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The Cryosphere
Discussions

Interactive comment on "Modelling seasonal meltwater forcing of the velocity of the Greenland Ice Sheet" *by* Conrad P. Koziol and Neil Arnold

Anonymous Referee #2

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This study couples together a surface drainage, ice flow and subglacial hydrology model to examine ice dynamics in the Russell Glacier catchment in Greenland. It's nice to see efforts to couple models like these together as, on their own, it is difficult to fully understand the system. Also, this is a good location to apply this type of study since I think more in situ data has been gathered here than anywhere else in Greenland. In particular, I thought the scenarios of increased melt input were helpful for assessing the dynamic impacts of basal hydrology. The authors argue that the primary output from their model is a confirmation of the hypothesis for an 'Alpine'-like drainage network at the margin of the Greenland Ice Sheet where an initial summer speed-up is followed by deceleration as efficient channel networks develop.

There was a thorough discussion in the paper about the different aspects of the model

and the tests that have been carried out. However, improvements can be made, particularly in the very strong support of the 'Alpine' drainage model, which is really only applicable to the very margin, while the model covers areas much further inland. Also I caution the authors against support of hypotheses applied to the whole of Greenland when Russell Glacier is a fairly slow-moving land-terminating glacier, unlike many of the regions where much of the mass loss is occurring by calving processes at tidewater glaciers. On the whole, I think this paper would be improved by toning down and clarifying some of the arguments as I detail below.

Major points:

The overwhelming argument in the paper is that water gets to the bed in the spring, grows channels and that causes ice deceleration. This is reported as the 'Alpine' model of drainage and, indeed, both the GPS data and the model outputs from this study support that, where the Greenland ice sheet is similar to an Alpine glacier (i.e. near the margin where lots of water gets to the bed and, most importantly, surface slopes are steeper), it does act similar to an Alpine system. However, when you go further inland, both the GPS data and the model in this paper support the argument that has been made in other papers (e.g. Meierbachtol et al, 2013, Andrews et al., 2014, Dow et al, 2015, none of which, by the way, were cited or discussed), that the shallow surface slope prevents large, efficient channels from forming and therefore more water input means higher velocity. This is not the Alpine drainage model. I strongly argue that the authors should recognize that their model outputs show this. The authors do discuss this inland acceleration within the main text but it is not mentioned in either the abstract or conclusion. This acceleration, particularly in the cases of increased melt input, is one of the more important outcomes of the modeling exercise and should therefore be highlighted.

Similarly, the role of channels in the drainage system is something that should be more carefully discussed by the authors. While subglacial channels can grow at high elevations under the Greenland Ice Sheet, it doesn't really matter until they start poaching water from the distributed system and therefore increase the system effective pressure i.e. presence of channels does not equal efficiency. Unless the surface slopes of the interior steepen considerable, there is no mechanism to efficiently remove water as the hydraulic gradient in channels will not be steep enough and therefore more water will mean faster velocities. So on page 21 where you report channels growing up to 50 km from the margin, that's fine, but these don't appear to be having much impact on the velocity, which is what you're interested in. That should be pointed out and the arguments that more channels inland will mean a velocity decrease, rephrased.

I think the authors need to less strongly argue that the model outputs fit the GPS velocity data well. For example on PG24 13-14 it is stated: 'many of the features observed in the GPS. . .are captured in the modeled velocities'. On many of the plots in Figures 7 and 8, the GPS velocity is slowing down while the model is speeding up (e.g. 2011 Site 4 day ∼203; 2011 site 5 day ∼195), and there are few places where I think the model and the GPS would have a good correlation on a day-to-day basis. This doesn't mean that the model outputs aren't useful and I'm not expecting the authors to do any more model runs. However, merely to be more careful with their language. For example, the authors could say: a reasonable fit between seasonal patterns from GPS velocities and modeled velocities.

Line-by-line comments:

PG1

1: can you specify what you mean by margin. How far inland does that go? This doesn't necessarily have to be in the abstract but should be noted somewhere in the main text.

1-2: The first line of the abstract is misleading since you then immediately say that meltwater also leads to deceleration.

PG2

8: where are these measurements?. . .accumulation or ablation zone? This is confusing following on from the previous sentence.

13: references needed at the end of this sentence e.g. Leeson et al, 2015

23: what do you mean by 'recent hydrological models'?

24: what feedback?

25: 'comparison to ice surface velocity measurements'

27: 'coupled ice dynamics/hydrology models'

29 (and 31): what do you mean by 'necessary elements'? In the context of modeling this is confusing since some of the models you discuss include finite element grids.

30: to drive the subglacial hydrology model?

31-31: repetition of 'different'

32: 'no model to date has included the full spectrum'. This suggests that you do include the full spectrum. From your description of the surface hydrology model it doesn't seem to me like there is anything different from the previous applications of this model. If you have adapted it, you should specify the main changes and why you think this represents the full spectrum.

PG3

1: 'This coupled model. . .'

18+20: repetition of 90 m resolution topography usage

31: can you specify which boundary conditions you are referring to here.

PG 5

3: why the assumption of -5 degrees C?

4: which Landsat8 image?

14: repetition of ‘in’

16: the first sentence is repetition of the 90m resolution for the third time

18: missing reference

20: define what you mean by a fracture area criterion. Also is this fast-drainage dataset driven by that fracture criterion checked against the satellite imagery record of lake drainages?

PG6

1: is that basal no flux condition accurate? Particularly on your upper boundary I would expect some basal water flux into your domain.

6: Ks here but K in Eq 1

10: Eq2 – what is h_cav?

11 (and 16): U_b is the basal sliding speed but u_b is the basal velocity? What is the difference?

16: how do you calculate basal drag? Also for u_b what is b in the (x,y,b) dimensions? You should also specify what v is.

PG7

3: what do you mean by incipient channel width?

5: what is x_c? If this is along-channel distance, what is the difference between this and r?

11-14: How did you choose the % of cells to change and the value to change them to? Did you sensitivity test this? For this whole paragraph you need to include units.

16: what is k?

PG9

6: Perhaps the removal of negative effective pressure is why you aren’t replicating the higher upstream GPS velocities.

PG10

3: unclear what you mean by an internal water table. References needed here.

PG11

18: why do you say ‘not shown’ for the sensitivity analysis? What about section 3.3?

10-12: I don’t understand the sentence beginning ‘Water routing…’. L10 say 70% routed into crevasses, L11 says 50%.

PG13

3-4: I find it confusing that the surface input is not varied. What do you mean by this?

5: which plots?

7: are you saying because sub-daily variability is subdued, you’ve averaged to daily? Otherwise this doesn’t make sense following the previous sentence.

9: which model output?

PG 14

3-5: I’m afraid it doesn’t look like a good fit to me at all. There’s a lot of variability in the GPS that aren’t captured in your model outputs and events in the model that doesn’t appear at all in the GPS. I think you have to have to be much more upfront about these poor fits.

PG16

3: how near the ice sheet margin?

4: low velocities through the summer melt season? Why is there no spring event at the beginning? This is a key of the 'Alpine' hydrology theory so it's worrying if the model doesn't produce that. If you are getting no spring event its possible your overwinter basal water pressures are too low. This is also possibly why your summer deceleration events are not below the winter mean velocity Overwinter the basal water pressure should be at overburden pretty much everywhere so any water input in the spring will cause a velocity increase. It would be good to include the overwinter steady state ice flux/basal water pressure diagrams, perhaps in supplementary material.

17-18: 'In 2009, site S6 shows a gradual. . .' do you mean the model or the data? Here and elsewhere be careful to specify.

33: Why qualitatively? A correlation should definitely be quantitative.

PG 17

7-8: 'In this section. . .' awkward sentence.

11: units! Please check the rest of the manuscript to make sure you include units for all of your numbers.

18: I don't understand this. Where does the water go? Also there should definitely be a velocity response (albeit a short one) to a lake drainage event. What is your model timestep by the way?

23: 50% of the initial nodes? What does that mean? Also why these % numbers?

30: how much was englacial storage increased and reduced?

33: day 2015?

PG18

1: I'm getting confused what the difference is between the calibration and the validation runs.

14: accelerating rate of speed-up is poor phrasing

15-16: modeled velocities decrease during periods of slower flow. The sentence is circular and doesn't make sense.

PG21

2: Why does figure 12 compare 2009 with 2011 x 4? Why not 2011 and 2011x4? From this figure it seems like melt increase has a significant impact on the velocity of most of your domain, just not at the very margin. This seems to be understated in your arguments, which are more focused on confirming a model that suggests more water = velocity decrease.

15: the fact that you have channels forming at higher elevations but still get speedup tells you a lot about the system that you haven't discussed. This feeds into one of my major comments about the presence of channels not necessarily facilitating efficiency. It depends how much water those channels remove from the distributed system and therefore the change in effective pressure.

PG25

10: a high aspect ratio of what?

16: cite Schoof et al (2014), The Cryosphere.

PG26

3: 'affected'

24: what is this constant sheet height scale? How does this link to the previous sentence?

27-32: these ideas have been around for a while so you need to reference this paragraph (e.g. Das et al, 2008, Doyle et al, 2013, Dow et al, 2015, Banwell et al, 2016). Also GPS data show that lake drainage have a large impact on ice velocities, just in the short term. What this means is that your model outputs are not valid for this particular lake hydrofracture example and therefore you have to be careful using them to make strong arguments.

PG 27

13: low aspect ratio and low sliding ratio. Ratios of what?

PG 28

18 (+PG30 6-7): This depends on what you class as the margin. This seems to only apply to within ∼40km of the terminus.

PG29

1-8: You're model doesn't support the assertion that channels further inland will cause slowdown. Even though you have more channels, there is still velocity increase at your upper site.

11-12: why would winter velocity decrease? This doesn't make sense if velocities are faster in the higher melt scenarios implying widespread distributed drainage. And what do you mean by offset?

Table 1:

-Ks is called sheet hydraulic conductivity in the text

-is K the hydraulic conductivity for the channel or the sheet? And do you mean m sˆ-1? 2m sˆ-1 is really fast!

- a critical layer depth of 1 m seems high given the basal bumps are 0.1 m high.

- what is the difference between Am and Sm?

Table 2:

- I don't think defining the number of seconds in a year is necessary.

Fig 1: caption needs a date for the satellite image

Fig 3: I don't really understand what this schematic is showing. It's not immediately obvious.

Fig 7/8: An indication of the elevation for each site would be useful here

Fig 14: It's really hard to tell the difference between the sheet and channel curves

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