionally, it would help saying that these are map-plane views (I think), of maybe the surface, or average, quantities.

Instead of n=1, n=3, the panels should be labeled with the codes of the model experiments e.g. n1xyz.

Velocity contours could also be shown for lower velocities, e.g. 0.1, 0.5, 1., 2., such that panels b and c show something interesting

288 now this mysterious term is called the "ocean foundation BC", where it was "elastic" in line 240. Just call this the normal stress. Also parenthesis is missing.

291 So why FRIS, if everything else, including GPS, is from RIS?

294 Don't call this "full-Stokes". Either you solve the Stokes equations, or you don't. But here you claim to solve with a viscoelastic rheology, which is again something quite different.

295 see 294, and please don't perpetuate this stupid "full-Stokes" thing.

297 Please specify which displacements these are: horizontal or vertical? One would expect to see velocities, given the theoretical section and the model description.

301 "medial line": better write "ice shelf center line"

308 better "For experiment n1xyz …"

323 This scaling makes the comparison with Figure 4 difficult. At least you should indicate the grounded portion on the horizontal scale, this might be estimated to be 0.1? Or even less, since the crosses are the stresses extracted at element centers, or element face centers (actually maybe integration points, unless projected to the nodes)?

325 Since you have symbols, please use those in the descriptions.

327 units of $w_a = 2\,m$. But this was already said in line 321.

328 Don't call yy "longitudinal", this is plain confusing. It is the cross-flow component.

329 where is $\pi/4\lambda$ in the figure? Please help the reader in the figure, or improve the description.

333 Probably first tell the reader that τ_{xy} is the boring component holding back the weight of the ice, that you would get irrespective of rheology.

333 Also show, and discuss, τ_E, this is the main point of the paper.

335 again, no full-Stokes. And as said just above, this part is boring since you get that with any rheology, it's just force balance.

Fig 5 Indicate the grounded part with a colored bar along the horizontal axis, or a vertical dotted line, or similar.

Also plot the effective stress, since this is the quantity that is crucial for the whole argument.

334 This section should go to the discussion.

336 Why is τ_{xx} increasing downstream? Is this already the effect of the end of the domain, or the standard ice shelf extensional stress?

340 again, why full-Stokes? This is visco-elastic!

348 No, you have not shown that, at least not in the paper. Why aren't any plots of the velocity-spectrum, together with the forcing, shown. Or at least curves of the time-variation of forcing and response.

After reading on I found Figure 6 which sort of shows this. So why is this not presented in the "Results" section, such that it could be meaningfully discussed?

355 I sincerely doubt this claim when using a spatial resolution of only 300 m. This effect might be quite important, but will be smoothed out by the horizontal and vertical approximation functions.

362 This period-doubling has been nicely shown in the theoretical part, although with some doubts concerning the problem with viscous/elastic stresses. Now one would like to see this frequency-doubling also in the FE model results. So it would be very nice to have a plot with forcing and response at different points on the domain.

Maybe the argument is that the stresses are highest at high tide, and therefore viscous deformation rates. But since everything is transient, and you have a visco-elastic membrane that is bent up and down, there must be location dependent delays.

362 Why at high and low tide? Vertical velocities are highest during rising and lowering. You could/should investigate this claim by just extracting the second invariant from your transient model runs.

365 “medial line”

Fig 6 It is not clear what is shown in this figure. Is this the FFT (frequency spectrum) of the model response? What was the strength of the forcing? It would be very helpful to also show the time series.

Panel (b) should be labeled with (m/d), as it shows velocities.

Panel (c) (missing) should show the effective stress in the shear margin.

This figure would also be a good place to compare the results from the three model runs, maybe as colored bars.

371 “…than the run time of 60 days”.

375 I think this is “vice versa” (good old Latin).

402 Again, no “full-Stokes”.

414 Add some discussion of additional, state-changing processes here (see general comment).