First of all, the question of transverse effects is barely addressed since all the plots of the 3D simulations are provided at the medial line, far from the lateral boundaries and no attention is paid to the transverse behavior (see also the comment about page 669 in the line-by-line section). If it is not relevant, it should at least be strictly pointed out and justified.

-The majority of results are given along the medial line because the GPS measurements of ice stream response to tidal forcing which inspired this investigation are made along the medial line, also the authors felt a simple line plot along one point would be easier to interpret than a plot that includes measures along other flow lines.

-That is not to say that lateral effects are not relevant, indeed they are very important, our goal in including them is to show that although a fixed sidewall leads to a change in the ice stream flow, it does not alter the mechanism that produces Msf modulation of velocities, something that has previously been called into question.

-In addition, the lateral affects are shown in figure 5 which plots the lateral change of Msf amplitude and phase. The authors felt was the most relevant full 3d plot to include in this study since it demonstrates that an Msf response is weaker near the sidewalls.

The introduction of a grounding line migration in the 2D model is an interesting feature which seems to lead significant change in the amplitude of the de-trended displacements but the only result provided is a qualitative observation, on the fact that the observed period is preserved. As a matter of fact, if the grounding line strictly migrates with the period of the tide signal, it is probably to be expected. A more thorough study of that problem should be carried on. At least, a plot relating the grounding line position to the resulting displacement should be provided.

-The reviewer is correct in pointing out that the grounding line migrates with the tide. However the effect on ice flow is difficult quantify/estimate without conducting a numerical study of the type given. This is related to, among other things, the fact that grounding line migration can have the opposite effect on flow to that of direct stress transmission ie. Velocity increases during high tide due to a reduction in basal shear stress.

-For the purposes of this study we feel that focusing on directly observable quantities such as the displacement at the surface is the best approach. We feel that addressing grounding line migration in more detail in the paper might distract from the main subject of our work which is the effect of tides on surface motion of ice streams.

-We are however happy to include a plot of grounding line position versus time for different loading periods if required.

The runs using a more complex tidal signal are almost not investigated. The only observation made from the results is that the 3D effects appears to operate even far from the lateral boundary but there is probably a simpler way of showing that and the relevancy of using a complex tidal signal is never given.

-Figure 5 shows how the amplitude and phase of the Msf tide change both with distance from the side wall and in an up-stream direction. In this particular run we forced the model with a realistic tidal forcing based on the CATS model.

-We can understand that it might be confusing that in most other examples we use a simpler forcing. However, we do feel that it is important to show that the Msf arises from a realistic tidal input as well.

-We will make it clearer in the revisions why a different forcing is used in this case.

Conversely, in the same section, the study of exponential decreasing of the amplitude of the signal is made using periodic forcing which are not of tidal nature with linear sliding and rheology. While the use of simplified signals appears relevant to me, motivating this study from the use of a real complex tidal signal with non linear rheology and sliding with almost no investigation of the resulting effects seems unnecessary.

-The non-tidal periodic forgings used for the length scale results were chosen for convenience, there is no need to use tidal periods in order to investigate the visco-elastic response and the choice of periods was made to get reasonable resolution of both the elastic and viscous responses of the model.

-The fact that our results closely match the derived analytical stress-coupling length scale helps to verify our numerical model and gives increased confidence in the accuracy of our numerical results. The analytical solution also shows, for the first time, directly how phase and amplitudes vary with distance upstream from the grounding line.

I am somewhat confused as to what the authors claim to have achieved with the analytical retrieving of the 12 days period bound on the tidal period they observed in the numerical simulation. The fact that for long loading period, the response of the viscoelastic material tends to the purely viscous response is to be expected and the observed smooth transition between elastic behavior and viscous behavior is a normal response of the viscoelastic model. The precise value of the loading period required to get rid of the elastic behavior is naturally completely dependent on the values of the parameters E, mu and eta.

-As we state in the discussion, the fact that the response of an ice stream to a periodic forcing can be a function of that forcing period is something that has been ignored by previous authors. This is particularly relevant for any investigation into the tidal response of ice streams since the largest tidal periods will be less than the Maxwell relaxation time and a purely viscous model will not give the correct response.

In addition, the 3D run is done using a very rapid sliding on a bed without topography (see also the comment about page 674 line 22 in the line-by-line section) and observed far from the lateral boundaries in the fully linear case (m = n = 1) which appears to me as a rather idealistic situation leading to a Stokes simulation very close to the shelfy-stream approximation. The almost exact match obtained between the analytical result (derived in a very simplified case) and the 3D numerical results mainly highlights, according to me, the over-simplification of both the numerical simulation and the analytical derivation. For instance, a comparison with a non-linear viscoelastic Stokes model could provide insights on the effect of the non linearities on this result.
Indeed, a crucial aspect of this work, as pointed out several times in the paper, is to corroborate the need for a non-linear sliding law to model the response of the ice-streams to tidal forcing. From that perspective, I am not sure to see the purpose of these fully linear experiments and calculations.

-We use simplified geometry because we are focusing on the general aspects of the response of ice streams to tidal forcing.

-By providing a direct solution to the linear case the analytical solution is helpful when identifying the non-linear aspects of the numerical solution.

-We agree with the reviewer that we did not give examples of how the length-scales of stress-coupling are affected by non-linear till behaviour. We will therefore conduct simulations investigating this in some detail and include an additional curve in the figure showing a non-linear example.

-The requirement of a non-linear sliding law is only a part of this study, our overall aims are to model an ice stream's response to the tides and gain some insight into their flow.

In a more general perspective, as it is pointed out in the introduction, what appears to me as a key aspect of the modeling of this process is the question of a possible net forward motion due to the non-linear coupling between tidal waves and the ice-stream/ice-shelf/sliding coupled problem. This major question is never brought up again in the paper and all the de-trended displacements plotted seems to have a zero mean, and therefore no real implication on a longer time scale flow (in terms of mass balance for instance). It appears to me that some quantifications could be relevant within such a study.

-The curves have zero mean because they are de-trended.

-The reason we chose not to provide a numerical estimate of the effect on net forward motion for this simple model is we felt it would be misleading since the magnitude of this effect will be dependent on the amplitude of the tidal response upstream, which we do not attempt to match quantitatively with observations.

-That being said, there is a shift in mean velocity. We will emphasise this point more strongly in the revised version of the paper.

The abstract and introduction are formulated in a rather misleading way, leading to think that the study of lateral effects and grounding line migration have been done in a complete 3D model (which is not the case). The last sentence of the overview states that issues are addressed "with a full 3-D model including grounding line migration" which is more than misleading.

-The authors accept that the phrasing here could be misleading, this was absolutely not the intention and these sentences will be re-worded to make it clear that grounding line migration and 3D affects are investigated separately.

<u>Line by line comments</u>

<u>Abstract</u>

p660-l4: Since a viscoleastic rheology is considered, the glaciology terminology "full Stokes" seems unappropriate and I would call it a non linear viscoelastic Stokes model.

-This is a question of terminology. We understand the point raised by the reviewer, but feel that including the term `full' is in line with commonly used terminology in glaciology.

p660-l7: Precise that long period modulations are qualitatively reproduced.

p660-l8: Precise that the inclusion of lateral effects and grounding line boundary are not considered in a couple way and not both on 3D runs. Precise what do you mean by "do not alter this result".
p660-l9-12: Precise that the stress-coupling length scale study is done on a fully linear model.

-We will make these points clearer in the revision.

Introduction

p661-l13: The authors speak about studying "the effects in the transverse direction" which is misleading according to the presented results

-We will add a more detailed discussion of the transverse effects in the revision.

Overview

p661-l27: "near, near", typo problem

-This will be corrected in the revision.

p662-l16 and further: I don't know much about tides and tidal components and almost no information is given on the components considered along the paper such as Msf, Mf, M_2, S_2, O_1, K_1 etc. At least a reference could be helpful.

-We will include some more detail on the tidal components in the revision along with a general description of tides around Antarctica.

p664-l10: This consideration on very high value for the exponent m raises the question of the use of a Coulomb-type friction law which appears interesting to me. Have you considered this type of sliding law?

-No we have no implemented a Coulomb-type sliding law. We agree that this could be very interesting and may do so in future work.

Methods

p665-l20: The parameter G is never defined with respect to E and mu

-We will add the relevant equation relating G to E and mu in the revision.

p667-l11: What is referred as the ice-bed interface? Is it the till-bed interface or the ice-till interface? If I understand correctly, the resulting sliding velocity is thus the sum of the till deformation and the sliding? Please, give some more precisions on that aspect of the model.

-The reviewer is correct. We will re-phrase any references to this aspect of the model to make it absolutely clear what is meant.

p669, subsection 3.4: From this description and the plot of the 3D domain on Figure 1, it seems that the 3D run was not refined at the grounding line, while you point out that the 2D runs requires a strong refinement at the grounding line. If this is the case, it appears to be a strong limitation on the quality and the reliability of the 3D runs.

-The 2D simulation requires a refined grid at the grounding line in order to correctly resolve the migration, as it has been shown in previous studies that the solution is very sensitive to grid size. Since the 3D simulation uses a fixed grounding line location there is no need to refine the grid around this point.

p669-l22: Provide a justification for plotting only the results at the medial line for all the 3D runs.

-This is covered in the first point.

Discussion

p674-l22: This result requires quite a few assumptions to be obtain. The main one is the assumption that h does not depend on x. In that case, it is crucial to include a bed and surface topography (i.e. variations around the mean slope). Otherwise, typically in the case of a parallel slab, the resulting velocity field would not depend on x and the calculation would make no sense.

-In the SSA approximation it is not in general the case that velocity will not depend on x even if the geometry does not. One can have a situation where ice thickness and surface slopes are constant, but the solution varies with x due to boundary conditions.

In other respect, I am not a specialist of SSA but I was wondering if it is safe to replace the viscous constitutive law by a viscoelastic one within the equation, since the derivation of such an equations relies on several assumptions and simplifications that, I believe, are made according to the viscous model.

-The only possibly somewhat questionable assumption that does not carry over directly to the elastic case is that of incompressibility. However, because the Poisson ratio is a function of loading period and close to 0.5 at tidal periods, this error is small.

Figures

Figure 5: Why does the plot stops at z = 30km since the domain is 32km wide?

-This will be corrected in the revision.

Figure 7: The y-axis of the upper-plot is labeled lambda and should be labeled L.

-This will be corrected in the revision.

We would like to thank the reviewer for their insightful comments which will help to greatly improve the quality of this manuscript.