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Earth Surface Dynamics Discussions

Interactive comment on "Basal shear stress under alpine glaciers: Insights from experiments using the iSOSIA and Elmer/ICE models" *by* C. F. Brædstrup et al.

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Reviewer 1

In this reply we comment on all remarks given by the reviewer and present the associated changes to the manuscript. The comments from each review have been copied into this document in grey and are marked with C for comment and a sequential number. The corresponding response is marked with R. In this reply we comment on all remarks given by the reviewer and present the asso- ciated changes to the manuscript. The comments from each review have been copied into this document in grey and are marked with C for comment and a sequential num- ber. The corresponding response

is marked with R.

C-1.1: There are two parts in this paper. The first part objective is to investigate the difference in basal shear stress from two models, one solving the Stokes equations (Elmer/Ice) and the second based on the 2nd order shallow ice approximation (iSOSIA), assuming the same glacier geometry. In the second part, using the iSOSIA model only, three different friction laws are compared on transient simulations accounting for bedrock erosion. The first part is used as a "validation" of the lower order model for the second part. My main concern is on the way the two models are compared using vertically averaged velocity and stress, which looks not correct. For erosion, because processes take place at the interface between the ice and the bed, the quantities should not be vertically averaged, but instead one should take care to use the local values estimated at the bed/ice interface. I therefore not understand the necessity of averaging the velocity and stress from Elmer/Ice for the comparison with iSOSIA. Moreover, I suspect that by doing so, the differences between both models are decreased. The reverse should be done instead: the iSOSIA bedrock velocity and stress should be evaluated (this is always possible from a vertically integrated model to estimate the 3D velocity field, and then the 3D stress field), and the comparison conducted using velocity and stress at the bed.

R-1.1: We thank the reviewer for the constructive comments. Based on the comments, we have modified the text and figures, and we feel this has improved the manuscript significantly.

The first comment is, however, based on a misunderstanding. Basal shear stress and basal sliding velocity are not, as understood by the reviewer, computed from depth-averaged properties. In both iSOSIA and Elmer/Ice, we extract the full Cauchy stress tensor at the bed, and use this to compute the bed-normal and -shear stress components from Eqn. 11.

Only the creep velocity shown in Fig. 1d and the horizontal longitudinal and transverse stress components in Fig. 2a are obtained by depth-averaging in both models. In Fig. 1d we can show the surface velocity instead, which would obviate depth-averaging of the creep-velocity. However, for the horizontal stress components, we can only compare depth-averaged values, as their depth-variation cannot be reconstructed from the iSOSIA output. We note, however, that focus is on the basal properties (bed shear stress and sliding rate, which are not based on depth-averaging anything in Elmer/Ice), and the horizontal stress plays a more indirect role in this study.

We realize that the text in section 2.4 (Comparing the output of iSOSIA and Elmer/Ice) has caused the misunderstanding. We have therefore rephrased text in this section to make it clearer that depth-averaging is not used for computing subglacial stress and sliding.

C-1.2: all along the manuscript, Elmer/ICE should write Elmer/Ice (see e.g. Gagliardiniet al., 2013).

R-1.2: Elemer/ICE has been changed to Elmer/Ice throughout the manuscript.

C-1.3: title: the title is a bit restrictive to the first part of the paper. You might think to a more general one that would include both objectives of the paper.

R-1.3: The title was initially more general: "Glacial dynamics in response to glacial erosion". However, after advise from the associate editor, we changed this to specifically highlight the comparison study between iSOSIA and Elmer/Ice. We prefer the present title because it so clearly signals the study's focus on 1) basal shear stress and 2) computational models.

C-1.4: p. 1144, l. 13: suble should be subtle?

R-1.4: Done

C-1.5: p. 1145, l. 16: These shear stress values should really be seen as mean value over a relatively large distance (>10m) as we know that, induced by cavitation for example, stress might concentrate at much higher values (e.g. Gagliardini et al., 2007), and that this stress concentration might play a key role in glacial erosion.

R-1.5: This is true. We have rephrased the sentence to reflect this.

C-1.6: p. 1148, 2.2: it should be mentioned if iSOSIA is a finite-element or finite-difference model.

R-1.6: The iSOSIA implementation used here is a staggered-grid finite-difference model as explained in the second paragraph of section 2.2.

C-1.7: p. 1148, l. 18: Stoke should write Stokes

R-1.7: Done

C-1.8: p. 1150, l. 2: the elevation used to compute the local temperature should not be bedrock elevation but the ice elevation when the bed is ice covered.

R-1.8: We use bedrock elevation in the mass-balance equation in order to avoid that a difference in sliding velocity, and hence ice thickness, influences the mass-balance. A constant and identical mass-balance function results in more transparent experiments, where secondary effects related to mass-balance do not mask the variations in stress caused by different sliding and erosion laws. We explain this in the paragraph below Eqn (3), and we have now strengthened this paragraph to more clearly motivate our choice of mass-balance function.

C-1.9: p. 1151, Eqs. (6) and (7): what does justify the choice of an exponent 2 for the Weertman and Empirical sliding laws? In absence of cavitation, the exponent in the Weertman sliding law should be the Glen's exponent, so 3. I would suggest to adopt a different notation for Cs as the values are different for all three laws.

R-1.9: A stress exponent of 2, or (n+1)/2, is in agreement with the model proposed by Weertman in 1957. For the empirical sliding law, exponents of both 2 and 3 seem to be commonly used. However, we agree with the reviewer that it makes sense to change the exponent from 2 to 3, also to increase the difference between the Weertman law and the empirical law (See also R22 below). The model experiment using the empirical sliding law (in experiment 3) has therefore been repeated (for erosion exponents of 1 and 2), and Figs. 8 and 9 have been updated.

C-1.10: p. 1152, l. 14: extruded is may be more adapted than expanded. Also the number of vertical layers should be specified.

R-1.10: This is a good suggestion. We now use extruded instead of expanded. The sentence already specifies the five vertical layers.

C-1.11: p. 1152, Eq. (10): doing the comparison on vertically averaged values is not correct (see main point).

R-1.11: See response to main point $R1$

C-1.12: p. 1153, Eqs. (11) and (12): "x" should be replaced by "·".

R-1.12: Yes, we agree. Done

C-1.13: p. 1154, l. 20: I would suggest to plot relative difference instead of absolute one.

R-1.13: We did try this, but we found the result to be misleading, mainly because areas of very low stress along the glacier margins result in very high relative errors (i.e. large difference of a very small number). A plot of absolute difference allows the reader to assess the actual error. The reader can then estimate the level of the relative error without the bias of small numbers by comparing the levels of the stress difference to the levels of actual stress.

C-1.14: p. 1155, l. 18: remarkable should be remarkably

R-1.14: Thank you. Done

C-1.15: p. 1157, 3.3: Some explanation should be given on the way the sliding law parameters have been chosen. Did you try to get similar velocities for the initial geometry? Similar final geometries?

R-1.15: Yes, we calibrated the constants to give a similar mean sliding velocity. This is now explained in section 3.3

C-1.16: p. 1158, l. 4: m = 1 is in contradiction with what is specified in the Legend of

Fig. 8 (m = 2). This should be corrected. If m = 2 in this experiment, then the sensitivity of the erosion exponent is not studied. You might think adding an experiment for all 3 friction laws with m = 1 (which must exist as you have results plotted in Fig. 9).

R-1.16: All experiments have been performed for both m=1 and m=2. We have modified the text to make this clear.

C-1.17: p. 1158, l. 12: I would suggest to use equation instead of rule.

R-1.17: Good idea. Done.

C-1.18: p. 1161, l. 5-16: this is an important point which is discussed here, but I think it should not restricted to the Coulomb-friction law only. The parameter in all 3 friction laws would evolve if the bedrock topography evolve, but this is true that it is certainly at a sub-grid scale.

R-1.18: This is another good point. We have expanded the discussion to include the other sliding laws as well.

C-1.19: Figs. 7 and 8: For an easier comparison, the output should be produced for the same stages of glacial erosion (20, 60, 80, 100 for example).

R-1.19: We agree. The figure has been updated.

C-1.20: Table 1: "yr" should be "a"

R-1.20: Done

C-1.21: Fig. 9: does it make sense to use normalised mean velocity here as the erosion is function of the absolute value of the velocity. At least, it should be mentioned how different are the mean velocity for the 3 friction laws at the beginning of the experiment.

R-1.21: We have updated Fig. 9 to show absolute values. The trends are similar, but we agree that the absolute values add relevant information to the figure.

Interactive comment on Earth Surf. Dynam. Discuss., 3, 1143, 2015.