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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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Review:

DESCRIPTION AND GENERAL COMMENTS

The large-scale numerical modeling of ice streams has commonly used approximations to momentum conservation that are valid for shallow ice (horizontal scales much larger than the ice thickness) and high slip ratio (basal motion much larger than deformational motion) closely related to the nearly-ice-shelf model of MacAyeal (1989). This manuscript thoroughly explores the response to boundary perturbations of this set of approximations (dubbed SSTREAM) to systematically illuminate for the first time its characteristics of transfer of bed topography and variations in basal lubrication to pat-

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terns of topography and velocity on the upper surface as well as timescales associated with transients.

While the scalings employed in the derivation of the SSTREAM approximation indicate the conditions of its validity, they do not by themselves quantify errors or indicate the characteristics of the failure. Gudmundsson here attacks this problem by systematically illustrating the conditions where the SSTREAM succeeds and fails by quantitative comparison to corresponding solutions to the full momentum equations (FS). Moreover, the manner of failure is thoroughly described. Some of the results have been earlier derived by Hindmarsh (1984 L1L2 approximation, which is equivalent to SSTREAM), but Gudmundsson focuses specifically on SSTREAM and his exploration is much broader. The extensive comparisons also provide the added benefit of a very accessible summary of the intriguing transfer characteristics predicted by the full solution (FS). The manuscript also compares the SSTREAM approximation with the more standard shallow-ice approximation used for modeling sheet flow of ice sheets (dubbed SSHEET).

The core of the paper is the derivation of analytical perturbation solutions for SSTREAM. These solutions are new in the literature. For the comparisons to FS, the manuscript relies on the analytical perturbation solutions for FS already derived and published by Gudmundsson (2003). The solutions of Johannesson (1992) provide analytical SSHEET behavior. The analytical set of solutions allows the comprehensive display of the response characteristics for the different solutions in a set of impressive figures that display an immense amount of structure.

During the last decade or more there has been considerable interest in inferring bed conditions of ice streams from observations of surface topography and velocity. A clear message to numerical modelers using SSTREAM approximation for this purpose is that SSTREAM gives unphysical perfect transfer of basal topography amplitude to the ice surface for horizontal scales going to zero. There are other model errors at small horizontal scale as well. This behavior has serious implications for the resolving power

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of attempts to invert surface observations to basal conditions and at the same time provides some guidance for filtering data to alleviate the problem.

The manuscript does not take up implications about the accuracy of SSTREAM for large-scale modeling of the transport of ice along ice streams. The following questions come to me. In fast ice streams moving over rugged bed topography and possibly heterogeneous till properties including sticky spots (MacAyeal Ice Stream could be an example), how does the small scale bed structure link to the large scale dynamics? Presumably the incorrectly SSTREAM-amplified transfer of smaller horizontal scale bed features included in an actual application would also result in an error in the drag from those features. How important could that be? In the usual thinking about glacier sliding, relevant horizontal scales for drag are decimeters to meters. Viewed from that perspective, one would expect that small scale features not correctly calculated by SSTREAM would be unimportant in any case for drag. But in ice streams moving at high speeds over beds lubricated with a very weak skin of till, could form drag over bed topography become relevant at substantially larger horizontal scales than usual thinking? From another related perspective, one might ask: below what scale should bed variations be regarded as roughness and analyzed within the basal boundary condition affecting basal speed but unimportant in the interior flow? Could the answer be different for SSTREAM and FS? While these questions can not be answered within the context of SSTREAM alone without reference to FS, this manuscript brings them clearly to mind, even without an explicit statement. Perhaps Gudmundsson might like to make it explicit, although with more careful logic and wording than I provide here. The predominant interest in ice streams concerns their influence on the mass balance of the ice sheets that they drain, so any insights about large scale dynamics could be valuable even if very brief.

Clear weaknesses in the development of the manuscript are that the analytical solutions considered are all restricted to small amplitude boundary disturbances and linear ice viscosity. Limited numerical modeling available in published literature cited in the

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manuscript indicates that the general characteristics displayed by the linear perturbation solutions show up similarly in solutions using non-linear viscosity. However, much more (probably numerical) exploration will be needed to pin down how important these weaknesses are for the quantitative information about length and time scales revealed by the linear perturbation solutions. All of this is acknowledged in the manuscript.

This manuscript is innovative, well-organized, clearly presented and rich in new information content. It is a valuable contribution to understanding of ice streams. It can be published essentially as is with the option to make minor modifications.

Below are more specific comments on details in the presentation and answers to specific questions addressed to reviewers.

SPECIFIC COMMENTS ON TEXT

Pg 24 lns 1 to 24: Add some more about relaxation time scales?

Pg 24 lns 1 to 22: The Abstract may undersell the paper.

Pg 25 ln 6: Suggest “as compared“ => “compared“

Pg 25 lns 6 and later: Is “vertical shear stress“ an adequate description? Consider “surface-parallel shear stress“ as a possibly better alternative. Please also consider using the singular “stress“ rather than the plural “stresses“ in many places.

Pg 25 ln 22: It would be good to also state the corresponding order of error for the SSHEET approximation in its usual application?

Pg 26 ln 15: How about a reference to the SSHEET approximate solutions, or are new ones derived in this paper?

Pg 26 ln 17: It would be nice, but not essential, to conclude the Introduction with some comment on how the results might guide practical modeling applied to actual ice sheets.

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Pg 26 ln 6: “u and v“ => “depth-independent horizontal velocity components u and v“

Pg 26 lns 6 7: Omit “The horizontal — over depth, and“ or possibly some other arrangement with the previous comment.

Pg 26 ln 11: “media“ => “medium“

Pg 27 lns 16 to 19: This specific time history may seem puzzling to some readers without a little motivation.

Pg 28 Eq 9: There is an implicit assumption of no bed profile change from bed deformation or erosion. It is standard but should be explicit especially for soft bed ice streams where possible the assumption could fail.

Pg 29 ln 2: “using“ => “used“

Pg 31 ln 10: “a spatial variation“ => “spatial variation“

Sections 2.1 and 2.2: I found the imbalance in detail for δ_b versus δ_c developments in the main text versus appendix sort of artificial (arbitrary?). I wonder whether it would be worthwhile to rebuild 2.1 with some of it in an appendix. Similarly have a little more in 2.2, e.g. the boundary conditions at least. The later discussion of δc in section 3.3 is quite extensive and interesting and it does not seem a wholly secondary part of the paper.

Pg. 34 ln 7: Omit “to this“

Pg. 35 ln 15: Actually the reference to Eq. (30) alone does not allow the stated comparison. A reference to a corresponding equation for FS is needed.

Fig. 1 caption: Add to end of first sentence: “for n=1, m=1“ (I think that similar info should be added to many relevant following figure captions as well. Complete captions are helpful, since it is difficult to back track to find it in the text.)

Pg. 35 lns 20 to 22. Having just looked at Fig. 1a (for C=30), the reader may be a bit

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confused by this statement which becomes true for very large C.

Pg.36 ln 15: “closer to the exact FS solution“ is sort of an understatement since SSHEET does not look anything like the “exact FS solution“.

Fig.3 caption: “ and t_r in years“ => “, and t_r is given in years“

Pg. 37 ln 22: How about “Bed topography perturbations“? (instead of “rock“ variations)

Pg. 39 ln 24 to ln 4 next page. This discussion refers explicitly to SSTREAM solution and not the real one. Be clear about that in this text.

Pg. 40 ln 17: Is there a particular significance to this inverse time?

Pg. 40 ln 18 “Where the transfer amplitudes are equal to, lets say 50,“ => “For example, a transfer amplitude of 50 with“

Pg. 40 ln 20: Omit “large“

Pg. 40 ln 21: Do you really mean “mean“ surface velocity? Isn’t this still the amplitude of the fluctuation?

Pg 41 ln 15: This 3.2.1 is the first subsection of section 3.2. Furthermore, it is short and the only subsection of 3.2. There is whole lot of stuff between the beginning of the section 3.2 and the beginning of this subsection. I suggest that all of the earlier 3.2 stuff could be one or more subsections. Or was 3.2.1 intended to be 3.3, reflecting a Latex error in the section level command?

Pg 42: The low dependence of T_{sb} on the sliding power m is related to the low effect of low effect of c variations on S? Or the low effect on w (Fig. 11 and discussion further on)?

Fig. 10 caption: I find the second to last sentence confusing! Are we talking about 4 SSTREAM curves or what? I suggest “the two SSTREAM curves“ => “the SSTREAM results“

Fig. 10: It would be useful to have an interpretation of what the scale (unit) on the vertical axis means.

Pg 43 ln23: Same as above. What is 1/2?

Pg. 45 ln 10: “difference between T_{ub} the T_{wc}“? Do you intend “the“ = “and“?

Pg. 45 lns 20 and 21: I am not sure what the message is here.

Pg. 45 ln 22: I suggest seeking a better title for section 3.4 that is descriptive of the calculation and refer to the coded name for the Benchmark experiment in the text.

Pg. 46 ln15: “qualitatively“ => “qualitative“

Pg 48 lns 4 to 6: Perhaps you mean the only previous systematic investigation. Work by MacAyeal and others before 2004 did use the SSTREAM theory to do inversions.

Pg. 51 ln 1: “equation“ => “equations“

Pg. 51 ln 4: “system of Eq. (A4)“ => “system of Eqs. (A2) to (A4)“

Pg. 51 ln 9: Check the equation defining T_b which appears to define a vector normal to the bed.

ANSWERS TO SPECIFIC QUESTIONS TO REVIEWERS

1) Does the paper address relevant scientific questions within the scope of TC? Yes, important concepts and quantitative characteristics about ice stream flow

2) Does the paper present novel concepts, ideas, tools, or data? Yes, a new mathematical solution and prediction of some complex properties of ice stream flow.

3) Are substantial conclusions reached? Yes.

4) Are the scientific methods and assumptions valid and clearly outlined? Yes.

5) Are the results sufficient to support the interpretations and conclusions? Yes.

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6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes.

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes.

8) Does the title clearly reflect the contents of the paper? Yes, but in a way it under sells it.

9) Does the abstract provide a concise and complete summary? Yes, except for one addition that I suggest above.

10) Is the overall presentation well structured and clear? Exceptionally for a highly mathematical paper.

11) Is the language fluent and precise? Yes. I have only a few editorial comments above.

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes.

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Possibly. I suggest consideration of a small reorganization with regard to main text and appendix.

14) Are the number and quality of references appropriate? Yes.

15) Is the amount and quality of supplementary material appropriate? NA

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