Dear Scientific Editor,

We would like to thank the three reviewers for their helpful comments on the paper. We are very grateful for their positive remarks on the capabilities of the ice-sheet model presented here. We have taken care to address their remarks to improve the quality of the manuscript. Especially as requested by referee #2, we have tried to put forward the new results of this paper, and as suggested by referee #3, it is now more apparent that our experiments are sensitivity experiments allowing to estimate bounds for the future ice-sheet mass loss. We hope that these changes will make the paper more attractive for a wider community.

The detail of how we have addressed the reviewer remarks is given in the supplement, where you will find a reply to all the comments and an highlighted version to show how the paper has been modified. You will see that some large parts have been struck out to be rewritten as suggested by the referees to improve the discussion/conclusions. It often remains minor corrections but it was not possible to keep a detailed highlighted track of the changes.

Sincerely yours,

Fabien Gillet-Chaulet

Reply to referee #1

I do not think the paper will attract a larger audience of readers interested in sea-level rise because the experiments, in spite of the model's high fidelity to material physics, are still too crude to be plausible. Specifically, I think that the perturbations to the basal traction are oversimplified, and have no confidence that the initial state plus 50 years of relaxation is a reasonable proxy for the modern GrIS. Nor do I see see significant advances over the SeaRISE experiments. This is a weakness of the paper, but not one that I think needs to be addressed before publication. I see this as more a paper for practitioners than planners.

The way the experiments are reported has been changed accordingly to comments from reviewer #3. These experiments are now reported as sensitivity experiments. Discussions of the experiments and conclusion have been improved. If we agree that there is no significant advances over the SEARISE experiments, spacial care has been taken here in the initialisation of the model, allowing a good comparison of the model results with the observed velocity structure and for the first time ice discharge at the scale of individual outlets, giving more confidence in the results. So we think that has such our sensitivity experiments will help to constrain possible bounds for the future ice sheet mass balance.

As such, my recommendations for the authors have to do with the things that other modelers would be interested in reading, and more revelations of what I think the shortcomings of model are. This would make it a better paper for modelers, and help other readers understand the limitations inherit in the prognostic runs that are performed. These authors, in particular, have an obligation to discuss model shortcomings, as theirs is among the very best models available right now. Their honesty in discussing model deficiencies has the potential to set the agenda for the entire modeling community's efforts. What follows are some of the deficiencies I noted. In each case I would ask the authors to investigate the degree to which they are true, with supporting graphics, and comment on why they might be occurring. The resulting paper would provide a much better reference, and help identify areas where more model development in needed.

We have taken care to reply to the referee comments to improve the discussions on the model shortcomings.

Rate of change of surface elevation: The authors are right to neglect these data (Prichard 2009) for assimilation purposes (page 2796, lines 14-17), because flux divergences are dominated by errors in bed topography (Seroussi et al. 2011). However, this data should not be neglected entirely. Especially after the relaxation process is done, the rates of change in surface elevation should be close to what the model is producing. I was surprised to see in figure 6 b that even after the relaxation, the rate of change is much different from Prichard 2009. Specifically the typical range near the outlets is 1-10, rather than 0-1 m/a, and the choice of absolute values prevents interpretation of the direction of surface elevation change. The direction is something that I'd be interested in knowing! If it's bad, let us know it is bad, and provide some commentary about what is happening, and why the modeled surface rate of change is so different from the observed. Also, the relaxed figure shows what looks like a classic fingerprint of numerical instability: a 'ringing' or high frequency oscillation of the ds/dt field in the upstream areas of Jakobshavns as well as several other outlet glaciers. This needs to be addressed. How is equation 5 being solved? Can you provide any evidence that it is being solved in a robust manner?

The discussion in the Relaxation section has been improved to explain the causes of the unphysical rates of change of the free surface and why relaxation is needed.
« One contribution to these initial surface changes comes from local ice flux divergence anomalies (Seroussi et al., 2011) due to uncertainties on the model initial conditions, including initial topography and model parameters. These anomalies disappear very quickly within a few years.

The other contribution is a response to the fact that many outlet glaciers are not able to evacuate the ice flowing through them and they need to thicken up. One explanation is that the ice thicknesses of many outlet glaciers is poorly known and they may be thicker in reality than the current geometry data allows for. The other explanation is that the lateral side of the mesh does not correspond exactly to the current position of the fronts of marine terminated glaciers for which we have no compilation for the whole GrIS. The ice then need to be transported towards the edge of the domain to be evacuated as discharge through the lateral boundary. This effect can be shown on the results for the ice discharge globally (Fig. 7) and locally for individual outlet glaciers (Table 2). Ice discharge is very low and underestimated for most of the smallest outlet glaciers at the beginning of the relaxation but increases with time as ice reaches the edges of the domain.»

For the direction of the surface elevation change we have added the sentence «*As shown by Fig.9a when running the same scenario for another 10 years, this remaining elevation change is mainly positive as these areas are still unable to evacuate all the ice flowing to them but the rate of change is within a physically acceptable range (Fig. 6).«*

Elmer/Ice is now a well established ice flow model using up-to-date methods for solving equations in a robust manner. Details on the numerics and validation of the model can be found in Gagliardini and Zwinger (2008). The following sentence has been added in section 2.1 Equations :

« All the equations presented above are solved using the ice flow model Elmer/Ice, based on the finite element code Elmer (http://www.csc.fi/elmer). Details on the numerical methods used to solve equations (4) and (6) and validation through established benchmark experiments can be found in Gagliardini and Zwinger (2008). »

Basal boundary condition and an apparent failure to conserve mass: I credit the authors for being very honest about a loss of 10% of the mass through the lower boundary (see lines 6-12 on page 2806), but I don't understand why it is happening. It seems like it has to do with equation 7, impenetrability, and bed roughness (lines 3-6 page 2795). I guess I thought that equation 7 entered as a Lagrange multiplier and was something that could be satisfied with accuracies similar to those of the state variables. I would really like to know more about what is happening here. Of course, I'm worried that others, myself included, might have similar problems, but at this point I simply do not have enough information to know exactly what is happening.

We have added a discussion on the method to enforce the non penetration condition in section 2.1 Equations

« We give a more detailed discussion in the implementation of the non-penetration condition, i.e the zero-normal velocity in Eq. (7), as the results on a rough topography will depend on the choice of the implementation strategy. With finite elements, normals are uniquely defined per elements from the nodes coordinates and elemental shape functions. At a given node, the discontinuity of the normal direction to the elements sharing the node can be large if the typical length scale of the basal roughness is larger than the mesh size.

-The non-penetration condition (Eq. 7) can be enforced weakly by elements under the form of a Robin condition as

***n (sigman)+Lambda un=0** (11)*

*where is a arbitrary large positive number. If the discontinuity of the normal direction, **n**, is large between adjacent elements, Eq. (11) will lead to **u=0** at the node shared by such elements. Enforcing Eq. (7) as a Dirichlet condition implies a linear combination of the three components of the velocity vector in the general case where **n** is not aligned with one of the coordinate axis. This can be done as in Leng et al. (2012) by using a local coordinate system with one direction aligned with the normal direction. A choice needs to be made on the definition of this normal direction as it is not uniquely defined, in general, at a given mesh node. The most immediate possibility is to take the average of the normal direction to the elements sharing the node. The mass flux through an element, computed as the integral of the scalar product of **u** with **n**, is then non null in general. This solution has been adopted in the following simulations. An alternative, newly implemented in the model, is to use mass consistent normals where the normal at a given node is a weighted average of the normal direction to the elements sharing* *the node. The weights are the nodal basis functions. This choice will insure that the zero-flux condition will be preserved globally, the non-null fluxes through the elements cancelling each other. However, it should be noted that this choice for the definition of the normal direction at the mesh nodes can be flawed with quadratic elements (Walkley et al., 2004). »*

Lateral boundary condition and the domain: In Figure 2, looking at the close ups, it does not appear that the extent of the domain matches the margin of the ice sheet.
I understand why this would happen; it isn't easy work to create a mesh like this. However, I'm curious about what is done in regions where there is not ice? Is a thin layer applied? What are the boundary conditions for terrestrially terminating glaciers? It looks like they should be stress free, is that correct? Ultimately, I think this becomes a big concern, as we see the flux emerging from the ice sheet sometimes increase, and other times decrease. What is happening during the relaxation period? Is the ice being transported outward, towards the edge of the domain? Is flux computed out the margin of the domain, or out the original ice margin? 50 years seems like an arbitrary value to make sure that the flux is transported from the edge of the ice to edge of the model domain, isn't it? Some more discussion of; 1) how the differences in the edge of the domain and the margin of the ice are reconciled, and 2) how the calculation of how the flux out is computed is needed.

An explanation on how the initial footprint is constructed has been added in sec. 2 .2 Mesh construction:
« There is no freely available product for current ice margins position and the initial 2D-footprint has been constructed from a 0-m thickness contour. »

A discussion on what is happening to the mesh has ben added in sec. 2.1 Equations
«During transient simulations the mesh is deformed elastically to follow the free-surface elevation with the constraint that the minimum thickness is equal to h_{min} =10m. The mesh nodes »

For the interpretation on the fluctuation of D see associated discussions in the model results sections (3.2 Relaxation and 4.2 results.
are not allowed to move horizontally. As a consequence, for outlet glaciers, the calving front position is fixed in time, however, land terminated glaciers can retreat inland but not advance beyond the initial footprint (Sec. 2.2). The ice-sheet volume is computed as the integral of the basis functions over the whole mesh. The ice discharge is computed as the ice flux through the lateral boundary as
***SEE Eq.** (9)*
As the glaciated area (i.e. the area where ice thickness is larger than h_{min}) can change with time, SMB is computed from Eq. (5) as
***SEE Eq.** (10) »*

Enhanced sliding: In the BF3 experiment, the authors continuously increase basal sliding by dropping the traction by a factor of 10. Maybe that is possible, none of us really knows the future, but this experiment is at odds with a large body of theory suggesting that basal sliding would tend to be reduced as the transmission of basal water becomes more efficient. They do acknowledge this, but carry on with the experiment anyway. I guess I'd rather see this particular experiment dropped. It's just a little too 'chicken little' for my tastes.

The way this experiment is now reported has been rethinked (cf comments from reviewer 3), and is now presented as a sensitivity experiment used to see what kind of forcing is required to sustain the actual rate of increase of mass loss from the ice sheet.

Page 2792 line 3 Stokes equations no need to capitalize 'equations'

This has been done

Page2794 line 16 horizontale should be horizontal

This has been corrected

Page 2795 line 18 speed (scalar), not velocity (vector), right?

This has been changed

Page 2796 line 15 specify that these are bed uncertainties. Also, point out that the rate of change of surface elevation gives a volume change, but that is not necessarily a mass change, due to firn densification.

We have changed the sentence as *« caused by the remaining uncertainties in the model initial conditions »* as the remaining uncertainties are not only the bed topography but also the ice viscosity for example. These uncertainties ad their effect are repeated in sevral instance in the text. For the firn densification this is true when looking at the observation but the model has no firn so that volume changes are mass changes, this could be misleading to discuss this issue here.

Page 2800 line 22-23 But this introduces large gradients at the boundary between balance velocity and INSAR velocities. Do you do anything about that?

No. We have added the justification :
« As these gaps are mainly located in areas of low speed in the central parts of the ice sheet, except in the south and south-east, no special care has been taken to insure continuity between the balance velocities and the observations. »

Page 2802 line 11 guaranty should be guarantee in this context.

This has been corrected

Page 2802 line 17 Unsufficient should be insufficient

This has been corrected

Page 2802 line 23 Control does not need caps.

This has been corrected

Page 2803 line 15 on should be changed to other

This has been corrected

Page 2803 line 26 I think that closeness to equilibrium should be shown in table 2 by including an additional column having the integral of the SMB over the catchment.

Integration of SMB on drainage basin with the anisotropic mesh has not been implemented yet, but will not add much to the discussion as it will only provide a global value. The closeness can be seen locally from the free surface rate of change (Figures 6 and 9, where we can see that most of the drainage basin are in equilibrium except in some places near the margins).

Page 2804 line 10 How can it evolve, isn't it somewhat fixed by the extent of the mesh?

The extent of the mesh is fixed but land terminated glacier can retreat (leaving a minimum thickness of 10m for the mesh), so that the « *glaciated* » area can decrease. This has been specified.

Page 2804 line 14 Gta^{-1} not Gta^{-2}

No it is Gta^{-2} , this is the derivative of D (Gta^{-1}) with respect to time, i.e. the slope of the curve in Fig7b.

Page 2804 line 23 “Future work might consider to use: : :” very awkward sentence, reword.

This sentence has been removed

Page 2803-2804 The word good is used 3 times. I don’t really know what good means in this context. Try and be more quantitative.
This part has been improved with quantification of the error on the ice discharge :
« A detailed comparison shows that the modelled ice discharge is generally underestimated. For the three largest outlet glaciers, where the topography is the best known, the agreement is within 1% for Helheim Glacier and 15% for Jakobshavn Isbrae, but D is largely underestimated by more than 50% at Kandgerdlugssuaq where the drainage basin still shows high rates of surface-elevation change (Fig. 6). Discharge at Petermann Glacier and the North East Greenland Ice Stream is also largely underestimated by up to 80% and these glacier are still thickening at high rates. This can be explain by the fact that their floating tongues are not treated explicitly in the model resulting in the neglect of the mass loss from melting below the shelves at the interface with the ocean. »

Page 2806 line 24 ‘retro-actions’ I’m not sure I know what you mean.

This has been changed to « *feedback* » and clarified.

Page 2809 line 11 ‘processes’ say which are most important.

This has been done

Page 2810 line 1 ‘self-similar’ what do you mean by this?

This is the term used by Price et al. ; check the reference for details.

Reply to referee #2

While the paper presents a very interesting piece of work, the intrinsic potential of the paper could be greatly improved by putting the major results in a broader framework. For instance, while it is not explicitly mentioned, the paper also underscores that to reproduce observed flow speed and mass loss of glaciers, higher-order physics is not sufficient, but resolving glaciers at high resolution (in which higher-order stresses play a decisive role) and initializing the model with robust inversions are essential building blocks for new-generation ice-sheet models and model predictions on decadal to century time-scales. A broader discussion could be given in the comparison of the numbers obtained from this work with numbers put forward by other authors. For instance, what is the main difference in approach of those studies? How do other GrIS model results compared to this study? What are their deficiencies? What is the further outlook in modelling the GrIS? What would be the potential effect of including calving? What can be done to improve the value of the projections (type of basal sliding perturbations)?
The conclusions should be made more firm, since the experiments are sufficiently robust. Major improvements should also be made to the English language to make the paper more readable, fluent and attractive. The native English co-author on the paper should definitely aid in an overhaul of the text. Many expressions are litteral translations from French (examples follow below).

The way the experiments are now reported has been change according to the remarks of referee #3. Conclusions are now more firm, with the clear identification of possible lower and upper bounds for the future GrIS mass loss. A discussion on the expected future developments of the model has been added at the beginning of the conclusion. Efforts have been made to improve the language.

I list below a series of more detailed remarks, some of them pertaining to the language. This is, however, not an exhaustive list of comments. The use of a spell checker could also help in eliminating a series of typos.
Especially the sections ’Results’ (and the discussion thereof) and ’Conclusions’ need some more work in bringing the important features upfront. If the authors take into account these elements, i find the paper acceptable for publication.

The sections have been changed and improved according also to take into accounts comments from reviewer 3. We have taken care of putting forward the new results and the perspectives to improve modelling of the GrIS.

Detailed comments
p2790 l7: ’most usual ice-sheet models’. Rephrase using ’current ice sheet models’, or present-day ice sheet models’

It has been changed to '*present-day ice sheet models*'

l18: rephrase: ’and on its own has a a stabilising effect’

It has been changed to *« and thus decreases the ice-sheet imbalance ».*

p2791 l13: Van der Veen

This has been done

p2792 l10-15: explain briefly why inverse methods are essential in modelling

This has been done :
« Inverse methods, by offering a robust framework to combine informations from the model and the data, are essential in modelling real systems but remain underused in glaciology. As a consequence, several parametrisations remain very poorly constrained limiting our ability to reproduce the current state of the ice sheet »

l16: ... a new generation continental scale …

This has been corrected

l18: Stokes equations

This has been corrected

p2794 l20: ’independent’ or ’fixed in time’

This has been corrected

l22: is a_s the prescribed accumulation? Shouldn’t this be the whole surface mass balanceÂ˘ aterm, including surface melt (as shown in Eq 5)

This has been changes to *« net accumulation «* as surface mass balance is used for the global surface mass balance, i.e. the integral of a_s, which could be a source of confusion

p2795 l16: equally distributed

This has been corrected

l18-19: Prior to the time-dependent simulation, the mesh size is optimised using the freely …

This part has been changed

l20: Mesh sizes decrease from 40km in the central part of the ice sheet to 1km in the outlet glaciers...

This has been corrected

p2796 l1-3: In general, ice sheets respond to changes in cliamte on multi-Century time scales, implying model spinup over long (glacial-interglacial) time scales.

This sentence has been changed to :
« As ice-sheet responses include long time scales (multi-century), forecasting change on decadalto-century time scales is essentially a short-term forecast. As such simulations are sensitive to the initial state, simulating the present conditions of the ice-sheet is crucial. »

l10: Moreover, inverse methods …

This has been corrected

l11: are currently restricted ..., hence limiting the ability to assimilate time series

This part has been improved

l12: ... we use two inverse …

This a been corrected

l16: ... in a diagnostic model is unphysical

this has been corrected

l17: Â˘aThe free surface is then allowed to relax compared to the observed surface for a period of 50 years

This has been corrected

l23: remove ’and are compared in the following’

This has been done

p2797 Remove first sentence

This has been done

l2: The method, detailed in A&G (2010), consists …

This has been corrected

l21: recently applied to

This has been corrected

p2798 l1: As in Morlighem …

This has been corrected

l3: which is valid only for Newtonian

This has been corrected

l6: remove ’here’

This has been done

l11: obtained by

This has been corrected

l14: change ’exact’ into ’valid’?

No, the derivative is « *exact* » in this case.

p2799 l2 and 7: obtained by or ’written as’

This has been corrected

l15: Surface ice flow velocities vary over several order of magnitudes between the interior of the ice sheet (slow flow) and the glacier outlets (fast flow). Therefore, Schaefer et al 2012 have shown that good convergence …

This has been corrected

p2800 l2: remove ’Here’

This has been done

p2801 l29: Similar to the velocity magnitude, …

This has been done

p2802 ll11: ... no guarantee that the actual …

This has been corrected

l17: Too coarse spatial resolution where the minimum …

This has been corrected

l19-20: rephrase

This has been corrected ; and item (iv) has been added for the point *« (iv) Too coarse spatial resolution of the data as,»*

p203 l4-5: remove 'not shown here' and add (not shown) at the end of the sentence.

This has been done

l6: insufficient

This has been corrected

l11: at the margins

This has been corrected

l11: unphysical very high: rephrase

« very high » has been removed

l23: what is meant by 'fronts that open' Are these flux gates?

This section has been improved, especially one can read :
« The ice then need to be transported towards the
edge of the domain to be evacuated as discharge through the lateral boundary »

p2804 l12: After this, ice discharge increase …

This has been corrected

l17: Petermann

This has been corrected

l18: imbalanced glaciers

This has been corrected

p2805 The section on Setup should be rephrased somehow. Maybe it would be more clear to use a few baulated equations to show how the perturbation is done. It is not clear whether the whole spatially non-uniform friction field is subjected to the same perturbation, or whether this is scaled. Either write this better in the text or use a couple of equations to make your point.

We have added equations to show how perturbations are done.

p2806: l24: change 'retro-actions' into 'feedbacks'

This has been changed

l26: Without dynamic perturbation …

This has been corrected

l28: ... around zero and lacks a particular …

This has been corrected

p2807: l1: as a consequence, the total ice volume …

This has been corrected

l17: Due to this acceleration Bottom section of this papge: this is very novel and should be emphasized, by for instance bringing this into a separate paragraph

This part has been improved

p2808: l6: discriminate between the …

This has been corrected

Bottom section: what happens when the ice sheet margins retreat further? Is there a remeshing? If yes, write why it is important; if no, explain in more detail the consequences for this.

It is now explained in Sec. 2.1 how the mesh is deformed. Moving margin and remeshing is the next required major development of the model and this is work in progress.

p2809: l8: allows

This has been corrected

l18: reference to Nick et al (2012) as well; modelling such processes on Petermann Glacier (J. Glac.)

This has been added

Bottom section: rework the section on sea level rise, bring upfront the major conclusions of the work with respect to the innovations and importance of initialization through inversion and the way forward.

The way the experiments are reported has been improved, especially the discussion on our estimations of possible bounds for the ice-sheet mass loss.

Reply to referee #3

General comments

General Note: I reviewed a previous version of this paper and I think The Cryosphere is a more appropriate venue for the publication of this paper. However, because that version of the paper was very similar to the present version, many of my comments and concerns about the paper remain the same.

The authors present prognostic simulations of the Greenland ice sheet over the next century conducted using the new (next?)-generation ice sheet model Elmer-ice. An introductory review of current generation models and their limitations serves to motivate the new developments of Elmer-ice reported on here. Note that this discussion should really be explicitly aimed at circa AR4 models, since there are currently a half dozen or so models that could also be called "next generation". The authors also discuss Elmer's capabilities, their initialization procedure, and the model output from a number of prognostic experiments accounting for perturbations to both surface mass balance and ice dynamics. The output from these perturbation experiments are presented as changes in ice sheet mass loss over time (and the resultant change in global sea-level) relative to a control run for which climate (surface mass balance) and dynamic forcing (basal sliding coefficients) are held steady at their initial values.

It is not explicitly mentioned that new generation is used in contrast with the model generation that has participate to AR4.
« The lack of skill of the ice-sheet models used for the fourth assessment of the Intergovernmental Panel on Climate Change, was one of the reasons behind the statement that a poor understanding of the importance of dynamic changes limited our ability to put an upper bound on the contribution of ice sheets to sea level by 2100 (Solomon et al., 2007). Since then, the inherent fundamental limitations of this models generation, that prevent proper modelling of the ice discharge, have been identified: »
But it is true that potentially a half dozen of current models could be included in our definition of 'new generation' models. Nevertheless, to our knowledge, a smaller number has been effectively used in the context of a continental transient applications

The capabilities of the model showcased in this paper are impressive and overall the model represents a significant technical achievement. The combination of meshing capability, solver capability, and data assimilation capability are all first rate and are currently matched by only one other modeling group in the world today. While the experiments reported on in the paper are certainly useful for probing the parameter space of future ice sheet behavior, it seems like that fact could be stated a bit more explicitly in the paper. Instead, it seems like the authors are trying to "sell" the results from the experiments as representative of particular "realistic" future scenarios for Greenland's evolution. For example, the primary thing we learn from the paper is that discharge from the ice sheet increases depending on how slippery the ice sheet bed might become in the future. This in itself is not surprising. But rather than trying to sell the results from these rather arbitrarily forced experiments as realistic possible futures for Greenland – which is very difficult, because the perturbations are largely arbitrary (e.g. how does a doubling of sliding everywhere correspond to some kind of realistic physical forcing, like a change in meltwater lubrication at the ice sheet bed?) – why don't the authors take the more honest and reasonable approach of treating the experiments as a sensitivity study designed for probing the possible bounds of future ice sheet mass loss and sea-level rise from Greenland?

We are now presenting the experiments as sensitivity experiments as suggested by the reviewer.

Another example is the conclusion made in the paper (and re-stated in the abstract) about the "increasing dynamic perturbation" experiment, whereby the model is forced by a decrease in the value of the basal traction parameter, everywhere, by 10x over one century. Since, as the authors admit, such a scenario is highly unlikely, why not argue that these results demonstrate exactly how drastic (and probably unrealistic) a dynamic perturbation is necessary in order to maintain long-term increasing rates of mass flux from the ice sheet? To me, the results of this experiment can be used to argue that such a dynamic change is highly unlikely, and thus can be used to place an upper bound on future dynamic mass loss and sea-level rise from the ice sheet. Further, it strongly argues against doing things like extrapolating the increasing rates of mass loss from the last decade into the future, because it demonstrates just what kind of dynamic forcing is required to justify doing so (that is, very unrealistic forcing). This conclusion seems equally or more important to me, and much more believable, than trying to convince the reader that the results represent a reasonable potential future for Greenland.

The way the experiments are presented has been changed and we have adopted the view of the referee to present the experiments as sensitivity experiments used to assess the possible bounds for the future contribution of GrIS to Sea level rise.

A related complaint is the statement from the abstract (and similar statements sprinkled throughout the main text) that "By conducting perturbation experiments, we investigate how current ice loss will endure over the next century". The implication is that the model initial condition is such that it captures current rates of observed ice sheet mass loss (thus, the model initial condition captures current-day transients) and that the applied perturbations are physically reasonable and/or constrained such that they are representative of what we expect the ice sheet to be subjected to over the next century. Neither of these is true, however, in terms of what is presented and discussed in the paper. Again, there seems to be plenty to learn here if the results are presented more honestly as coming from perturbation experiments aimed at bounding the likely future sea level rise contribution from Greenland (i.e., it seems unlikely that we can expect dynamic mass loss of >114 mm by 2100, because now we have some idea about what kind of dynamic forcing is required in order for that to happen).

This has been clarified now. The end of the abstract has been changed to
« From this initial state, we investigate possible bounds for the next century ice-sheet mass loss. We run sensitivity experiments of the GrIS dynamical response to perturbations in climate and basal lubrication. We find that increasing ablation tends to reduce outflow and thus decreases the ice-sheet imbalance. In our experiments, the GrIS initial mass (im)balance is preserved throughout the whole century in the absence of reinforced forcing, allowing to estimate a lower bound of 75 mm for the GrIS contribution to SLR by 2100. In one experiment, we show that the current increase in the rate of ice loss can be reproduced and maintained throughout the whole century. However, this requires a very unlikely perturbation of basal lubrication. This result is used to estimate an upper bound of 140 mm from dynamics only for the GrIS contribution to SLR by 2100.».
And the main text has been changed accordingly.

The issue about basal boundary conditions resulting in mass loss of 10% of the total discharge through the basal boundary is a bit disturbing. This is not a small number of mass that is not being conserved. I am not an expert on finite elements but I do know that there are methods for enforcing conservation both locally and globally. If the mesh is at fault for some reason, shouldn't a more high fidelity mesh be used? In general, this issue seems to be swept under the rug a bit too much.

A paragraph has been added to the way to enforce the non penetration condition in our finite element model. See reply to reviewer 1.

Specific comments
p.2789
line 7: ": : : only represent rapid ice flow in an approximate fashion." Be specific about what you mean here. What does "approximate" mean? What is the specific problem? Resolution? Wrong gov. equations? Simplification of boundary conditions? All of these?

There is no room in the abstract to detail explicitly the problems. This is done in the introduction:
"(i) most of the observed changes are located on narrow outlet glaciers and the dominant source of increased discharge is the combined contribution of many small glaciers (Howat et al., 2007; Moon et al., 2012) that cannot be captured individually by models typically running with grid resolutions from 5 km to 15km;
(ii) Low order approximations of the Stokes equations do not hold for these outlet glaciers where the scale of horizontal variations in basal topography and friction are of the same order as the ice thickness (Pattyn et al., 2008; Morlighem et al., 2010);
(iii) Inverse methods, by offering a robust framework to combine informations from the model and the data, are essential in modelling real systems but remain underused in glaciology. As a consequence, several parametrisations remain very poorly constrained limiting our ability to reproduce the current state of the ice sheet. The most uncertain parametrisation is the drag exerted on the ice by the underlying bed, this can vary by several orders of magnitude depending on the bedrock roughness and water pressure (Jay-Allemand et al., 2011). »

Line 11-12: The statement that one needs to be solving Stokes is not necessarily true. Depends on lots of things, like resolution, bed roughness, etc. In many places lower order approximations are appropriate (e.g. even SIA is ok in the interior).

The sentence says « *to reproduce the pattern of rapid ice flow* », rapid ice flow in concentrated in narrow outlet glaciers that require high resolution usually show high roughness of basal topography and roughness. Hypothesis of the lower order approximations are clearly violated in these conditions.
It is now clearly mentioned in the main text that these lower approximations do not hold in these areas.
« (ii) Low order approximations of the Stokes equations do not hold for these outlet glaciers where the scale of horizontal variations in basal topography and friction are of the same order as the ice thickness (Pattyn et al., 2008; Morlighem et al., 2010); »

Line 18-19: ": : :if destabilizing processes continue : : :". This is misleading relative to what is presented in the paper. Yes, this was the result of one of the experiments but by your own admission that experiment is unrealistically extreme. This makes it sounds like increasing rates of mass loss into the future are not unlikely but I think the more reasonable interpretation of the experiment results is that the are unlikely.

The end of the abstract has been changed accordingly.

p.2790
line 13: water filling crevasses does not soften them, the latent heat from water does.

It has been modified as :
« increased surface runoff filling the crevasses can result in a softening of the outlet glaciers lateral margins through cryo-hydrologic warming and hydraulic weakening of ice »

Line 24- . "The lack of skill of current generation : : :" This discussion is a bit dated and disingenuous, since there are a number of new models now that have shown good skill in reproducing observations of rapid change (and these models have been published on as well).

It is now explicitely mentioned that 'new generation' is used in opposition with the generation that has participated to AR4. :

« The lack of skill of the ice-sheet models generation used for the fourth assessment of the Intergovernmental Panel on Climate Change, was one of the reasons behind the statement »

p. 2791 (i) – (iii): It sounds like you are condemning all approximations to Stokes (e.g. even 1storder, LIL2, etc.) as inappropriate. This is not true, as they are still valid approximations over large areas of the ice sheets. I agree that Stokes is required in some areas, but not everywhere all of the time.

The order of the items has been changed (see above) to expressed that low order approximations do not hold in the narrow outlets where we observe rapid ice flow.

Line 25: "We present the first model : : :" Is this statement entirely true? ISSM has many of these same capabilities.

Yes ISSM share the same capabilities, but there is no publication yet with a transient full-Stokes continental scale application. A reference to ISSM has been added in the reference list.

Line 26-27: The reference to Seroussi et al. is out of place or incorrect here. The anomalous surface mass balance implied from the initialization procedure used here is only partly due to errors in ice flux data (as discussed in Seroussi et al.). The bulk of it is due to the fact that your initialization procedure is tuning a single model field, the basal sliding parameter, when in fact there are many other inconsistencies between your initial model state and the velocities you are trying to "fit" (e.g. incorrect internal temperatures and ice softness).

Reference to Seroussi et al. is correct here.
Seroussi et al. discuss that the ice flux anomalies are primary due to errors in the bedrock topography (and not in ice flux data), which is one of the model initial condition. We agree that uncertainties in the bed topography are not the only remaining uncertainties in the model initial conditions and it is now explicitly mentioned *« remaining uncertainties in the model initial conditions »* and this has been specified where needed in the main text.

p.2792:
Section 2.1: Logically, it seems like the governing Stokes equations should be presented first, since they are generic, followed by the constitutive law, which is not (currently, this order is reversed).

The order has been conserved.

Section 2.1: Somewhere in here, mention where the non-linearity comes from explicitly. That is, from the strain-rate dependence of the viscosity.

It is explicitly mentioned that the effective viscosity is a function of the strain-rate thus non linear, and it is a well known result in glaciology ; This has not been changed.

Line 20 - : Save the information on how you get the temperature field for the discussion of initialization? It seems awkwardly force in here. It would suffice to say that you use a constant temperature field for A(T) and that you discuss the details further below. Also, it would be good to state why it is justified to hold the temperature field fixed over time. Presumably because it will change little on a 100 yr timescale (which I agree with, but it should be stated).

This has been modified and moved latter in the section :
"Values of parameters prescribed in this study are presented in Table 1. A proper initialisation of the temperature field would require a spin-up over at least a full glacial cycle, which remains too computing time consuming with our model. In our application, the initial field is bi-linearly interpolated on the finite element mesh from the temperature field computed with the shallow

ice model SICOPOLIS (Greve, 1997) after a paleo-climatic spin-up as in Seddik et al. (2012). In order to avoid an initialisation shock, as these two models do not have the same physic, the ice temperature field is then kept constant. When considering the low conductivity of ice, this assumption remains valid for the short time scales considered here. Because the ice-sheet basal temperature conditions remain uncertain, basal sliding is not restricted to areas where the ice temperature has reached the pressure melting point and an optimal friction coefficient in (Eq. 7) is inferred by inverse methods at each node to reproduce the observed surface velocity field (Sect. 3.1).»

p.2793: line 23-24: The equation for the boundary condition should be written out instead of just given in writing.

It is written (Eq. 7 and 8).

p. 2794:
line 3-6: Overall, I don't think enough information is given as to why the flux is not conserved at the base (e.g. why the no-penetration bc cannot be enforced). There are methods to do so, why aren't they implemented here? If the amount of mass loss was trivial, it might not matter, but 10% of the discharge flux is a pretty large number.

A discussion on the implementation strategy of the non penetration condition has been added (cf reply to reviewer 1)

Line 6-9: clarify that you are applying "hydrostatic pressure" as a b.c. at the ice-ocean lateral boundary.

This has been clarified.

Line 17: The "metric tensor" is mentioned in terms of constructing the mesh but you haven't really explained what it is or why / how it is used. My impression is that the Hessian of the velocities is used as a weighting factor for deciding where to focus resolution, and maybe the description would be more informative if it was simplified to a statement of that nature.
In terms of the meshing, it is not clear here if YAMS does the mesh optimization or if that is something the authors came up with. A few more references on the meshing might be adequate to cover questions like this.

This section has been improved :
« Anisotropic mesh adaptation is now widely used in numerical simulations especially with finite elements, as it allows to refine the mesh where needed to capture the flow features within a certain accuracy without increasing the computational cost excessively. The method is generally based on an estimation of the interpolation error used to adjust the mesh size so that the discretisation error is equally distributed over the whole domain. It can be shown that an estimate of the interpolation error induced by the meshing is obtained from the Hessian matrix of the modelled field, allowing to define an anisotropic metric tensor at each node (Frey and Alauzet, 2005). As the target surface velocity field is known, our metric tensor is constructed from the Hessian matrix of observed surface speed (Joughin et al., 2010) shown in Fig. 1a. Starting from a first regular mesh of the 2-D-footprint of the GrIS based on Delaunay triangulation, we use the freely available anisotropic mesh adaptation software YAMS (Frey and Alauzet, 2005) to optimise the mesh sizes according to the given metric map.»

p.2795: line 16-17: Again, the ice flux divergence anomalies are not entirely the result of the issues discussed in Seroussi et al. They are largely the result of the fact that your initial state, even though optimized to match observed velocities, are not in equilibrium with the current ice sheet geometry because of, e.g. incorrect or missing model transients, incorrect/unknown rheological properties, etc. By referring to Seroussi et al., it sounds like you are arguing that the mismatch between the flux divergence and the surface mass balance – resulting in very noisy and unphysical vertical velocities at the surface - is entirely due to uncertainties in the geometry data for the ice sheet. This is not true.

The pattern of ice flux divergence anomalies is similar to those obtained by Seroussi et al., if they argue that this is mainly due to error in the bed topography, we agree that this is also due to the *« remaining uncertainties in the model initial conditions »*

p.2797:
line 5: Not clear to me why you are discussing the ice sheet surface topography here.

This has been clarified :
« As the direction of the ice velocity is mainly governed by the ice-sheet topography, we disregard the error on the velocity direction and the cost function is expressed as the difference between the norm of the modelled and observed horizontal velocities as »

p.2798:
line 4-5: When discussing the regularization, note why it is necessary; to enforce a “smoothness” constraint.

This has been changed to :
« To avoid over fitting of the data and improve the conditioning of the problem a smoothness constraint is added to the cost function under the form of a Tikhonov regularisation term penalising the spatial first derivatives of as: »

p.2799-2800:
Section 3.1.4 - 3.1.5: Note somewhere in this section whether or not the optimized values of B^2 are consistent with the basal thermal conditions (i.e. does sliding take place if and only if T = T_pmp?).

This is now done in Sec. 2.1
“Because the ice-sheet basal temperature conditions remain uncertain, basal sliding is not restricted to areas where the ice temperature has reached the pressure melting point and an optimal friction coefficient in (Eq. 7) is inferred by inverse methods at each node to reproduce the observed surface velocity field (Sect. 3.1).”

p.2801:
(ii) Note that optimization on multiple parameters simultaneously (e.g. a 3d enhancement factor) could improve the fit between model and observations.

This is now discussed in conclusion.
“The main remaining uncertainties in the model are the ice viscosity field, the bedrock elevation and the position of the ice margins. Future work will benefit from new observations and from the developments of inverse methods to constrain these uncertainties (Arthern and Gudmundsson, 2010; Pralong and Gudmundsson, 2011; Morlighem et al., 2010; Petra et al., 2012). »

p.2802:
line 14-16: Again, I think you are assigning to much blame for the unrealistic surface vertical velocities (that is, the result of the flux divergence) to the uncertainties in ice thickness, when most of them are due to the uncertainty in other parameter values that result in, e.g. and incorrect vertical velocity structure.

We clearly mention that these anomalies are due to the model initial conditions.
« One contribution to these initial surface changes comes from local ice flux divergence anomalies (Seroussi et al., 2011) due to uncertainties on the model initial conditions, including initial

topography and model parameters ».
A result from Seroussi et al. is that if the initial topography is smoothed these anomalies disappear, and latter Morlighem et al. show that you can reduce these anomalies if you invert for the basal topography. So clearly the bed topography in the model partly explain these unrealistic vertical velocities. But we agree that the problem is more complex and that there is other uncertianties in the model initial conditions. A remedy would be to ad these ice flux divergence anomalies in the cost function and to invert for multiple parameters to make them compatible. This is discussed in the conclusion.

Line 19-23: The problem discussed here is probably also due to the fact that the thicknesses for many outlets are poorly known and underestimated (i.e., they need to thicken up in order to evacuate the ice flowing to them because, in reality, they actually are thicker (deeper) than the current geometry data allow for).

This has been modified as :
« The other contribution is due to the fact that many outlet glaciers are not able to evacuate the ice flowing through them and they need to thicken up. One explanation is that the ice thicknesses of many outlet glaciers is poorly known and they may be thicker in reality than the current geometry data allows for. The other explanation is that the lateral side of the mesh does not correspond exactly to the current position of the fronts of marine terminated glaciers for which we have no compilation for the whole GrIS. The ice then need to be transported towards the edge of the domain to be evacuated as discharge through the lateral boundary »

p.2803:
line 9-10: somewhere here, tell us where your surface mass balance comes from (Ettema et al?).

There is the reference to Ettema et al.

line 10: The ice extent (lateral I assume?) does not evolve much : : : but can it? Isn't the mesh extent fixed ahead of time? Can the margin advance and retreat freely? Only retreat (e.g. formerly ice filled cells become ice free)?

We now refer to glaciated area as the mesh extent is fixed but land terminated glacier can retreat leaving an unglaciated area where the minimum mesh thickness is 10m.

Line 20-23: Again, most of the uncertainties that require the surface relaxation (or alternatively, would require a "synthetic" surface mass balance, given by the flux divergence field) here are NOT due to uncertainties in the bedrock elevations, but due to uncertainties in the model initial conditions.

Bedrock elevation is one of the model initial condition uncertainty.

p.2804:
Section 4.1: Somewhere you need to give appropriate credit to the SeaRISE effort, who designed and organized the data for at least a few of the experiments reported on here. Either reference the website (e.g. for the data), or the publications that have been submitted, or both.

These has been done :
« These forcings are taken from the freely available SeaRISE data compilations (http://tinyurl.com/srise-umt) »

Section 4.1: Note that the in the first beta forcing scenario (halving its value), the change is implemented as a step function.

This has been done

Line 18-20: But this last scenario IS unrealistic. Decreasing basal friction temporarily and in a few locations (e.g., I doubt that anyone things that all of Greenland is going to surge all at the same time) is a lot different than decreasing it by an order of magnitude everywhere. The more honest way to report this would be as a sensitivity analysis, as discussed above.

We now report this experiment as :
«As has been shown for a surging glacier (Jay-Allemand et al., 2011) , can vary by several orders of magnitude with only small changes in the water pressure so that the magnitude of the imposed changed would not be unrealistic if applied in few locations and for short time periods. However, it seems highly unlikely that the GrIS will surge all at the same time, so that this last sensitivity experiment is used as an high end scenario of the possible future for the GrIS mass loss.»

p.2805:
line 10-12: As discussed above, the 10% of discharge mass loss through the basal boundary seems substantial and problematic. This is not a small number, and one fundamental requirement for an ice sheet model would seem to be conservation (at least to a much smaller tolerance than this).

This has been discussed.

p.2806: line 7-10: I think that what is written here could be interpreted incorrectly. That is, one might read this as implying that one needs to reduce the friction parameter by 1/2 everywhere from your initial condition in order to bring your model in line with current observations. Is that what you mean?

This part has been changed to :
« When looking at the total discharge only, this perturbation allows to compensate the model initial uncertainties as this value is more in agreement with current estimates based on observations (Rignot et al., 2011). However, this is done at the expense of the velocity pattern as we see higher velocities on the interior of each drainage basin (Fig. 1e) compared to the current observations (Fig. 1a). A progression of high velocities areas upstream of each drainage basin may be expected as ablation will reach higher elevations in the future, increasing water pressure and basal lubrications in these areas. However, the expected effect in low elevation areas is not clear as the increase of water availability should contribute in the formation of efficient drainage systems resulting in a decrease of the water pressure and basal lubrication (Schoof, 2010). With this experiment, we investigate how an initial ice-sheet imbalance of -200Gta-1, which is close to the current observations, can evolved within a century.»

Line 15-17: “This pattern is expected : : :”. This is not an entirely honest statement, as there is at least as much evidence arguing that excess runoff will have very little effect on ice sheet mass flux (as efficient drainage systems will buffer the impact of increased melt on basal sliding).

See reply to previous comment .

Line 20-: See discussion above about how this experiment is interpreted and reported on, which I think could be made substantially better (and more honest).

This experiment is now interpreted as follow :
“With an increasing dynamic perturbation (C2 BF3), the discharge increases continuously from 300Gta-1 to 1400Gta-1 after 100 yr (Fig. 8c), and the ice sheet is loosing mass throughout the 100-yr simulation (Fig. 8a, b). The acceleration of the ice discharge produced with this

scenario is comparable with observations from the last decade (Rignot et al., 2011). The velocity pattern at the end of the century (Fig. 1f), shows large areas of high velocities (>100ma-1) far inland of each glacier. Such a scenario seems unlikely as it would required a sustained surge of the whole ice-sheet in the same time, which is excluded if efficient drainage systems develop as a result of increased water supply (Schoof, 2010). Nevertheless, This experiment shows that, if it is mechanically possible to sustain over a whole century the currently observed GrIS rate of mass loss, it requires nearly unrealistic maintained dynamical perturbation allowing to estimate an upper bound for the ice-sheet contribution to SLR by 2100. »

p.2807-2808:
The section describing the comparison of observed vs. modeled rates of surface elevation change doesn't add very much and seems like a distraction. I would suggest removing it from the paper.

It has been left.

p.2808:
line 5-9: You are really only partially satisfying one present-day initial condition, that of approx. matching the observed velocities. I'm not saying this is trivial, but there are certainly other conditions you are not matching (e.g. geometry, observed rates of change, internal temperatures, etc.). You might be more specific about what you mean here by "present-day conditions".

This has been specified:
« to satisfy the essential pre-requisite of simulating the observed velocity structure of the ice sheet. »

Line 14-16: We already know that surface mass balance AND discharge will govern the ice sheet's future evolution (this was a conclusion from AR4 afterall), so this is a somewhat trivial conclusion to be making here.

The value of ice Discharge is usually not reported by the model running simulations of the GrIS so that it is useful to recall this trivial conclusion here.

p.2808-2809, line 25-line 2: It's not clear to me how the "extrapolation" was done here and how it is being compared to the value from Price et al. I think the latter value they are referring to (90 mm of SLR by 2100) contains approximately equal parts SMB and dynamic discharge. Again, unclear if/how these two values should be compared, or how the extrapolated value reported on here is obtained.

This has been improved.
"In our experiments with constant basal conditions, the rate of decrease of D and SMB are similar, resulting in a nearly constant annual mass balance. In these conditions, a probable lower bound for the ice-sheet contribution to SLR is given by assuming that the currently observed ice-sheet imbalance will be preserved over the whole century. Taking an averaged value of -300Gta-1 for the ice-sheet mas balance in 2010 (Rignot et al., 2011) leads to a total contribution to SLR of 75mm by 2100. This value can be compared to the 46mm (40mm from SMB and 6mm from dynamics) given by Price et al. (2011) as an estimate of the committed SLR due to the observed last decade ice-sheet dynamics. Our value is closer to their estimated upper bound (85mm including 45mm from dynamics) which is estimated by assuming a selfsimilar response of the ice sheet to a 10 yr-recurring forcing in the future decades (Price et al., 2011) . »

p.2809, Line 2-8: The "continually increased" sliding perturbation scenario is discussed here as if it is realistic. Again, it seems like the more honest way to report these results is to note that, in order to come close to the Pfeffer et al. upper bounds (that are quoted here), you have to do something fairly crazy and unrealistic to the model (decrease friction everywhere by an order of mag. over a century). Thus, both the Pfeffer kinematic arguments and the modeling conducted here are in approx. agreement on what might be an upper bound for dynamic mass loss from Greenland over the next century. This seems like a more honest way to discuss these results (as opposed to hinting that this kind of behavior might actually be expected).

This has been improved.
"We have shown that, conversely, if the forcing is continuously increased, there is sufficient ice available to sustain the current rate of increase in discharge over an entire century. However, in our model, this is achieved with an unrealistic perturbation of the basal lubrication field. In these conditions, a probable upper bound for the ice-sheet contribution to SLR is given by assuming that this rate of increase will be preserved in the future. Taking a discharge anomaly of 100Gta-1 increasing at rate of 9Gta-2 for year 2010 (Rignot et al., 2011), leads to a contribution to SLR of 140mm by 2100 from dynamics only. This value is on the lowest half of the values obtained from kinematic considerations assuming low (93mm) and high (467mm) scenarios (Pfeffer et al., 2008)."

Technical corrections
The word "enhancing" is used often throughout the paper. It would be better to use a specific word, e.g. "enhancing : : : sea-level rise" —> "increasing : : : sea-level rise".

p.2789
Line 12: "unstructured mesh" —> "variable resolution unstructured mesh" ?

This has been done

p.2790
line 8: "The flow of an ice sheet : : :". I think this should be a new paragraph, but at the same time, this statement doesn't seem relevant here. Shouldn't it be moved to where the gov. equations / model are discussed?

This is now in a new paragraph but it has not been moved.

p.2791
(iii) "The most uncertain parameterization : : :" —> "The most uncertain process"

The parameter needs to be constrained from inverse methods.

line 18: provide a general ref. for "Elmer/Ice code".

This has been done

p.2792:
Suggest a new paragraph at the end of this section (before section 2 starts). For example, "We first discuss the governing equations, the model, and our meshing strategy, followed by a discussion of our initialization procedure. We then discuss the model perturbation experiments and the result of those experiments" (or something like this).
This plan of the paper is anounced in the previous paragraph.

p.2793:
line 22: "accumulation: —> "accumulation rate"

This has been changed to :
« net accumulation rate »

p.2794:

Line 6-9: clarify that you are applying "hydrostatic pressure" as a b.c. at the ice-ocean lateral boundary.

This has been done.

Line 19: "YAMS" = : : : ?

This part has been clarified.

Line 24-25: provide a link or ref. for the searise dataset used here?

This has been done

p.2795:
line 10: Omit "full Stokes" (the inverse methods in the discussion don't need to be limited to only Stokes models).

The sentence has been changed and the discussion is limited to full-stokes
« Moreover, if the model developped by Heimbach and Bugnion, 2009 offer the ability to do transient data assimilation, inverse methods applied to full-Stokes ice-sheet modelling are currently restricted to diagnostic simulations »

Line 12: "basal friction field" —> "basal friction parameter" ? You aren't constraining the friction field with your initialization procedure, you are constraining the parameter relation basal velocity to basal traction/friction.

It has been changed to *« basal friction coefficient »*

Line 20: "main outlets" —> "main outlet glaciers"

This has been changed

Line 23: Here you refer to the "basal friction coefficient", which is how it should be referred to elsewhere in the paper.

Yes this has been changed

p.2796:
line 15: the notation "d_B J_0" is not clearly described.

This has been improved.

Line 16: the symbol "|.|" is not used with equation 10 but with equation 9 (and should be discussed after equation 9, not 10)

it is used in (10) and discussed at the right place .

p.2797:
Line 18: "To avoid unphysical negative : : :" —> "To avoid unphysical (e.g. Negative) : : :"

The change of variable used here is really used to avoid "negative" values.

p.2799:
line 1: suggest rewriting sentence as, " : : : using a spatially varying step size rather than the fixed-step size used in the original gradient descent algorithm of A&G (2010)."

This has been changed.

Line 10: "reverse communication MODE."

This has been changed

p.2800:
line 2-3: Provide a reference for the "L-curve" method?

This has been done

p.2801:
line 8: "twin experiments" is awkward. What does this mean?

« twin experiment » is the right locution when synthetic data are generated by the model and used to validate the inverse model

(iii) "Unsufficient" —> "Insufficient"

This has been changed to *"Too coarse resolution"*

(iii) " : : : and of the data as, for example, the ice thickness : : :". This should be a new topic (item iv?) as it doesn't have anything to do the other topic in item (iii).

An item (iv) has been added.

p.2802:
line 11: "essentially" —> "especially" ?

This has been changed

line 13: clarify that by "climate" here you mean "surface mass balance"

This has been changed

p.2804:
line 3-4: Even though one of the two methods can't be conclusively argued for, note which one you use for the rest of the simulations reported on here.

This this mentioned explicitly just after

Line 9: clarify that "climate forcing" here is only surface mass balance, since you are holding temperatures fixed.

This has been done

Line 18-19: Instead of "enhanced", be more specific and just say that beta is reduced linearly by one order of magnitude over 100 yrs.

We have added the equation to present the perturbation.

Line 22: "thereafter" —> "hereafter"

This has been done

p.2805:
line 24: "other retro-actions" is awkward. What does this mean, other feedbacks?

This has been changed to *"feedbacks"*

Tables / Figures
Table 3: Suggest adding a column of currently observed ice flux for these same outlets?

This column is already in table 2

Figure 1: This figure should be made bigger. It is extremely difficult to discern any of the details of the flow structure (e.g., in order to compare them). Also, the colorbar labels are essentially unreadable.

High resolution figures will be given (with improved color bars) at publication time. But we want to keep the figures altogether so that we can not increase the size.
We do not give a discussion on the details of the velocity sturcture but only on the main patterns of the velocity field, which remains largely visible in these figures.

Figure 2 (caption): ": : : with the Robin inverse method. Colored boxes show close-up views for various outlet glaciers of interest."

This has been done

Figs. 4, 5: Can't read the colorbars here. Use white letters and larger font size?
Figure 9: Again, can't read the colorbar here (too small / blurry).

Color bars in Fig.4 and 5 have been improved with white letters and larger font size.
Figures have been given with a low resolution for the review ; however, high resolution figures will be given at publication time.

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Greenland Ice Sheet contribution to sea-level rise from a new-generation ice-sheet model

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Abstract

Reply to referee 1;Reply to referee 2;Reply to referee 3;

Over the last two decades, the Greenland Ice Sheet (GrIS) has been losing mass at an increasing rate, enhancing its contribution to sea-level rise (SLR). The recent increases in ice loss appear to be due to changes in both the surface mass balance of the ice sheet and ice discharge (ice flux to the ocean). Rapid ice flow directly affects the discharge, but also alters ice-sheet geometry and so affects climate and surface mass balance. ~~The most usual~~ Present-day ice-sheet models only represent rapid ice flow in an approximate fashion and, as a consequence, have never explicitly addressed the role of ice discharge on the total GrIS mass balance, especially at the scale of individual outlet glaciers. Here, we present a new-generation prognostic ice-sheet model which reproduces the current patterns of rapid ice flow. This requires three essential developments: the complete solution of the full system of equations governing ice deformation; a variable resolution unstructured mesh to usefully resolve outlet glaciers and the use of inverse methods to better constrain poorly known parameters using observations. The modelled ice discharge is in good agreement with observations on the continental scale and for individual outlets. ~~By conducting perturbation experiments, we investigate how current ice loss will endure over the next century. Although we find that increasing ablation tends to reduce outflow and on its own has a stabilising effect, if destabilisation processes maintain themselves over time, current increases in the rate of ice loss are likely to continue.~~ From this initial state, we investigate possible bounds for the next century ice-sheet mass loss. We run sensitivity experiments of the GrIS dynamical response to perturbations in climate and basal lubrication. We find that increasing ablation tends to reduce outflow and thus decreases the ice-sheet imbalance. In our experiments, the GrIS initial mass (im)balance is preserved throughout the whole century in the absence of reinforced forcing, allowing to estimate a lower bound of 75 mm for the GrIS contribution to SLR by 2100. In one experiment, we show that the current increase in the rate of ice loss can be reproduced and maintained throughout the whole century. However, this requires a very unlikely perturbation of basal lubrication. This result is used to estimate an upper bound of 140 mm from dynamics only for the GrIS contribution to SLR by 2100.

1 Introduction

The currently observed acceleration of mass loss from the Greenland Ice Sheet (GrIS, Rignot et al., 2011; Schrama and Wouters, 2011; van den Broeke et al., 2009; Wouters et al., 2008) is a concern when considering its possible contribution to future sea-level rise (SLR). Approximately 60 % of the acceleration rate in mass loss from the GrIS has been attributed to a change in the surface mass balance (SMB, Rignot et al., 2011; van den Broeke et al., 2009). However, several studies have revealed a dynamic response of the ice sheet, in which acceleration and thinning of most outlet glaciers are shown to be responsible for a substantial increase in ice discharge (Howat et al., 2007; Pritchard et al., 2009; Joughin et al., 2010; Moon et al., 2012). These studies show a high spatial and temporal variability in glacier acceleration, suggesting that simple extrapolation of the recent observed trends cannot be justified, and realistic projections of the contribution of GrIS to SLR on decadal to century time scales must be derived from the forecasts of verified ice-flow models driven by the most reliable projections of climatic (atmosphere and ocean) forcing.

The flow of ice in an ice sheet is characterised by a very low Reynolds number and is governed by the Stokes equations (e.g. Greve and Blatter, 2009). The outlet-glaciers dynamics are strongly controlled by basal and seaward boundary conditions. These boundary conditions have recently been altered by ongoing climate change: ~~increased surface runoff can result in a softening of the lateral margins of the outlet glaciers by filling the crevasses~~ increased surface runoff filling the crevasses can result in a softening of the outlet glaciers lateral margins through cryo-hydrologic warming and hydraulic weakening of ice (Van der Veen et al., 2011) and can enhance basal lubrication by reaching the bed through moulins (Zwally et al., 2002); ocean warming and processes happening at the front have likely triggered the recent acceleration of numerous outlet glaciers by reducing the back-stress at the front as their floating tongues thin and/or retreat (Howat et al., 2007).

Despite the recent efforts to model these processes (Nick et al., 2009; Schoof, 2010), incorporating them and validating the results produced has not been the focus of most modellers running continental scale models (Vaughan and Arthern, 2007): most of those ice-sheet models were primarily designed to run over glacial cycles and so did not need to reproduce the decadal to annual sensitivity that recent observations have highlighted and which are likely to be significant on decade-to-century projections (Truffer and Fahnestock, 2007).

~~The lack of skill of the current generation of ice-sheet models in reproducing observations was one of the reasons behind statements made by authors of the last assessment of the Intergovernmental Panel on Climate Change, who noted that a poor understanding of the importance of dynamic changes limited our ability to put an upper bound on the contribution of ice sheets to sea level by 2100.~~

~~Fundamental limitations, inherent to the current generation of ice-sheet models, prevent the proper modelling of the ice discharge:~~

(i) ~~these models are based on approximations of the Stokes equations that do not hold in areas where the scale of horizontal variations in basal topography and friction are of the same order as the ice thickness~~ (Pattyn et al., 2008; Morlighem et al., 2010);

(ii) ~~most of these changes are located on narrow outlet glaciers and the dominant source of increased discharge is the combined contribution of many small glaciers~~ (Howat et al., 2007; Moon et al., 2012) ~~that cannot be captured individually by models typically running with grid resolutions from 5 km to 15 km~~;

(iii) ~~several parametrisations remain very poorly constrained due to the under use of robust inverse methods. The most uncertain parametrisation is the drag exerted on the ice by the underlying bed, this can vary by several orders of magnitude depending on the bedrock roughness and water pressure~~ (Jay-Allemand et al., 2011).

The lack of skill of the ice-sheet models generation used for the fourth assessment of the Intergovernmental Panel on Climate Change, was one of the reasons behind the statement that a poor understanding of the importance of dynamic changes limited our ability to put an upper bound on the contribution of ice sheets to sea level by 2100 (Solomon et al., 2007).

Since then, the inherent fundamental limitations of this models generation, that prevent proper modelling of the ice discharge, have been identified:

(i) most of the observed changes are located on narrow outlet glaciers and the dominant source of increased discharge is the combined contribution of many small glaciers (Howat et al., 2007; Moon et al., 2012) that cannot be captured individually by models typically running with grid resolutions from 5 km to 15 km;

(ii) Low order approximations of the Stokes equations do not hold for these outlet glaciers where the scale of horizontal variations in basal topography and friction are of the same order as the ice thickness (Pattyn et al., 2008; Morlighem et al., 2010);

(iii) Inverse methods, by offering a robust framework to combine informations from the model and the data, are essential in modelling real systems but remain underused in glaciology. As a consequence, several parametrisations remain very poorly constrained limiting our ability to reproduce the current state of the ice sheet. The most uncertain parametrisation is the drag exerted on the ice by the underlying bed, this can vary by several orders of magnitude depending on the bedrock roughness and water pressure (Jay-Allemand et al., 2011).

We have developed a new generation ~~of~~ continental scale ice-sheet model (Little et al., 2007; Alley and Joughin, 2012) that overcomes these difficulties: by employing parallel computing and the Elmer/Ice code (http://elmerice.elmerfem.org/), we solve the full system of Stokes equations over the entire GrIS (Sect. 2.1). We use an anisotropic mesh-adaptation technique (Sect. 2.2) to distribute the discretisation error equally through the entire domain (Frey and Alauzet, 2005; Morlighem et al., 2010). For the construction of the initial state, we use two inverse methods (Arthern and Gudmundsson, 2010; Morlighem et al., 2010) to constrain the basal friction coefficient field from observed present-day geometry and surface velocities (Sect. 3.1). While other recent models have included some similar features (Price et al., 2011; Seddik et al., 2012; Larour et al., 2012), we present the first model to use all three developments simultaneously to produce prognostic simulations. Due to ice flux divergence anomalies (Seroussi et al., 2011) caused by the remaining uncertainties in the model initial conditions, the free surface is then relaxed for 50 yr (Sect. 3.2). ~~From this initial state we conduct sensitivity experiments to investigate how current ice loss will endure over the next century (Sect. 4).~~ From this initial state, we run sensitivity experiments of the GrIS dynamical response over one century to perturbations in climate and basal lubrication (Sect. 4). In conclusion, we discuss how this results can be interpreted in terms of possible bounds for the future ice-sheet mass loss and contribution to SLR by 2100.

2 Model description

2.1 Equations

We consider a gravity-driven flow of incompressible and non-linearly viscous ice flowing over a rigid bedrock.

The constitutive relation for ice is assumed to be a viscous isotropic power law, called Glen's flow law in glaciology (Glen, 1955):

$$\tau_{ij} = 2\eta\dot{\epsilon}_{ij} \ , \tag{1}$$

where $\boldsymbol{\tau}$ is the deviatoric stress tensor, $\dot{\varepsilon}_{ij} = (u_{i,j} + u_{j,i})/2$ are the components of the strain-rate tensor, and $\boldsymbol{u}$ is the velocity vector. The effective viscosity η is expressed as

$$\eta = \frac{1}{2}(EA)^{-1/n}\dot{\epsilon}_e^{(1-n)/n}, \tag{2}$$

where $\dot{\varepsilon}_e = \sqrt{\dot{\varepsilon}_{ij}\dot{\varepsilon}_{ij}/2}$ is the strain-rate second invariant, E is an enhancement factor, $A(T)$ is the rate factor function of the temperature T relative to the temperature melting point following an Arrhenius law:

$$A = A_o e^{(-Q/[R(273.15+T)])}. \tag{3}$$

In Eq. (3), A_o is the pre-exponential factor, Q is an activation energy, and R is the gas constant.

The ice flow is computed by solving the Stokes problem with non-linear rheology, coupled with the evolution of the upper free-surface, summarised by the following field equations and boundary conditions:

$$\begin{cases} \mathrm{div}\boldsymbol{u} = 0 \\ \mathrm{div}\boldsymbol{\sigma} + \rho_\mathrm{i}\boldsymbol{g} = 0 \end{cases} \quad \text{on} \quad \Omega, \tag{4}$$

$$\partial_t z_\mathrm{s} + u\partial_x z_\mathrm{s} + v\partial_y z_\mathrm{s} = w + a_\mathrm{s}, \quad \text{on} \quad \Gamma_\mathrm{s}, \tag{5}$$

$$\boldsymbol{\sigma}\cdot\boldsymbol{n} = \boldsymbol{0}, \quad \text{on} \quad \Gamma_\mathrm{s}, \tag{6}$$

$$\begin{cases} \boldsymbol{t}\cdot(\boldsymbol{\sigma}\cdot\boldsymbol{n})|_\mathrm{b} + \beta\boldsymbol{u}\cdot\boldsymbol{t} = 0 \\ \boldsymbol{u}\cdot\boldsymbol{n} = 0 \end{cases}, \quad \text{on} \quad \Gamma_\mathrm{b}, \tag{7}$$

$$\begin{cases} \boldsymbol{n}^T\cdot\boldsymbol{\sigma}\cdot\boldsymbol{n} = -\max(\rho_\mathrm{w}g(l_\mathrm{w}-z),0) \\ \boldsymbol{t}^T\cdot\boldsymbol{\sigma}\cdot\boldsymbol{n} = 0 \end{cases}, \quad \text{on} \quad \Gamma_\mathrm{l} \tag{8}$$

On the domain Ω, Eq. (4) expresses the conservation of mass and the conservation of momentum. The Cauchy stress tensor $\boldsymbol{\sigma}$ is defined as $\boldsymbol{\sigma} = \boldsymbol{\tau} - p\boldsymbol{I}$ with p the isotropic pressure. The gravity vector is given by $\boldsymbol{g} = (0,0,-g)$ and ρ_i is the density of ice. Equation (5) expresses the evolution of the upper free surface Γ_s with a prescribed net accumulation rate $a_\mathrm{s}(x,y,t)$. We note $\partial_i z$ the partial derivative of upper surface $z_\mathrm{s}(x,y,t)$ with respect to the horizontal~~e~~ dimension $i = (x,y)$. A proper treatment of grounding line dynamics has been developed for three-dimensional full-Stokes simulations (Favier et al., 2011) but remains computationally challenging on a whole-ice-sheet application. Here, ice shelves and grounded ice are not treated differently and the lower surface elevation z_b is ~~time independant~~ fixed in time.

On the boundaries, $\boldsymbol{n}$ and $\boldsymbol{t}$ are the normal and tangential unit vectors. The upper surface Γ_s is a stress-free surface (Eq. 6). A linear friction law is applied on the lower surface Γ_b (Eq. 7). On the lateral boundary Γ_l, the normal component of the stress vector is equal to the hydrostatic water pressure exerted by the ocean, with ρ_w the sea water density, where ice is below sea level l_w, and is equal to zero elsewhere (Eq. 8). The remaining components of the stress vector are null (Eq. 8).

During transient simulations the mesh is deformed elastically to follow the free-surface elevation with the constraint that the minimum thickness is equal to $h_\mathrm{min} = 10$m. The mesh nodes are not allowed to move horizontally. As a consequence, for outlet glaciers, the calving front position is fixed in time, however, land terminated glaciers can retreat inland but not advance beyond the initial footprint (Sec. 2.2). The ice-sheet volume is computed as the integral of the basis functions over the whole mesh. The ice discharge is computed as the ice flux through the lateral boundary Γ_l as

$$\mathrm{D} = \int_{\Gamma_l} \boldsymbol{u}.\boldsymbol{n} \mathrm{d}\Gamma \quad . \tag{9}$$

As the glaciated area (i.e. the area where ice thickness is larger than $h_{\min}$) can change with time, SMB is computed from Eq. (5) as

$$\mathrm{SMB} = \int_{\Gamma_s} (\partial_t z_s + u\partial_x z_s + v\partial_y z_s - w)\mathrm{d}\Gamma \quad . \tag{10}$$

Values of parameters prescribed in this study are presented in Table 1. A proper initialisation of the temperature field would require a spin-up over at least a full glacial cycle, which remains too computing time consuming with our model. In our application, the initial field is bi-linearly interpolated on the finite element mesh from the temperature field computed with the shallow ice model SICOPOLIS (Greve, 1997) after a paleo-climatic spin-up as in Seddik et al. (2012). In order to avoid an initialisation shock, as these two models do not have the same physic, the ice temperature field is then kept constant. When considering the low conductivity of ice, this assumption remains valid for the short time scales considered here. Because the ice-sheet basal temperature conditions remain uncertain, basal sliding is not restricted to areas where the ice temperature has reached the pressure melting point and an optimal friction coefficient β in (Eq. 7) is inferred by inverse methods at each node to reproduce the observed surface velocity field (Sect. 3.1).

All the equations presented above are solved using the ice flow model Elmer/Ice, based on the finite element code Elmer (http://www.csc.fi/elmer). Details on the numerical methods used to solve equations (4) and (6) and validation through established benchmark experiments can be found in Gagliardini and Zwinger (2008). We give a more detailed discussion in the implementation of the non-penetration condition, i.e the zero-normal velocity in Eq. (7), as the results on a rough topography will depend on the choice of the implementation strategy. With finite elements, normals are uniquely defined per elements from the nodes coordinates and elemental shape functions. At a given node, the discontinuity of the normal direction to the elements sharing the node can be large if the typical length scale of the basal roughness is larger than the mesh size.

-The non-penetration condition (Eq. 7) can be enforced weakly by elements under the form of a Robin condition as

$$\boldsymbol{n}\cdot(\boldsymbol{\sigma}\cdot\boldsymbol{n})|_{\mathrm{b}}+\Lambda\boldsymbol{u}\cdot\boldsymbol{n}=0 \tag{11}$$

where Λ is a arbitrary large positive number. If the discontinuity of the normal direction, $\boldsymbol{n}$, is large between adjacent elements, Eq. (11) will lead to $\boldsymbol{u}=0$ at the node shared by such elements. Enforcing Eq. (7) as a Dirichlet condition implies a linear combination of the three components of the velocity vector in the general case where $\boldsymbol{n}$ is not aligned with one of the coordinate axis. This can be done as in Leng et al. (2012) by using a local coordinate system with one direction aligned with the normal direction. A choice needs to be made on the definition of this normal direction as it is not uniquely defined, in general, at a given mesh node. The most immediate possibility is to take the average of the normal direction to the elements sharing the node. The mass flux through an element, computed as the integral of the scalar product of $\boldsymbol{u}$ with $\boldsymbol{n}$, is then non null in general. This solution has been adopted in the following simulations. An alternative, newly implemented in the model, is to use mass consistent normals where the normal at a given node is a weighted average of the normal direction to the elements sharing the node. The weights are the nodal basis functions. This choice will insure that the zero-flux condition will be preserved globally, the non-null fluxes through the elements cancelling each other. However, it should be noted that this choice for the definition of the normal direction at the mesh nodes can be flawed with quadratic elements (Walkley et al., 2004).

2.2 Mesh construction

~~Anisotropic mesh adaptation is now widely used in numerical simulations especially with finite elements. The method is based on an estimation of the interpolation error used to adjust the~~

~~mesh size so that the discretisation error is equi-distributed. First, we mesh the 2-D-footprint of the GrIS. The metric tensor is based on the Hessian matrix of observed surface velocities shown in Fig. . The mesh size is optimised before the simulation using the freely available software YAMS . The mesh size decreases from 40km in the central part of the ice sheet to a minimum resolution of 1km in the outlet glaciers as shown in Fig. . The mesh is then vertically extruded using 16 layers. The resulting 3-D mesh is composed of 417 248 nodes and 748 575 wedge elements.~~

~~The bedrock and surface topography are taken from the freely available SeaRise 1km present-day data set and are based on the digital-elevation models where new data have been added on three of the main outlets (Jakobshavn Isbrae, Helheim and Kangerdlugssuaq).~~

This part as been improved in response to the reviewers comments

Anisotropic mesh adaptation is now widely used in numerical simulations especially with finite elements, as it allows to refine the mesh where needed to capture the flow features within a certain accuracy without increasing the computational cost excessively. The method is generally based on an estimation of the interpolation error used to adjust the mesh size so that the discretisation error is equally distributed over the whole domain. It can be shown that an estimate of the interpolation error induced by the meshing is obtained from the Hessian matrix of the modelled field, allowing to define an anisotropic metric tensor at each node (Frey and Alauzet, 2005). As the target surface velocity field is known, our metric tensor is constructed from the Hessian matrix of observed surface speed (Joughin et al., 2010) shown in Fig. 1a. Starting from a first regular mesh of the 2-D-footprint of the GrIS based on Delaunay triangulation, we use the freely available anisotropic mesh adaptation software YAMS (Frey and Alauzet, 2005) to optimise the mesh sizes according to the given metric map.

The resulting mesh is depicted in Fig. 2. ~~The~~ Mesh sizes decrease from 40 km in the central part of the ice sheet to ~~a minimum resolution of~~ 1 km in the outlet glaciers. The 2-D mesh is then vertically extruded using 16 layers. The resulting 3-D mesh is composed of 417 248 nodes and 748 575 wedge elements.

The bedrock and surface topography are taken from the freely available SeaRise 1 km present-day data set (http://tinyurl.com/srise-umt) and are based on the Bamber et al. (2001)

digital-elevation models where new data have been added on three of the main outlets (Jakobshavn Isbrae, Helheim and Kangerdlugssuaq). There is no freely available product for current ice margins position and the initial 2D-footprint has been constructed from a 0-m thickness contour.

3 Initial state

As ice-sheet responses include long time scales (multi-century), forecasting change on decadal-to-century time scales is essentially a short-term forecast. As such simulations are sensitive to the initial state, simulating the present conditions of the ice-sheet is crucial Vaughan and Arthern (2007). Available observations of the current state of the ice sheet include the ice-sheet geometry (bedrock and free-surface elevations, e.g. Bamber et al., 2001), surface velocities (e.g. Joughin et al., 2010) and rate of change of the surface elevation (e.g. Pritchard et al., 2009). If timeseries of these observations are available for the last decade, observed changes in velocity and surface elevation are certainly the results of transient boundary forcing that are still not fully understood and modelled. Moreover, if the model developed by Heimbach and Bugnion (2009) offers the ability to do transient data assimilation, inverse methods applied to full-Stokes ice-sheet modelling are currently restricted to diagnostic simulations ~~and are not able~~ , hence limiting the ability to assimilate timeseries. In this application, we compare ~~here~~ two recently developed inverse methods to constrain the basal friction coefficient field (β in Eq. 7) from a given geometry and surface velocity field, considered as representative of present day conditions (Sect. 3.1). Due to ice flux divergence anomalies caused by the remaining uncertainties in the model initial conditions, the free-surface-elevation rate-of-change computed in a diagnostic model is non-physical (Seroussi et al., 2011), and the available observations are not used here to constrain the model. The free surface ~~elevation~~ is then allowed to ~~diverge from the observations with a relaxation period of~~ relax compare to the observed surface for a period of 50 yr (Sect. 3.2). The performance of the model to simulate present day conditions is then assessed by comparison of the estimated ice discharge for the main outlet glaciers (Rignot and Kanagaratnam, 2006).

3.1 Inverse methods

Two variational inverse methods (Arthern and Gudmundsson, 2010; Morlighem et al., 2010) are used to infer the basal friction coefficient field $\beta(x,y)$ ~~and are compared in the following~~. Both methods are based on the minimisation of a cost function that measures the mismatch between modelled and observed velocities. The two methods are briefly outlined below.

3.1.1 Robin inverse method

~~We briefly review the method detailed in~~ The method, detailed in Arthern and Gudmundsson (2010), consists in solving alternatively the *Neumann*-type problem, defined by Eq. (4) and the boundary conditions Eqs. (6) and (7), and the associated *Dirichlet*-type problem, defined by the same equations except that the Neumann upper-surface condition (Eq. 6) is replaced by a Dirichlet condition where observed surface horizontal velocities are imposed.

The cost function that expresses the mismatch between the solutions of the two models is given by

$$J_o = \int_{\Gamma_\mathrm{s}} (\boldsymbol{u}^\mathrm{N} - \boldsymbol{u}^\mathrm{D}) \cdot (\boldsymbol{\sigma}^\mathrm{N} - \boldsymbol{\sigma}^\mathrm{D}) \cdot \boldsymbol{n} \mathrm{d}\Gamma, \tag{12}$$

where superscripts N and D refer to the Neumann and Dirichlet problem solutions, respectively.

The Gâteaux derivative $d_\beta J_o$ of the cost function J_o with respect to the basal friction coefficient β for a perturbation β' is given by:

$$d_\beta J_o = \int_{\Gamma_\mathrm{b}} \beta' (|\boldsymbol{u}^\mathrm{D}|^2 - |\boldsymbol{u}^\mathrm{N}|^2) \mathrm{d}\Gamma, \tag{13}$$

where the symbol $|.|$ defines the norm of the velocity vector and Γ_b is the lower surface.

Note that this derivative is exact only for a linear rheology and thus Eq. (13) is only an approximation of the true derivative of the cost function when using Glen's flow law (Eq. 1) with $n > 1$ in Eq. (2).

3.1.2 Control inverse method

The control method has been introduced by MacAyeal (1993) and recently applied ~~with~~ to full-Stokes ice flow modelling by Morlighem et al. (2010). The method relies on the computation of the adjoint of the Stokes system. As in Morlighem et al. (2010), we assume that the stiffness matrix of the Stokes system is independent of the velocity and thus self-adjoint, which is ~~exact only for a~~ valid only for Newtonian rheology, i.e. when $n=1$ in Eq. (2).

As the direction of the ice velocity is mainly governed by the ice-sheet topography, we disregard the error on the velocity direction and the cost function is expressed as the difference between the norm of the modelled and observed horizontal velocities as

$$J_o = \int_{\Gamma_s} \frac{1}{2}\left(|\boldsymbol{u}_H| - |\boldsymbol{u}_H^{\mathrm{obs}}|\right)^2 \mathrm{d}\Gamma, \tag{14}$$

where $\boldsymbol{u}^{\mathrm{obs}}$ are the observed velocities and subscript H refers to the horizontal component of the velocity vector.

The Gâteaux derivative is obtained by

$$\mathrm{d}_\beta J_o = \int_{\Gamma_b} -\beta' \boldsymbol{u}\cdot\boldsymbol{\lambda}\,\mathrm{d}\Gamma, \tag{15}$$

with $\boldsymbol{\lambda}$ the solution of the adjoint system of the Stokes equations.

Again, this derivative is exact only for a linear rheology and thus is only an approximation of the true derivative of the cost function when using Glen's flow law (Eq. 1) with $n>1$ in Eq. (2).

3.1.3 Regularisation

To avoid non-physical negative values of the basal friction coefficient, β is expressed as

$$\beta = 10^\alpha. \tag{16}$$

The optimisation is now done with respect to α and the Gâteaux derivative of J_o with respect to α is written as:

$$\mathrm{d}_\alpha J_o = \mathrm{d}_\beta J_o \frac{\mathrm{d}\beta}{\mathrm{d}\alpha}. \tag{17}$$

To avoid over fitting of the data and improve the conditioning of the problem a smoothness constraint is added to the cost function under the form of a Tikhonov regularisation term penalising the spatial first derivatives of α as:

$$J_{\mathrm{reg}} = \frac{1}{2}\int_{\Gamma_\mathrm{b}} \left(\frac{\partial \alpha}{\partial x}\right)^2 + \left(\frac{\partial \alpha}{\partial y}\right)^2 \mathrm{d}\Gamma. \tag{18}$$

The Gâteaux derivative of J_{reg} with respect to α for a perturbation α' is obtained by

$$\mathrm{d}_\alpha J_{\mathrm{reg}} = \int_{\Gamma_\mathrm{b}} \left(\frac{\partial \alpha}{\partial x}\right)\left(\frac{\partial \alpha'}{\partial x}\right) + \left(\frac{\partial \alpha}{\partial y}\right)\left(\frac{\partial \alpha'}{\partial y}\right) \mathrm{d}\Gamma. \tag{19}$$

The total cost function now writes

$$J_{\mathrm{tot}} = J_o + \lambda J_{\mathrm{reg}}, \tag{20}$$

where λ is a positive ad-hoc parameter. The minimum of this cost function is no longer the best fit to observations, but a compromise (through the tuning of λ) between fit to observations and smoothness in α.

3.1.4 Minimisation

~~At the surface of an ice sheet, the magnitude of the velocities differs by several order of magnitude between the interior and the outlets. For this reason, in another application of the model, it has been shown~~ Surface ice flow speed vary over several order of magnitudes between the interior of the ice sheet (slow flow) and the glacier outlets (fast flow). Therefore Schäfer et al. (2012) have shown that good convergence properties are obtained with the Robin inverse method by using a spatially varying step size rather than the fixed-step used in the original gradient descent algorithm of Arthern and Gudmundsson (2010). We apply here another strategy where the Gâteaux derivatives given by a continuous scalar product represented by the integral on $\Gamma_{\rm b}$, Eqs. (17) and (19), are transformed in a discrete euclidean product when discretized on the finite element mesh. The area surrounding each finite element node on $\Gamma_{\rm b}$ is then included in the gradients used for the minimisation. This leads to good convergence properties with our unstructured mesh as large elements correspond to low velocity areas, and vice versa.

The minimisation of the cost function $J_{\rm tot}$ with respect to α is done using the limited memory quasi-Newton routine M1QN3 (Gilbert and Lemaréchal, 1989) implemented in Elmer in reverse communication mode. This method uses an approximation of the second derivatives of the cost function and is then more efficient than a fixed-step gradient descent.

3.1.5 Results

The observed velocities, shown in Fig. 1a, are a compilation of data sets obtained from RADARSAT data at different dates during the first decade of the 2000's (Joughin et al., 2010). We choose this compilation as it gives the best coverage. For glaciers that have been accelerating, it therefore provides a kind of average value for this period. But for these glaciers, the surface topography has also diverged from the surface topography used here. A proper comparison of the model results with observations of the 2000's would therefore require coherent data sets for both the topography and the velocities, which are currently not available. For the inversion, gaps in data have been filled by the balance velocities available in the SeaRise dataset. As these gaps are mainly located in areas of low speed in the central parts of the ice sheet, except in the south and south-east, no special care has been taken to insure continuity between balance and observed velocities.

To compare the performance of the two inverse methods and asses the dependence of the results to the initialisation of the basal friction coefficient β, the initial ~~friction~~ field is given by

$$\beta(x,y) = \max(10^{-4}, \min(1.0/U_{\rm bal}, 10^{-1}))\,\mathrm{MPa\,m^{-1}\,a} \tag{21}$$

for the Robin inverse method where U_{bal} is the balance velocity, and by

$$\beta(x,y) = 10^{-4}\,\mathrm{MPa\,m^{-1}\,a} \tag{22}$$

for the control inverse method.

The optimal regularisation parameter λ in Eq. (20) is chosen using the L-curve method (Hansen, 2001). The L-curve is a plot of the optimised variable smoothness, i.e. the term J_{reg} in Eq. (20), as a function of the mismatch between the model and the observations, i.e. the term J_o in Eq. (20). The L-curve obtained with both methods is given in Fig. 3. The term J_{reg} is identical for the two methods, whereas J_o is not (Eqs. 12 and 14). This leads to different values of the regularisation parameter λ in Eq. (20), which allows to tune the weight of the regularisation with respect to J_o.

For the Robin inverse method, when increasing λ from 0 to 10^9, the roughness of the basal friction coefficient field, represented by J_{reg}, decreases by several orders of magnitude with a small decrease of the mismatch between the model and the observation, represented by J_o. This decrease of J_o may be due to the fact that the gradient used in the model, Eq. (13), is not the true gradient of J_o due to the non linearity of the ice rheology. The regularisation, for which the gradient is exact, therefore improves the convergence properties of the model. For higher values of λ, J_{reg} still decreases but with a concomitant increase of J_o as the basal friction coefficient field becomes too smooth.

The L-curves obtained with the two methods are very similar. The minimum J_o is obtained for the same order of magnitude of J_{reg}. The optimal value for λ is chosen as the minimum value of J_o, i.e. $\lambda_{\mathrm{Robin}} = 10^8$ for the Robin inverse method, and $\lambda_{\mathrm{CI}} = 10^{11}$ for the control inverse method.

The surface velocity field obtained after optimisation of the basal friction coefficient field with the Robin inverse method is shown in Fig. 2. Our implementation reproduces very well the observed large-scale flow features (Fig. 1a) with low velocities in the interior and areas of rapid ice flow, restricted to the observed outlet glaciers, near the margins. The largest outlet glaciers (Jakobshavn Isbrae, Kangerlugssuaq, Helheim, ...) and their catchments are well captured by the anisotropic mesh and the modelled velocity pattern is in good agreement with the observations. Smaller outlet glaciers down to few kilometres in width are also individually distinguishable.

The absolute and relative errors on the surface velocities at the end of the optimisation are shown in Figs. 4 and 5, respectively. Similar to the velocity magnitude, the absolute error varies by several order of magnitude between the interior and the margins. The relative error is only few percents in most of the interior where ice is flowing faster than few meters per year. This error is usually higher very locally near the margins. The highest relative errors are located in the North in Petermann Glacier and in the North-East Greenland ice stream where long floating tongues are present but not explicitly taken into account in this application of the model. The remaining differences between modelled and observed velocities can come from four main reasons:

(i) non convergence of the minimisation: it has been shown on twin experiments where the minimum is known (Arthern and Gudmundsson, 2010), that the gradients of the cost function derived analytically for a linear rheology, Eqs. (13) and (15), work well in practice with a non-linear rheology. But there is no guarantee ~~in general~~ that the actual minimum will be found (Goldberg and Sergienko, 2011), especially in real applications where the curvature of the cost function is very low.

(ii) Remaining uncertainties: adjusting the sliding coefficient can compensate only partly for errors associated with the uncertainties on the other model initial conditions.

(iii) ~~Insufficient resolution of the model~~ Too coarse spatial resolution of the model where the minimum mesh resolution is lower than those of the velocity data, so that the model will not be able to capture all details, especially for the smallest outlets;

(iv) Too coarse spatial resolution of the data as, for example, the ice thickness is not sufficiently known in most outlet glaciers.

It is difficult to test the first hypothesis as the minimum is unknown but both the cost function and the norm of the gradient decrease during the minimisation and both inverse methods leads to very similar results (not shown for the control inverse methods), so that we are confident to be close to the actual minimum.

Errors shown in Figs. 4 and 5 account both for the error on the direction and on the magnitude of the modelled velocities compared to the observations. The direction of the flow is mainly governed by the ice-sheet topography and adjusting the sliding coefficient has little effects on the flow direction. The cost function used with the control inverse method only accounts for the difference between the velocity norms and not for the direction. Both inverse methods lead to similar errors so that we assume that most of the absolute error is representative of the error on the velocity norm. For the outlets, a common feature ~~, not shown here,~~ is that model velocities are lower than the observations along the central flow lines but higher along the shear margins (not shown here). This can be explained by insufficient resolution of the model and/or of the data, or remaining uncertainties on the ice viscosity for example.

3.2 Relaxation

~~When running prognostic simulations from this initialisation, the free-surface elevation shows unphysical very high rates of change essentially on the margins (Fig). The free surface of the ice sheet is therefore allowed to relax during a 50-yr time-dependent run, forced by a constant present-day climate .~~

~~One contribution to these initial surface changes comes from ice flux divergence anomalies due to uncertainties on the bed topography and on the model parameters. These anomalies disappear very quickly within a few years. Another contribution results from the fact that the lateral side of the mesh does not correspond exactly to the current position of the fronts of the marine terminated glaciers for which we have no compilation for the whole GrIS. Due to these geometry problems at the front, the ice discharge at the beginning of the relaxation is strongly underestimated for most of the smallest outlets (Table). The mass balance being largely positive due to the SMB term, this results in a thickening of the outlets, until the time when the ice fronts open to evacuate the mass excess.~~

~~At the end of the relaxation, the surface velocities, given in Fig. , still show good agreement with the observations, with ice discharge still concentrated in known outlet glaciers. Each~~

~~drainage basin is then relatively close to equilibrium, and in consequence computed discharge values from the main outlet glaciers are compared with observations before the recent glacier accelerations (Table , observations given by for 1996, or 2000 when not available). A detailed comparison shows generally good agreement in areas where the bed topography is well known (e.g., Jakobshavn Isbrae, Helheim Glacier). Agreement is less good in the North where the floating tongues in the model still show a large imbalance of the free surface. The total computed discharge for the relaxed solution is around 300 Gt a^{-1}. Up to now, the magnitude of the computed ice discharge in a continental ice-sheet model has only been addressed by by tuning the boundary condition at the ice front to reproduce the observations only on three major outlets.~~

~~The various terms of the mass balance equation during the relaxation are given in Fig. . The ice extent does not evolve much during this relaxation and, as a consequence, the surface mass balance is constant and equal to 400 Gt a^{-1}. The ice discharge increases very quickly during the first twenty years to reach 200 Gt a^{-1}. After, the ice discharge increase slows down. The relaxation is stopped after 50 yr. At this time, the rate of increase of the ice discharge is around 1 Gt a^{-2}. The free surface is nearly at equilibrium except in a few areas near the margins but the rate of change is of the same order of magnitude as the accumulation/ablation (Fig.). For the reasons cited previously, Peterman Glacier and the North East Greenland Ice Stream are the most imbalanced at the end of the relaxation. The change in surface elevation between the beginning and the end of the relaxation exceeds several hundred meters in some places. This difference is large but is of the same order of magnitude as the uncertainty on the ice thickness in some areas of the GrIS margins. This remains the main limitation of the model where only the upper surface is adjusted whereas most of the uncertainties come from the bed elevation. Future work might consider to use the bed elevation as a control variable as in , and will benefit from new observations. During this relaxation the ice volume increases by less than 0.5 %. Running the model longer should lead to a steady state where the ice discharge should offset the surface mass balance as shown in Fig. when running the same constant climatic scenario for another century.~~

This part as been improved in response to the reviewers comments

When running prognostic simulations from this initialisation, the free-surface elevation shows non-physical ~~very high~~ rates of change especially at the margins (Fig. 6). The free surface of the ice sheet is therefore allowed to relax during a 50-yr time-dependent run, forced by a constant present-day surface mass balance field (Ettema et al., 2009).

One contribution to these initial surface changes comes from local ice flux divergence anomalies (Seroussi et al., 2011) due to uncertainties on the model initial conditions, including initial topography and model parameters. These anomalies disappear very quickly within a few years. The other contribution is a response to the fact that many outlet glaciers are not able to evacuate the ice flowing through them and they need to thicken up. One explanation is that the ice thicknesses of many outlet glaciers is poorly known and they may be thicker in reality than the current geometry data allows for. The other explanation is that the lateral side of the mesh does not correspond exactly to the current position of the fronts of marine terminated glaciers for which we have no compilation for the whole GrIS. The ice then need to be transported towards the edge of the domain to be evacuated as discharge through the lateral boundary. This effect can be shown on the results for the ice discharge globally (Fig. 7) and locally for individual outlet glaciers (Table 2). Ice discharge is very low and underestimated for most of the smallest outlet glaciers at the beginning of the relaxation but increases with time as ice reaches the edges of the domain.

The time needed to offset these uncertainties and reach an equilibrium is difficult to estimate in advance and will be different for each drainage basin. The relaxation duration is arbitrarily fixed to 50 years. At the end of the relaxation, the surface velocity structure, given in Fig. 1b, remains similar to the observations, with rapid ice flow areas still concentrated in known outlet glaciers. The various terms of the mass balance equation during the relaxation are given in Fig. 7. SMB is nearly constant and equal to 430 $\mathrm{Gt\,a^{-1}}$ during the relaxation showing that the glaciated area has not changed dramatically. Most of the ice discharge increase happen during the first twenty years to reach 200 $\mathrm{Gt\,a^{-1}}$, after this increase slows down. During this relaxation the ice volume increases by less than 0.5 %. Running the model longer should lead to a steady state where the ice discharge should offset the surface mass balance as shown in Fig. 8 when running the same constant climatic scenario for another century.

At the end of the relaxation, the rate of increase of the ice discharge is around 1 $\mathrm{Gt\,a^{-2}}$ and the free surface is nearly at equilibrium except in a few areas near the margins. As shown by Fig. 9a when running the same scenario for another 10 years, this remaining elevation change is mainly positive as these areas are still unable to evacuate all the ice flowing to them but the rate of change is within a physically acceptable range (Fig. 6). The change in surface elevation between the beginning and the end of the relaxation exceeds several hundred meters in some places. This difference is large but is of the same order of magnitude as the uncertainty on the ice thickness in some areas of the GrIS margins. This remains the main limitation of the model where only the upper surface is adjusted to compensate the remaining uncertainties in the model initial conditions.

During this relaxation period with a constant forcing, each drainage basin has been driven toward a steady state and the model results should preferentially be compared with observations before the recent glacier accelerations. Computed discharge values from the main outlet glaciers are compared with observations given by Rignot and Kanagaratnam (2006) for 1996, or 2000 when not available). A detailed comparison shows that the modelled ice discharge is generally underestimated. For the three largest outlet glaciers, where the topography is the best known, the agreement is within 1% for Helheim Glacier and 15% for Jakobshavn Isbrae, but D is largely underestimated by more than 50% at Kandgerdlugssuaq where the drainage basin still shows high rates of surface-elevation change (Fig. 6). Discharge at Petermann Glacier and the North East Greenland Ice Stream is also largely underestimated by up to 80% and these glacier are still thickening at high rates. This can be explain by the fact that their floating tongues are not treated explicitly in the model resulting in the neglect of the mass loss from melting below the shelves at the interface with the ocean. The total computed discharge for the relaxed solution is around 300 $\mathrm{Gt\,a^{-1}}$. Up to now, the magnitude of the computed ice discharge in a continental ice-sheet model has only been addressed by Price et al. (2011) by tuning the boundary condition at the ice front to reproduce the observations only on three major outlets.

The evolution of the total volume and ice discharge obtained with the basal friction coefficient field optimised with the two inverse methods are very similar. This is true for each individual outlet as shown in Table 2. The two inverse methods perform similarly and neither can be favoured in view of these results or in terms of computation performance.

4 Sensitivity experiments

We use the relaxed solution of the Robin inverse method as the starting point to investigate the GrIS mass balance over one century.

4.1 Set-up

The climate forcing in the model impacts ice dynamics only through the net accumulation rate at each surface mesh node as the ice temperature field is kept constant. Two SMB scenarios are used, the first (C1), corresponds to keeping present conditions (Ettema et al., 2009) constant with time; the second (C2) represents an ensemble of 18 climate models forced under the IPCC A1B emission scenario. These forcings are taken from the freely available SeaRISE data compilations (http://tinyurl.com/srise-umt)

Here, we especially focus on the GrIS dynamical response to an increase in basal lubrication. Perturbations are introduced by applying three homogeneous changes in the basal friction coefficient β from the initial field $\beta_i(x,y)$ inferred by the inverse method (Sec. 3.1).

(i) no perturbation with β unchanged form the initial field (BF1):

$$\beta(x,y,t) = \beta_i(x,y) \quad \forall x,y,t \tag{23}$$

(ii) a constant perturbation, introduced as a step function, with β divided by two and then kept constant (BF2);

$$\beta(x,y,t) = \frac{1}{2}\beta_i(x,y) \quad \forall x,y,t \tag{24}$$

(iii) a continuously enhanced perturbation with β reduced by one order of magnitude over one century (BF3).

$$\alpha(x,y,t) = \alpha_i(x,y) - t/100 \quad \forall x,y \tag{25}$$

with $\beta = 10^{\alpha}$. As has been shown for a surging glacier (Jay-Allemand et al., 2011) , β can vary by several orders of magnitude with only small changes in the water pressure so that the magnitude of the imposed changed would not be unrealistic if applied in few locations and for short time periods. However, it seems highly unlikely that the GrIS will surge all at the same time, so that this last sensitivity experiment is used as an high end scenario of the possible future for the GrIS mass loss.

Further destabilisations introduced by changes in the seaward boundary conditions or weakening of the lateral margins are excluded from this study. Experiments are hereafter referred to by the climate forcing (C1 or C2) and the basal friction scenario (BF1, BF2 or BF3).

4.2 Results

We evaluate the results of the perturbations by considering ice-flow velocities (Fig. 1), the ice-sheet total mass balance (Fig. 8), discharge values from the main outlet glaciers (Table 3) and free-surface-elevation rate-of-change (Fig. 9). For the ice-sheet total mass balance (Fig. 8), we present the total ice-sheet volume and volume change from the starting point, converted to sea-level equivalents (Fig. 8a). The annual mass balance (MB, Fig. 8b) is obtained as the time derivative of the ice-sheet volume, and the respective contribution of D and SMB is given in Fig. 8c. The difference between MB and SMB-D is the ice flux lost through the bottom boundary (see Sect. 2.1, discussion on boundary condition Eq. 7). In all the applications it corresponds approximately to 10 % of D.

With constant conditions (C1_BF1), the model tends to reach a steady state where the modelled discharge balances the current SMB ($430\,\mathrm{Gt\,a^{-1}}$, Fig. 8c). Neither the glaciated area, nor the surface velocity pattern, change dramatically during this experiment (Fig. 1c) and the ice-discharge shows a small increase in the main outlets (Table 3).

The climatic perturbation used here (SMB scenario C2) shows a reduction of SMB of approximately only $100\,\mathrm{Gt\,a^{-1}}$ after one century (Fig. 8c), which is a lower bound of the forecast given by current climate models (Fettweis et al., 2008). Changes in the ~~marginal extent~~ glaciated area between the three perturbation experiments detailed below lead to only small differences in the total SMB after one century (Fig. 8c). These differences are one order of magnitude lower than those between the various climate models (Fettweis et al., 2008) but do not account for the possible feedback between the ice-sheet topography and climate as the prescribed accumulation is only a function of position and time. Investigating this feedback requires proper coupling of high resolution ice-sheet and climate models. ~~Other retro-actions could arise from surface elevation changes but this could be constrained more precisely only by coupling ice-sheet and climate models.~~

Without dynamic perturbation (C2_BF1), the computed ice discharge is of the same order of magnitude as the SMB and decreases at an equivalent rate (Fig. 8c). The resulting annual mass balance is nearly centred around zero and ~~shows no~~ lacks a particular trend over the century (Fig. 8b); As a consequence, the total ice volume is nearly constant (Fig. 8a). For this experiment, the velocity pattern (Fig. 1d) remains similar to the present one. The increase of ablation for this climate scenario is higher in West Greenland. This leads to thinning of the marginal ice in this area, resulting in a retreat of land-terminated glaciers and in a decrease in ice discharge of marine terminated glaciers (Table 3).

With a constant dynamic perturbation (C2_BF2), halving β before the simulation results in an almost immediate doubling of the ice discharge bringing it close to $500\,\mathrm{Gt\,a^{-1}}$ (Fig. 8c). ~~in agreement with current estimates based on observations.~~ When looking at the total discharge only, this perturbation allows to compensate the model initial uncertainties as this value is more in agreement with current estimates based on observations (Rignot et al., 2011). However, this is done at the expense of the velocity pattern as we see higher velocities on the interior of each drainage basin (Fig. 1e) compared to the current observations (Fig. 1a). A progression of high velocities areas upstream of each drainage basin may be expected as ablation will reach higher elevations in the future, increasing water pressure and basal lubrications in these areas. However, the expected effect in low elevation areas is not clear as the increase of water availability should contribute in the formation of efficient drainage systems resulting in a decrease of the water pressure and basal lubrication (Schoof, 2010). With this experiment, we investigate how an initial ice-sheet imbalance of $-200\,\mathrm{Gt\,a^{-1}}$, which is close to the current observations, can evolved within a century. The computed total discharge decreases throughout the 100-yr simulation at a rate equivalent to the rate of decrease of SMB except during the first decade where it decreases faster probably as a reaction to the initial perturbation (Fig. 8c). As a result, after the first decade the annual mass balance shows no particular trend (Fig. 8b) and the ice sheet loses mass at a nearly constant rate throughout the century (Fig. 8a). With this experiment, the retreat of land-terminated glaciers in the west coast induced by the increase of ablation is compensated by their acceleration, allowing to drain more ice to the areas subjected to high ablation. Again, the reduction of the discharge throughout the simulation is higher in the west coast as more ice is melted before reaching the margins of the domain.

With an increasing dynamic perturbation (C2_BF3), the discharge increases continuously from $300\,\mathrm{Gt\,a^{-1}}$ to $1400\,\mathrm{Gt\,a^{-1}}$ after 100 yr (Fig. 8c), and the ice sheet is loosing mass throughout the 100-yr simulation (Fig. 8a, b). The acceleration of the ice discharge produced with this scenario is comparable with observations from the last decade (Rignot et al., 2011). The velocity pattern at the end of the century (Fig. 1f), shows large areas of high velocities ($>100\,\mathrm{m\,a^{-1}}$) far inland of each glacier. Such a scenario seems unlikely as it would required a sustained surge of the whole ice-sheet in the same time, which is excluded if efficient drainage systems develop as a result of increased water supply (Schoof, 2010). Nevertheless, This experiment shows that, if it is mechanically possible to sustain over a whole century the currently observed GrIS rate of mass loss, it requires nearly unrealistic maintained dynamical perturbation allowing to estimate an upper bound for the ice-sheet contribution to SLR by 2100.

A important output of these experiments is that the ice discharge shows an extremely rapid adjustment to perturbations as shown at the beginning of the relaxation (Fig. 7b) and in response to the dynamical perturbations (for both experiments BF2 and BF3, Fig. 8c). However, the spatial and temporal variability of the forcings affecting the basal and seaward boundary conditions of the ice sheet remain poorly constrained, limiting the forecast ability of the models.

Together with surface velocities, rates of change of the free surface elevation are nowadays widely measured and available (Pritchard et al., 2009). All the glaciers that have been accelerating recently also show a dynamical thinning. Even if these observations have been used by flow models as a post-validation to try to discriminate between the destabilising processes (Nick et al., 2009; Price et al., 2011), they have not been used in a proper inverse method so far. Because the homogeneous dynamical perturbation applied here is probably too crude, we do not compare our results to observations but provide a qualitative discussion of the surface elevation changes computed after 10 yr of simulations (Fig. 9).

With experiment C1_BF1, as discussed previously, some drainage basins are still not at equilibrium and their outlets are still thickening due to an SMB higher than the computed discharge. In the interior, the surface elevation change is nearly zero. With a climate perturbation only, i.e. experiment C2_BF1, the margins in the west and south east are thinning and the thickening of Petermann Glacier and the North East Greenland Ice Stream is less pronounced. These differences with experiment C1_BF1 mostly come from the change in the SMB term.

Experiments with a dynamical perturbation, i.e C2_BF2 and C2_BF3, show an additional dynamical thinning associated with the acceleration of the ice, upstream of each drainage basin. Downstream, near the margins, this gives two different behaviours. (i) If the decrease of the basal friction coefficient also produces an acceleration and an increase of the discharge sufficient to offset the mass excess coming from upstream, the whole ice stream shows a dynamical thinning. This is the case in the south east, and for the Jakobshavn Isbrae and the Heilhem Glacier. (ii) If the acceleration and the increase of the ice discharge are not sufficient, this results in a dynamical thickening of the margins. This is the case in the north west and for the Kangerdlussuaq Glacier. For the land terminated glaciers in the south of Jakobsahvn Isbrae, the dynamical perturbation induces a less pronounced thinning.

In some aspects, these results therefore agree with observations and show an interesting regional variability of the response to the dynamical perturbation. The link between surface runoff, basal hydrology and basal sliding will have to be investigated more in-depth to make quantitative comparisons.

5 Conclusions

We have shown that our implementation of a model with the correct treatment of longitudinal stresses, sufficient resolution to resolve medium to small outlet glaciers, and a careful initialisation of ice-flow parameters allows to satisfy the essential pre-requisite of simulating the ~~present-day conditions.~~ observed velocity structure of the ice sheet. Neither of the two recent inverse methods used to infer the basal friction coefficient field from the knowledge of the ice sheet topography and ice surface velocities can be favoured in view of our results.

Due to the remaining uncertainties in the model initial conditions, the ice-sheet surface has been allowed to diverge from the observations during a relaxation period of 50 years. This is the main limitation of the model as this relaxation drives the model toward a steady state, limiting our ability to capture the currently observed transient response of the GrIS. We have shown that, after the relaxation, the ice discharge obtained globally and for individual outlet glaciers is in good agreement with observations of the late 1990's. The main remaining uncertainties in the model are the ice viscosity field, the bedrock elevation and the position of the ice margins. Future work will benefit from new observations and from the developments of inverse methods to constrain these uncertainties (Arthern and Gudmundsson, 2010; Pralong and Gudmundsson, 2011; Morlighem et al., 2010; Petra et al., 2012). However, these methods remain restricted to diagnostic simulations and proper methods, such as automatic differentiation of the true discrete adjoint model (Heimbach and Bugnion, 2009), still have to be applied to higher-order ice flow modelling to capture the observed transient response of the ice sheet.

More specific projections will arise as the ice sheet is driven by more complete and precise climate scenarios, and with greater understanding of processes at the front of outlet glaciers. Our new-generation continental-scale ice-sheet model is well suited to incorporate such information as it becomes available. However, our current model experiments are significant and general conclusion can already be drawn.

Results confirms that the overall mass balance of the GrIS is sensitive, not only to changing SMB, but in large parts, to ice discharge. Our model shows a rapid adjustment of the ice discharge in response to dynamical perturbations. This is supported by other models that include processes at the ice front (Nick et al., 2009, 2012) and by recent observations (Howat et al., 2007). Moving ice margins and a physically-based treatment of calving are essential future development of the model to move from sensitivity experiments to realistic projections of the ice discharge evolution.

Results show that, unless the perturbation is continuously enhanced, an increase of the surface ablation reduces the discharge, stabilising the ice sheet. This demonstrate that, even on a century timescale, discharge and surface mas balance anomalies cannot be treated independently to estimate the GrIS contribution to SLR. This effect could be even higher due to the possible feedback between surface elevation and surface mass balance. Proper coupling with local surface mass balance models will then be required to improve the model predictive ability.

The discussion below has been improved

We use the modelling conducted here and the currently observed ice-sheet mass balance to estimate possible bounds for the future GrIS contribution to SLR.

In our experiments with constant basal conditions, the rate of decrease of D and SMB are similar, resulting in a nearly constant annual mass balance. In these conditions, a probable lower bound for the ice-sheet contribution to SLR is given by assuming that the currently observed ice-sheet imbalance will be preserved over the whole century. Taking an averaged value of $-300\,\mathrm{Gt\,a^{-1}}$ for the ice-sheet mas balance in 2010 (Rignot et al., 2011) leads to a total contribution to SLR of 75 mm by 2100. This value can be compared to the 46 mm (40 mm from SMB and 6 mm from dynamics) given by Price et al. (2011) as an estimate of the committed SLR due to the observed last decade ice-sheet dynamics. Our value is closer to their estimated upper bound (85 mm including 45 mm from dynamics) which is estimated by assuming a self-similar response of the ice sheet to a 10 yr-recurring forcing in the future decades (Price et al., 2011) .

We have shown that, conversely, if the forcing is continuously increased, there is sufficient ice available to sustain the current rate of increase in discharge over an entire century. However, in our model, this is achieved with an unrealistic perturbation of the basal lubrication field. In these conditions, a probable upper bound for the ice-sheet contribution to SLR is given by assuming that this rate of increase will be preserved in the future. Taking a discharge anomaly of $100\,\mathrm{Gt\,a^{-1}}$ increasing at rate of $9\,\mathrm{Gt\,a^{-2}}$ for year 2010 (Rignot et al., 2011), leads to a contribution to SLR of 140 mm by 2100 from dynamics only. This value is on the lowest half of the values obtained from kinematic considerations assuming low (93 mm) and high (467 mm) scenarios (Pfeffer et al., 2008).

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Table 1. List of parameter values used in this study.

Parameters	Values	Units
E	2.5	
$A_o(T < -10°\mathrm{C})$	3.985×10^{-13}	$\mathrm{Pa^{-3}s^{-1}}$
$A_o(T > -10°\mathrm{C})$	1.916×10^{3}	$\mathrm{Pa^{-3}s^{-1}}$
$Q(T < -10°\mathrm{C})$	-60	$\mathrm{kJ\,mol^{-1}}$
$Q(T > -10°\mathrm{C})$	-139	$\mathrm{kJ\,mol^{-1}}$
g	9.8	$\mathrm{m\,s^{-2}}$
n	3	
ρ_w	1025	$\mathrm{kg\,m^{-3}}$
ρ_i	910	$\mathrm{kg\,m^{-3}}$
h_min	10	m

Table 2. Model discharge for individual outlets and the whole ice sheet in $\mathrm{Gt\,a^{-1}}$: after 1 yr (RI_1a) and after 50 yr (RI_50a) of surface relaxation with the Robin inverse method, after 50 yr of surface relaxation with the control inverse method (CI_50a), and observations from 1996 (or 2000 when not available, and converted from $\mathrm{km^3\,a^{-1}}$ to $\mathrm{Gt\,a^{-1}}$ using a uniform density of 910 $\mathrm{kg\,m^{-3}}$ for ice) (Rignot and Kanagaratnam, 2006) (Obs.).

	RI_1a	RI_50a	CI_50a	Obs.
West				
Jakobshavn I.	30.2	24.4	26.1	21.5
Sermeq kujatd	0.4	7.8	7.9	9.1
Rink	13.8	9.7	10.5	10.7
Hayes	0.1	4.1	3.7	9.0
East				
Daugaard-Jensen	0.0	7.2	7.4	9.1
Kangerdlugssuaq	0.1	9.9	11.1	25.3
Helheim	10.3	23.7	24.0	23.9
Ikertivaq	0.2	22.5	22.8	9.2
North				
Petermann	0.0	1.9	4.0	10.7
Nioghalvfjerdsbrae	0.0	4.5	4.6	12.3
Zachariae I.	0.5	6.7	7.0	9.0
Total	65.1	306.7	326.0	325

Table 3. Model discharge for individual outlets and the whole ice sheet in $\mathrm{Gt\,a^{-1}}$ after one century for the various experiments.

	C1_BF1	C2_BF1	C2_BF2	C2_BF3
West				
Jakobshavn I.	22.7	16.9	27.2	108.5
Sermeq kujatd.	8.5	3.8	8.8	48.3
Rink	10.6	9.9	13.8	31.4
Hayes	4.7	2.3	5.1	26.0
East				
Daugaard-Jensen	8.0	9.0	12.8	35.9
Kangerdlugssuaq	11.8	11.5	16.4	47.0
Helheim	24.9	23.8	30.6	82.6
Ikertivaq	24.3	20.3	24.7	59.6
North				
Petermann	5.8	1.1	5.4	34.9
Nioghalvfjerdsbrae	6.6	4.8	11.6	58.2
Zachariae I.	7.5	6.1	13.7	66.3
Total	362.7	252.4	387.1	1404.5

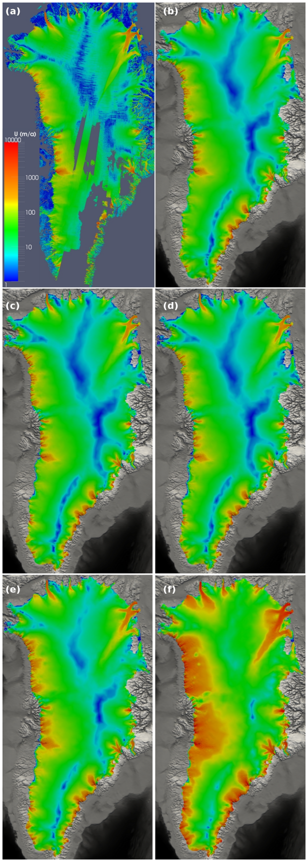

Fig. 1. GrIS surface velocities. **(a)** Observed surface velocities on the original regular $500\,\mathrm{m} \times 500\,\mathrm{m}$ grid; Computed surface velocities: **(b)** after relaxation; after one century for **(c)** experiment C1_BF1, **(d)** experiment C2_BF1, **(e)** experiment C2_BF2 and **(f)** experiment C2_BF3.

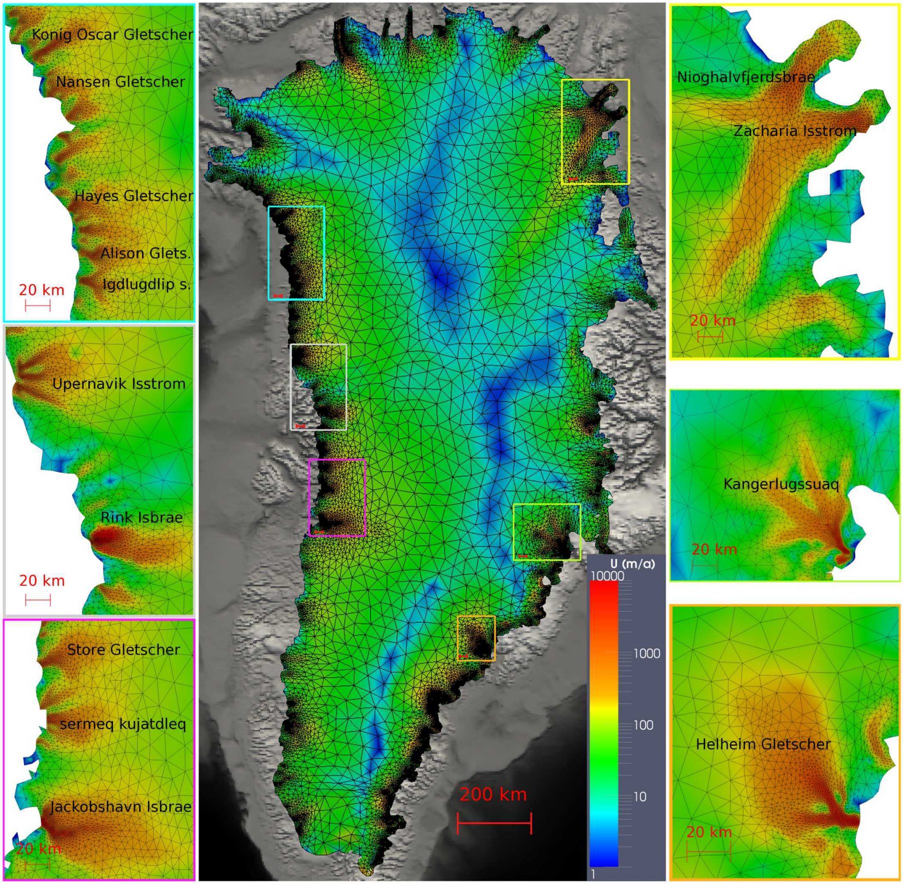

Fig. 2. Unstructured finite element mesh and model surface velocities after optimisation of the basal friction coefficient with the Robin inverse method. Colored boxes show close-up views for various outlet glaciers of interest.

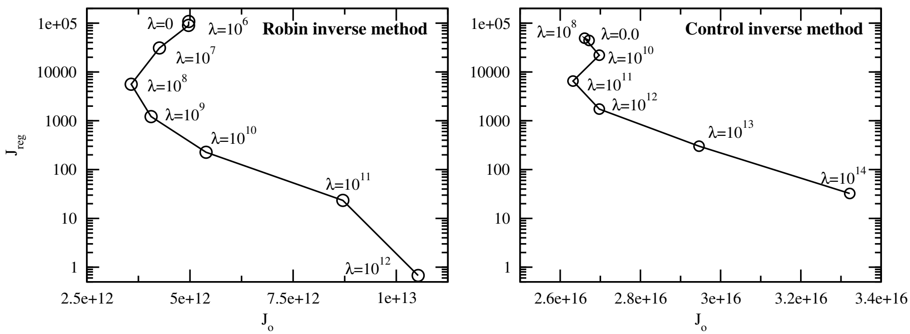

Fig. 3. L-Curve obtained with the Robin (left) and Control (right) inverse methods.

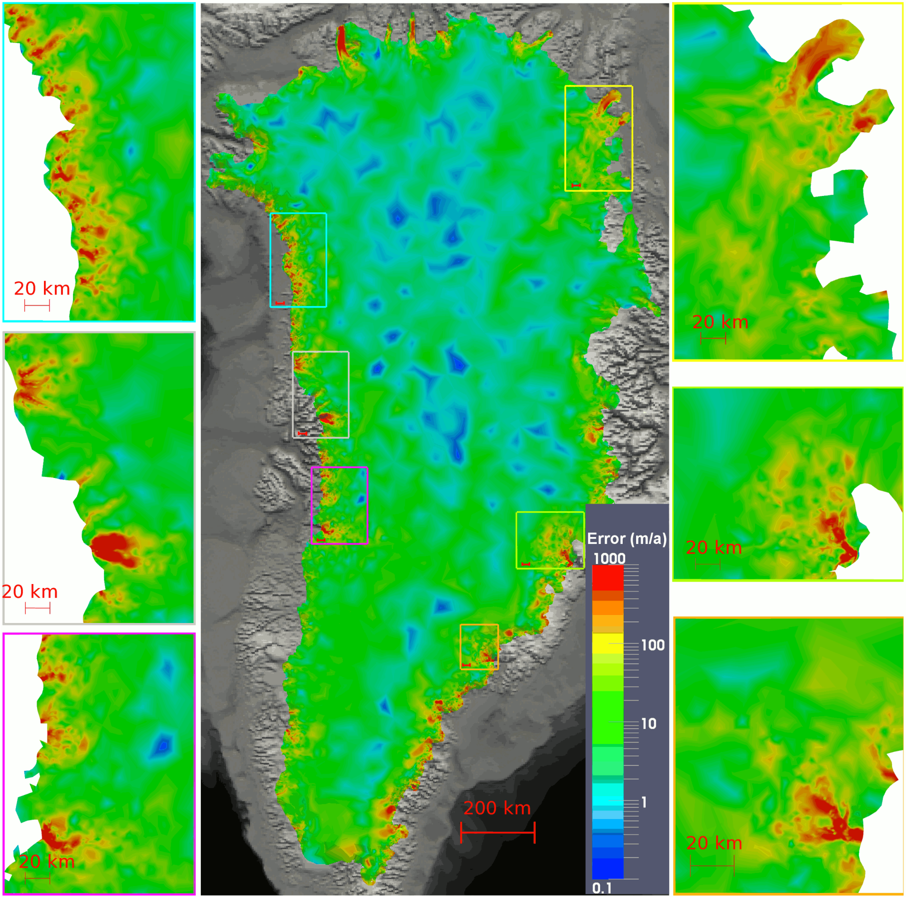

Fig. 4. Absolute error on surface velocities $|\boldsymbol{u}^{\mathrm{mod}} - \boldsymbol{u}^{\mathrm{obs}}|$ in ma^{-1} at the end of the optimisation using the Robin inverse method.

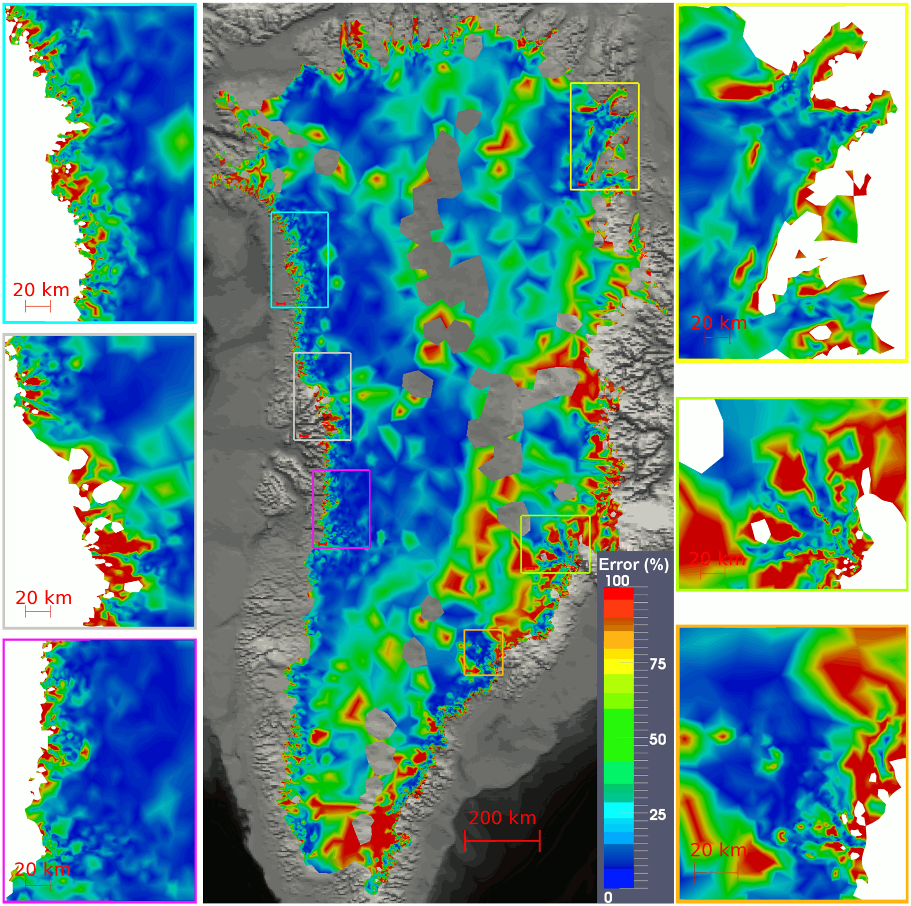

Fig. 5. Relative error on surface velocities $|\boldsymbol{u}^{\text{mod}} - \boldsymbol{u}^{\text{obs}}|/|\boldsymbol{u}^{\text{obs}}|$ in % at the end of the optimisation using the Robin inverse method. Areas where $|\boldsymbol{u}^{\text{obs}}| < 2.5\,\text{m}\,\text{a}^{-1}$ have been removed from display.

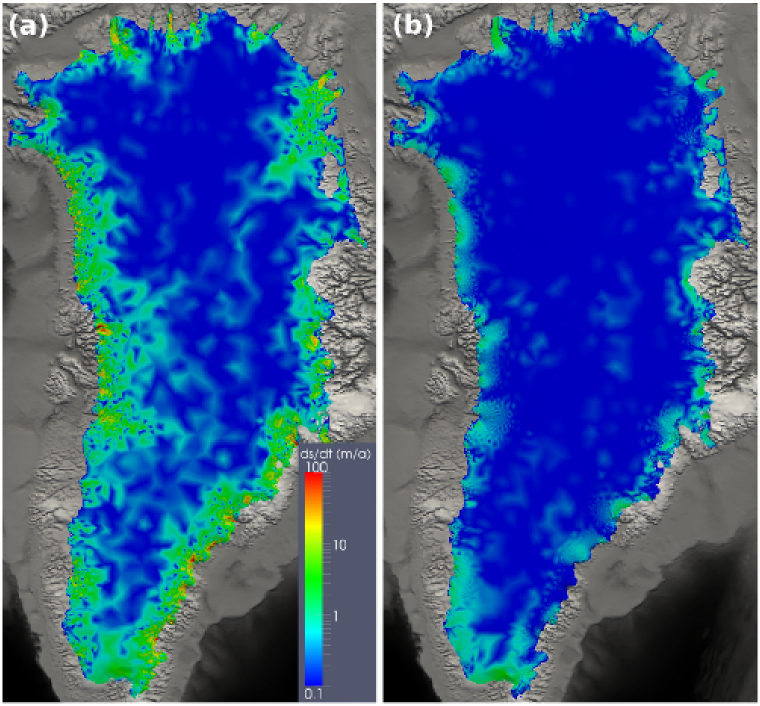

Fig. 6. Free surface elevation absolute rate of change: **(a)** after 1 yr of relaxation; **(b)** after 50 yr of relaxation.

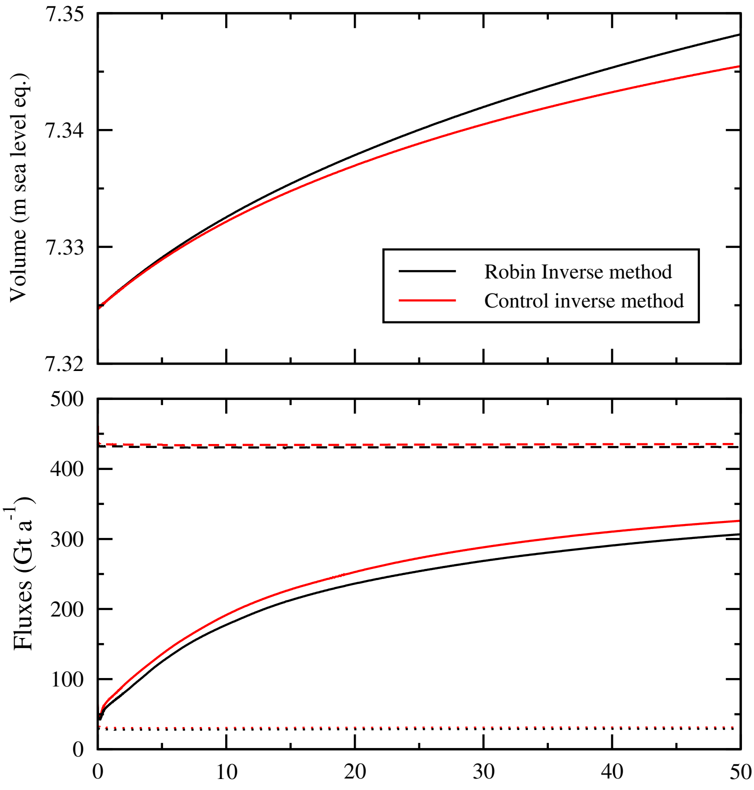

Fig. 7. GrIS mass balance during relaxation: (top) Evolution of the total ice volume in meters of sea level equivalent and (bottom) evolution of the discharge (solid lines), SMB (dashed lines) and flux through the bedrock (dotted lines) in $\mathrm{Gt\,a^{-1}}$.

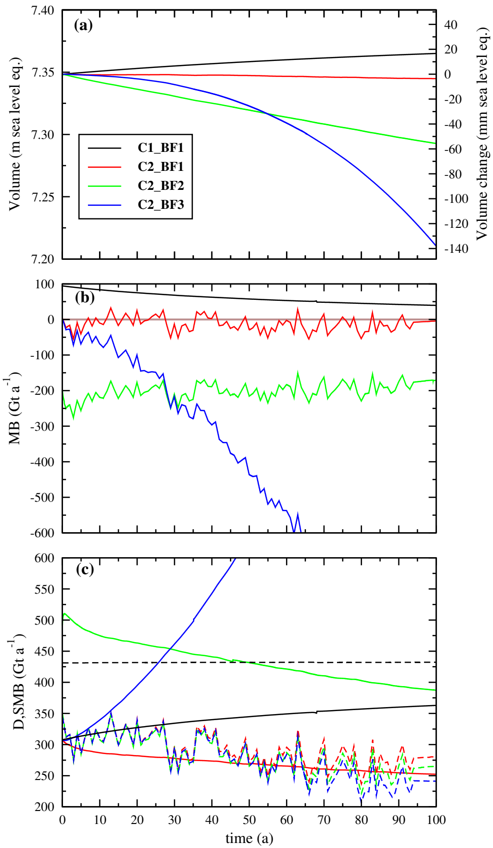

Fig. 8. GrIS future mass balance as a function of time: **(a)** total ice volume (left axis, meters of sea level equivalent) or volume change from initial time (right axis, milimeters of sea level equivalent), **(b)** annual mass balance (the brown line correspond to 0) and **(c)** discharge (D) (solid lines) and SMB (dashed lines) in $\mathrm{Gt\,a^{-1}}$.

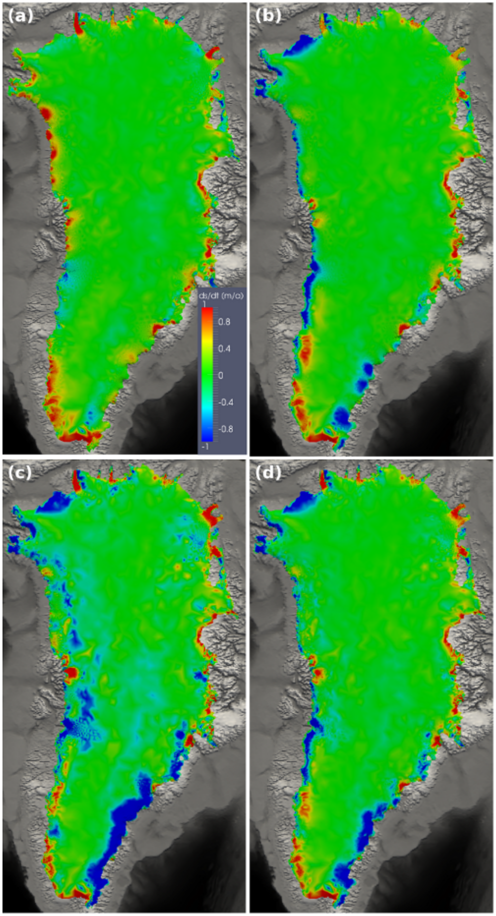

Fig. 9. Free surface elevation rate of change after 10 yr for experiments **(a)** C1_BF1, **(b)** C2_BF1, **(c)** C2_BF2, and **(d)** C2_BF3.