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Interactive comment on "Influence of anisotropy on velocity and age distribution at Scharffenbergbotnen blue ice area" *by* T. Zwinger et al.

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Review comments on "Influence of anisotropy on velocity and age distribution at Scharffenbergbotnen blue ice area" by T. Zwinger and others.

1. General comments

This paper presents numerical modeling of ice flow conditions of the blue ice area at Scharffenbergbotnen. Modeled ice flow fields are used to compute ice age distributions over the surface and the results were compared with field data. Because the model results did not show reasonable agreement with observed ice velocity and age, additional experiments were performed with prescribed anisotropic ice fabrics. As a

result of these experiments, the authors concluded that ice anisotropy plays a key role in the ice flow regime and ice age distributions in the studied region.

This is an interesting and valuable work which applies a "state-of-the-art" ice flow model to a very complex ice flow field in Antarctica. The Scharffenbergbotnen is a deep and narrow valley surrounded by nunataks, where ice flows into the valley, emerges to the surface, and forms a blue ice area. The authors employed a full Stokes high resolution model (Elmer/Ice), which is able to take into account higher order stress terms arise from complex bed geometry. Relatively abundant field data are available in the studied region and they are used for the validation of the modeling. Such approach is useful for testing the performance of the latest modeling techniques and also for planning field measurements and sampling in the future. The first author has been leading the development of the Elmer/Ice model and working on many other glaciers using the same model. Thus, the modeling results are thought to be reliable.

Because of the reasons above, the presented work meets the interests of the journal readers and details are beneficial for those engaged in this field of cryosphere science. The manuscript is generally well written in the introduction and methodology sections. In my opinion, however, interpretations and discussions on the modeling results are not sufficient in the present form. Specifically, I suggest the authors to compare the results with field data more carefully and discuss other possible factors missing in the model. Figures are nicely prepared, except for several points which should be revised for clearer presentation.

Here, I provide several concerns, specific comments to each part of the text, and technical corrections. I encourage the authors to improve the manuscript, considering the comments listed below.

2. Major concerns

(1) Comparison of the modeling results with field data

Computed ice velocity and age are compared with field observations and ice core data. The authors concluded that the isotropic experiment was not able to reproduce the observations, whereas the results of the anisotropic experiments showed sufficiently good agreement with the observations. However, these comparisons were not performed in a quantitative manner. Figures 2, 3, 6 and 7 are the basis of their argument, but quantitative analyses were not performed. Because the authors have the details of the velocity and age data (Tables 1 and 2), it is not difficult to compare the data one by one to compute statistical values. Because errors are expected to be large in the ice age data, the comparison should be made more carefully with consideration of error bars. Scattered plots of computed and field data with error bars may be helpful for the readers to understand how much the modeling results agree/disagree with the field data.

(2) Interpretations of the insufficient results from the isotropic model

The authors concluded that anisotropic ice rheology is the reason why isotropic model failed to reproduce the observed ice velocity and age. I found this argument is too strong as compared to the evidence provided in this paper. The agreement with the field data was better in the anisotropic experiments. However, the single maximum ice everywhere in the glacier is a highly unlikely assumption, and thus it also implies the field data are not reproduced by a likely anisotropy distribution. I understand the assumption used for Figure 7a is a more realistic assumption, but the results disagree with the data. The authors state that other sensitivity tests were performed by tuning enhancement factor, surface temperature, and geothermal heat flux, but their results are not shown. This is pity as I expected changes in ice temperature might have a large influence on the results. I also suspect the uncertainty in the bed geometry affects the englacial velocity field. Surface elevation change in the past is also relevant to the ice age. Old ice near the bed might have emerged to the surface after the change in the surface elevation. Additional experiments with 100 m thicker ice may give insights into such possibilities. These are all my speculations, but still possible interpretations. I

encourage the authors to perform and show the results of sensitivity tests by changing parameters possibly influence the ice velocity and age. Unless strong evidence is shown, I suggest the author to be modest about the conclusion. It is interesting enough to propose the anisotropy as one of the important factors in the studied region. Listing other possible factors is also beneficial for those work on similar problems in the future.

(3) Interpretation of the results

The results presented in the section 5 contain interesting and important information related to the ice flow and temperature regime in the blue ice area. Unfortunately, the details of the modeling results are not explained very well except for surface horizontal velocity and ice age. I suppose many readers, including me, are interested in other aspect of the results. For example, vertical component of the surface velocity can be compared with surface mass balance measured in this region. Englacial temperature field under the influence of complex ice flow regime and bed geometry is also worthy of discussion. Ice flow field obtained by the anisotropic fabric also needs more interpretations because the influence of anisotropy is not intuitive. Single maximum fabric resists to deformation to one direction (or stress regime), but it deforms more to the other directions (or stress regime). Significantly slower ice motion obtained with the anisotropic fabric can be discussed with englacial stress and strain regimes.

3. Specific comments and Technical corrections

Title: » This title gives a strong impression about the role of the anisotropy at Scharffenbergbotnen. In my opinion, a more general title something like "Numerical modeling of ice velocity and age distribution at Scharffenbergbotnen blue ice area" suites the paper as it stands.

page 3060, line 10: » . . . and too slow vertical ones "obtained in the model".

page 3060, line 15: . . . both two-dimensional and three dimensional flow models. » Use of the two models should be mentioned earlier?

page 3061, line 11: The with respect to . . . » Something wrong?

page 3063, line 4: the outside ice-sheet » Not clear where it is.

page 3063, line 6-8: As the elevation . . . » Is this a well-established idea? Any citation?

page 3063,line 14: ...increases towards the center of the valley. . . » Increases from where to the center of the valley? What do you mean by "the center of the valley"?

page 3063, line 25 complete slate-blue » This is very difficult to find. Text labeling on the map is helpful.

page 3063, line 29: . . . including the band . . . » What is "the band"?

page 3064, line 9-10: . . . before the full impact of . . .. » Not clear what is meant.

page 3064, line 27: to follow the deepest surface incline path » Do you mean something like "along the direction of the maximum inclination"?

page 3065, line 16: varied by about -9 to +3K » citation?

page 3067, line 3: collinearly » It sounds strange to me because stress-strain relationship is nonlinear in Glen's flow law.

page 3067, line 7: » second invariant of the strain rate "tensor".

page 3067, line 22: Martin and Gudmundsson (2012) » The publication year disagree with the reference list.

page 3069, line 18-19: mean average temperature » What do you mean?

page 3070, line 2-3: . . . pointing against the outward facing surface normal Eq. (13) » pointing into the glacier?

page 3070, line 7: freeze-on condition for . . .... all over this boundary. » fixed boundary condition (u=0) at the bed?

page 3071, line 3: By the nature of the problem, » This is not clear.

page 3071, line 3: . . . the first of these items are . . . » . . . the first of these items is . . .?

page 3071, line 7: » Delete one of "the".

page 3071, line 13: » Delete "and"?

page 3072, line 4: 4.1 Prescribed anisotropy » It's odd to have only one subsection in section 4.

page 3072, line 12: vertical flow depth » Not clear to me. Do you mean somewhat "vertical displacement from the initial deposition on the surface"?

page 3074: 5.2 Simulations using 2-D flow line model » Figure 4 and 5 show nice plots, but they are not well explained in this section. Each panel is even not referred in the text (Figure 4a, Figure 4b, . . .). I would like to read more details about the results by pointing out specific part of the figures. For example, the authors state that "they now even underestimate the observed flow speeds in places." (line 14-15), but not clear where and how much the results underestimate the observations .

page 3074, line 7: naturally, as at the surface . . . » Why do you think it's natural? Usually, deformation near the bed is more important than that near the surface.

page 3074, line 9: much flatter » Not clear.

page 3074, line 11: . . . more towards the lower end of the valley . . . » Do you mean "down the valley"?

page 3074: 5.3 Simulations using prescribed fabrics in 3-D » I have the same impression as I wrote above for the section 5.2. I suggest the author to explain the results more in detail and compare them with the field data in a quantitative manner. For example, the paragraph starting from line 25 can be improved by giving numbers instead of "slightly lower" or "slightly too slow".

page 3075, line 9: Fig. 7 » Fig. 8?

page 3075, line 12-13: (as stream lines are not expected to cross surface moraines) » Why do you expect not? Why does the model compute such an unrealistic result?

page 3075, line 17-19: i.e., the resulting physical time . . . » I think this is unnecessary rewarding.

page 3076, line 26: Fig. 7 » Fig. 8

page 3077, line 12-14: Strong fabric development . . . » These two citations report ice fabrics at very different conditions from the study site of this paper. They are not convincing evidence of strong anisotropy near the surface of blue ice area.

page 3077, line 17-19: Even if . . . » I cannot follow this argument. Why can it evolve rapidly?

page 3078, line 3: . . . non-linear evolution of the valley . . . » What kind of evolution?

page 3078, line 25-26: This suggests that the BIA was formed . . . » I cannot follow this argument. Which part of the text discusses this point?

page 3080, line 12: » Please check the publication year, 2005 or 2003?

Figure 1: » This plot is beautifully drawn, but very difficult to read when it is printed. It should be large enough to read the details when it appears in the final paper.

Figure 4 and 5: » What are the three color dots at the upper right corner of each panel?

Figure 5a-c: » I understand these are results of the 2D model. If yes, the texts at the upper right corner are not correct.

Figure 5 caption: Comparison of absolute velocity . . .. » Comparison of absolute "horizontal" velocity . . ..

Figure 8: » This plot is also difficult to read when it is printed. It should be very large or needs some modifications. The numbers for the color code "-5000 -4e+4 -2e+4 0" should be "-5000 -4000 -3000 . . .".

Interactive comment on The Cryosphere Discuss., 7, 3059, 2013.