Application of a regularised Coulomb sliding law to Jakobshavn Isbræ, West Greenland

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Response to Referee Comment #2 (https://doi.org/10.5194/egusphere-2024-1040-RC2)

Referee comments are in black, author responses are in blue, *suggested changes are in italics blue*.

In this manuscript, Trevers et al. explore how well a range of commonly used sliding laws – linear-viscous, Weertman, and Coulomb-friction – perform in a model at mimicking observed speeds along the main trunk of Greenland's Jakobshavn Glacier during the time period between 2009 and 2019. They find that, while no single sliding law with static (fixed in time) parameters does a good job of matching observed velocities for the entire time period, a regularized Coulomb-friction law does a much better job (relative to the others tested) at matching observations during the time period from 2012-2016, during which the glacier exhibited both the highest overall peak speeds and the largest range in annual speeds. The authors go on to discuss the reasons behind these different model behaviors, after which they hypothesize that a regularized, Coulomb-friction sliding law with a spatially variable regularization parameter may provide the optimal choice for best-matching observed speeds (without requiring time-dependent optimization, something that is obviously not a viable choice if the ultimate goal is to use the optimized model for projections of future change).

Overall, this is an informative paper that is well written and presents interesting and useful findings. It confirms and expands upon related findings from other recent work and will be appreciated by readers of The Cryosphere. Most of my comments below are minor and editorial in nature, aiming to improve the readability of the paper. I do think that the overall direction and findings of the paper could be more clearly hinted at and summarized up front. Because of the way the abstract and introduction are currently written, I was anticipating that the focus was going to be more on what was gained and learned from conducting a time-dependent initialization / assimilation of observations. As currently written, the conclusions may leave the reader a bit unsatisfied; having queued up the interest, we are left wanting to see the results of the proposed optimal sliding law, That is, a Coulomb-friction law with a spatially variable, optimized, regularization parameter. While I realize that may be beyond the scope of the current work, it could be nice to end with the proposal to conduct future work to this end, if that is indeed the author's ultimate intention.

We are glad that the reviewer found the manuscript to be interesting, and would like to thank them for the helpful and insightful review.

We agree that the current structuring of the abstract and introduction don't make it clear for the reader quite what the motivation and aim of the study are, with too much of a focus on the time-series inversion which isn't the main focus of the study. It is not our intention to carry out future work to investigate how to spatially optimise the fast-sliding speed.

We will make some changes to the abstract to point that the time-dependent assimilation is required to make the linear viscous law work, and that this presents a limitation. We will *highlight that the regularised Coulomb law is able to reproduce the observed velocities without this requirement.*

We will also add a paragraph to the end of Section 1.1 to lay out the motivation, goals and structure of the paper.

Abstract

3: "…range OF sliding laws…"

5: "…the OBSERVED large seasonal and …"

5: "… AND THAT the assimilation of regular …" ("while" does not seem appropriate here since the suggestions regarding the sliding-law-type and the assimilation of velocities are two distinct and very different topics).

7: "was" -> "is" ? Note that the tense in most of the rest of the abstract is present, not past (e.g., in next sentence you say "we find" rather than "we found").

7: It might be more informative here to say " … able to reproduce the range of speeds observed during the period of peak flow, from 20XX-20YY" (or something like this). After readying the full paper it seems to me that this is the more important and interesting conclusion to highlight.

7: "Finally, we find …" (missing comma)

We will make all these recommended changes

Last sentence – maybe make this a bit more clear, e.g. if you are putting forth a proposal for such a sliding law as part of this contribution.

We will add a clause to this final sentence to point out that this is beyond the scope for this study.

Main Text

12-15: You might also add a reference here to Hillebrand et al., who conducted a somewhat similar set of exercises to try and model the behavior of Humboldt Glacier, Greenland, and found similar w.r.t. importance and sensitivity of power-law exponent in sliding law. There may also be some other findings from that paper w.r.t. sliding law types, param. values, calibration against observations, etc. that are relevant to / warrant some discussion here (The Cryosphere, 16, 4679–4700, 2022 https://doi.org/10.5194/tc-16-4679-2022).

21-20: As above, the findings from Hillebrand et al. may be relevant to discuss here.

We will add the Hillebrand et al. reference at this point in the text.

37: "… ice tongue, which …" (comma after "which")

47-48: "…flow speeds, in excess of 18km/yr, …" (missing commas?)

66: Maybe clarify "… block-structured mesh refinement …"?

We will change this.

91: "…soft ice, which …" and "… viscous ice, which …" (comma after "which")

98: Again, comma after "which"? Note that I'll stop mentioning this explicitly from here on and just suggest the authors check the remaining manuscript and ensure this is used correctly and consistently throughout (some places in do use a comma after which and some do not).

We will add commas at all the suggested locations, and will check the text for any other places where commas would be appropriate and consistent

118: Move the colon forward? E.g., suggest "… with respect to C and phi: since we seek to unknown fields …"

We will rearrange this sentence

Section 2.1.3: Did you try without the timeseries regularization? If so, what were the results? Or, put another way, would it make sense here to provide a bit of additional information on what motived this choice (as opposed to just taking the straight-up, best-fit optimization fields for every individual timeslice? I.e., it's not entirely clear what you get from the time-lagged portion of the optimization (unless I've misunderstood it).

The temporal regularisation enables the time-series inverse model to fill in locations where gaps in the data are too large for the spatial regularisation to cover, so long as the reference timeslice has good spatial coverage.

We will add a sentence to clarify this. We will also add another sentence to highlight that the purpose of the time-series regularisation is just to produce the time-series of C/phi inputs for the hindcast model.

142-145: Was there any independent check done on the accuracy of the DEMs constructed in this way? It seems like simply accumulating dh/dt year on year for many years in a row could also result in the accumulation of error. Alternatively, were any estimates made of the potential errors in the constructed DEMs and/or the potential impact on those errors on the simulated velocities?

Due to time constraints we did not carry out any checks or error/sensitivity analysis relating to this.

152: What does "as available" mean here? Were there 3 month periods for any given year where obs. vel. data were not available (and, if so, what was done to generate velocities for those time periods)?

This refers to the fact that the velocity datasets have gaps in.

We will add a sentence to this paragraph to point out that the regularisation helps to fill in the gaps.

152-154: Were the vels. for the faster moving trunk generated from feature tracking rather than interferometry? Perhaps in this section you could clarify if interferometry or feature tracking was used for the calculation of velocities (or if a combination of methods was used for velocities covering each scene at each time period).

These used a combination of speckle-tracking and interferometry. *We will include this detail.*

160-164: Clarify if the temperature spin-up is done as part of the BISICLES model or using some other model. And what sort of temperature model is used? Does it count for both horizontal and vertical advection? A few additional details would be appreciated.

These details will be added to this paragraph.

2.2.2: I'm confused about how the time-series optimization works. Is the same reference state always used (i.e., the first quarter of 2009) or is a new reference state – linked to the optimization from the prior quarter – used each time?

The same reference state is used for each quarterly timeslice. We did try running it sequentially, i.e. using the outputs from the previous timeslice as the initial guess for the next timeslice. We found that this tended to exaggerate the magnitude of variations in the resulting C and phi outputs, but did not substantially affect the resulting velocity fields, so we preferred to use the same reference state each time.

This will be clarified in the text.

179-180: "... AN ice sheet surface ... and TO reduce ..."

This will be corrected.

188: "...were calculated by equating Tau_b WITH ITS OPTIMZED VALUE (?) in the relevant expressions."

This will be clarified in the text.

179-184: It's unclear to me how the generated DEMs factor in here. If you relaxed the initial sfc elevation via forward modeling, then presumably that sfc is much different from that of the initial DEM. Did you accumulate anomalies from the DEM differences to your modeled sfc elevation? Were the DEMs just used for the optimization of the sliding coefficients, etc. ...but then not necessarily consistent with the model ice sheet surface for those same time periods?

It's applied both in the timeseries inverse model to produce the surface elevations there, but then also as a prescribed thickness change rate in hindcast experiments upstream of the calving front (see below).

190-197: Is the calving front position is specified by observations or calculated? Initially here, it sounds like you calculate the calving rate required to match the observed ice front. But then below that you say that it's only the centerline that gets this treatment and the rest of the ice front (?) is scaled according to this rate and the local velocity. In that case, does any other ice front position than the centerline match the observed ice front position over time? A little additional clarification would help here.

Yes, this treatment is only applied along the central flowline. It would be significantly more complicated to apply this treatment everywhere along the calving front, or to multiple flowlines. Scaling the calving rate like this still enables the ice stream front to advance and retreat in step.

We will add an extra sentence following Equation 16 to mention this.

198-200: Perhaps this addresses my question above. It sounds like the observed thinning rates (inland of 15 km) are applied to the model sfc state. Is this in addition to or instead of any thinning that the model calculates for these locations? Is the mix of observed vs. modeled thinning rates between 15km and the terminus just a linear combination based on the distance along the flowline between the two regions?

The prescribed thinning beyond 15 km is instead of any modelled thinning. Essentially, an additional accumulation/ablation component is calculated to account for the difference between the modelled thickness change and the prescribed thickness change. For positions for than 15 km from the calving front, this component is added in full. Less than 15 km, this component is partially added with a factor that scales linearly with distance from 0 at the calving front to 1 at 15 km.

207-208: Clarify – linear interpolation was done between the quarterly inputs determined from inversions?

Yes. This will be clarified in the text.

257: “and required the inference of changes in” ... could this just be “and required changes in”? If the inference part is important, then it seems like you may want to make the additional clarification in this sentence that you are talking about the modeling of these velocity changes (as opposed to the actual changes themselves).

We will change “explained” to “modelled” to clarify this.

261-263, 265-267: I think it could worth explaining more clearly and early on in the paper, when you first discuss the methods and different sliding laws used, that you are not advocating here for time-dependent optimization of basal slide parameters (since in some sense, as you point out here, this is “cheating” a bit). Rather, this is your baseline for clearly identifying if / that changes in basal sliding / basal traction are required to fit the observations. After that, your goal is to find the best sliding law and set of fixed parameters for that law that also allow you to best match the time-varying observations. This is something that you might also consider trying to clarify / prepare the reader for a bit better in the introduction and abstract.

We agree that the goal of the paper needs to be made clearer upfront. We hope that the changes already discussed to the abstract and introduction, as well as to the time-series inversion methods section, will provide more direction for the reader on the first time of reading.

270-282: Another way to think about this (or possibly explain it) is that ungrounding and loss of basal traction at one point has to be made up for at some other location (in order to obey force balance); the loss of support at the bed requires the transfer of stress, laterally or longitudinally, to another part of the bed. For a more linear sliding law, this transfer can be quite local (neighboring grid cells) but for a coulomb-friction law and a semi-uniform failure strength in a region (here, the grounding zone), this is not possible. That stress transfer will simply increase traction at neighboring regions, leading to failure of the bed there as well. In this sense, the Coulomb-friction law leads to / requires a non-local transfer of stress.

Yes, this is correct. *We will add a sentence to this section to point out the local/non-local stress transmission between the two laws.*

282-283: “Through this mechanism ... is able to account for in effective pressure without explicitly ...”. Can you elaborate further on what you mean here? I’m not sure I follow exactly (and maybe other readers will have a similar problem).

On evaluation this was a poorly thought out sentence and a bit misleading.

We will point out instead that the regularised law is accounting for the loss of traction when cells come afloat, but not to changes in effective pressure resulting from other changes in basal properties.

290: “The results from further …”. Something missing from / wrong with the beginning of this sentence that makes the rest of it confusing.

This sentence was missing a clause, this will be added in to make the sentence make sense.

292-293: Can you be explicit here if / that the transition between “slow” speeds and the faster speeds allowed by a Coulomb law is in fact given by the value of u_0 (i.e., if u_0 is 1000 m/yr, do we expect linear-viscous behavior below that speed and Coulomb behavior above it?).

Yes. We will alter this sentence to make this a bit clearer. It should also be noted that the transition is smooth and doesn’t occur abruptly at u_0. A sentence to point this out will be added.

308-309: Practically speaking though, is there much difference? In each case, you would need to figure out / specify the spatially varying parameter field values. Do you think that a spatially varying u_0 is more physically reasonable / intuitive than a spatially varying m?

We agree that there may not be much difference in practice, but we suggest that the regularised law provides a more natural way to model the dynamics since it combines both regimes and the fast-sliding speed governs the transition. *We will add a sentence to make this argument.*

310-320: Be explicit here that by “the fast sliding speed” you mean the value of u_0 (?).

This will be changed to use “u_0” throughout this section for clarity.

Again, in 317 you say that (presumably) this value subsumes the role of effective pressure. Can you be clear about what you mean by that and why? Is this simply a mathematical / functional argument because it appears in the denominator as effective pressure often does (and so both u_0 and eff. pressure have an inverse relationship with basal traction)? Note that this may be the same question I’m asking about above for lines 282-283.

We will add a few sentences here to flesh out this argument. Essentially, C and u_0 both partially subsume the effect of unknown spatial variations in the effective pressure. Effective pressure will change near the grounding line in response to thickening or thinning. A non-regularised law cannot account for these changes with a static value of C, but we argued in Section 4.1 that the regularised law can account for changes in traction resulting from grounding line motion. Therefore it might be optimal to have a spatially varying u_0, instead of subsuming all the effective pressure variation into C. Over longer periods with more significant grounding line retreat, temporal variation may also be necessary.

328: “We explore the use of different sliding laws” … add some more here to clarify to what end? E.g. “… in addressing this limitation.”

We will add “...with non-evolving inputs to address this limitation”

332: “… may vary”. Spatially? Temporally? Both?

Both. We will clarify this.

Figures

Figure 1 (caption): The meaning of “Annual year start ice fronts” is a little bit unclear here, and an awkward way to start this sentence. Consider revising? “… from SAR intensity images OF Lemos et al …”

We will reword this

Figure 4: Clarify in caption what the grey region represents?

This is the period of maximal sliding speed and retreat, and is referenced in Figures 5 and 6. *We will clarify this.*

Figure 5a: Suggest making the symbols in the legend larger (i.e., use filled circles rather than dots). The colored dots on their own there are difficult to see and their colors are difficult to discern from one another (the clouds of dots in the actual figure body are ok as is).

We will increase the circle size.

Figure 8: The actual line colors in the figure here do not seem to agree with the colors as described in the caption. "Vertical-dashed lines indicate the value of u_0". This is confusing since you just said that the colors of the lines (at least for two of them) are for different values of u_0. Do you mean that the dashed-line represents the values assumed over a certain region for the modeling done here?

Thanks for pointing out the colours! I had changed the colour scheme but forgotten to change the caption.

We will the colour scheme again and update the caption along with it. The vertical dashed lines just identify the u_0 values for the corresponding curves. The scale in the right hand panel doesn't go up to 10000 m/yr so this value isn't shown. *We will update the caption to better explain this.*