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The Cryosphere Discussions

Interactive comment on "How accurate are estimates of glacier ice thickness? Results from ITMIX, the Ice Thickness Models Intercomparison eXperiment" *by* D. Farinotti et al.

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General comments:

This paper presents a comparison of models which compute thickness of glaciers and ice caps from surface observations. Ice thickness estimation from surface information is a classical problem in glaciology, but it is still very important and unsolved issue. Several algorithms have existed from earlier time, and more approaches and applications have been proposed and tested for the last 10 years. Such recent development benefits from growing amount of satellite derived data (e.g. DEMs and velocity map). Because increasing number of studies are presented in this field, I find it very important and timely to call an experiment to compare performance of existing models.

The authors designed an interesting experiment. The selected 18 glaciers have a large variety of geographical and geometrical settings, which enabled the authors to analyze the robustness and weakness of each model. In addition to the real existing glaciers, synthetically generated glaciers are included in the test cases, which is useful to study the physics behind the models. The authors also made a fairly good job in presenting a large amount of data produced by 17 different models. I particularly acknowledge their effort to use the ice thickness data as much as possible to evaluate and rank the model results. Ranking models is tricky, but this paper follows a quantitative criteria, and the descriptions are careful and modest.

This kind of model comparison is useful for researchers in the field because it helps to (1) evaluate the accuracy of each model and (2) to find direction to improve the models. Moreover, it is beneficial for researcher not exactly in the field because it provides (3) insight into the robustness of the modelling in general and (4) overview of the modelling approaches and techniques. In my opinion, the paper is well done in terms of (1)-(3) and a little more effort is needed to improve (4). Otherwise, the paper is well constructed and carefully written. I list several points, hoping the paper becomes more readable and useful for readers not in the field of ice thickness modeling.

1. Classification of the models The models are classified into four approaches (1. mass-conservation only, 2. mass-conservation and momentum equation, 3. shallow ice approximation, 4. artificial neural networks) (line 109-112). However, the descriptions on the models are not grouped nor ordered according to the physics behind. Moreover, no detailed explanations are given for these four approaches, and the model descriptions are mostly on technical issues. This makes it difficult to distinguish the models, and follow the interpretations on the model comparison results. I suggest the authors to elaborate on Section 4, so that physical basis and fundamental equations of the four approaches are given, and model descriptions are ordered and structured in terms of the approach. Once basic information and equations are given, each model description can refer the information to explain how they solve the problem and what is

the difference from other models using the same approach.

2. "unpublished" models Several models are "unpublished". Because details are not explained, results from these models are difficult to evaluate and increase uncertainties in the overall discussion. If there is no written material available as a reference, details of the models should be provided in Supplementary Material. Without knowledge of "how they computed the model", it is not much useful to include the results into the comparison and statistical treatment.

3. Averaging the model results One of the conclusions of the paper is "accuracy of the ice thickness estimation improves by averaging results of different models" (e.g. line 5-7, 396-400 and 554-556). Given the large variations over the models, I found this argument too simplistic. It would be OK, for example, if all the models rely on the same physics and only detailed treatments are different. In this case, models are classified into four groups and the number of models in each group are different. I wonder if the same argument stands if models belong to one of the groups are averaged. Otherwise, does it mean all of the approaches are equally wrong but to different directions? To get into this point, classification of the models and understanding of the difference between the models are important. I hope to see more logical interpretation on why the accuracy improves after averaging.

4. Rate factor A I understand that each model used their own value for the rate factor A. Since this is a crucial parameter for ice thickness estimation, I suggest the author to indicate the value for each of the models. I am also concerned about the use of constant A for cold and temperate glaciers. "Huss" applies a temperature dependent A (line 233), but not clear for the others. I also wonder if this value is taken as a tuning factor in some models. Can you provide more information when you describe the model approaches in Section 4?

Specific comments:

line 9: "sensitivity to input data consistency" » I understood this only after reading the

manuscript. Please consider to rewrite.

line 34: ". . . inverse modelling of ice thickness from glacier ice flow ..."?

line 35: "additional properties" » What exactly? Basal slipperiness? something else?

line 38: "a rapid pace:" » "a rapid pace."?

line 70: Can you describe more about the sample glaciers and ice caps? For example, how many calving glaciers? Mean, maximum and minimum glacier length and area? Temperate of cold? Can you also describe here, why ice sheets are excluded from the experiment?

line 82: "Consistent glacier-wide estimates . . ." » What exactly do you mean by "consistent"?

line 84: "separate tiles" » Not very clear

line 94: "accumulation and ablation zone" » "accumulation and ablation zones"?

line 98: "suitable size and shape" » Do you mean the model did not reach a steady state?

line 124: "non-physical behavior incompatible with ..." » What do you mean?

line 140: I suppose the rate factor A is a very important parameter for estimating ice thickness from velocity. I was a little surprised to see different value was used in each model. Are they tuned in each model? I suggest to indicate the values used for A for all the models.

line 141: "effective mass balance" » What exactly is this?

line 249: "designed for alpine glaciers" » What would be a problem for ice caps? Too flat surface geometry? How did you compute thickness when surface slope is zero?

line 315: "vertical gradients" » What is this?

line 333: "constant forcing" » Do you mean constant SMB?

line 340-344: "Requirement of input data" is a fundamental feature of the models. This should be explained in the previous section and used to classify the models.

line 343-344: "the average composite ice thickness (i.e. the composite of the local average thickness computed from the ensemble of provided solutions)" » Is this the average of all provided thickness for a certain grid point? It is not clear and confusing.

line 344-3435: "local ensemble spread" » Is this a technical term? If not, please define more precisely. Something like "range of computed ice thickness variation at each grid point"?

line 371: "possible reason are" » "possible reason is"

line 392-394: I wonder if the ice thicknesses computed by the models are independent because the models are classified into several approaches.

line 405: "branched nature" » If this was the reason, larger deviations are expected near the confluence area, but it appears not.

line 453-462: I am a little surprised at this relatively short discussion on the comparison with the volume-area scaling method. Since volume-area scaling is commonly used for large scale ice volume estimation, its accuracy and reliability is a big concern of the readers. Isn't it useful to elaborate more on this issue, for example, by changing the parameters c and gamma?

line 463: "average composite solution provided by the ensemble of models" » This is not very clear expression.

line 551: "The relative low..." » "The relatively low..."

line 572: "data were known precisely" » In my opinion, the synthetic glaciers were better reproduced by the models because the glacier thickness is distributed as expected from our knowledge and this knowledge is used in the models. In this regard, "precisely

known" may not be a primary reason why the performance was better in the synthetic cases. I agree that accurate and more complete data sets are important for the models, but the logic here is a little problematic.

Figure 7: This plot is interesting, but very difficult to read useful information. I understand that this plot can be replaced by a set of plots showing the results of a model applied for all the glaciers (Figures S1 and S2), and another set of plots showing the results for a glacier computed by all the models. If you use Figure 7 in the paper, can you provide the latter set of plots in Supplementary Material?

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