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Interactive comment on "Of the gradient accuracy in Full-Stokes ice flow model: basal slipperiness inference" *by* N. Martin and J. Monnier

S. L. Cornford (Referee)

s.l.cornford@bristol.ac.uk

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article

Review of N Martin and J Monnier, TCD (2013) vol 7 p 3853

Stephen Cornford

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This paper deals with one of the common inverse problems in ice sheet dynamics: determination of the basal traction coefficient from satellite observations of the surface velocity. Specifically, it examines, the consequences of contructing the gradient of the misfit function by solving either the full adjoint problem, or by solving an approximate problem derived by neglecting derivatives of the effective viscosity (the self adjoint approximation). It also connects the two methods as end points of a sequence of approximations, which I thought was an interesting generalization. The self-adjoint approximation is easier to derive and use, and is widespread, so discussion of its value and limitations should be important to the ice sheet modelling community.

I thought that the numerical experiments carried out in this study were sound and the results were interesting, and well suited to publication in The Cryosphere. I was especially pleased to see the difference between the two methods discussed in terms of the wavelength of features that could be recovered. I did, however, find the manuscript awkward to read in parts, not so much because of the material, but the presentation. I will outline some of the difficulties I had below, but there are a number of convoluted sentences and paragraphs. I think these are all minor issues, but that the paper should be carefully examined for correct and sensible use of language.

1 Specific comments

1.1 Gradient and scalar product tests

Should equation (16) be

$$\frac{j(k+\alpha\delta k)-j(k+\alpha\delta k)}{2\alpha}=\frac{\partial j}{\partial k}.\delta k+...?$$

The definiton of $|I_\alpha|$ in eq (17) would be the same, of course.

It is not so suprising that $|I_\alpha - 1|$ is always around 1 for the 'self-adjoint' gradient since the terms that are neglected are comparable to the terms included, though it is useful to see that the full-adjoint (with its more complicated implementation) behaves well and that the incomplete reverse accumulation has an error that decays at a constant rate. Since the gradient is used primarily to provide a descent direction, I would also have been curious to see a test that differentiated more clearly between an approximate gradient that had an incorrect magnitude but a correct (or nearly correct) direction, and one with an incorrect direction. I think the scalar product test mentioned in sec 2.5.2 would have been useful, but there seem to be no plots for that, or indeed any further mention of it.

1.2 Discussion of the slip ratio

The authors note that it is harder to recover the oscillations in β as the slip ratio increases because the flow is no longer much retarded by friction and instead only by the topography and the lateral boundary conditions. For example, 'For $r > 1$ it is not

the quality of the data that prevents an accurate reconstruction of β but the non-local behaviour of the flow'. I think this is a bit misleading, because the amplitude of the oscillations is decaying as r grows, and therefore failure to recover them represents a smaller error. Compare figures 9c and 9d, for example. In (c), with r = 0.5, the self-adjoint method recovers about half of the f0 wave, which has an amplitude of 0.005. In (d), with r= 5, the self-adjoint method also recovers about half of the f0 wave, but that now has an amplitude of 0.0025, so the ampitude of error is lower. 9e shows essentially random results for both methods, but with a smaller amplitude of error still (0.0005).

2 Presentation issues

I don't mean to pick on the items below in particular, but as examples of the hard-to-read style that appears in good portion (but not all) of the paper. Rather than point out more examples, I hope the authors will be able to improve the style of writing.

- Abstract, line 9 'maximum of 20' should be 'minimum of 20'
- p3854, lines 18-19: In other respects, the ice dynamic is known to be highly sensitive to the state of the bedrock and therefore to its modelling .The basal slipperiness is consequently a crucial parameter in the perspective of controlling ice flows'. This is rather convoluted, when all that is meant, perhaps, is that the observed surface flow velocity does not appear in the momentum equations, but the basal slipperines does
- p 3860, lines 1-3: 'This term plays a role of convexification for the optimal control problem and thus restricts the region of admissible parameters to smoothly varying fields'. No explanation is give of the relationship between convexity and

smoothly varying fields. ‘This term introduces a bias toward smoothly varying fields’ would be simpler, and require no further explanation.

3 Technical corrections

- p 3860, lines 17-19: ’It plays a role...’ refers to a plural, so should be ’They play a role’.
- p 3863, line 22 (and elsewhere): MacAyeal (1993) is usually called the shallow-shelf or shelfy-stream approximation (fast sliding) rather than the shallow ice approximation.
- Figures 6,7,9,11. The horizontal axes are labelled ‘Frequency, Hz’, but should be ‘Wavenumber , m^{-1}’