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.

Authors' Response to Anonymous Referee #2

We thank the referee for their careful and thought-provoking commentary on our discussion paper "Modeling the elastic transmission of tidal stresses to great distances inland in channelized ice streams." These comments have improved the readability and focus of 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.

This is a generally well written paper, using numerical experiments to explore the transmission of stress generated by tidal forcing at the end of an ice stream. The primary novelty is the inclusion of lateral resistance, which is argued to cause significant decay of longitudinal stress with distance upstream of the grounding line - at least more than has been previously assumed. It is concluded that stress is unlikely to be transmitted more than 2 times the ice stream width, but this is inconsistent with observations from Rutford ice stream. An alternative explanation, the propagation of the tidal signal within the subglacial hydrological system, is advocated.

The arguments seem reasonable, and I think the paper could be published, but I think that before this happens a number of aspects need to be cleared up, and some of the explanations need to be considerably improved. I found it quite difficult to tell what was actually solved in the models, some of the approximations that are made need to be acknowledged more readily as such,

The detailed description of our models—in particular the applied boundary conditions—is apparently less clear than we intended. We have revised and expanded our discussion of the modeling approach to more clearly describe our model configurations, assumptions, and solutions. In particular, we have incorporated and modified information previously presented in Sections 2 and 2.1 into a new Section 2.2 (Applied Boundary Conditions) to clearly and concisely identify the boundary conditions used in our 2D (Section 2.2.1) and 3D (Section 2.2.2) models.

and there needs to be some consideration about whether the conclusion is specific to the one set of observations that is mostly considered (Rutford) or holds more generally. In particular, for this latter point, if the stress can be transmitted up to 2 ice stream widths upstream, that could easily be up to 100km for larger ice streams, particularly if one takes into account the possibility of margin weakening etc.

Our original wording left the scope of our conclusions unclear. In addressing the referee's comment, we have modified Section 7 (Conclusion) to clearly differentiate between conclusions applicable to narrow ice streams and conclusions that are generally applicable to ice streams independent of geometry. Additionally, we have expanded the discussion of other ice stream geometries (Section 6.2) to explicit discuss larger ice streams (such as Whillans Ice Plain) and to provide our perspective on the transmission of tidal stresses inland of the grounding line on these wider ice streams.

One of the aspects of the model that I found questionable was the treatment of the grounding line as being fixed. In reality the grounding line would move as the tide goes up and down, and by assuming it is fixed it is not clear that the stresses near the grounding

line would be properly resolved. Related to this is the model in appendix C, where it is not explained what boundary conditions are imposed on the ice shelf at the grounding line (it should really be a 'free' boundary).

We feel that our modeling approach demonstrates that assuming a fixed grounding line does not affect the transmission of stresses far inland of the grounding line. The original manuscript did not include an explicit discussion of our grounding line assumptions. In order to alleviate concerns over our modeling assumptions, we have specifically addressed the grounding line as part of our revisions to the manuscript described below in the next comment.

I found the description of the models, in particular the boundary conditions imposed at the grounding line, to be rather unclear, and I think this needs to be improved.

As both this referee and the other anonymous referee felt that the original discussion of the boundary conditions used in our models was unclear, the authors acknowledge that the discussion of model configurations and applied boundary conditions needed to be revised for clarity. Incorporating and modifying information presented in Section 2 and 2.1, we have added Section 2.2 (Applied Boundary Conditions) to clearly and concisely identify the boundary conditions used in our 2D (Section 2.2.1) and 3D (Section 2.2.2) models.

All three appendices seem to be about aspects of this boundary condition and ways in which it can be simplified - I think it would actually be clearer to combine these together and make a single appendix all about describing in greater detail what conditions are used for the different models.

With the addition of Section 2.2, we believe the improved explanation of the applied boundary conditions in our models also makes the distinction between Appendices A, B, C clearer as well. While we appreciate the referee's suggestion of a unified appendix describing modeling boundary conditions, given that each appendix presents a "stand-alone" detail of the modeling rationale, we have opted to keep the structure of the appendices the same.

In appendix B, given that you have a three dimensional model so need to impose boundary conditions at all heights z, I don't really see why it is any harder to impose the full loading condition than the simple condition.

The referee is correct that imposing the full loading condition, as presented in Appendix B, is not more difficult than imposing the simple loading condition from a computational standpoint. However, the observations of Antarctic ice stream behavior that suggest the ocean tides influence the motion of these ice streams are fundamentally related to the *variability* of the ice stream's behavior with the *change* in ocean tidal amplitude. As such, the important boundary conditions for these models are not the static background stresses of the ice stream and the ocean tides, but rather the fluctuating stress over a tidal cycle. This variable tidal stress is incorporated in our models as the "simple" loading condition.

In appendix C it needs to be made clear how and where these results are actually used for the rest of the study.

Originally, Appendix C was not directly referenced in the main the manuscript, but we have modified the manuscript to refer to Appendix C when this analysis is used.

Although I am quite happy with the suggestion of hydrological control, I think that the section in 6.3 should be expanded somewhat, as I felt it seemed rushed and not explained fully. In fact, I would really like to see a more complete analysis of this model including a diffusion equation for the pore pressure distribution driven by the tide, but I leave it at the authors' discretion as to whether they include this. As it stands, however, there are no results of this model shown except an analogy with Gudmundsson (2007) - this analogy should be spelt out more, and some result shown to back up the claim on 2144, line 19 that the observations from Rutford ice stream can be 'explained' using this model. That explanation has largely been the point of this paper, but it seems to run out of steam before completing it.

The goal of Section 6.3 was to propose a viable alternative to a nonlinear basal sliding law as a potential mechanism for explaining the observed interaction between ice streams and the ocean tides. While we do not agree with the referee's suggestion that we "r[a]n out of steam" and "rushed" the analysis of our hydrologic model, the referee's suggestion that the analogy to Gudmundsson [2007] could be more clearly demonstrated is valid. This comparison has been rewritten and slightly expanded to more directly demonstrate that our hydrologic model is analogous to the form of Gudmundsson's basal stress model.

We thank the referee for their thoughtful suggestion that we incorporate a diffusion equation to govern the spatial and temporal evolution of the pore pressures in the subglacial till due to the loading of the tides. Ultimately, we feel that such a modification of our hydrological model is beyond the scope of the current manuscript, although incorporating diffusion is an excellent suggestion for future development of this hydrologic model.

Throughout, there are odd phrases that are not well written or are grammatically incorrect – a thorough proof-reading, especially of the appendices, is required.

We hope we have removed the "odd phrases" in the manuscript.

Specific points

1. Section 1.1, and Table 1 - the distinction between observed tidal flexure and observed tidal stress should be made clearer. There is also some ambiguity about what 'stress transmission' really means. What is observed is not presumably not the stress - it is something else like seismic activity or changes in surface motion. Best to make clear what is actually observed since that is what you need to explain (in some ways the conclusion of this paper is that it is not really stress transmission - at least not through the ice).

The referee is correct that the GPS, seismic, and other observations from Antarctic ice streams summarized in Table 1 are not direct measurements of tidal stresses, but rather observations that have previously been thought to indicate the communication of tidal stresses. We have made the following revisions:

- A sentence has been added Section 1.1 to specifically describe the connection between some surface observations of ice stream motion to the influence of the ocean tides.

- Our definition of what we refer to as "stress transmission" is found in the final sentence of the first paragraph of Section 1.2. To help clarify this definition, the parenthetical notation of terminology "stress transmission" has been added to this sentence.
- The column header in Table 1 previously labeled "Tidal Stress Transmission" has been relabeled "Tidally-Modulated Observations." Additionally, the caption of Table 1 has been revised to reflect this change to the Table header.

2. 2123, line 25 - flow-line models do not have to assume no lateral stress at the margins; it is quite common to parameterize lateral shear stress (proportional to flow speed, say).

The referee is correct that flow-line models can parameterize the effects of lateral shear. The sentence at 2123, line 25 has been rewritten to state that the flow-line models specifically discussed in Section 1.2 do not incorporate the effects of lateral shear. Other minor revisions to Section 1.2 have been made to more clearly indicate that the discussed models *choose* to neglect lateral stresses rather than that these models *must* neglect lateral stresses.

3. 2125, line 21 - this sentence does not read well. Might be good to explain that it is the deviatoric stress that is important when the rheology is made non-linear, and also that the hydrostatic component of pressure (which is being neglected here) is included when considering the stress to apply at the grounding line (see appendix).

This sentence has been rewritten for clarity.

4. 2126, line 3 - the applied force is equal to the 'excess' hydrostatic pressure? It should not be equal to the hydrostatic pressure. Make sure all the variables are defined.

The referee is correct; the applied stress is the "excess" hydrostatic pressure due to changes in the tidal amplitude. This comment has been addressed as part of the revision to the discussion of our models' boundary conditions (i.e., the addition of Section 2.2).

5. 2127, line 11 - seems like the section on page 2135-2136 would fit much better here, where you're explaining the viscoelastic rheology, rather than providing similar discussion in different places.

We have incorporated this comment into our modifications to the discussion of model configuration and boundary conditions, such that the discussion of our applied viscoelastic rheology is no longer spread between two different sections of the manuscript.

6. 2128, line 1 - what does it mean to say the ice shelf is included 'explicitly'? You need to be more explicit about what the boundary conditions are - a lot of this discussion is relegated to the appendices, but even there it is not very clear what is actually done, and which of the different models are being referred to.

The intended meaning of "explicitly" in this sentence is to refer to our 2D models that include an ice shelf. This comment has been addressed as part of the revision to the discussion of our models' boundary conditions (i.e., the addition of Section 2.2).

7. Figure 2 is not very clear. It appears as if it's showing the model domain in part (a), but on reading the text I think I understand that the ice shelf is never included explicitly as

part of the domain, which is what it looks like in this figure. In part (b), what are the two insets on the left actually showing? The axes need labels. It should be clearer what 0 and 'full' ice stream width refer to on the main panel - do they refer to the transition between fixed and sliding basal conditions?

We believe that the referee's difficulties in interpreting Figure 2 are generally related to deficiencies in the discussion of model boundary conditions in the main text rather than problems inherent to the figure. We have addressed these issues as part of the revised discussion of the models' boundary conditions.

In regards to comment (a), the ice shelf is shown as an option in the 2D models because some of our 2D models did include an ice shelf. We have modified the text to clarify that the ice shelf is explicitly treated in some models. We believe these changes will prevent readers from having the wrong impression that the ice shelf is never treated explicitly.

In regards to (b), the referee's suggestions for improving the inset to Figure 2B are appreciated, and have been incorporated into a revised figure.

8. For the three dimensional models, it is not made clear what the domain is, and what are the boundary conditions applied at the lateral edges?

These issues have been addressed as part of the revision to the discussion of our models' boundary conditions (i.e., the addition of Section 2.2).

The figures show a 10km width 'stream', but presumably this is the region of free slip bed, and the actual domain is wider?

The referee is correct; the actual model domain is wider. This has been discussed more fully as part of the revised discussion of the models' boundary conditions.

In section 4, it is then discussed what the effect of weakening 'the margins' is, but what appears to be done is to weaken all of the domain outside of the middle of the stream - i.e. not just the margins. There is also some confusing discussion about the 'margin width', and position of the margins in this context.

The referee is incorrect in the assertion that the entire domain outside of the middle of the ice stream is weakened. In our models incorporating weakened margins, the only portion of the model domain that has reduced elasticity is the portion of the model within the ice stream. The entirety of the model domain outside of the streaming portion of the model is unmodified. Section 4 and Figure 2 have been modified to more clearly indicate the portion of the model domain that has reduced elasticity to avoid future confusion.

The position of margins is surely controlled by where the transition from basal slip to fixed conditions occurs, rather than by this additional imposition of a change in ice strength.

Indeed, the transition between basal sliding and the fixed basal condition does control the width of the ice stream in our model that incorporate a variable ice strength. However, we do not suggest that that the change in ice strength controls the position of the margins. Rather, our modeling approach is to *a priori* impose the change in ice strength to coincide with the location of the transmission from the fixed basal condition to basal sliding. As part of the revisions to Section 4 described in the previous comment, the discussion of model configuration has been revised to clearly state that the basal boundary condition controls the ultimate width of the ice stream, independent of the ice strength imposed in a given model.

9. 2133, line 3 - this paragraph is not at all clear, and needs to be revisited. In particular the 'note' in the second sentence is very vague - what is the 'marginal damage relationship', and what are 'compliant margin models' in line 11?

We have revised the sixth paragraph of Section 5 to more clearly define the terms "marginal damage relationship" and "compliant margin models."

10. 2135, line 4 - typo Gudmundsson.

This typo has been corrected.

11. 2137, line 9 – this comment that the behavior could be approximated as a linear viscoelastic effect seems to be at odds with the earlier comment about Gudmundsson's work finding nonlinear interaction between modes giving rise to a fortnightly oscillation.

This portion of the third paragraph of Section 5 has been rewritten to more clearly outline the modeling rationale behind modeling individual tidal frequencies instead of applying a more realistic combined tidal loading function.

12. Figure 11 - the shear margins, which I think should be at the outer edges |y| = 5km in this figure, do not appear to have very different viscosity here, as the text suggests - in fact, it appears to be more just that the centre of the ice stream (where the lateral shear is zero) has a noticeably large viscosity, rather than there being particularly weak shear margins.

To some extent, the referee's comment is semantic, as an increased viscosity in the center of the ice stream is equivalent to a decreased viscosity in the margins of the ice stream, just with a different reference viscosity. However, the referee is correct that the viscosity of the center of the ice stream is larger due to the lack of lateral shear stresses. If we adopt the referee's perspective that the central ice has increased viscosity relative to the "normal" viscosity of an ice stream, then the underlying assumption that the shear margins are potentially decoupled from the surrounding ice due to reduced viscosity caused by increased deviatoric stresses is incorrect. In either case, we have demonstrated that incorporating the viscoelasticity into our models did not strongly decouple the ice stream from its lateral margins.

13. Figure 12 - the labelling of 'nondeforming bed' is not accurate I think? Else there would be no ice stream there. It is not clear to me why there needs to be the implied sudden cut off between the tidally influenced region and that region that is not influenced - a smoother transition would work just as well, and is probably more realistic.

The referee misinterprets the meaning of the label "nondeforming bed" due to the unfortunate placement of the label in the original figure. The label was intended to indicate that the bottom of the till layer defined as the location of the "nondeforming bed."   We did not intend to imply an abrupt transition between "highly-weakened till" and a "nondeforming bed," as the referee suggests. Upon review of the figure, we have decided that defining the base of the till layer as nondeforming is unnecessary, and to avoid potential confusion, the label has been removed.

14. 2141 - point 4 here does not seem to be a 'difference' between the models, but rather a comment about this model.

The referee is correct that our fourth point is not really describing a difference in modeling approaches. This fourth point has been reformatted as a separate paragraph.

15. 2144, (15) - the h here is presumably not the same h as in (14) or (16)? Need to use different notation.

The referee is correct that the variable *h* used in Eqn (15) is not the same as the variable *h* used in Eqn (14) and (16). The notation used in Eqn (15) has been changed.

16. 2145, line 6 - has any evidence been shown for this for ice streams other than Rutford? If so I missed it.

This conclusion has been rewritten to more accurately represent the breadth of the conclusion.

17. 2147, line 15 - the sentence starting here is excessively long and does not seem to make sense.

To improve the clarity of the final paragraph of Appendix A, the paragraph has been rewritten.

18. 2148, line 20 - the flotation condition at the grounding line would suggest that the grounding line moves as the tide goes up and down. Are you referring to an average water level here?

The referee is correct, that we are referring to an average water level at this location. The discussion has been rewritten to more clearly denote that the flotation condition (i.e., that the ice stream is neutrally buoyant at the grounding line) is used to calculate the average water level.

19. 2149, (B2) - σ_{flex} does not seem to have units of stress, and is therefore an odd choice of notation. Is this correct?

Unfortunately the original manuscript incorrectly labeled Eqn. (B1) (a two-line equation) as Eqns. (B1) and (B2). The numbering of the equations in Appendix B has been updated to correctly identify the number of equations present (two).

The referee is correct, and equation has been rewritten/re-derived such that σ_{flex} is now in units of stress. We have decided that factoring out the time dependence of the equation, in the form of $F_{Tide}(t)$ does not serve a purpose except to confuse the meaning of Eqn. (B1), and as such, the time dependence of the stresses is shown in variables σ_{flex} and Δh. The labels of the variables present after Eqn. B1 has been updated to reflect the updated equation.

20. 2149, (B3) - should have no + on the right hand side?

This addition sign in Eqn. (B3) is a typo, and was intended to be a multiplication sign. The equation now reads: $\sigma_{applied} = \rho_W g\Delta h(t)$. As discussed in our response to specific comment 19, $F_{Tide}(t)$ has been included as the time dependence of the tidal amplitude Δh and Eqn. (B3) has been corrected to being labeled as Eqn. (B2).

21. 2152, line 4 - the dashed line corresponds to a 'constant loading function' - what does this mean? I struggle to understand where the spatial x dependence for this case comes from in figure C1.

The "constant loading function" refers to a model with $EI\frac{\delta^4 w}{\delta x^4} = \rho g\Delta h$, where $\rho g\Delta h$ is a constant, as described in the fourth paragraph of Appendix C. To avoid confusion, a parenthetical identification of this function as the "constant loading function" has been added to the end of the first sentence of this paragraph. The spatial (*x*) dependences of this case shown in Figure C1 come from the dependence of the deflection *w*, and thus the upper edge stress σ_x, on the spatial coordinate *x*, as described in Eqn. C2.