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The Cryosphere Discussions

Interactive comment on "Data assimilation using a hybrid ice flow model" *by* D. N. Goldberg and O. V. Sergienko

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General Comments

This paper presents the derivation of an adjoint to the increasingly popular 'hybrid' class of ice sheet models. An important distinction is drawn between adjoint formulations that include non-linearities in viscosity (termed *complete*), and those that do not (*incomplete*). Hydrid models treat the conservation of momentum by solving the vertically integrated Stoke's equations with the following additions: strain softening includes terms due to vertical shear, vertically averaged velocity is solved for, and driving stresses are modified by adding the longitudinal stresses resulting from solution to the vertically integrated equations. Hybrid formulations are popular because the map-plane momentum balance reduces to a single 2 dimensional layer, in other words a single two dimensional, elliptical PDE. This offers a clear computational advantage over fully 3 dimensional systems arising from first order, or full Stoke's systems.

The adjoint system is useful for determining the sensitivity of the system to changes in parameters, and also for data assimilation. Essentially, solving the adjoint system provides gradients of state variables with respect to parameters, and delivers the derivatives with the same computational effort that is required for the forward problem. To my knowledge, a thorough development of the adjoint of the hydrid equations has not been done. Nor has the adjoint of the hydrid equations been used to assimilate a range of synthetic and real data. The distinction between incomplete and complete adjoints is also a nice addition. Hence, this paper is novel, and presents worthwhile results that will be cited by others. I recommend that it be published; pending a few changes that I think will improve the text and the quality of the analysis.

Specific Comments

I am still unclear about the ability of the so-called 'incomplete adjoint' to determine the basal traction field. The author's results are mixed and seem conflicting. It appears that in idealized geometries, the incomplete adjoint can fail to find a reasonable minimum, or match to the observations. However, in realistic geometries it does very well and the distinction between incomplete and complete doesn't appear to be useful. As there are numerous authors currently using incomplete adjoints, the results of the paper would be truly significant if they demonstrated that the approach was only valid in certain circumstances. However, as the paper now reads, I find it hard to draw any firm conclusions about the necessity of using the complete vs. the incomplete adjoint. This might be addressed with an expanded discussion, but more experiments would be more convincing.

Technical Comments There are a host of easily corrected issues having to do with style or tone of the paper. I will not pretend to know too much about this, but hope the authors will read each of these, think carefully about whether or not improve the clarity

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of the paper, and take action.

- The capitalization scheme throughout this paper is odd. I don't think "First Order" needs to be capitalized. Nor does “Optimal Control” (page 2203). Then again, I think “Lagrange” should be.
- Page 2204: “models account for all the stress terms...” I'm not sure I agree with this. While the terms are there, the hybrid models do not resolve them in a vertically explicit fashion, and (I think) that's an important distinction.
- Page 2205: If you're not up against a page limit, I think more detail should be put into explaining the forward model. This short section left many important details unclear to me. This should be a simple cut and paste from Goldberg 2010. I think that my difficulty was understanding how, and when the vertically explicit terms $\partial_z \nu u_z$, $\partial_z \nu v_z$, $\frac{1}{4}u_z^2$, and $\frac{1}{4}v_z^2$ are resolved.
- Page 2206: I think you mean “plan” view or “map-plane”, but plane view? Line 8; functional derivative -> cost function derivative? Line 13: “where $\mathbf{u}$” -> “where $\mathbf{u_s}$”. Line 25: “input parameters” -> “input parameters for the computational cost of a single forward solve”.
- Page 2207 Line 2, I'm confused about the use of the word ‘linear’ here, is that right?
- Page 2208: Line 7, you should probably discuss the common case of a Nuemann boundary condition, which you presumably used on the PIG exercise? Eq. 12, curly braces on K missing.
- Page 2209 Line 27 “An important point to remember...” This is an excellent point, but does not seem to be borne out by the experimental data. This is what I was getting at above, but in a way it comes down to providing better support for this critical statement.

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- Page 2010 Line 20: Can you offer more justification for using a FO solution as a target for a hydrid model? Given that you've got the hydrid solution (I think), should the experiment be done with hydrid solutions? Line 23: Is it? Don't we expect there to be differences that arise from the different models that are used? Please explain more. Page 2215 Line 22: "was poor", or was it impossible?

- Figures: Make the labels consistent with the text. "Approx adjoint" -> "incomp. Adjoint"?

- Fig 5. I think c and b labels are reversed.

Interactive comment on The Cryosphere Discuss., 4, 2201, 2010.

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