Author's response to anonymous referee #2.
"Brief Communication: Can recent ice discharges following the Larsen-B ice-shelf collapse be used to infer the driving mechanisms of millennial-scale variations of the Laurentide ice sheet?"

Overview

This paper quantifies the influence of ice-shelf collapse on ice-stream motion in the Laurentide ice-shelf, with the intent of understanding whether this could be the cause of Heinrich events. It is the first such paper to do this, and whether this mechanism is viable is something worth investigating. I am half-convinced by this paper, but General Points 2-7 give my reasons for wondering whether a more detailed study will give different results. A particular concern is that there is no effort to demonstrate that the calculated speed-ups are sufficient to have caused HE's.

Referee Coment: *I think that more modelling results are needed, and particular attention needs to be paid to Point 7; could these episodes be causes of Heinrich Events. This is not the same as saying that model runs need to be done; more needs to be presented to show how the mecahnisms works, and whether it is a viable explanation for HEs.*

Authors Comment: We would like to thank the anonymous reviewer for raising several interesting points concerning our manuscript. In the revised manuscript, we will attempt to modify the presentation of the modeling results in order to reflect your concerns. Please find a response to each comment below.

General points

Referee Comment 1 (RC 1): *1. 3117. I understood that one of the mysteries of HEs was that they occurred during cold periods. I understand that increasing melt is a modelling mechanism for getting rid of ice-shelves, but what is the justification for having ice-shelves break-up during cold periods? Do you reject this correlation? - or can you think up of a justification?*

Authors Comment 1 (AC 1): It is true that initially it seems paradoxical that there would be increased melt and ice shelf break up during cold periods. However, it has recently been suggested (and simulated) that cold periods in Greenland (i.e. stadials) prior to HEs could be accompanied by subsurface oceanic warming events (Shaffer et al, 2004; Marcott et al, 2011; Brady et al, 2011, Gutjahr et al, 2011). The mechanism responsible lies with the fact that during stadials oceanic convection sites are strongly reduced. This favors a decrease in the sea to air released heat and expansion of the sea ice, but also implies a warming of the subsurface waters by reduction of vertical mixing. This warming peaks at a depth between 500 and 1100 meters, corresponding with the basal layer of the ice shelves. The associated heat flux reduces the ice-shelf thickness through enhanced basal melting. The crevasse depth eventually grows large relative to the ice-shelf thickness, thereby favoring a major calving rate. Thus, subsurface oceanic warming during Greenland stadials would provide the appropriate mechanism leading to increased melting of the ice shelves during cold periods. This is now explicitly addressed in the new version of the manuscript.

RC 2: *The grid resolution is not mentioned at all, - looking at Figure 2, I would guess that it is 20km. I am concerned that low resolution has enhanced the upstream propagation of effects, in the same as* *numerical diffusion is stronger in coarse grids. You certainly need to quote the grid resolution, and you need to demonstrate that it is not exaggerating the horizontal extent of propagation.*

AC 2: We apologize for omitting this information from the text and we will add a discussion of this point to the revised manuscript. The ice sheet model resolution used for these simulations is 40 km. This responds to the necessity to accomplish a good compromise between resolution and the spatio-temporal scales. A 20 km grid resolution, for example, would imply an approximately eightfold increase in the computational time for these simulations, which is not currently practical when dealing with runs of more than several kyrs on the hemispheric scale. Nonetheless, comparing our results with several other studies leads us to believe that neither the dynamic upstream propagation nor diffusion are exaggerated here. Firstly, the propagation of the force imbalance related to the ice-shelf breakup is here treated under the Shallow Shelf Approximation (SSA) thus including longitudinal stresses. This approach has been successfully applied to hemispherical-scale studies (Peyaud et al, 2007, Alvarez-Solas et al, 2011) but also to individual ice streams or ice-stream basins on considerably finer resolution (MacAyeal, 1989; Hulbe and MacAyeal, 1999) even for recovering observed velocities through inverse modelling. The “dragging ice shelf” approach for treating ice streams in the SSA has also been used under larger resolution runs but with idealized configurations (Bueler and Brown, 2009). In this later paper, the possible effects of grid resolution on the ice streams dynamics are discussed, and no obvious convergence of the results to the finer resolution were found, suggesting that even coarser ice streams were well treated under this approach. It is important to note that this approach represents a considerable improvement with respect to other studies devoted to the Laurentide dynamics. These were carried out with similar grid resolution but that only based on the Shallow Ice Approximation (SIA) and therefore they simulated a speed-up propagation that suffers from strong numerical enhancement (via artificial propagation of changes in the surface slope, point by point). More specifically, and related the next point, Payne et al (2004) estimate the values for the characteristic response times of the diffusive terms and the kinematic-wave (as a result of the grounded-line stress imbalance) to be ~20 and ~120yrs, respectively, for a length scale of 200 km. Regarding our results, the perturbation exerted by the ice-shelf removal diffuses towards the ice-stream source within ~100 yrs while the kinematic wave does so within ~700 yrs, which considering a length-scale of ~1000 km gives quite similar numbers. This is also the case when comparing (the diffusion term) with the Nick et al, 2009 paper (~2.5 yrs for a length-scale of 20 km). These numbers can now be roughly estimated by eye helped by the new figures 2 and 3 attached here. It is also important to note that we are beginning to carry out and analyze new GRISLI runs under idealized configurations (roughly based on the HEINO project; Calov et al, 2010) in order to systematically quantify the effects of buttressing removal on the inland dynamics. These preliminary results (beyond the scope of this brief communication) carried out with different grid resolutions (between 40 and 15 km) do not show significant differences on the upward speed-up propagation. On the other hand, we have to recognize that one possible exaggeration related to the grid resolution lies with the ice stream length. Whereas the existence of a large, extended and very active Hudson-Bay/Strait ice stream (as we simulate here) is suggested by several data and modeling studies, our grid resolution may not properly capture several tributary glaciers. We believe however that the total related ice discharge in response to the ice-shelf perturbation would result with the same order (See point 7).

RC 3: *There are quite a few similar calculations regarding the effect of removing buttressing - for example Payne et al. , GRL, 2004; Nick et al., Nature Geoscience, 2009. A comparison of your results with theirs would be useful. They show broadly the same pattern.*

AC 3: Thank you for pointing this out. A comparison with these papers has been now added to the new version of the manuscript. Indeed they show a similar pattern, which in fact we believe responds to the fact that the physics required to simulate such processes is successfully captured with the GRISLI model used here (despite its coarse resolution, which is related to the above point).

RC 4: *Figure 1. The graphics are pretty poor - are you plotting the LIS as such to make it comparable with Le Brocq's data? It doesn't really work. A minium level of detail is a contour map of LIS with velocity magnitude color-coded on, so that we can see what is being modelled. I don't see the point of having the zoom of the Peninsula.*

AC 4: The idea behind the figure was too give a rough impression, comparing the Laurentide Ice Sheet and Antartica, highlighting where the major ice streams being compared are. We have added contour information and made the figure more clear in the new version as well as we removed the zoom of the Peninsula. A new figure1 is also attached here.

RC 5: *Likewise, why plot the velocity data from the Crane Glacier. This is a tiny glacier; what are your reasons for expecting this to scale up? Surely the topographic setting is completely different?*

AC 5: The spatial scale of Crane Glacier is quite different from the Hudson Straight ice stream, however from the perspective of the force balance, both cases have important similarities. In both cases, a fast flowing ice stream flows into an embayed (buttressed) ice shelf. When the ice shelf is removed, the pattern of the velocity response is largely similar. The response occurs on different time scales, but this should be expected from the different sizes. Again, the idea is not to say these are identical scenarios. Rather, the similarity of the response lends credibility to the notion that the millennial-scale variability of the Laurentide could be a result of changes in ice shelf buttressing conditions.
This point is somehow related to the referee's general point 3, since the spatial scales of the Greenland and Pine Island cases analyzed in Payne 2004 and in Nick 2009 papers are also much smaller (similar to Crane Glacier), but showing, as the referee pointed put, a similar pattern. We think that showing the Crane Glacier profile is indeed not an indispensable point but that keeping it would help the reader to visualize the analogy that motivated this paper.

RC 6: *There is no indication of the amount of grounding-line retreat. I would expect Figure 2 to show this. What is the origin in both cases - the maximum grounding line? You need to mark where the grounding line is on each of the lines.*

AC 6: Figure 2 has now been significantly improved. According to this comment we decided to include a whole new figure where the cross profile of the Hudson Strait ice stream (at different time steps) is shown. We apologize for not commenting on the grounded line issue in the previous version. As this new figure 2 shows, the grounded line does not migrate during our simulations. This has indirectly been done on purpose, since as suggested by others (e.g. Schoof 2007), the issue of grounding line migration represents an entire issue by itself. The non-migrating case shown here facilitates the task by focusing on the inland dynamic effects of the ice-shelf breakup.

RC 7: *While ice velocities do increase markedly, they only increase by a factor of two. How much extra ice is released, with the melting of the shelf and the increased ice flow. Isn't the period of increased* *velocity determined by the period over which you increase the melt? Would there by a HE signal if you allowed the shelf to regrow immediately.*

AC 7: The released ice represents a mean flux of 0.04 Sv during the first 1000 years of increased ice-shelf melting, but continues during approximately other 1000 years (compared with a control run with no ice-shelf removal) with a weaker mean flux of 0.02 Sv corresponding to the phase of a regrowing ice shelf. Then, to the question: *Isn't the period of increased velocity determined by the period over which you increase the melt?,* the answer is: not directly, because when enhanced melt stops the ice shelf needs time to regrow in order to buttress again and decrease velocities.
Concerning the question of an instant ice-shelf regrowing, it is likely that in that case no major discharges candidate for a HE could be created. Nevertheless, data suggest that stadial periods (not necessarily Heinrich stadials which are longer) cover at least ~1 kyr and that according to the oceanic subsurface warming mechanism, enhanced basal melt would stand long enough to maintain a substantial discharge. This iceberg surge could moreover further affect remaining oceanic convection, favoring the conditions for warmer subsurface waters and colder surface and thus giving an explanation to the longer duration of Heinrich stadials with respect to non-Heinrich stadials.
Coming back to the important point about whether the simulated acceleration is a plausible cause of HEs, it is important to note that the above mentioned ice discharge implies a sea-level rise of about 2 meters which agrees with the isotopic-modelling-based estimation of Roche et al. (2004), and represents a total amount of $190 \cdot 10^4\ km^3$ of ice, well above the required minimum estimated by Roberts et al (2011) using an iceberg/sediment model ($60 \cdot 10^4\ km^3$). This discussion has been added to new version of the manuscript.

Minor points

RC 1: *3115: lines 25. References to Alley and Whillans, Vaughan and others a bit out-of-date; the observations cited later on surely suggest opposite?*

AC 1: Yes, the idea here was to highlight the fact that despite the fact that theoretical work predicted a significant response to a potential buttressing removal, the limited observations at that time (no major ice-shelf breakups) could not confirm such a statement.

RC 2: *3117 What is MacAyeal's L1 equation?*

AC 2: This sentence has been modified in the new version.
What we meant by MacAyeal's L1 equation is the first governing equation in MacAyeal (1989), which describes the vertically integrated balance of horizontal forces and considers the same case for ice streams and ice shelves when adding a basal dragging to the first.

RC 3: *3118 'spreads anisotropically' - a bit clumsy. Do you mean that it's an ice tongue extending farther in the predominant flow direction?*

AC 3: "Spreads anisotropically" is meant to imply that there is none stress present at the ice-shelf boundaries that determine a favored direction of spread. We have modified this description to be more clear.

RC 4: *3119 'half as sensitive' - not a clearly defined phrase. The index on the buttressing parameter is around half that of the index on the thickness. Make this statement more precise.*

AC 4: Thank you, we have now clarified this statement.

RC5: *Figure 2. What is the cause of the velocity oscillations? Presumably numerical - shouldn't we be worried?*

AC 5: The main cause of the wiggly curves shown in the previous version of the manuscript is related to the chosen profile points used. This profile was based on single points connected to each other from the ice stream source to the grounding line. Thus we followed the ice stream point by point, without any criteria based on continuity and therefore some chosen points could be placed slightly outside the profile creating these apparent oscillations. We now carefully followed maxima in velocity by considering the 8 neighbors, and then averaging the profile with distance weighting. The development of the hybrid approach (SIA+SSA) in GRISLI has been possible thanks to a quite stable numerical scheme.

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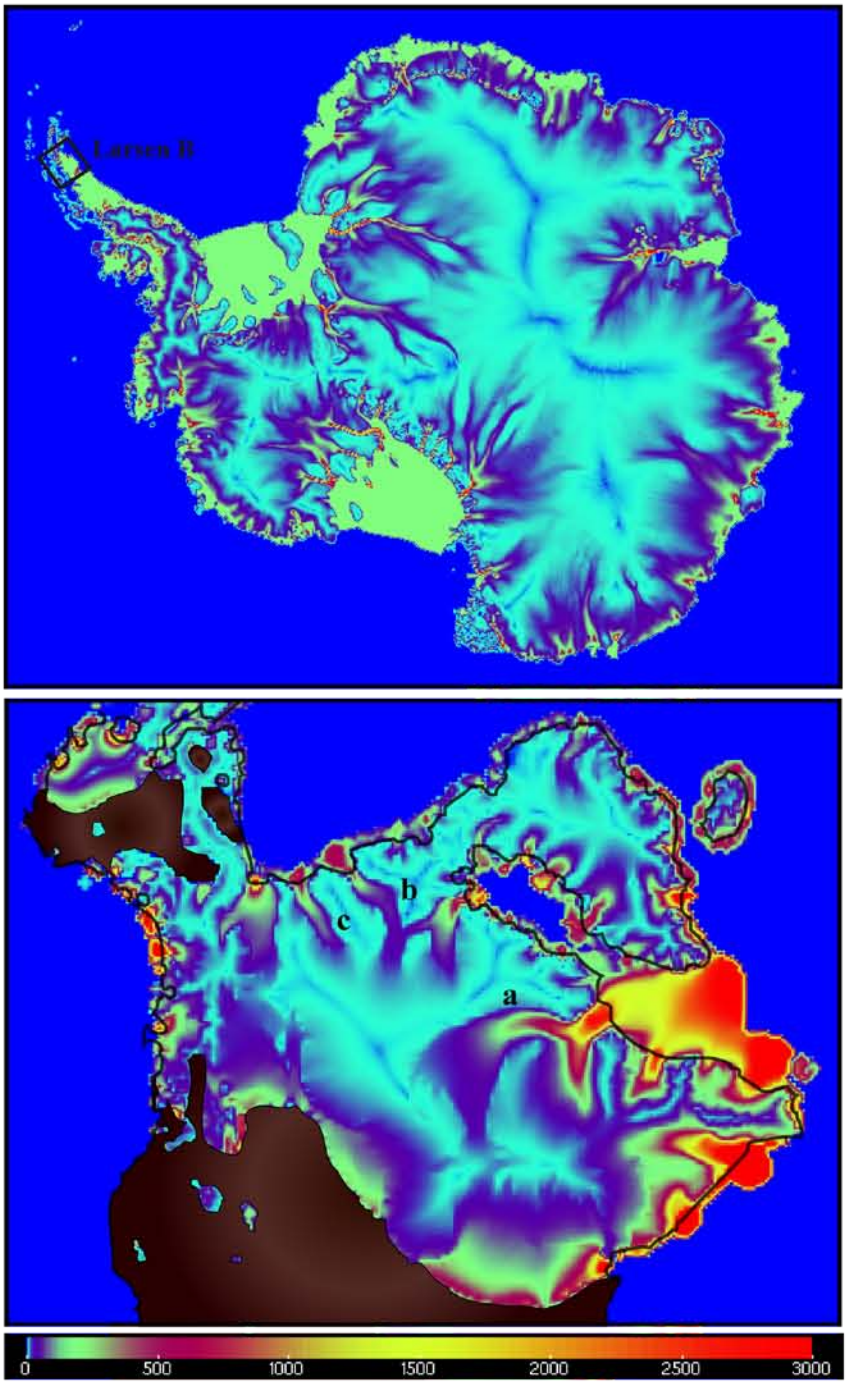
New figures



Ice velocity (m/a)

Fig 1. Top: Present-day Antarctic surface ice velocities obtained from the SeaRISE data website (Le Brocq et al., 2010). Bottom: Simulated Laurentide ice velocities during the last glacial maximum. ***a***,***b*** and ***c*** illustrate the location of the Hudson-Bay/Strait, MClure Strait and Amundsen Gulf ice streams, respectively.

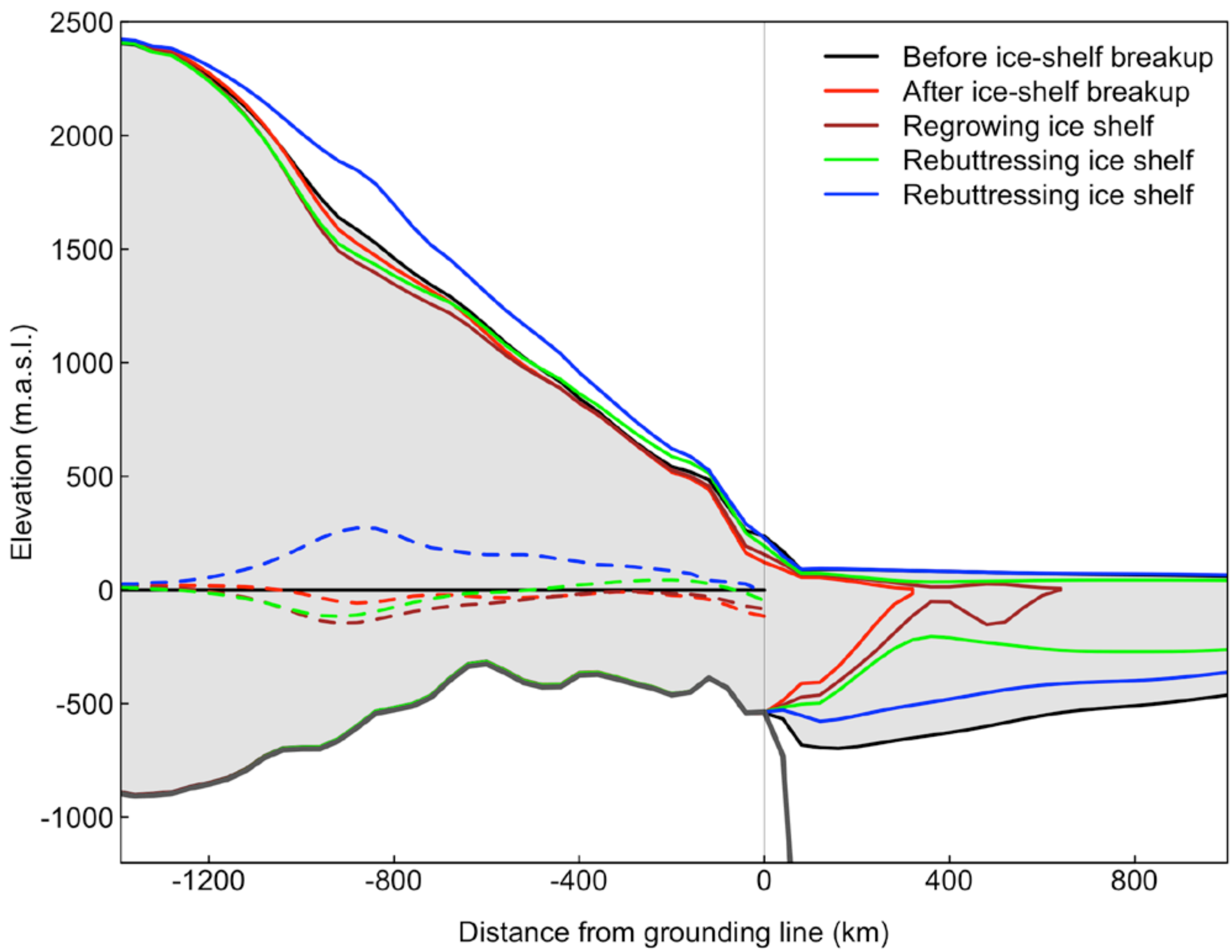


Fig 2. Simulated along-flow profiles of surface elevation. Colors indicate different phases of the Hudson-Bay/Strait ice stream with respect to the Labrador ice shelf status and go from black (initial unperturbed steady state) to red (after 500 yrs of enhanced basal melting), brown (1100 yrs after the onset of the perturbation; 100 yrs after the end of the enhanced basal melting period), green (1900 yrs after the onset of the perturbation; 900 yrs after the end of the enhanced basal melting period) and blue (3000 yrs after the onset of the perturbation; 2000 yrs after the end of the enhanced basal melting period).

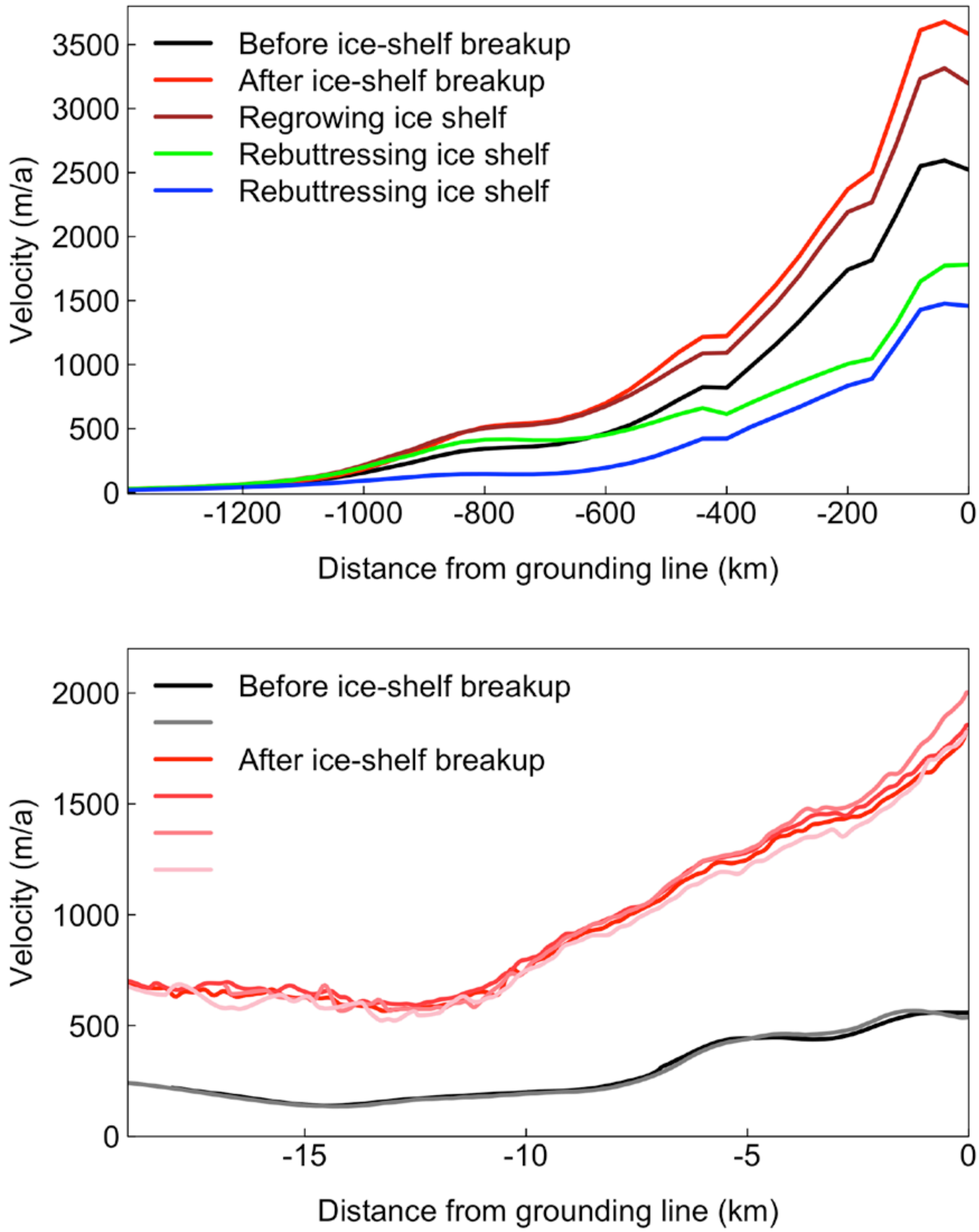


Fig 3. Top: Simulated along-flow profiles of ice velocity. The lines are color-coded for time as in Fig.2. Bottom: Surface ice velocity of the Crane Glacier profile; derived from the satellite data published by Rott et al. (2011) and shown in their Fig. 6. The different profiles, from black to light pink, correspond to December 1995, December 1999, October 2008, November 2008, April 2009 and November 2009.