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The Cryosphere
Discussions

Interactive comment on "Inversion of geothermal heat flux in a thermomechanically coupled nonlinear Stokes ice sheet model" *by* Hongyu Zhu et al.

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This paper deals with the difficult inverse problem of inferring the geothermal heat flux under an ice sheet under strict assumptions of non-slip and cold basal conditions. The authors derive the infinite-dimensional forms required for generation of a Newton's method. The gradient and Hessian of the proposed least-squares objective function are fully (thermomechanically) coupled, which is an advance over work which has been done previously, which often uses so-called incomplete adjoints. The paper explores the limiting resolution at which geothermal heat flux can be recovered given assumptions of data density and uncertainty. Additionally, the authors explore the effects of using an operator-splitting approach for the adjoint problem.

Major Comments:

While this paper effectively makes its point regarding the numerics of the problem in question, I don't think that it provides enough glaciological relevance to be published in the Cryosphere as is. Thus I propose two options: first, that the paper be resubmitted to Geoscientific Model Development, where the paper may get the appreciation it deserves based more solely on its mathematical merit, or second, that a substantive discussion of the possible implications that this paper's results might have for practical glaciology be added. Some examples of the latter might be the addition of a section that discusses whether, given estimates of error in contemporary remotely-sensed datasets, this method has any promise towards use on real ice masses. Obviously, the methods presented in the paper are limited to cold conditions. Where might these assumptions be valid? What additional factors might complicate the analysis in the real world? Furthermore, the work presented here is done at a rather low resolution. Is this a result of computational efficiency, and would this be a major limiting factor with respect to inverting for heat flux in real glaciers?

Minor Comments:

Generally: I think the paper could benefit from some compaction, both at a small scale (e.g. eliminating as many unnecessary subjective adverbs, such as "significantly", as possible) and also at a large scale (some sections read a little bit like a textbook, cf. P19 L6).

P1 L13: "small wavelength is ambiguous, consider "short-wavelength" instead.

P2 L2: Stokes' equations are always "full," else not Stokes' equations.

P2 L2: It's clear that the model is coupled, so the word "multiphysics" isn't relevant (here and elsewhere).

P2 L2-7: Consider transposing this first paragraph with the second. As it stands, the (brief) literature review splits the problem description.

P2 L8-18: The authors might consider addressing the importance of geothermal heat in determining whether the bed is frozen or not at a given location, which has implications for ice dynamics (via sliding) that very likely exceed rheological effects.

P2 L31: ill-posed is redundant in this line.

P2 L31: The continuous problem is also a PDE-constrained optimization problem, it's just being discretized. Maybe reword to make this more clear.

P2 L32: "Only hope" is a bit strong. See for example Pollard and DeConto (2011) "A simple inverse method for the distribution of basal sliding coefficients under ice sheets, applied to Antarctica" for a counter-example.

P3 L5: The second sentence in the paragraph restates the first, and is not more precise.

P3 L13-35: These sections are a bit too hand-wavey. If the conclusions about one-way versus two-way coupling are solely a result of the work contained herein, this discussion should not be attempted with such depth prior to presenting evidence for said results. If this information is generally understood and valuable as context for the work to be described herein, references should be added.

P4 L12: The statement that "Section 7 provides concluding remarks" is not necessary.

P4 L14-18: All of this information has already been clearly stated in the introduction and may not be necessary here.

P4 L19: Perhaps just "ice can be modelled as" is sufficient here.

P4 L20: "Conservation of" rather than "balance of" seem more appropriate.

P5 L2: Is simultaneous inversion of the flow law exponent and geothermal heat flux undesirable? If so, state as much.

P5 L28: Since this is primarily a glaciology journal, rather than a mathematical one, please provide a basic reference when talking about weak forms.

P7 L9: "i.e." not necessary.

P8 L12: Clarify that the test functions are acting as Lagrange multipliers.

P8 Eq. 22: I would like to see a little bit more information about $\mathcal{B}$. It is reasonably obvious how it can be constructed (i.e. after discretization evaluating the basis functions at the locations of the observation points). However, since the least-squares misfit isn't integrated over Γ_t, I fail to see how by equating it with something that is (namely the adjoint stress) can allow the strong form to be recovered. It seems to me that the author's are performing an additional integration of Γ_t that is not being shown in the paper, and I would like to see some additional specificity here.

P8 L30: Consider using underbraces to specify what part of the adjoint stress comes from where (and what gets omitted when using the linearized approximation). This is interesting because it becomes clear that there exists an additional term derived from the thermomechanically coupling term that I have never seen considered before.

P9 L3: Throughout the work, the authors use several different notational conventions to refer to effective strain-related quantities (i.e. $\dot{\epsilon}_{II}$, $\dot{\epsilon} : \dot{\epsilon}$,tr$(\dot{\epsilon}^2)$). It would be better to use a consistent notation throughour (I would favor either the invariant or colon notation, but not the trace notation).

P9 L22: Without stating what this new functional is, I can't really determine if the computed variations are correct. Even referring back to Petra (2012), I can't tell what this Lagrangian on the gradient referred to in the text actually looks like. Is it *only* the forward and adjoint equations imposed via new Lagrange multipliers, or is there a term to be minimized as well? It's not clear from the text.

P10 L5: The incremental forward problem seems to also resemble the adjoint problem, particularly in the changes to the viscosity term relative to the self-adjoint case. Is there a more compelling reason why the forward and incremental forward problems should be more closely related than the forward and adjoint problems?

P10 L25: Same question as above.

P11 L7: Please make clearer from the outset that the content of this paragraph is paraphrased from Petra (2012), and that a more thorough discussion of the topic is included there.

P11 L10: Can you cite a reference showing why neglecting terms involving the adjoint variable guarantees positive-definiteness in the Hessian? Petra (2012) did this as well, but similarly did not cite a reason for why this should be true.

P11 L29: Check out the "citet" command in bibtex to get citations that look like "Isaac et al. (2015)", rather than "(Isaac et al., 2015)".

P12 L4–6: This front matter is redundant.

P12–13 Sec. 4.1: The specification of the finite element spaces splits the discussion about streamline upwinding. Consider transposing the first and second paragraphs.

P13 L20: The discussion of Galerkin vs. non-Galerkin spaces requires a reference.

P14 L7: This point was already stated in the previous paragraph.

P14 L13: Since the adjoint temperature isn't used here except as a means to facilitate optimization, is the lack of equivalency in the small mesh limit between the discrete and continuous (rather, non SUPG) temperature all that meaningful?

P14 L17: This sentence provides no information.

P14 L21–27: I'm not sure that this is true; the authors spent the previous paragraph talking about how the advantage of OTD is that one can use stabilization terms that vanish as the residual does, but that this creates other problems. This section would seem to suggest (wrongly) that it is DTO that has this property. I could of course be wrong, in which case the authors might humor me by re-writing this section more transparently.

P14 L32: Perhaps "We consider both a two-dimensional flowline case and a three-dimensional map plane case" would be more clear.

P14 L33: $s(x)$ is usually reserved for surface elevations in glaciological parlance, where $H(x)$ is reserved for ice thickness. In these examples the two appear to coincide because of the flat bed, but this could become confusing for other examples.

P14 L33: Why this particular choice of geometry? There are analytical solutions that are somewhat more realistic than a cosine, such as the Vialov profile. I don't think it matters much with respect to the results, but I think that the choice should be elaborated upon a little bit.

P16 L5: This could be explored with a little bit more depth. Throughout the remainder of the work it seems as though the methods presented herein are selecting overly smooth solutions. Could this be a result of using too-aggressive regularization? Additionally, γ computed via Morozov's discrepancy principle would be different for each problem. Was it computed independently for each example, and how did it vary? Would using a smaller value of γ allow for better resolution of the small scale variability in geothermal heat flux or is the lack of detail indeed a result of the smoothing nature of Stokes' flow?

Figure 3: The chosen colormap is a little difficult to read. Try something higher contrast.

P17 L2: Once again, I'm not sure whether these results are showing the limits of data recoverability or if they're showing the choice of regularization parameter. A bit more exploration of the latter topic would help to clear this up.

Figure 6: I cannot tell the difference between cyan and blue in this figure.

P19 L6: I am extremely skeptical of the capacity for a model with 2 elements (quadratic or not) to accurately capture the vertical structure of the temperature field, particularly when said temperatures are subject to boundary layers (as the authors noted). Is it possible that this low vertical resolution is contributing to some of the error in the recovered solution? I suppose that since the same model is being used to generate the surface velocities that this effect would be lessened, but then that brings up the additional issue of whether the overly simplified temperature fields induced by the low resolution are leading to an easier job of recovering the geothermal heat field.

Figure 9: Add legends to (a) and (b). Also, it would be useful to see an additional plot of the cosine of the angle between the two-way coupled descent direction and the exact gradient. Perhaps we would see some "uphill traffic" there as well.

Appendix: Probably not necessary to include this; a reference to any numerical analysis text ought to be fine.

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