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The Cryosphere Discussions

Interactive comment on "Present-day mass changes for the Greenland ice sheet and their interaction with bedrock adjustment" *by* M. Olaizola et al.

M. Olaizola et al.

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General Comments

AC: We thank reviewer 2 for his thoughtful comments and reply to them below.

RC: Olaizola and coauthors analyse a set of experiments with a model for the Greenland ice sheet including an isostatic module to account for bedrock adjustment due to the ice loading history. The main goal of this paper is to question the possibility of present day bedrock subsidence as a consequence of past surface mass balance changes as proposed by Wu et al. (2010). While the paper presents a nice set of schematic experiments and (given a thorough reworking) may be interesting in its own right, it fails in my eyes to achieve its goal for two reasons. First, it produces a bedrock response for a given surface mass balance model without variations which may well be the largest unknown when reconstructing past ice loading changes. From the given experiments, it is not possible to strictly exclude present day subsidence of the distribution and magnitude shown by Wu et al. (2010). Second, the analysis of the model results is limited by too much simplification, reducing the complex interactions between ice loading and bedrock in the discussion to a local and linear problem of changes in mean values.

AC: It is not correct to state that there are no variations in the model. The SMB is actually varying as a function of a) surface elevation and b) annual temperature as is explained in the manuscript. We illustrate this in Figure 1 and 2 of the present document using the initial and final SMB fields of the experiment 'Holocene and last deglaciation, ice core data'.

It is clear that the initial state used in some of the experiments (those in which the last glacial period is consider) is not an accurate configuration of the ice sheet, nevertheless represents a possible ice-geometry under the surface mass balance scheme that we are using. The initial state of the ice sheet depends

on mass balance, ice thickness and bedrock topography, being all variables unknown for the past ice history. The only variable that is well known back in time is temperature, therefore the variable we used to reproduce a surface mass balance field.

The crux of this manuscript is that changes in bedrock uplift are always concentrated in the marginal zone of the ice sheet and not in the centre. This is independent of the mass balance used. We show that it is possible to have subsidence (albeit lower), the crucial point is that a pattern with the largest subsidence in the central parts of Greenland is not possible, at least not for the past 7000 years. This is summarized in Table 1, where all experiments show a very different imprint from the one presented by Wu et al. (2010). Regardless of the exact form of the SMB field, realistic changes in ice thickness are always concentrated near the margins, and this is reflected in the bedrock adjustment pattern.

The interaction between changes in ice thickness and in bedrock elevation is indeed not linear as the referee indicates. They are also not treated as such in the model. The mean values of ice thickness and bedrock change are used to explain and analyze the results in a more quantitative way, and to select time slices for which the spatial patterns over the entire ice sheet are shown. We will stress this better in the text (for example in comments p3456.l11 and p3463.l6.

General Comments

RC: It may not be very rewarding to write a paper with the main conclusion that another paper is wrong' but it is interesting and necessary for the scientific community. I therefore appraise the attempt of Olaizola and coauthors. However, the line of argumentation is not well developed and the set of presented experiments does not achieve this goal. The manuscript lacks scientific precision and needs a thorough reworking to remove inconsistencies and improve the language. Some non-exhaustive examples are given in the detailed comments below. The discussion of the results is largely focused on mean values of bedrock adjustment and ice loading changes, while spatial variability is key to understanding the complex interactions.

While the authors seem to be aware of the complexity the wording suggests that we are looking at a local, linear and instantaneous response of the bedrock. In fact, the interaction is non-linear and non-local in time and space.

Throughout the entire manuscript changes in ablation and precipitation are discussed that (according to my understanding of the model) cannot be derived from the SMB gradient method. It is not clearly explained where this information comes from nor is it backed up by data or figures that show these relations.

AC: This study is an attempt to quantify how the bedrock response may look from an ice sheet modeling perspective with the aim that this may help the interpretation of GRACE data. Obviously this is important because GRACE data need to be corrected somehow for GIA. Either a priori or in a simultaneous solution as proposed by Wu et al. (2010).

We agree with the reviewer that the spatial patterns of bedrock change are important, they are shown and discussed throughout the whole manuscript and summarized in Table 1 where they are a vital piece of evidence.

Since the interaction between variables is not linear, we introduced the use of SGVE model to calculate bedrock changes. It turns out that this is not providing fundamentally different results than the ELRA model, which is simpler but also non-linear and non-local in time and space. The crux is, in fact, that ice mass changes are concentrated near the margin irrespectively of the chosen bedrock model. Hence patterns of uplift and subsidence are lined up with the ice margin and not radially symmetric from a central ice sheet point as the Wu pattern suggest. The idea was not to imply a linear or instantaneous response (a main point along the manuscript is the discussion of the lag of the bedrock changes with respect to changes in the ice thickness), nevertheless we will emphasize the non linearity of the system even further, as is exemplified in the specific comments below. We will also add a simple calculation of the required mass change for an elastic response which yields the pattern reconstructed by Wu et al. 2010.

Regarding the SMB, we tried to clarify that SMB gradients are the result of spatial changes in net ablation (i.e. negative or positive SMB, respectively). We assume here that these gradients can be used to account for changes in positive or negative SMB due to variations in climate forcing.

The discussion will be done in terms of a decrease or increase in the SMB rather than in terms of precipitation and ablation. An example of the resulting changes in the manuscript is given below (comment p3463.l11).

Specific Comments

Abstract:

RC:Abstract - seems a bit detailed and thus, long. Perhaps it could be shortened to the main points. **AC: We agree. The abstract will be shorter. We changed some of the sentences as follows.**

RC: p3456.l11 'This subsidence appears to be counterintuitive since the ice sheet is loosing mass at present.' Bedrock subsidence for an ice sheet which is loosing mass would only be counterintuitive if one would expect an instantaneous response. This is clearly not the case. Should be reformulated. **AC: This subsidence appears to be counterintuitive if one assumes an immediate (elastic) response since the ice sheet is loosing mass at present. It may however be a delayed response to past changes.**

RC: p3456.l19 'Under a sine forcing of the annual temperature, that mimics the temperature variations in the Holocene ...' Should say 'annual mean temperature' and mention which period of the forcing is used. **AC: Changed to: Under a sine temperature forcing with a period of 1000 years, that mimics the annual mean temperature variations in the Holocene**

RC: p3456.l22 Replace 'Although,' by 'However,' **AC: Changed.**

RC: p3457.l10 'This undermines results suggesting that recent loss is only half of the regular ice mass loss changes.' Replace 'regular' by 'previously published'. Also clarify what is compared here: mass loss or mass loss changes. **AC: Changed to: This**

undermines results suggesting that the recent mass loss derived from GRACE data is only around half of the previously published values.

Main text:

RC: p3458.l5 'In this study we tested the hypothesis that the recent negative trend in the integrated SMB over the GrIS intuitively leads to ice thinning and hence an average uplift of the bedrock response, which is in disagreement with the results by W10.' This needs some clarifications. First of all, overall MB is not only controlled by SMB but also by dynamic ice discharge. Second and as mentioned above, given the long response time of bedrock adjustment it is not at all intuitive that ice thinning and uplift are instantaneously coupled. I think you have to reformulate your hypothesis here.

AC: We rephrased it to the following:

An average bedrock subsidence as suggested by W10 would intuitively be the result of an increase in ice loading, either in the past or present. Since there is presently no indication of increasing SMB (Ettema et al., 2009) that could explain bedrock subsidence in the central part of the ice sheet, we test the hypothesis that any SMB variability in the past is responsible for the inferred bedrock subsidence.

RC: p3458.l7 'an average uplift of the bedrock response,' Should be 'an average uplift of the bedrock,' **AC: Changed.**

RC: p3458.l12 'This is done with a coupled ice sheet-bedrock model driven by variations in mass balance.' Should be 'This is done with a coupled ice sheet-bedrock model driven by variations in surface mass balance.' **AC: Changed.**

RC: p3458.l17 'The first experiment schematically mimics climate fluctuations during

the Holocene following a sine function' Mention the period and amplitude of the sine function here. **AC: Changed to: The first experiment schematically mimics climate fluctuations during the Holocene following a 1K-amplitude sine function with a 1000 yrs period.**

RC: p3458.l21 Your use of 'PD' is not consistent in the text. Sometimes it seems to mean year 2010 or similar and sometimes it means the recent past or the entire Holocene. This has to be revised in the entire manuscript. **AC: Changed to: In page 3464, line 5 changed to: We consider PD (t=0 kyrs) as the initial time of the reference state that results from the initialization of the model (all temperature anomalies are relative to the PD conditions). In addition, the use of PD will be revised along the manuscript.**

RC: P3458.l21 'Those results are performed for two different bedrock models.' Should be 'Those experiments are performed for two different bedrock models.' Could say here that this is done with a more complex model to validate the simpler standard model.

AC: Changed to: This experiment was performed for two different bedrock models, the ELRA model, used as the standard model in the rest of the experiments, and the more complex SGVE model to validate the previous one and to study the possible influence of the model choice in the resulting bedrock pattern.

RC: P3458.l25 Is there a model description paper available, has the model been used in other studies? The model description in the document is rather sparse and should be extended in case no references exist.

AC: We agree, the following references to other studies where similar models have been used will be added in page 3458: van de Wal (1996), van de Wal (1999), Helsen et al. (2012) (included in the References in the Supplement.)

RC: p3459.l9 The conversion of temperature changes to elevation changes is in my eyes rather counter-intuitive. The opposite would make much more sense. It should be made clear that this is just a way to simplify the calculations and not an attempt to simulate the physics of the system.

AC: The SMB scheme used in this work is not only a way to simplify the calculations, which, moreover, is physically based. It is also an attempt to provided a SMB field that can evolve in time, as a function of temperature, translated into changes in elevation, see Helsen et al. (2012).

RC: p3459.l16 I do not see the need to reproduce figure 1 from Ettema et al. here since it is a paper freely available for the reader. In general, compared to the other model components that are hardly described at all, a lot of detail can be found here about the SMB component. I would rather suggest a short overview of how SMB is calculated and a critical discussion of the assumptions of this method and their consequences. The most important one I can see is that present day SMB gradients are assumed to also hold for a completely different climate regime (LGM), which should be questioned at the very least.

AC: A detailed description of the SMB scheme, as well as the figure 1 taken from Helsen et al. (2012) (I guess the reviewer means Helsen instead of Ettema) were provided in more detail because the article was still in review at the time of submission of the current manuscript. Since it has been published we will follow the reviewer advice and we will shorten the description.

We will indicate the following in the manuscript:

With the SMB gradient method, a spatially homogeneous climatic temperature perturbation is transferred into a regionally variable SMB field. The response of the SMB to these perturbations, in a specific grid point, depends on the sign and magnitude of the SMB gradient (parameters b_{abl} and b_{acc} in Fig. 4 in Helsen et al.

(2012)). This response is quite straightforward in the ablation area, where an increase in temperature leads to a SMB decrease. The magnitude of this response depends on the spatially variable parameter b_{abl}. In the accumulation area, values of b_{acc} can be either positive or negative, thus causing opposite effects on the SMB. We assume here that the present-day SMB gradients can be used for different climate states throughout the deglaciation. This may be questionable during glacial conditions, but it ensures a dynamic SMB forcing, also for initially non-glaciated areas around the present-day ice sheet and it improves if the conditions are close to the present-day configuration. In Figure 1 and 2 of this document, a SMB field for PD is presented, obtained from an initial SMB corresponding to a colder climate (-10K). The initial SMB evolves accordingly to the SMB gradient method and reaches a final configuration for PD similar to the reference one (see Fig. 1 in Helsen et al. (2012)), with an area of high accumulation located in the southeast and ablation along the margins. Thus, the SMB method is self consistent, in the sense that produces a SMB for glacial conditions that can evolve to a SMB field for PD which is, if not the same, very similar to the reference one from Ettema et al. (2009).

RC: p3459.l21 Clarify the difference or relation between Hc and the equilibrium line.
AC:The equilibrium line separates the ablation zone and the accumulation zone in the ice sheet. Hc is a critical altitude defined locally, for every grid point, which marks the transition between the use of the a and b coefficients. Nevertheless any mention to Hc will be excluded from the manuscript since more information can be found in Helsen et al. (2012).

RC: p3460.l10 Specify what parameters are used for the flow law and for the sliding law. The model description should be generally completed and extended here if no reference is available. This could also go into an appendix, but in its present form, the description is not sufficient.

AC: Yes, references will be added as well as a table with the values of all the constant used in the model.

RC: p3460.l5 'The temperature field is solved by ...' should be 'The ice temperature evolution is calculated by ...'

AC: Changed.

RC: p3461.l19 What kind of initialisation is actually performed? What has been done with the observed data of Bamber. p3461.l20 Is 20 km x 20 km also the ice sheet model resolution? This should be added in the model description.

AC: Changed to: To initialize the model, bedrock topography and ice thickness were taken from Bamber et al. (2001) with a spatial resolution of 20 km x 20 km and the present-day reference SMB field and surface temperature from Ettema et al. (2009). The model runs with these initial values for 200 000 years until a steady-state is reached.

RC: p3462.l1 Do you mean 'mass balance variations' or rather 'surface mass balance variations'? Clarify.

AC: There is no reference to mass balance variation at that page or the previous one.

RC: p3462.l5 This is not a last millennium experiment! Change the section title. The first and the second experiment are both idealised experiments and this should be clear also from the section titles.

AC:Changed to: Idealized last millennium experiment.

RC: p3462.l8 When looking at Kobashi et al. (2009) it is very difficult to see how your

forcing signal could possibly be an approximation to the reconstruction. There is a lot of variability on many different time scales. It is clear that the ice sheet integrates most of the shorter term climate variability and the bedrock is again an integrator if ice loading changes. But this argument has to be raised before you can ignore all frequencies below 1/kyr altogether. It could help to see a smoothed version of Kobashi to get the point across. I could also not readily extract a magnitude of 1K from from the data. For later interpretations of little ice age cooling and others it would help to explain what part of the sinusoidal should be interpreted as the present day.

AC: The sine function is chosen to simplify the forcing (this is done as a first step, real more complicated temperature variations were consider in following experiments) and to show how changes in bedrock elevations due to recent temperature variations could possible look like. In Figure 3 of the present document, the sine forcing used in the last millennium experiment and the temperature variations from Kobashi et al. (2009) can be seen together, as well as the chosen 1K amplitude and the part of the sinusoidal corresponding to present-day. Similar results were obtained using the actual data from Kobashi et al. (2009), an alternation between bedrock uplift and bedrock subsidence between the southeast and southwest, shown in Figure 4 and 5 of this document.

RC: p3462.l8 Mention over which spatial domain averages are made.

AC: In pg 3462l10 Changed to (spatial average over all the grid points in the domain excluding ocean points)

RC: p3462.l15 The interesting aspect of modelling isostatic rebound with a coupled model is that there are feedbacks in the system due to similar adjustment time scales of ice sheet and bedrock. This is not a linear system! It is misleading to state that 'temperature forcing ... leads to ice thickness changes ... lead to a bedrock response' when the opposite is also and equally true. It should be mentioned that both bedrock adjustment and ice thickness changes can change the temperature forcing in your model setup.

AC: We agree that is not a linear system. Nevertheless, the model is forced by temperature variations, reflected in changes in ice thickness. The bedrock responds accordingly to these changes as we solve the equation in a coupled way. As a result there is a new surface elevation in every grid point that in turn modifies the SMB, therefore, can influence ice thickness, but this will not, by any means, affect the temperature forcing in the model. As we show the phase relation between the parameters studied might indeed be such that at the same time temperature increases and bedrock increases or decreases.

RC: p3462.l16 I would suggest to insert 'The total length of the simulation is 60 kyr.' before 'We focused the analysis' in line 14. It may also be useful to declare the first 57kyr a spin-up experiment and analyses the 3000 year experiment as such. Time indications like in line 26 would be easier to read in this framework.

AC: Changed.

RC: BTW, I do not really see evidence in figure 2 for a quasi steady state. Should mention that and could say that for your purpose remaining variability is not an issue.

AC: In figure 2 of the manuscript, only the last 3000 years of the simulation are considered, as is explained in the text, to see the quasi-steady state it would be necessary to plot the whole period of the simulation, which is pointless. The remaining variability is, instead an issue, what we are analyzing. Only the last part of the simulation is considered, to filter the spin-up effect. We clarified the text to stress this better. We will also change Fig 2 and the time scale will be from -3000 yrs to 0 (PD). moreover the label 'T' would be change to 'dT' and 'T=0' to 'T=PD temperature' (answering comment regarding p3463.l9)

Changed to: The total length of the simulation is 60 kyr, We focused the analysis

on the last 3000 years of the simulation to filter the spin-up effect.

RC: p3462.l19 Could you clarify how you determine that there is a 200 year lag? Is it not possible that your lag is 200 years plus some multiple of the 1000 year forcing period instead? For instance 2200 years comparable to the lag you find in the other experiments.

AC: The time lag is calculated as the time between the moment when the ice thickness derivative is zero (in this moment there is a change from ice thickening to ice thinning) and the moment when the derivative in bedrock elevation has a value of zero. Since the forcing is periodic, similar responses follow every cycle of temperature variations, with similar values of this time lag. The bedrock always relaxes towards a steady state unless there is a new change in loading. So it cannot be a multiple of the 1000 year forcing period.

RC: p3463.l5 Is this really the only ablation area for the given forcing? That would be surprising. Would be good to have a figure showing the SMB distribution for selected configurations also in the following.

AC: It is not the only ablation area but it is clearly the most dominant one as ice thickness changes are largest and the bedrock response is most pronounced. For this 'weak' variations in the temperature forcing there are actually no large changes in the shape of the SMB field that remain mainly equal to the reference field obtained form Ettema et al. (2009) (see Helsen et al. (2012) Figure 1).

RC: p3463.l6 'As a result, ice thickens and the bedrock subsides' Again, this is simplifying the complex behavior to a linear system. Should always mention at least that the bedrock response is delayed. Also in the following.

AC: Changed to: As a result ice thickens and the bedrock subsides (blue colors).

RC: p3463.l9 ‘When the temperature increases to positive values ...’ Is it absolute temperature or temperature forcing that turns positive? Clarify. **AC: Changed to: when the temperature increases more than the reference value (PD temperature) so it is dT rather than the absolute temperature. We changed the text and figures accordingly**

RC: p3463.l11 ‘This is due to the enhanced precipitation ...’ I thought your model works with SMB gradients? I do not see how you can distinguish between different components of the SMB at this stage. Clarify. See also argument around ablation changes in line 16.

AC: Changed to: This is due to an increase in SMB that results in ice thickening. In the accumulation areas, where the positive SMB is the dominant factor, an increase in temperature is reflected in an increase in the SMB (reflecting a higher precipitation). Although, over the rest of the island, the temperature increase is followed by a decrease in SMB, which results in less ice volume

RC: p3463.l24 ‘... an ablation area located in the south west and an accumulation region in the southeast.’ Again, are these the only regions of positive and negative SMB?

AC: It is not the only ablation area, but clearly the most dominant one as is shown by the bedrock response.

RC: p3464.l1 Should be made clear that this is a schematic experiment. At any rate, the order should be reversed to mention last deglaciation first and then Holocene.

AC: Since the title of the section would be change it will be clearer that it is an schematic experiment. We disagree in changing the order of the experiments. The firs step is analyze the influence that present changes in ice thickness had

in the pattern of bedrock elevation changes, and then, study the influence in the pattern due to ice thickness changes that occurred further back in time.

RC: p3464.l6 ‘Finally, the temperature oscillates around PD as ...’ should be ‘Finally, the temperature oscillates around its PD value as ...’

AC: Changed

RC: p3464.l11 It is necessary to be more detailed about where thickening and thinning occurs. Looking only at mean values does not suffice in this discussion.

AC: To do this it will be necessary to add all plots of dHi and SMB for every selected point. This will greatly reduce readability of the paper. Ice thinning occurs at the margins, being stronger in the southwest as it is reflected in the pattern of bedrock elevation changes. The discussion has been done not just in terms of mean values, but this allows a more quantitative description, and an overall description as stated before. In Table 1 we combine the average rates and the patterns together

RC: p3464.l12 Remove repeated sentence ‘This is a consequence of more ablation’

AC: Changed

RC: p3464.l13 Again, it is not clear to me how you know that precipitation is enhanced. BTW, if that would be the case, it should have been increasing from the beginning of the experiment.

AC: Yes, the analysis should be in terms of SMB.

RC: p3465.l5-10 I do notunderstand why the model is not initialised correctly? It should not be difficult to introduce a spin-up procedure that does not lead to problems at the beginning of the run.

AC: A proper spin up has been performed.

RC: p3465.l24 Same as above (p3464.l1) applies to the title here.

AC: We disagree since in this case we are using ice core data, therefore the experiment is not so idealized.

RC:p3466.l22 ' ... we carried out the experiment presented in Sect. 3.1' Should briefly repeat what this experiment is.

AC: Changed to: 3.1, where the temperature variations for the last millennium are represented by a 1K-amplitude sine function with a period of 1000 yrs,

RC:p3466.l23 'found a time lag of the bedrock' What is the bedrock lagging? For which experiment? Clarify.

AC: In the previous sentence, p3455l22 is written which experiment and section we refer to. To be clear we changed it to: In this experiment we found

RC: p3466.l24 Without any former motivation, this is the first time the Little Ice Age comes up in this manuscript. I really do not see how the presented schematic experiments justify such a conclusion.

AC: The link to the LIA has been done because we tried to find out if a bedrock subsidence at the central parts of Greenland could be the result of a SMB increase in the Holocene. One possible period of SMB increase could be the LIA, and the resulting lag for the bedrock response indicates that at PD the bedrock could be reacting to changes in ice thickness that occurred during the LIA. The next reference to the Little Ice age (LIA) would be add in the Introduction:

Introduction. p3458.l17 Changed to: ... bedrock response. In order to find out if bedrock subsidence at the central parts of Greenland could be the result of a SMB increase during the Holocene, for example due to the Little Ice Age (LIA), we carried out an experiment that schematically mimics climate fluctuations during the past millennium following a sine function, and describes a quasi steady-state behavior.

Conclusion p3466.l22 Changed to: In reality, temperature variations are more complex than the sinusoidal function used in the experiment. Nevertheless, if we translate this value into a real situation, we can obtain agreeable results, implying that nowadays the bedrock is still reacting to changes in ice thickness that happen 200 years ago. This is compatible with the LIA that occurred between the years of 1,400 and 1,900.

RC: p3467.l1 My guess would be that the time lag is largely determined by the forcing period rather than by the physical parameters of the system. Explain what happens to your lag time when you change the forcing period and why.

AC: The relaxation time is the main physical parameter of the model that controls the delay of the bedrock response to ice thickness changes. Nevertheless, variations in this delay are largely determined by the forcing period. If changes in temperature with the same intensity occur during a longer period of time, the same happens with changes in ice thickness, and variations occur slower. As a result, the bedrock reacts slower as well, and the time lag increases.

RC: p3467.l17 'ice changes take place' should be 'ice changes have taken place' Despite the fact that the authors seem to be aware of the fact that uplift and subsidence are lagging ice loading changes, the wording is in most cases not precise and suggests a direct and linear response of the system. This should be corrected in the entire document.

AC: Changed

RC: p3467.l18 This statement is not generally true. It depends on what time scales one is looking at. It is well possible to assume a long-term thickening of the central part of Greenland of low magnitude which leads to subsidence, while high amplitude marginal variability of zero long-term mean would not induce strong bedrock changes at the margins.

AC: We are looking at the same time scale for changes in the central part as well as along the margins. In Greenland, there is a high amplitude variability of non-zero long-term that should be reflected on the bedrock elevation. We therefore changed the text to:

Assuming that the entire ice sheet has undergone a spatially uniform climatic history, any realistic mass balance forcing will lead to a stronger response along the margin than in the center as ice thickness changes are larger near the margin.

RC: p3468.l15 It seems that in this model the subsidence in the centre is an ongoing response to central accumulation increase. Which is logical since it is in this area where the ice thickens. The focus on mean rates of bedrock adjustment obscures this result, but it looks to me that a slightly different SMB forcing could result in a pattern shown by Wu. In the following you continue with the simplified model, which does not show that signature. This is quite surprising.

AC: We do not think that a slightly different SMB would result in the pattern presented by Wu, since we performed different experiments with different SMB, as the SMB changes with time, and with variations in temperature. For example, the SMB used in the 'Holocene and last deglatiation, ice core data' experiment is different at the beginning and at the end of the simulation (Figure 1 of this document). The issue is that if accumulation increases the ice sheet starts to

adjust dynamically in such a way that the excess accumulation flows to the lower regions and the largest thickness changes therefore occur near the margins not in the central parts.

RC: p3469.l4 You are also using a constant lapse rate (gamma) in your conversion. What is the difference? Clarify.

AC: Changes in SMB occur as a function of temperature, translated to a change in elevation using a lapse rate indeed, but surface elevation is a function of time and space, and varies with temperature, and bedrock elevation changes. As a consequence, SMB variations are not only the result of a constant lapse rate as is the case in the PDD method. This is clearly explained in the SMB description, for more information the reference to Helsen 2012 will be added.

RC: p3469.l15 Again 'lag of the bedrock response' to what?

AC: to ice thickness changes

RC: p3469.l16 'This implies that for the PD conditions, after 10 kyr of deglaciation, the bedrock is adjusted to the ice load reduction and an average bedrock uplift is present in Greenland.' If bedrock is uplifting at this time it is clearly still responding to ice load reduction during deglaciation. What are you implying?

AC: Changed to: This implies that a bedrock subsidence caused by a delay on the bedrock response that is still reacting to a net past ice accumulation on the glacial period, is not possible for PD conditions (after 10,000 yrs of the end of the last glacial era). In fact, the bedrock is already adjusted to the ice load reduction and an average bedrock uplift is present in Greenland.

Language

RC: Although I could understand the manuscript, my impression is (I am not a native English speaker myself) that the language of the manuscript should be further improved. Tables and Figures RC: Table 1. Nice idea to add the little figures here. But

because the scale is not possible to read, it is impossible to compare against the two last ones. It would be better to (ask for permission to) reproduce them in your own colour scale.

RC: Figure 1. As mentioned in the comments above I do not see the need to reproduce this figure here. If the method is well explained and critically discussed in the text, I do not think it is necessary.

AC: Figure 1 will be removed.

RC: Figure 2. to 5. Figure labels and axis markers are a bit on the small side and difficult to read. A sans-serif font should be used to improve the legibility. It would help to have all figures on the same scale, possibly increasing the contour interval for the present day panels to show more detail if desired.

AC: Changed.

RC: Figure 2. (a) It could be useful to plot -Hb instead to make the proposed relationship to Hi more visible. On first view it looks like Hi is lagging Hb. Caption should read 'temperature variations *during* the Holocene'; 'We present the last cycles *where* a new *quasi*-steady-state is reached'; '... is at *its minimum* ...'

AC: Caption changed.

RC: Figure 3. (a) Should mention and explain where the variability comes from in this experiment.

AC: Changed.

RC: Figure 4. Caption should read '*For the* selected time points' **AC: Changed.**

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Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/5/C2128/2012/tcd-5-C2128-2012-supplement.pdf

Interactive comment on The Cryosphere Discuss., 5, 3455, 2011.

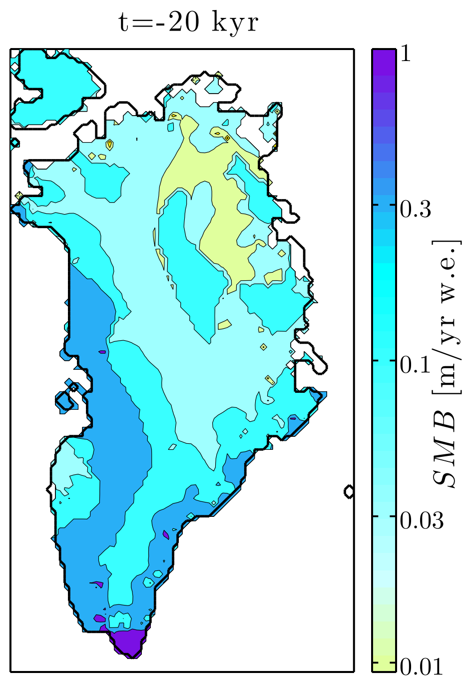

Fig. 1. SMB field corresponding to the experiment {\bf Holocene and last deglaciation, ice core data}. Initial SMB corresponding to t =-20000 yrs (obtained from a steady-state simulation for a temperature

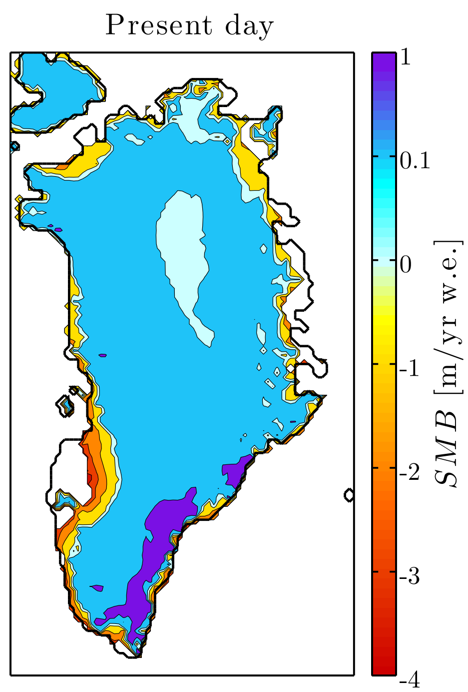

Fig. 2. SMB field corresponding to the experiment {\bf Holocene and last deglaciation, ice core data} SMB for PD (t=0 yrs).

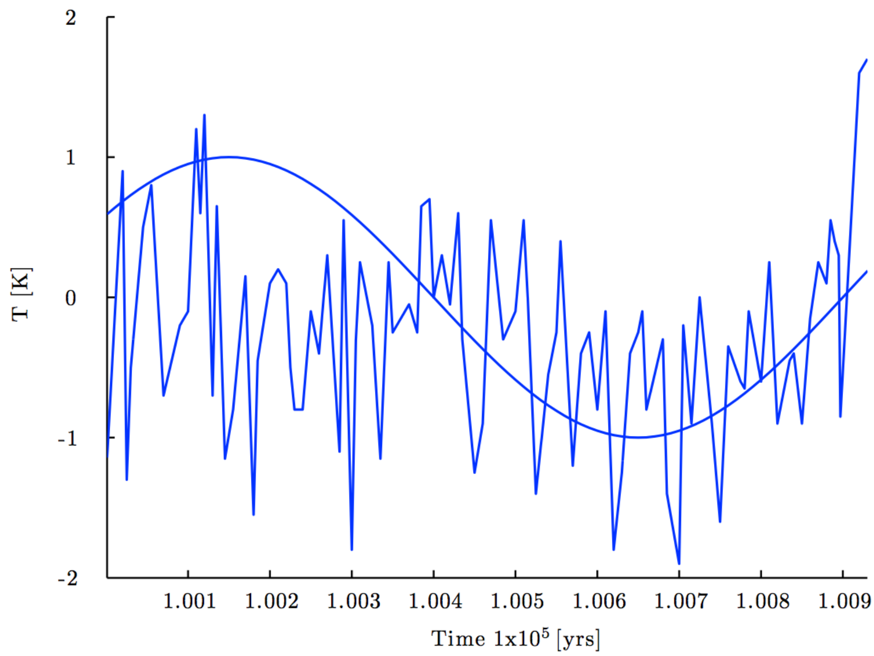

Fig. 3. Sine forcing temperature used in the idealized last millennium experiment and the temperature variations from Kobashi et al. (2009).

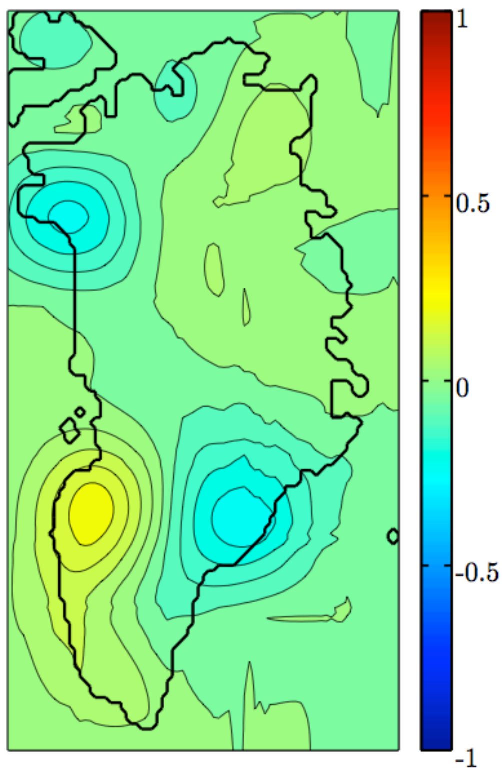

Fig. 4. Bedrock elevation changes in mm/yr for a temperature forcing from Kobashi et al. (2009) at t=1.0038

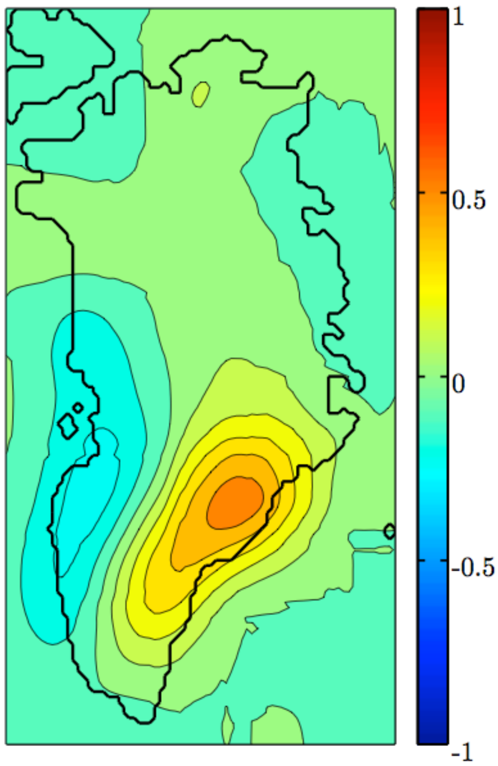

Fig. 5. Bedrock elevation changes in mm/yr for a temperature forcing from Kobashi et al. (2009) at t=1.007.