Authors' Response to Anonymous Referee #1

We thank the first anonymous referee for their careful and thought-provoking commentary on our paper "Modeling the elastic transmission of tidal stresses to great distances inland in channelized ice streams." Their comments improve the readability and have helped us focus our manuscript. For clarity of this review, the referee's comments will be shown in bold with the authors' response shown in plain-style text beneath.

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 (~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.

The referee's primary concern with the conclusions of our manuscript is that a narrow ice stream model, as was used for Rutford Ice Stream, does not allow for the universal statement that tidal signals observed on *all* ice streams must be explained independently of direct tidal load at the grounding line. We agree with the referee, as demonstrated by our final sentence of the paper, where we state that "for *channelized* [emphasis added] ice stream, such as Rutford Ice Stream, and *perhaps* [emphasis added] other tidally-modulated ice streams as well, stress transmission through the subglacial hydrologic network is the most-likely mechanism for the tidal modulation of ice stream motion […]." Our use of the term "channelized" ice stream is meant to convey the same meaning as "narrow" ice streams. We have modified sections 6.2 and 7 to more clearly identify which aspects of our models apply to narrow ice streams, and which apply generally to ice streams independent of their relative width-to-length.

As an aside, we do not agree with the referee's assertion that Rutford Ice Stream is represented by an ice stream that is ~10 kilometers wide. Based on aerial and satellite imagery, our estimate of Rutford Ice Stream's width was approximately 30 kilometers. In reviewing Section 6.1, we found that there was a typographical error that may have led to this confusion, as the paper currently references Table 4, when the correct table is Table 5. Additionally, we have rewritten the first sentence of section 6.1 to explicitly state the model width used to represent Rutford Ice Stream (30 kilometers).

It is not clear from the description whether the 3D model was used only to simulated 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.

While we do not discuss the variability of our model results with ice stream width in detail, Table 5 summarizes model results for an array of models that include the effects of increasing the model width on the observed length-scale of stress transmission L_{TR}. To more clearly identify the geometries explored within our models, we made the following changes:

- A sentence describing the range of 2D model geometries has been added to Section 3.1.
- Section 3.2 was restructured in response to these and other referee comments. Specifically in response to the above comment, a sentence was added to the first paragraph of Section 3.2 that describes the ranges of model geometries considered in this manuscript.
- Portions of Section 3.3 have been rewritten for clarity.
- A column was added to Table 5, specifically describing the ratio of *Ltr* to model width. In conjunction with this addition to Table 5, the analysis conducted in Section 6.2 has been slightly modified. As we have already considered model geometries beyond 10 kilometers in width, we feel that more clearly identifying these other model geometries is sufficient to address the referee's curiosity regarding other model geometries, and thus additional (new) models will not be considered for this manuscript.

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.

Where practical, the manuscript has been revised to use more concise language and sentence structure. Additionally, the referee's specific suggestion regarding the manuscript's abstract is appropriate, and the abstract has been rewritten.

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.

The referee is correct in stating that the model of Walker et al. (2012) is a one-dimensional flow-line model. The reference to Walker et al. 2012 has been removed from page 2122, lines 23-25, as the discussion of this paragraph was focused on the modeling results of Gudmundsson (2011).

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.

Both referees felt that the original discussion of the boundary conditions used in our models was unclear, so we have revised the discussion of the model set-up and the applied boundary conditions to more clearly outline the applied boundary conditions. Incorporating and modifying information presented in Section 2 and 2.1, we have added Section 2.2 (Applied Boundary Conditions) to identify the boundary conditions used in our 2D (Section 2.2.1) and 3D (Section 2.2.2) models.

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.

Indeed, we were inconsistent. In keeping with the *x-z* coordinate system used in Figure 2(a), we have updated our discussion of our two-dimensional models to exclusively use an *x-z* coordinate system (e.g., in Eqn 4(a), Table (3), Figure 3, and Figure 4).

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

Yes, the creep coefficient, *A*, used in the Arrhenius relationship (Eqn (9)) is temperature dependent. Eqn (9) should read:

$$A = 2.4 * 10^{-24} \exp\left(\frac{-6 * 10^{4}}{8.314} \cdot \left[\frac{1}{T} - \frac{1}{263}\right]\right) Pa^{-3} s^{-1} \text{ for } T < 263K$$

$$A = 3.5 * 10^{-25} \exp\left(\frac{-1.39 * 10^{5}}{8.314} \cdot \left[\frac{1}{T} - \frac{1}{263}\right]\right) Pa^{-3} s^{-1} \text{ for } T > 263K$$

which matches the suggested values of Cuffey and Paterson (2010).

Figs. 3-4, 6, 8, B2: Though the plotted colors can remain log10 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.

We have modified the labels in these figures to indicate the stress values themselves and to present stress in units of kPa.