**The Cryosphere
Discussions**

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Interactive comment on "The effect of the north-east ice stream on the Greenland ice sheet in changing climates" *by* R. Greve and S. Otsu

Anonymous Referee #1

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This paper is fairly well written, well organized, and it is easy to follow the progression of the model experiments done. However, the motivation for pursuing some model experiments at the expense of others (some of which seem obvious and in some cases necessary), seems to be lacking. For example, it is unclear how or if the two main topics investigated here - evolution of NEGIS and its impact on future mass balance of the GIS, impact of melt-induced sliding - are related to one another. Both have something to do with how the mass balance of the GIS might change in the future, but to focus on parameterizing the effects of the NEGIS while ignoring the fact that the major outlet glaciers are not adequately accounted for seems very problematic. At the minimum, it is not justified by the authors. By all recent observations, the outlet glaciers are where big changes are taking place and it seems necessary to at least also try and parame-

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terize their effect on the ice sheet's discharge. Further, and maybe more fundamentally, the primary processes that are explored in the model (sliding in the area of the NEGIS and melt-induced sliding as a result of climate warming) are implemented in such a way that I am not convinced that the main conclusions of the paper are supported by the modelling conducted. Overall, I think this work may eventually lead to a very positive contribution to The Cryosphere. But as it currently stands, I think there are too many unanswered questions and simplifications to view the results presented here as anything more than speculation. My recommendation is that the paper eventually be accepted but only after it undergoes major revisions.

GENERAL COMMENTS

It is unclear to me why the focus here (concerning the effect of ice streams and/or outlet glaciers on the future evolution of the GIS) is on the NEGIS. The authors have more-or-less neglected trying to get the flux of the outlet glaciers correct during their model-observation minimization exercise. Yet, the outlet glaciers are the areas in the GIS that need a better understanding at present (where lots of big changes - acceleration and thinning - are taking place) and it is certainly clear that they have a major impact on the ice sheet's mass balance. It seems that, without have the outlet glaciers adequately parameterized in the model, it will be difficult to confidently say anything about how the NEGIS will affect the mass balance of the ice sheet in the future.

Similarly, the authors note that the largest disagreements between the model ice thickness and the observed thickness are in the areas of the NE and the SW. The former is treated by allowing for the affects of the NEGIS (faster sliding) but the authors fail to comment on the obvious question that follows - what is necessary to 'fix' the misfit in the SW? An obvious first guess is that flux through the outlet glaciers there is under represented. It certainly is through JI (according to thickness misfit in Figure 4, 8), so why not discuss what must be done to improve the misfit there as well? A similar adjustment to the sliding prefactor would probably work. A similar argument applies to the locations near the Kangerdlugssuaq and Helheim glaciers on the east coast. The

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model obviously over predicts the thickness there, so why not try to reduce the misfit in that region as well by increasing the rate of sliding? We expect that these glaciers are going to be important to the ice sheet thickness based on their large (observed) fluxes, so why not at least try to get the initial starting geometry of the ice sheet right by accounting for that? Taking this into account could alter what you need to do in the area of the NEGIS to improve the misfit there. If there is a good reason or technical argument (e.g. grid resolution is too poor?) for why the tuning exercise is not applied to the outlet glaciers as well, it should be noted in the paper. As it stands, there is an obvious hole where this discussion should take place.

The conclusion that, because the authors have calibrated the effect of the NEGIS for the ice sheet at present, they have a good handle on how it will affect the ice sheet in the future seems very misleading to me. This is one of two technical or model implementation issues that I disagree with. In the model, the area occupied by the NEGIS is fixed spatially according to an estimate for its present-day lateral extent and the tuned, sliding prefactor is held steady over time. How then is the NEGIS supposed to change or evolve realistically over time? It can't migrate spatially because the area of fast sliding is fixed (as far as I can tell) and it can't speed up or slow down (independent of the geometry) because the sliding prefactor is fixed. In this sense, it is not surprising at all that the future mass balance of the ice sheet is not significantly affected by the NEGIS. It cannot really change under the constraints placed on it in the model. There are no horizontal stresses present in the model and the basal boundary condition does not evolve, so the only way it can evolve is through changes in the ice sheet geometry. For something like an ice stream (and possibly even an outlet glacier), I would argue that all evidence points to evolution of the basal boundary condition as being the most important thing w.r.t. changes in flux over time. Dealing with this in a predictive model is cutting edge stuff (can/does anyone do it reasonably well at this scale yet?) and so it is not surprising that it is not accounted for here. However, the whole paper (title included) is then somewhat misleading as this is billed as the main topic.

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I don't know how novel it is, but it is interesting to me to see that the authors use a spatially variable geothermal flux. I think some obviously missing discussion is noting how that effects the initial configuration of the ice sheet relative to the case where a constant geothermal flux is used (i.e. are some areas thinner/thicker, flowing faster/slower?). It would be nice to see some quantification for how important this might be is. Further, w.r.t. the discussion about the misfit between the initial model configuration and the observations, one could also argue that the general trend of the thickness misfit is too thick in the south, not thick enough in the north. That, in turn, might suggest some dependence on the choice of geothermal flux - approx. 4x greater in the northern part of the ice sheet than in the southern part - which would affect the rate of ice discharge. I think that Figures 4 and 10 may show some support for this as well (see more detailed comment below for p.50, lines 10-19).

My second major technical / model implementation concern is the way the link between meltwater and sliding has been treated. First, I would argue that Parizek and Alley (2004) isn't the best place to start here. I think there are plenty who would agree that assuming that the rate of sliding will increase (continually?) as the amount of melt increases is a vast (and dangerous) over simplification of how subglacial hydrology really works. While we don't know all the details for how subglacial hydrology works, this assumption seems to run counter to what we do know. For example, we know from observations on valley glaciers (temperate and polythermal) that sliding speed generally does increase early in the melt season as meltwater from the surface enters the basal water system. This is, presumably due to the existence of a distributed drainage system in which the basal water pressures (and sliding speed) increase for a period of time early in the melt season. Later in the melt season, however, as the flux of meltwater continues to increase, the basal water system generally adjusts by becoming channelized. At this point, even though the flux of meltwater into the basal plumbing may increase still further, sliding speeds are observed to decrease. This aspect of the meltwater-sliding relationship is simply ignored by Parizek and Alley (2004) and it is ignored here as well. Second, there needs to be more justification for why the authors

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decide to test (and report on) gamma = 1.0 and 5.0 a/m in their melt-sliding relation when their analysis, based on the only available observations (Zwally et al., 2002), suggests that a value of 0.1 is justified (see further, detailed discussion of this topic below in 'appendix'). This is particularly important because the appendix is referenced in the main text to suggest that this justification is presented, when in fact, there is almost no justification at all given (the same argument they make could be used to say that the value of gamma should be smaller). My own attempt at justifying it results in some fairly alarming implications w.r.t. the observations that the value of gamma is supposed to be based on (again, see further discussion below). Unfortunately, when the reader looks into the details of this, he/she walks away with the feeling that the additional values quoted for gamma have simply been tweaked by the authors to get a more exciting result. This may not be the case, but if there is some justification for choosing a larger value of gamma, it needs to be clearly stated.

SPECIFIC COMMENTS

Abstract

(p.42, 2-4) Information about how NEGIS was discovered is not relevant here. Should be in intro material (and it is).

(p.42, 5) 'dynamic/thermodynamic' ... how about 'thermomechanical'?, which is slightly shorter?

(p.42, 5-6) Acronym and definition of SICOPOLIS should go in intro. Here, it is adequate to just say '...a large-scale ice sheet model'.

(p.42, 8) Are 'present-day' observations ice core records? If so, this seems like a funny way to characterize them. They are really paleoclimate records. Also, 'GCM' is not defined ... perhaps not necessary? Does TC have a policy on the use of acronyms like this?

(p.42, 10) 'normal slowly-flowling areas of the ice sheet'. What does this mean, internal

deformation only? Certainly the outlet glaciers, which are also very important to the ice sheet's mass balance at present, are not slow flowing.

(p.42, 12-15) The 'consequences' (?) of the NEGIS and melt-induced sliding are two very distinct issues that don't seem to be directly related. I'm not sure they should be mentioned in the same sentence (and it is somewhat unclear why they are considered together in the same study). At least make it clear that they are two separate issues by separating them in the abstract.

(p.42, 15-end) I'm not sure either of these claims are fully supported by the modelling, as discussed above (and in the appendix discussion).

Introduction

(p.43, 1-2) Is this a reference to support this statement? Perhaps it is obvious?

(p.43, 3-12) Again, curious why only NEGIS and JI are discussed. We know of many other large, important outlet glaciers in Greenland that have also accelerated dramatically in recent years. Is JI considered an 'ice stream' vs. an outlet glacier? What is the distinction here?

(p.43, 24-26) The topic of 'surface-meltwater induced speed-up of basal sliding' doesn't seem related to the topic of how NEGIS will affect the evolution of the ice sheet. Are they related?

Paleoclimatic simulations

(p.45, 21-26) suggest 'A map of spatially variable geothermal heat flux beneath the Greenland ice sheet was constructed using the spherical-harmonic representation (to degree and order 12 ***is this info important here? If not, suggest it is omitted***) of the global heat flux by Pollack et al. (1993). Optimum values of the heat flux at the four ... and Dye 3 were obtained by matching simulated and observed basal temperatures and by interpolating ...' I'm a little unclear on the meaning of the last part - are the authors simply saying that they interpolated the tuned geo. flux map onto the model grid? Were

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the spherical-harmonic coefficients tuned to match the estimated and simulated basal temperatures noted?

(p.46) Note that 'equation (1)' is not really an equation. Is this standard TC format? I would call this a table, not an equation.

(p.46, after line 5) It would be interesting to note somewhere (maybe not here, but somewhere in discussion) how the use of a spatial geo. flux affects the model results relative to using a constant value (what is normally done).

(p.46, equation (2)) The negative sign is confusing here. Is basal drag defined to be in the direction opposite of the driving stress? Does it have something to do with the exponential part of the expression?

(p.46, 10-13) It would be better to say something like 'Pb=rho*g*H is the ice overburden pressure, where rho is x, g is y, and H is z'. Also, where are the values for C, p, and q taken from? A ref. to a prev. Greve paper would be adequate.

(p.47, 8) The dependence of the thickness misfit on the geothermal flux could be rather easily tested by just replacing the spatially variable flux with a constant value. This seems like an important test to do, even before tweaking the sliding prefactor in order to simulate increased drawdown of the ice sheet due to ice streams/outlet glaciers. There seems to be some correlation between the pattern of geo. flux and the pattern of thickness misfit.

(p.47, 8-10) Why do the authors only discuss the importance of the NEGIS w.r.t. correcting the misfit using ice dynamics? Obviously, if increasing the rate of sliding beneath the area of the NEGIS improves the misfit, then doing similar for the area beneath the JI should help to improve the misfit there. For that matter, there is a large overestimation of thickness near Helheim and Kangerd. Glaciers on the east coast - why not try and correct this as well? If there is an argument for why the misfit correcting process should not also account for the importance of outlet glaciers (which, combined

are probably more important to the mass balance of the ice sheet than the NEGIS) then it should be stated here.

(p.48, 3-6) '...the location of the NEGIS has been identified according to information about its length and width (Fahnestock ...) and by using the map of balance velocities from Bamber et al. ( ), in which the NEGIS and its margins are well identifiable (Fig. 1)." But these are balance velocities, not 'real' velocities. Is the data from Joughin not adequate for this purpose? It looks like it is based on Figure 9. Can you note why you choose not to use the real velocities to outline the area of the ice stream? Is there a reference maybe that supports that the balance and measured velocity fields are very similar?

(p.48, 17-20) ... but it is pretty well established by now that real values for p should probably be >=10 (i.e. any underlying till is probably closer rheologically to plastic than to linear viscous). Admittedly, using p=1 has some historical precedent, but at least be honest about that and note that p=1 probably does not give results representative for a real till.

(p.49, 16) 'So we conclude that the linear sliding law ... must be discarded'. Why is this important? It is not clear to the reader if we learned something useful here. Is the implication that the ice stream is not underlain by a linear-viscous till?

(p.49, 19-20) 'In other words ...'. I think this statement is not entirely supported by the modelling. First, the sliding law is just a guess and certainly represents a simplification for how 'sliding' actually takes place within the ice sheet. Second, without including higher-order physics in the model (e.g. horizontal stresses), it seems doubtful that the model can really do a very good job of approximating ice flow in fast flowing regions like the ice streams and outlet glaciers, where we know that horizontal stresses are important. Third, while reducing the mean of the misfit for the velocity data, the exercise discussed has done almost nothing to improve the RMS of the velocity misfit; the model is still, in general, substantially over and under-representing the velocities in the NEGIS

area. Thus, the velocity data are not really fit that well in the model. The thickness misfit has improved because, in general, ice in the NEGIS area is now going faster than it was in the trial run, where the NEGIS was neglected. This is, however, a fairly obvious result. From figure 9, it looks like the increase in velocity in the region of the NEGIS has occurred over a much wider area than is shown by the actual, measured velocity field (the observed ice stream is a relatively narrow, faster feature, relative to what looks to be two relatively wider, and slower flowing, outlet glaciers in the model).

(p.49, 23-26) It should be clarified here that the authors are only talking about the misfit in the NEGIS region. Again, I'm confused why the authors didn't try to improve the misfit in the regions near JI, Helhiem, Kangerd., etc. Surely the geometry of the initial ice sheet, which is incorrect because of this neglected tuning, could affect the results for later model runs. Particularly because the only way the NEGIS might be expected to change w/ climate change is through the ice sheet geometry (there is no way for the basal boundary conditions to change spatially or in time).

(p.49, last sentence) This seems like a rather odd and/or obvious conclusion. Did the authors expect that by accounting for enhanced sliding in the NEGIS region ice flow in other portions of the ice sheet would be affected? There are no horizontal stresses here, no way for the basal bc to evolve or grow spatially over time, so changes in geometry are the only way that the other parts of the ice sheet could be effected. Obviously the ice sheet geometry is going to be affected locally, where sliding in the NEGIS region has been enhanced, but in a shallow ice model, this will have limited importance. I'm not sure what the point is here.

(p.50, 5-9) I'm glad to see this discussed and I agree. One consequence of the ice stream being less localized is that, in order to minimize the thickness misfit, you have increased the velocity over a larger area of the ice sheet but with a smaller magnitude than occurs in real life (that is, the ice stream is narrower, longer, and faster in real life than in the model). So, to some extent, it remains questionable how well the ice stream is actually captured by the model tuning exercise.

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(p.50, 10-19) The pattern of frozen vs. thawed bed seems to coincide somewhat with the pattern of over/under thickness prediction shown in Figure 4 (overpredicted ice thickness where the bed is cold, underpredicted where it is warm). Again, it seems like it would be important to check the importance of this by using a constant geo. flux in at least one model run.

Global warming simulations

(p.51, 5-9) Are these sea-level temperatures that are then related to temperature on the ice sheet surface through a lapse rate?

(p.51, 21-22) In order to make this conclusion, there should probably be some more model runs in which something other than the mean temperature and/or precipitation increase per degree C is used. For example, use the high estimate for temperature, the low for precip., and vice versa, as well as the mean values. Does this statement still hold? As it stands this statement doesn't really take into account the uncertainty in the sensitivities that are clearly noted in the previous paragraph. To me, this test seems somewhat more interesting than what is discussed as the main thrust of the paper (i.e. the influence of the NEGIS, which as parameterized here, can't really do much to change its influence on the ice sheet anyway).

(p.51, 27-28) Again, this is largely due to the fact that the thing that controls the fast flow in this area, the value and the location of the sliding prefactor, is held constant and steady over time. I don't think this conclusion is really supported because you aren't actually allowing basal sliding to evolve at all over time, which would be the case in real life.

(p.52, 7) 'provoking a fast reaction ... on increased surface temperatures.' is a poor choice of words here. It would be better to be more specific and say exactly what you mean by describing the link that you imagine exists (or that Zwally et al. discuss).

(p.52, 20-26, and in general) See comments above w.r.t. link between sliding and melt

and specific comments below (in appendix) about using a larger value of gamma.

(p.53) I have a hard time seeing the discussion on this page as anything more then speculation. There is no real basis given (here or in the appendix) for choosing the large values of gamma (1 and 5 a/m) as far as I can see, in which case we are not really looking at someone's best guess for what might happen so much as how the ice sheet might decay rapidly if there was a really really strong feedback between increases in temperature and increases in sliding. Because there is no real justification given for applying this very strong feedback, I don't see how these results can be justified as predictions.

Conclusions

(p.54, 6-7) The large RMS in the velocity misfit still suggests that the model velocity field of the NEGIS is not right, and I think this is also shown by Figure 9.

(p.54, 9-10) As noted above, I think the 'conclusion' that sliding within the NEGIS is 3x that in the rest of the ice sheet is a vast oversimplification and not wholly supported by the modeling conducted here.

(p.54, 16-17) Again, the two larger values of gamma used in the melt-sliding relation is not justified adequately.

(p.54, 19-end) '...unless it behaves in an unexpected way by dramatically increasing its area or ...' Again, my concern that the evolution of the NEGIS is not adequately allowed for here makes these assertions suspect. According to the way the NEGIS is implemented in the model, it is in fact, not allowed to increase in size or speed up significantly (as far as I can tell anyway). This doesn't tell us anything about how the ice stream might actually evolve over time.

Appendix

The authors have chosen the largest values for the integrated PDD factor and acceleration in the Zwally et al. (2002) data. It seems like it would be more honest to take

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some kind of mean of the values presented in that data, which span four years.

Following is my attempt at justifying the use of gamma = 1.0 or 5.0, and the implications that come from it, w.r.t. the field observations that the initial estimate for gamma is based on. If one assumes that gamma is as large as 1.0 and/or 5.0 a/m, and that other variables in Equations A7 remain fixed, one is forced to assume that the 'physical' variable changing to give a larger gamma is the value of delta Vb, the velocity acceleration, that results from seasonal meltwater input. Extending that argument back to the Zwally et al. (2002) data, one comes up with the following results (the velocities that would have to be measured at Swiss Camp to support the particular choice of gamma):

For gamma = 1.0 a/m, the mean annual velocity at Swiss Camp increases seasonally from ~117 m/a to 437 m/a (delta Vb of 0.88 m/day instead of 0.088 m/day); a seasonal increase by nearly a factor of 4.

For gamma = 5.0 a/m, the mean annual velocity at Swiss Camp increases seasonally from ~117 m/a to 1720 m/a (delta Vb of 4.4 m/day instead of 0.088 m/day); a seasonal increase by nearly a factor of 15.

I would argue that these values are simply too large to support being used without some additional justification. Should something else in the equation be changed instead? For example, should a smaller value of M and/or C be used? Simply saying that the value of gamma is 'not representative for the entire ice sheet over longer time-scales' provides no justification at all. It just ends up looking like the value of gamma is tweaked to get an interesting result from the model.

TECHNICAL COMMENTS

(p.43, 7) remove 'by contrast'

(p.43, 11-12) suggest 'The NEGIS branches into ... where flow velocities are up to 1.2 km/a'.

(p.43, 16) suggest '... reference simulation that excludes the NEGIS is described in ...'

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(p.43, 18) suggest omit 'based on data'

(p.43, 21) Define 'CE' and 'WRE'

(p.43, 22-23) Info in parentheses could be omitted here, as it is a detail that is discussed later on.

(p.44, 3) suggest '... of ice sheets' $->$ '...of an ice sheet'

(p.44, 8-9) suggest omit 'which follows Paterson's recommendation'

(p.44, 9) suggest 'A particular feature ...' $->$ 'A unique feature ...'

(p.44, 11) suggest 'adequate' $->$ 'realistic'

(p.44, 14) Does 'Tau_iso' need to be included in this sentence?

(p.44, 16-17) suggest '(iii) the global sea level ...' $->$ '(iii) global sea level, which defines the land ... glaciation, and ...'

(p.44, 18-20) suggest 'All computations are carried out on a 10-km, horizontally-spaced, Cartesian grid, in a [polar?] sterographic projection with standard parallel ...'

(p.44, 25) Include a reference to 'sigma coordinates'?

(p.45, last sentence of prev. paragraph) suggest 'Standard values for the relevant physical parameters are used for the simulations (Table 1)'.

(p.45, 6) suggest Omit 'doubled'

(p.45, 7) suggest 'is that of ...' $->$ 'is the same as "hf_pmod2" described by ...'

(p.45, 10) suggest omit 'results of'

(p.45, 16) Define LGM and when it is (i.e. it is generally not assumed to occur simultaneously everywhere on the globe).

(p.45, 20) suggest '...to global sea level (see Greve (2005) for additional information).'

(p.45 to top of p.46) suggest 'This leads to the following values of heat flux at the ice-core sites ...'

(p.46, 6-8) suggest 'Basal sliding is based on a ... law in the form given by Greve (1998, 2005), modified to allow for sliding at values just under the melt temperature (as discussed further by Hindmarsh ...)'.

(p.47, 12) suggest '(observed values from Bamber et al., 2001; Joughin et al., 2001)'

(p.48, 6-7) suggest 'A mask file ...' $->$ 'This information was used to identify which points from the model grid correspond to the area of the NEGIS'.

(p.49, 7) suggest '... are far less convincing'. Are far less 'satisfactory'? Convincing is the wrong word to use here.

(p.50, 22) 'fate' is a bad word choice here. How about 'response'?

(p.50, 23) Again, define WRE

(p.51, 1) AOGCM = ?

(p.51, 3) suggest '1.3 ... 3.1' $->$ '1.3-3.1'

(p.51, 5) similarly, suggest '2.7-7.8%'

(p.51, 10) suggest 'In the following ... will be considered. The model is run from 1900 until 2500 CE'.

(p.51, 19) suggest 'evolution of the ice volume that result ...'

(p.51, 28) 'becomes also' should be 'also becomes'

Table 1: 'Standard physical parameters used in the model'. Rather than discussing when the ice was deposited, does it make more sense to refer to its 'age'? For the given values of E, should be presented as '1, 3*' rather than '1 / 3*' (latter reads like a

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fraction).

Figure 1: How about just saying that 'brown areas are ice free'. Also, authors should note that NEGIS and JI are labelled, rather than saying that they are 'clearly visible'. Define 'a.s.l.', here and in other similar figures.

Figure 2: 'History of glacial index, g ...' or 'Time series of glacial index, g ...'

Figure 3: 'equation (1)' should probably be presented as a short table of values and referred to as such.

Figure 12, 13, 14: In all of these figures, 'NEGIS / m=3, WREXYZ' should probably be written as 'NEGIS, m=3, WREXYZ'

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