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Interactive comment on "Insights into ice stream dynamics through modeling their response to tidal forcing" *by* S. H. R. Rosier et al.

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1 Summary of the content

This study investigates the effects of tidal forcing on the flow of grounded ice in terms of modulation of the horizontal surface velocities using a nonlinear viscoelastic rheology. A solid overview of the previous studies is proposed followed by a presentation of the Stokes viscoelastic model and the modeling of the base of the grounded ice as a nonlinear viscous till layer coupled with a nonlinear Weertman sliding law. The floating ice modeling is achieved by imposing a time dependent water pressure representing the tide. A contact algorithm for the movement of the grounding line is also presented.

The first part of the paper consists in results of the simulation of the 3D model with

a fixed grounding line. The authors find that they qualitatively reproduce the result of a previous 2D paper (Gudmundsson 2011) at the medial line of the symmetrical 3D flow. The result is that the use of a non-linear Weertman sliding law is required to simulate the long period modulation of the horizontal velocities, particularly the Msf period.

Effects of the grounding line migration are then studied on a 2D situation with a refined mesh at the grounding line and they show that the phenomena is qualitatively preserved with a moving grounding line.

The third part uses a more complex tide signal on the 3D model and the component (amplitude and phase) of the Msf signal within the horizontal surface velocities upstream of the grounding line are plotted as a 2D plot on almost the whole surface of the domain. The amplitude plot seems to demonstrate that the simulation presents 3D effects even close to the medial line.

On that basis, a more systematic analysis is carried out using a linear rheology and a linear friction law. The numerical result, observed at the medial line, is an exponential decay of the amplitude of the signal with the upstream distance to the grounding line, and the decreasing rate depends on the loading period with an apparent limit for loading period greater than 12 days. An analytical calculation is proposed, in the linear case, on the 1D shallow-shelf approximation modified with a viscoelastic rheology in order to understand that 12 days bound on the loading period for the rate of exponential amplitude decreasing.

2 General comments

In the present state, this study appears as a preliminary approach for an interesting yet complex question on the response of a viscoelastic ice-stream to tidal forcing. However, there are many issues regarding the 3D modeling and the 2D modeling, the numerical/analytical comparison and the meaning of the results. The present investigation, while certainly of interest, is somewhat disjointed and lacks of a clear scientific objective. I recommand to rework the paper along the comments pointed hereafter, and to present the results in a conclusive form.

3 Specific comments

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 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 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. 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.

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, ν and η.

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.

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 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.

4 Line by line comments

Abstract

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

model.

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.

Introduction

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

Overview

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

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.

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?

Methods

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

p667-l11: What is referred as the ice-bed interface? Is is 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.

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.

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

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 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 simplificatons that, I believe, are made according to the viscous model.

Figures

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

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

Interactive comment on The Cryosphere Discuss., 8, 659, 2014.