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

Anonymous Referee #2

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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.

General comments:

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.

Detailed comments:

Abstract ———

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.

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.

p3456.l22 Replace "Although," by "However,"

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.

Main text ———

p3457.l15 "... several studies have been published showing an increased loss in MB" Replace by "... several studies have been published showing an increased mass loss"

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.

p3458.l7 "an average uplift of the bedrock response," Should be "an average uplift of the bedrock,"

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."

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.

TCD

5, C2110–C2118, 2012

Interactive Comment

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.

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.

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.

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.

p3459.l16 I don't 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.

p3459.l21 Clarify the difference or relation between Hc and the equilibrium line.

p3460.l5 "The temperature field is solved by ..." should be "The ice temperature evolution is calculated by ..."

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 refer-

TCD

5, C2110–C2118, 2012

ence is available. This could also go into an appendix, but in its present form, the description is not sufficient.

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.

p3462.l1 Do you mean "mass balance variations" or rather "surface mass balance variations"? Clarify.

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.

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.

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

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.

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 spinup experiment and analyses the 3000 year experiment as such. Time indications like in line 26 would be easier to read in this framework.

BTW, I don't 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.

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.

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.

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

p3463.l9 "When the temperature increases to positive values ..." Is it absolute temperature or temperature forcing that turns positive? Clarify.

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

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?

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.

TCD

5, C2110–C2118, 2012

Interactive Comment

p3464.l6 "Finally, the temperature oscillates around PD as ..." should be "Finally, the temperature oscillates around its PD value as ..."

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.

p3464.l12 Remove repeated sentence "This is a consequence of more ablation"

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.

p3465.l5-10 I don't understand 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.

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

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

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

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

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.

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.

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.

p3468.l15 It seems that in this model the subsidence in the centre is an ongoing response to central accumulation increase. 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.

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

p3469.l5 "The SMB model formulation has an influence on the results" Obvious conclusion, but seems to be in conflict with p3462.l25. Clarify!

p3469.l15 Again "lag of the bedrock response" to what?

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?

Language ———

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 ——————

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.

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

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.

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* ..."

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

Figure 4. Caption should read "*For the* selected time points"

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