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Interactive comment on "Modeling the elastic transmission of tidal stresses to great distances inland in channelized ice streams" *by* J. Thompson et al.

Anonymous Referee #1

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General comments

The manuscript presents the results of a modeling study investigating the tidal effects on the ice stream behavior. The authors consider both elastic an viscoelastic types of rheology, and use a two-dimensional (cross-section view) and three-dimensional models. There are many novel features in this study, e.g. rarely used viscoelastic rheology and the use of a three-dimensional model in such investigations. The results are interesting and fairly well presented, and the manuscript can be published after minor revisions.

My major comment concerns the main conclusions that the tidal loads have a too strong of a decay, due to the ice-stream lateral boundaries, to explain ice-stream surface observations, hence, it is necessary to invoke tidal response of subglacial hydraulic system in order to explain these observations. One of the major results of both 2D and 3D modeling simulations is that the tidal effects decay exponentially over the length of two-three widths of an ice stream. Although the authors aim to explain tidal signals observed on Rutford Ice Stream, which is fairly narrow ($\sim$10 km wide), other ice streams where tidally modulated displacements are observed, Bindschadler and Whillans, are wider. Therefore, on ice streams with the length being a few widths, it is potentially possible to observe the exponentially decaying tidal signals. By no means I want to put words in someone's mouth (and the authors have a subsection discussing the different ice stream geometries), but perhaps it would be more appropriate to state that on **narrow** ice streams, i.e. several ice thicknesses, the most likely cause for tidal surface signals is due to the tidal effects on subglacial hydraulic system, and on **wide** ice streams, i.e. several tens of ice thicknesses, it is potentially possible to explain the observed tidal signals on the surface of ice streams by the tidal load at the grounding line.

It is not clear from the description whether the 3D model was used only to simulate a 10 km wide ice stream or any other width was considered. It would be very interesting to know, whether the observed exponential decay holds for ice streams with progressively increased width and unconfined ones as a limiting case. Considering computational costs, I leave to the authors discretion to decide whether to add such analysis to this study or not.

Minor comments

Overall, the manuscript is well written, however, in my view, it can be made a bit conciser. For instance, the first ten lines in the abstract can be reduced to a couple of

sentences. The same information is repeated in the Introduction.

page 2122, line 23-25: Walker et al. (2012) use a vertically integrated model, so it's a one-dimensional, flow-line, not a two-dimensional model.

page 2125, eqn(1) and lines 6-8: either here or in Fig. 2 the boundary conditions need to be explained. For instance, it is unclear what is prescribed at the most upstream vertical boundary for both 2D and 3D models. For the 3D model it is unclear what kind of conditions are implemented at the lateral boundaries. Though, it is a matter of a personal preference, since the 3D model is a horizontal extension of the 2D model, it might be better to use $x - z$ instead of $x - y$ coordinates for the 2D model. Moreover, Fig 2(a) have $x - z$ labels.

page 2135: eqn(9): I believe that a first factor in this Arhhenius relationship (3.5×10^{-25}) is different for T <263 K and T >263 K. The authors need to double-check that.

Figs. 3-4, 6, 8, B2: Though the plotted colors can remain $\log_{10}$ of stress values, it would be better if the color labels indicate the stress values themselves. Also, a traditional glaciological unit of stress is kPa, so it might be better to use it in all plots.

Interactive comment on The Cryosphere Discuss., 8, 2119, 2014.