

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

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General Comments

In this paper the authors present and apply a numerical ice approximation to A) modeling changes in the flow speed of the NEGIS due to mass-balance perturbations and B) the Greenland Ice Sheet due to Mass Balance perturbations and enhanced basal lubrication from increased meltwater input.

The logic in the methodology and progression of experiments are clearly presented (aside from the typos) and easy to follow. Beyond that, I have little positive to say about the paper. My problems fall within the following three general themes, upon which I elaborate below:

1. Why are we still applying coarse-resolution, “shallow ice” models to

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ice streams and margins?

2. Why do we only care about the NEGIS?

3. Do we expect that the entire ice sheet will experience the “Zwally Effect”?

Since my problems rest at the very foundation of this study, I have a hard time envisioning any practical revision that would render it worthy of publication. I think 10 years ago, before Huybrechts [e.g. Huybrechts, 2002; Huybrechts et al., 2002] and EISMINT [e.g. Payne et al., 2000], this paper would have been good for giving us a 1st order constraint on ice sheet response and showing us where we needed improvement. But that’s been done, investigators like F. Pattyn [e.g. Pattyn, 2002; 2003] and A. Payne [e.g. Payne et al., 2004] have given us better models, and the recent changes of these Ice Sheets have shown us how important these models should be [e.g. Rignot and Kanagaratnam, 2006; Thomas et al., 2004]. This paper offers little in the way of advancement within the field, one horrendous assumption (point 3 below) and, therefore, little probability of citation. I recommend rejection.

Specific Comments

1. Why are we still applying coarse-resolution, “shallow ice” models to ice streams and margins?

We have known for some time that using a shallow ice approximation to model ice streams is a bit like removing screws with hammer – with enough brute force you can just about get it to work, but don’t expect to use the screw again. With a highly non-linear sliding law and a pre-determined region of flow enhancement you can get the ice to flow faster where it’s supposed to, which may be OK for simulating quasi-steady state behavior, but there’s no way that these empirical fits will stay the same under a substantial change in boundary conditions [see Payne et al., 2004; Vieli and Payne, 2005]. Even in the steady state case, the model present

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in the paper fails to reproduce observations for obvious reasons. It would be expected that the regions of fast flow, where the driving stress exceeds the basal drag, would be thicker in a shallow ice model than reality.

For the transient case, we actually need to get the physics right, not just the current map. The variations in the position and speed of the Siple Coast ice streams [Siegent et al., 2004] are a case in point. If we were to delineate of enhanced flow based on the current velocity field and then run the model backward we would not recreate these variations. Similarly, I don't think it's justifiable to simply delineate a region of enhanced flow around the NEGIS, perturb the mass balance and/or basal drag, and expect for this area to stay the same. According to non-shallow ice models like those of Payne et al. [2004] this is probably not the case. With a plastic bed, which is suggested by your best sliding fit, the region of $m=3$ will likely expand under acceleration as stress/speed gradients increase to makeup for our weak bed (we're actually seeing this in action at Jakobshavn (see Joughin et al. [2004]) and the big glaciers on the east coast [Howat et al., 2007]. Even more troubling, there's no discussion of this possible weakness of the model. And then there's the problem with spatial resolution. With a 10km grid (and shallow ice) we are completely ignoring all of the outlet glaciers around the coast, including the ones that have recently doubled their rate of discharge [Howat et al., 2007; Rignot and Kanagaratnam, 2006](By the way, the IPCC would disagree with your reference to the ice sheet's mass-balance as being \approxslightly negative$$;, as would R. Thomas, whose older work you cite. Please read his more recent assessment of mass balance in Thomas et al. [2006]). Granted, it may not yet be realistic to run models at high enough spatial resolution to capture the dynamics of these glaciers (also you would have to include the \approxhigher-order$$ terms), but there needs to be some discussion of this shortcoming. But, again, do we really need another shallow ice model paper where the conclusion is that we predict some moderate amount of mass loss over the next n centuries, but this estimate does not include the changing dynamics of outlet glaciers that have doubled the ice sheet's rate of mass loss in the past 10 years?</p></div><div data-bbox="825 697 934 719" data-label="Text">Full Screen / Esc</div><div data-bbox="808 754 950 777" data-label="Text">Printer-friendly Version</div><div data-bbox="810 811 947 833" data-label="Text">Interactive Discussion</div><div data-bbox="823 868 936 890" data-label="Text">Discussion Paper</div><div data-bbox="364 934 399 959" data-label="Page-Footer">S69</div><div data-bbox="854 931 904 958" data-label="Page-Footer">EGU</div></html>

2. Why do we only care about the NEGIS?

This question relates to the last part of my 1st question, but, more specifically, why do the authors only treat the transient behavior of the NEGIS? Couldn't, and shouldn't, the same attention be put into the other regions of fast flow, like the Jakobsavn catchment? The authors may find that while one contribution isn't significant, the combined effect of many regions accelerating can translate into a substantial increase in mass-loss. Of course, as I stated above, I don't think this model is the right tool for the job anyways, so I guess it doesn't matter.

3. Do we expect that the entire ice sheet will experience the Zwally Effect?

I was astonished by the Authors applying a surface meltwater coefficient over the entire Ice Sheet. By doing this, the authors assume that water is penetrating to the bed everywhere there's melting. Many papers [e.g. Alley et al., 2005; van der Veen, 2007] show that penetration is highly unlikely through thick polar ice, such as what covers most of the interior and northern ice sheet. Certainly they don't really expect for water to be penetrating the ice sheet way up in the percolation zone, where there are no crevasses or moulins? I have a hard time even hypothetically extrapolating the measurements of [Zwally et al., 2002] at Swiss Camp (which is ~ at the ELA and relatively close to the margin) everywhere. This is precisely why [Parizek and Alley, 2004] adopted their approach. Therefore, I think this is a highly unrealistic experiment, using a model we know is wrong.

Technical Corrections The writing isn't very good, with lots of passive voice where active would be smoother, but it's passable. There are typos everywhere. Here are a few: Abstract, Line 2: imaginary; Page 43, Line 1: increase strongly; relative to what? Do you mean non-linear? (if so, your PDD function in the more is wrong). Page 48, Line 4: informations; Page 50, Line 21: What do you mean by effect; Page 51, Line 8: I

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know English is ridiculous when it comes the plural declensions, but should be either and precipitation is assumed; or and the precipitation rate is assumed; Page 54, Line 2: ice sheet was simulated;

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