# Manuscript: Evolution of ice-shelf channels in Antarctic ice shelves

## Response to reviewer's comment RC C309

Thank you for your constructive comments which helped to improve the current version of the manuscript, particularly in the way the model setup is explained. Virtually all of the proposed points have been implemented in the revised version. Attached is  a point-by-point response. Reviewer's comments are marked italic, responses are marked in bold. In the revised manuscript changes are marked in bold.

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*1 - The fact that the ice-shelf is not at the hydrostatic equilibrium is not only true in the channels vicinity. This is also occurring in the vicinity of the grounding line (GL), and can be shown both on observations [e.g. Anandakrishnan et al., 2007] and modelling [Lestringant, 1994; Durand et al., 2009]. The point is that specificity is not accounted for in the modelling as it seems that at the inflow BC (at the GL), the ice thickness is imposed to the floatation thickness. This is not clearly specified (it is only said that the mass flux is specified at this boundary). I would first suggest to be more precise on the landward BC. What is the horizontal velocity profile (homogeneous i guess)? Is only the horizontal velocity imposed? Not the vertical and lateral? Is the ice thickness imposed? Then, I am wondering how strong is this latter hypothesis. From Figs. 2 and 3, one can see that the melt is not imposed right at the GL, but few km downstream (how many?). Why? How are influenced the results if the melt is imposed closer to the GL, as expected from observations. My understanding is that by prescribing the ice thickness at the GL, the hydrostatic equilibrium is forced there and the melt has then to be artificially shifted downstream the GL if one wants to observe the bridging effect. I think this modelling point should be at least mentioned and the influence of a fixed geometry at the GL discussed.*

**Agreed with comment and implemented in revised version in section 2.4. The simulations presented here do not include the hydrostatic imbalance which naturally occurs at the grounding-line (indicated in the initial manuscript  by  "excluding grounding-line dynamics" (p. 1611, l. 5)). On the contrary, hydrostatic equilibrium is forced. During the ice-shelf evolution, the landward thickness can evolve freely and the inflow velocities are adjusted so that the mass flux remains constant (for example: initial conditions are 500 m landward thickness and 100 m a$^{-1}$ inflow velocities, lateral and vertical velocities are initially zero; after relaxation to steady state the landward boundary is 436 m with an inflow of 115 m a$^{-1}$ for the unconstrained cases).  Because hydrostatic equilibrium is forced at the landward boundary, melting is initiated farther downstream to avoid numerical complications.  This was not well enough explained and is now more detailed in section 2.4. Due to the model simplifications, no claims can be made about how channel initiate at the grounding-line and what the effects of the hydrostatic imbalance would be. The focus here is how channels evolve downstream of the grounding-line and how that compares to data which were collected comparatively close to the ice-shelf front of RBIS.**

*2 - My second main concern is about the use of the surface topography in the vicinity of the channels and how it compares from the observation and the modelling. From Fig. 1 it is obvious that these channels are visible from the surface. I am then wondering why the measured surface topography (especially the one transverse to the channels) is not compared to the modelled one? I agree that the real surface topography is not only the result of the channels but also the perturbation of the accumulation distribution by the presence of a depression above the channels, which is certainly too complex to be accounted for in the modelling. But, it might be that some signatures of the surface topography are still observable and could be compared to the modelling. At least, this should be discussed.*

**Agreed with comment and implemented in revised version in Figures 4c and 5c. New Figures 4c and 5c now include the modeled and measured surface topography. Both figures agree qualitatively: Narrow channels with bridging are equally incised at the ice-shelf bottom as the wider channels where bridging is negligible. However, the corresponding depression at the surface are significantly shallower for the**

**narrower channels. This is discussed in the revised version.**


*3 - As I said in the introduction, the strength of the work is certainly to couple both observations and modelling. In some sense this is also its weakness because the modelling should have been performed using the Roi Baudouin Ice Shelf geometry, which would have allow to get more specific conclusions (about melt rate for example) on the observed channels. I know it would have been a more challenging modelling effort, but the choice of a simplified and synthetic geometry should be better justified.*

**Ok and partially implemented in section 4. A real case application would give more quantitative results and that is something to be done in the future. However, a number of variables are yet uncertain, to name only two which have been discussed in the original version: (1) how does the surface mass balance change inside the channels and how does the change depend on the channel orientation relative to the main wind direction? The radar data shown here (and, for example, in Langley et al., 2014 (GRL, doi: 10.1002/2013GL058947)) indicate that this effect can be quite strong and it is not straightforward to model it quantitatively (as stated in your own comment #2); (2) does the density change inside the channels? If so, this imprints the traveltime-to-depth conversion of shallow layering and the corresponding SMB estimate, as well as the hydrostatic inversion. The advantage of using a basic synthetic geometry is that all mechanisms of the channel creation/advection/decay can be easily distinguished. Following the suggestions given here, this is more clearly motivated in the revised version in section 2.4.**

*page 1604, line 9: Inverting surface elevation for ice thickness -> Inverting surface elevation assuming hydrostatic equilibrium for ice thickness*

**Ok, inserted.**

*page 1604, line 22: I am not sure the Schoof (207) reference is relevant for this sentence.*

**Ok, removed.**

*page 1605, line 16: entirely Rignot and Steffen (2008). -> entirely (Rignot and Steffen, 2008).*

**Ok, inserted brackets.**

*page 1608, line 25: the choice of i = 900 kg/m3 should be discussed. Other works related to ice-shelves hydrostatic equilibrium, as the cited one by Holland et al. (2011), are using a higher value.*

**Ok, also in-line with the comment of Reviewer 2 it was updated to 918 kg m$^{-3}$ which is a standard value. The initial reasoning was that pure ice density is only reached in very deep ice, but there was no strong justification for using 900 kg m$^{-3}$.**


*page 1610, line 20: from crevassetops -> from crevasse tops*

**Ok, implemented.**

*page 1611, 2.4 Model setup: missing information should be added (see main point 1). Also, I guess that as in the previous works using Elmer/Ice, you have specified a viscous spring at the base of the ice-shelf to account for the depth dependency of the sea hydrostatic pressure? This should be mentioned.*

**Ok, implemented in revised section 2.4. The viscous spring has been used and is explained and referenced in revised version.**

*page 1613, line 19: the choice of applying or not the lateral friction is not clearly discussed. It seems that it is switched on or off in an ad hoc way, but its effect is not really discussed. Would the results be similar if instead of*

*applying lateral friction to decrease the main flow the inflow flux itself would simply be decreased (and no lateral friction applied)? In other words, are there other effects induced by lateral friction than decreasing the main ice flow of the ice-shelf?*

**Ok. Also in-line with comments from Reviewer 2 this is better explained at the end of section 2.4 and in the discussion. The point of applying lateral friction is to reduce longitudinal stretching rates to provide a scenario which can explain the enhanced horizontal shearing across a channel in the observations. This effect does not occur if longitudinal stretching is too dominant (as is the case for the unconstrained scenarios).**

*page 1615, line 22: which is too large at the channel trough, and too small at the channel flanks -> (?) which is larger at the channel trough, and smaller at the channel flanks*

**ok, implemented.**

*page 1615: I am wondering if having two channels in one model conducts to the same results/conclusions than having two simulations of one channels at a time. In other words, are the two channels interacting and influencing each other, or are they sufficiently distant not to interact? Is the purpose of having two channels with different melt distribution for the modelling MS4 only to have one plot showing two channels at a time? This point should be discussed/specified.*

**Ok, implemented in sections 3.2 and 4. In terms of the two channel's amplitudes and bridging there is no significant interaction between the two channels because they are sufficiently far apart. The purpose of having two channels is to more easily compare the simulations to the radar data.**

*Table 1: I would suggest to add a column with the number of channels (1 except for MS4 for which it is 2)*

**Ok, implemented.**

*Figure 2: I would suggest to remove the unnecessary black background*

**Ok, implemented.**

*Figure 3c: is the channel amplitude? It is not clear from the legend of the figure.*

**Misunderstanding: Channel amplitude was indicated by Y-Label. Legend marks melt scenarios.**

*Figures 4 and 5 are two small. The axe texts are difficult to read. I understand the paper was initially prepared for GRL, which has a limited number of figures. I would suggest to split these two figures in four figures to make them more readable. It should be also specified in the Legend of Fig. 4 that the two channels for the case MS4 have different melt distribution, as specified in Table 1.*

**Ok, implemented in new format/captions of Figures 4-7.**