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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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LETTER TO REVIEWERS ——————-

Since one of the comments was very brief, and some of what I will say here addresses comments in both, I will try to address both sets of comments here. Several typos were pointed out, and they will be fixed in the revised manuscript.

There was a question of terminology regarding the Blatter/Pattyn/FO balance. When I wrote the J Glac paper that this manuscript references, I decided that the relevant literature had not yet decided what to call the balance. And so I went with the rationale that textbook definitions are more final than journal article definitions, and followed Greve and Blatter (2009).

There was a bug found in the code used to do the PIG inversions. To be specific: as discussed in the text, the boundary values of the forward model are set to the observation velocity values at those locations, i.e. \vec{u}*. I had accidentally set the boundary conditions of x-velocity (u) to the boundary values of y-velocity (v). This error was only at the upstream boundary, no other boundaries and not in the interior. It was stressing the inversion by forcing high basal traction near the upstream boundary. The new Fig. 5 will reflect the inversion with the bug fixed; however i don't know how you will see it since i cannot upload the revised manuscipt yet. anyhow there is no longer a region of high basal stress along the top boundary.

The fixed bug addresses a comment from the "anonymous" reviewer about artifacts of the boundary. I believe the high basal stress near the southern boundary (right x-boundary) is real based on what others have shown (e.g. Joughin et al 2009). The fact that it goes to zero right at the boundary is something that i felt was too technical for the manuscript, but I believe it is because of the homogeneous boundary condition in the adjoint equation; since \lambda and \mu are zero at the boundary, the search direction for \beta may be underestimated in the boundary cells. Since the initial guess is one of low basal traction, basal stress might remain lower in those cells than it would if the boundary extended further. (To answer another of J.V. Johnson questions, the entire boundary is kinematic.)

The following addresses some points raised by J.V. Johnson:

– regarding the depth-integrated nature of horizontal stress terms in the hybrid models, my statement has been qualified.

– I talked a little bit more about the model in Goldberg (2010), but I still don't think this paper is the place for repeating technical details. The paper has been published now, so it is readily available to interested readers.

– Page 2206, like 25: this was a good point. thanks.

– Page 2207, the use of "linear" is correct: "adjoint" refers to the adjoint operator of the linearized forward model.

– In the inversions of synthetic observations, my original thinking was to use velocities from a model that was close to, but not exactly, the forward model, thereby injecting some sort of uncertainty into the inversion. but a better way to do this would have probably been to add some noise to the results, and had i used the hybrid output as synthetic observations, i don't think the results would have been noticeably different. At any rate, i still think that the synthetic observations experiments stand as examples of the best that the inversion scheme can possibly do, in terms of the "observations" being completely compatible with the forward model.

– The biggest issue with the manuscript, in my opinion, is that the advantage shown by the complete adjoint is not seen in the inversion using the PIG data. One should now look at figure 5(c), which reflects the fixed bug mentioned above. There is now a bit more of a difference in convergence rate, at least more than before. Still, it is not nearly as noticeable as in the synthetic experiments. I have worded the manuscript to acknowledge this. I think the fact that fixing the bug made a small difference supports a statement made in the revised manuscript: that the better the quality of the observations in terms of compatibility with the forward model, the bigger the advantage of the complete adjoint. (I tested this out by taking the output of the forward model in the PIG inversions and using that as synthetic observations, and performing more inversions. The difference in performance between complete and incomplete adjoints was similar to that shown in Fig. 4: the incomplete adjoint either converged more slowly than the complete, at least at first, or reached a point after which J did not decrease. I did not include any of this in the manuscript as i thought it would be too confusing to the reader.)

One more statement – the idea suggested above – that the advantage of the complete adjoint depends on compatibility of observations with the forward model – may seem useless as this will never happen. But it is my hope that in the future inversions for

unknown parameters will integrate the momentum solve with the time-dependent continuity equation, and in such a framework it may still be important to consider the full adjoint, as i now point out in the last paragraph of the conclusions section.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/4/C1634/2011/tcd-4-C1634-2011-supplement.pdf

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