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Interactive comment on "Influence of high-order mechanics on simulation of glacier response to climate change: insights from Haig Glacier, Canadian Rocky Mountains" *by* S. Adhikari and S. J. Marshall

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1 General Comments

First of all: **this was enjoyable reading**. The paper is short, largely consistent and to the point. Apart from minor issues, I basically found only four points I want to have clarification on (in the order of my subjective importance):

1. From the text I conclude that you use one and the same mesh for computing all

three approximations to the Stokes equation – if not so, indicate the differences (and ignore the rest of this paragraph). I see an issue with the SIA (=SD in your paper) approach here in that sense, that SIA is based on the assumption of flatness, which basically has to hold also for your discretization. By the nature of its approximation, SIA cannot resolve any bedrock feature of length below the given flow-height divided by the aspect-ratio. In other words, running SIA on unit aspect-ratio meshes is like driving a truck on a twisting and turning go-cart track: You might get along with it – or not completely in your case, as you significantly had to reduce the time-step size (i.e., drive the large truck very carefully) – but it is just not appropriate. Smooth your DEM and enlarge your horizontal mesh size and you will see that the SD solution will get way more stable and less patchy. In that connection, the typical resolution of your DEM would be a valuable information.

2. In contrary to the often applied surface relaxation pre-processing steps (i.e., short time prognostic runs allowing the errors in DEM to adjust) (e.g., Zwinger & Moore, 2009, Gillet-Chaulet et al., 2012), you seem to immediately start your diagnostic as well as prognostic results from the initial shape you obtain from the DEM. As pointed out in (Zwinger & Moore, 2009), flaws in the DEM (especially in Full Stokes) can lead to artificial local discrepancy in velocity field (in some case order of magnitude off the real value). Can you elaborate why this does not seem to be the case in your application?

3. You are anyhow careful in your conclusions, but perhaps it would be good to even stronger point out that this is just a single glacier study and generalizations should be taken with precaution.. Nevertheless, I agree with some of the principal findings. From an unintentional mistake made during the runs of a paper you cite in this article (Zwinger & Moore, 2009), I can confirm that at low flow speeds and retreat scenarios the 'no model at all' option actually on shorter timescales (a few years) does not make too much of a difference, as surface mass bal-

ance is in charge. But on longer time scales, especially looking at Fig. 13, I am not fully convinced that with Haig you just got lucky and the geometry with its over-deepening part makes the area and volume change just accidentally look the same for all three approximations while you have that significant discrepancy between the thicknesses obtained with the different approaches. The deviating thickness tells me, that dynamics actually does matter. In other more provocative words: I do not think that Haig (nor another ice body) is the mother of all glaciers. And, especially as we now start to understand the strong non-linearities and seasonal changes introduced by hydrology, like you, I emphasize that more of these studies shall be made on different type of glaciers – with the nice side-effect that we "Full-Stokers" have a perspective to keep our jobs. Not to forget about the field glaciologists bringing us the valuable data the modelers depend on.

4. What influence does it have to ignore the to the approximation corresponding contributions of the strain-rate tensor in the effective strainrate (the square-root of second invariant of $\dot{\epsilon}$) within the viscosity? Or are you always taking the full-blown strainrate?

- *Does the paper address relevant scientific questions within the scope of TC?:* yes.
- *Does the paper present novel concepts, ideas, tools, or data?:* yes, to a certain extent
- *Are substantial conclusions reached?:* yes, but perhaps be more careful with the generalization
- *Are the scientific methods and assumptions valid and clearly outlined?:* yes
- *Are the results sufficient to support the interpretations and conclusions?:* yes, what comes to Haig glacier

- *Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?:* yes
- *Do the authors give proper credit to related work and clearly indicate their own new/original contribution?:* yes
- *Does the title clearly reflect the contents of the paper?:* yes
- *Does the abstract provide a concise and complete summary?:* yes
- *Is the overall presentation well structured and clear?:* yes
- *Is the language fluent and precise?:* yes
- *Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?:* yes
- *Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?:* no
- *Are the number and quality of references appropriate?:* yes
- *Is the amount and quality of supplementary material appropriate?:* yes - as none given and none needed

2 Detailed Comments (sorted by their occurrence)

page 1711, line 9: *The derived flowline . . . .* Indicate how you *derived* (visual, some algorithm, only at surface?) the flowline, also with respect to the fact tat actually there is a reported discrepancy between τ_d and the SD result (which should match).

page 1716, line 1: As a general information, you might want to mention that your equations are presented in *index notation* as well as that *Einstein's convention* for summation over same indexes is applied.

page 1716, line 11: What is the basis of your iso-thermal assumption? Usually, we can assume that if we know that we are dealing with a complete temperate glacier – but then the freeze-on condition at the bedrock gets questionable. Nevertheless, your "tuning" of A, which can be interpreted as a tuning with respect to temperature, as A usually is expressed as an Arrhenius-factor, reveals $T < T_{pm}$ (so below pressure-melting). I think you are fine, but some words justifying that assumption would be good.

page 1719, line 12: $\tau_{ij}(s) - p(s)\delta_{ij} \approx 0$. This is wrong – at least for Full Stokes. First of all, the r.h.s. should still be a second-order tensor, but apart from that, the correct boundary condition you solve – actually it is the natural boundary condition for the Stokes-Solver in Elmer/Ice – is $\sigma_{ij}n_j = (\tau_{ij}(s) - p(s)\delta_{ij})\, n_j = t_i = p_{atm}n_i \approx O_i$, with n_i being the surface normal, p_{atm} the atmospheric pressure, σ_{ij} the Cauchy stress and O_i the zero vector. In other words, you do not have a in all its components vanishing Cauchy stress tensor (check the output on the surface), but rather a in all its components vanishing stress vector t_i.

page 1719, line 22: Zwinger & Moore (2009) – as stated on page 219 of their paper – used the commercial pre-processor Gambit, and not Gmsh for meshing the footprint. If you want to have an explicit reference (and increase my citation index) you might refer to e.g. Gillet-Chaulet et al. (2012), where the initial mesh (although unreported in the paper) before refinement has been made with Gmsh.

page 1720, line 9: *The mesh is then extruded using ElmerGrid ....* If the version has not changed dramatically since I last viewed it, to my knowledge ElmerGrid (for the reader: ElmerGrid is an auxiliary program in the Elmer package to manipulate meshes) does not have any DEM-interpolating extrusion features. I guess

you meant ExtrudeMesh. Then the reader might be also interested by the method used for approximating/interpolating the DEM data onto the footprint mesh (in case of ExtrudeMesh that would be the inverse-distance approximation). Hint: you could use a low exponent and a larger cut-off radius for the inverse radius to smoothen out the finer bedrock structures for your SIA runs.

page 1720, line 10: *...with ten vertical layers.* This does not sound like a lot, although it could be sufficient. Did you make any mesh-sensibility analysis, especially to the vertical resolution?

page 1722, line 5: *...non-local stresses ....* What makes longitudinal and transversal stresses less local than vertical shear stress? I guess you rather refer to the local (in columns) confinement of variable dependencies within SIA equations.

page 1736, Fig. 1: Where are the annotations a), b) and c) you are referring to? Plus, perhaps annotate Haig glacier in the photo

page 1737, Fig. 2: This is more of a matter of taste, so not a demand. I know you describe the units in the caption, but why not also put them directly to the color bar? Same applies to all figure-panels showing areal results (Figs. 3, 6, 9, 10, 12 and 13)

page 1741, Fig. 6 : If you compare results between different models, in my opinion you should apply the same data-range to each of the columns. Same argument counts for Fig. 9 and Fig. 12, eventually also Fig. 13.

References

Gillet-Chaulet, F., Gagliardini, O., Seddik, H., Nodet, M., Durand, G., Ritz, C., Zwinger, T., Greve, R., and Vaughan, D. G.: Greenland ice sheet contribution to sea-level rise from a new-generation ice-sheet model, The Cryosphere, 6, 1561-1576, doi:10.5194/tc-6-1561-2012, 2012.

Zwinger, T. and Moore, J. C.: Diagnostic and prognostic simulations with a full Stokes model accounting for superimposed ice of Midtre Lovénbreen, Svalbard, The Cryosphere, 3, 217-229, doi:10.5194/tc-3-217-2009, 2009.

Interactive comment on The Cryosphere Discuss., 7, 1707, 2013.