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The Cryosphere Discussions

Interactive comment on "Analytical analysis of small-amplitude perturbations in the shallow ice stream approximation" *by* G. H. Gudmundsson

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This paper continues a research program by its author aimed at understanding the performance of simplified glacier flow models by comparing their linearized response to bed perturbations with the Stokes equations that these simplified models are intended to approximate. This objective is important in the context of ice sheet modelling as ice sheet simulation codes cannot in most circumstances employ the Stokes equations to model the flow of an ice sheet, but have to rely on simplified equations.

The model which is considered in detail in the present paper is the so-called shallow stream approximation (also called the 'shelfy stream approximation' in other quarters to underline its relationship with ice shelf models), which is commonly used to describe fast-flowing parts of ice sheets.

The paper is basically fine as it stands. I have not attempted to reconstruct all the calculations in the paper (mostly due to the profusion of symbols that occur, which could undoubtedly be streamlined), but have checked that they are consistent with a few special cases that are easier to calculate, and based on the quality of the author's previous work, there is no doubt to me that his findings are correct.

If pressed for a criticism, I would have to say that it produces no big surprises, neither in the method chosen nor in its conclusions (namely that the shallow-shelf approximation holds for large slip ratios and for wavelengths that are large compared with ice thickness — these are precisely the basis for the derivation of the shallow stream model). As such it is possible that the paper could be shortened somewhat as most readers will fall into one of two categories: those who do not need anywhere near as much detail as is presented in the present manuscript, and those who will not follow the theoretical development regardless of the degree of exposition given.

In terms of presentation, it may instead be worthwhile tying the results of the linearization more strongly to the assumptions behind the shallow stream approximation. First, a comment on the derivation of the shallow stream approximation in this paper. One page 25 (specifically, line 19), the author announces that the slip ratio of an ice stream must be of $O(\delta^{-1})$ in order for the shallow stream approximation to hold. I do not believe that this is true, if I understand the common definition of slip ratio correctly. Following the discussion on page 51, it is clear that if $u_b^\star$ is $O(1)$ and $\partial_{z^\star} u^\star = O(\delta^2)$, as the author indicates is necessary for his derivation of the shallow stream approximation, then the slip ratio (ratio of sliding velocity to shearing velocity, the latter being velocity difference between surface and bed) must be of $O(\delta^{-2})$ — not $O(\delta^{-1})$!

However, a slip ratio of $O(\delta^{-2})$ is actually not *necessary*, it merely ensures that the error in the shallow stream approximation due to omitting geometrical terms (of $O(\delta^2)$) is comparable with the error due to omitting shearing effects (of order of one over slip ratio). The shallow stream model can in fact also be derived for other slip ratios, in which case one can show that the error incurred is no longer of $O(\delta^2)$ but essentially

TCD

2, S4–S8, 2008

of $O(\delta^2) + O(\nu)$, where ν is slip ratio (this remains true for all ν for Newtonian ice, but not for Glen's law).

As the numerical results indicate a divergence of the shallow stream approximation from the Stokes equations for short wavelengths (larger δ) and slower sliding (large ν), I think it would be worth investigating if the error estimate above is indeed useful. I notice that *all* the graphical output in the paper shows plots of transfer functions etc. versus wavelengths - how about showing plots at, say fixed wavelength but treating slip ratio as a variable? Or contour plots like fig 8b with wavelength on the horizontal axis and slip ratio on the vertical?

Other points:

- page 27, line 11: m is arbitrary but had better be positive. . .
- page 28, line 15: the domain of the shallow stream problem is the projection of the actual domain onto the xy plane, and doesn't need to be transformed. In the same vein, I'd use the depth integrated version of the kinematic boundary conditions instead of (8) and (9) (saves space):

$$\partial_t \Delta s + \partial_x [u(\Delta s - \Delta b)] = 0,$$

  which follows straightforwardly from the fact that u is independent of depth in the ice.
- equations (15)–(20): you're making this far harder to read than necessary by defining five (!) new variables that are functions of the wave vector (k, l), namely p, t_p, t_r, ξ and j. Also, the insistence on Laplace transforms is superfluous; I can solve constant coefficients odes (which is what I get after Fourier transforming) without resorting to contour integration).
- page 33: if $1/\bar{C} \ll 1$ it doesn't follow that $\bar{C} = O(\delta^2)$!

- page 38, line 10: ‘THe SSHEET solution is only similarlu accurate for wavelengths larger than about 100 ice thicknesses’ — this surely depends on the value of $\bar{C}$. How does this wavelength change with $\bar{C}$?
- page 40, line 19: ‘lets say . . . ’ ?? Even let’s would be a little colloquial, I should think. On line 22, I would write ‘As Figs. 7a . . . ’ (lose ‘the’)
- page 41 line 12 ‘. . . used in Figs. . . . ’ (not using)
- page 42 line 12: ‘Presumably the most interesting . . . ’ — odd phrasing.
- page 42 lines 13 and 14: The differences between $m = 1$ and $m = 3$ are only significant for wavelengths longer than about 100 ice thicknesses. You’re using $\bar{C} = 100$; is there a relationship, at least for SSTREAM (presumably not for SSHEET, as commented on before)?
- page 42, line 20: ‘It is not clear if this insensitivity of T_s to the value of the sliding law exponent m for wavelength[s] smaller than the one given by Eq (34) is also true of the FS solutions’. Firstly, correct ‘wavelengths’. Secondly, I would argue that it *will* also be true of FS solutions. I wrote two notes on moderate-wavelength ($\sim$ ice thickness) perturbations with a full system and arbitrary sliding laws in the limit of a high slip ratio. In the second of these (Schoof, C. 2005. A note on inverting ice-stream surface data. J. Glaciol., 51(172), 181–182.), I show that treating the inverse of slip as a a small parameter in which an asymptotic limit is sought (much as SSTREAM represents the limit $\delta \rightarrow$ and $\bar{C} \rightarrow \infty$) leads to a model for transfer of perturbations from the base of the ice to the surface in which the precise form of the sliding law (linear, Weertman or otherwise) does not matter at all — this is essentially because at leading order in the slip ratio the sliding velocity stays spatially constant at the bed, regardless of any perturbations in the sliding parameters. Hence one would expect the FS results to be insensitive

to m at least for moderate wavelengths and for high slip ratios. (As I'm in nit-picking mode, I should add that this comment also applies to the statement that 'The only study on the effects of non-linear sliding on the bed-to-surface transfer amplitudes seems to be that of Raymond and Gudmundsson (2005))

- page 43, line 21: 'long wavelengths' (no hyphen)
- page 44, line 13: Arguably, $\bar{C}/2 \sim \bar{C}^2/(2(1+\bar{C})$ when $\bar{C} \gg 1$, so asymptotically the two limits agree in the parameter range in which SSTREAM holds, as is to be expected.
- page 45 line 4: 'are possible' — not 'possibly'
- page 47 line 2: Let's change 'lets' once more.
- page 47, first paragraph of section 4. In a sense, this states the obvious. SSTREAM is based on omitting certain terms which are small when wavelengths and slip ratios are large; when they are not, these terms start to matter and this leads to discrepancies with the true solution. So? — all this seems to be saying is that one should know the limitations of a model before using it…
- page 48 line 21: orders of magnitude
- page 48, bottom: it is presumably not advisable to use SSTREAM at such resolutions, or at least not to expect results that accurately reflect physical reality at these scales.

Interactive comment on The Cryosphere Discuss., 2, 23, 2008.