

## ***Interactive comment on “Performance and applicability of a 2.5D ice-flow model in the vicinity of a dome” by Olivier Passalacqua et al.***

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[final]pdfpages This manuscript presents a study the performance of a 2.5D model versus a full 3D model and its applicability in the vicinity of a dome. Ice flow is complex and boundary conditions are not easily parameterized and well constrained by observations. Ice flow is described by a set of thermo-mechanically coupled non-linear

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differential equations and the numerical solution of these equations is computationally very demanding. Simulations of ice flow are often done on simplified systems, and a commonly used approximation is to reduce the 3D set of equations to a 2.5 flowline version. Investigating the applicability and performance of these models is therefore an interesting contribution to the community.

It is important to note that this test has not been done before. One reason is that only recently complex models that solves the full set of stress balance equations have become available to do the test. These models are still so computationally demanding, however, that simplifications of the equations are required for many purposes.

The study is focused and well-structured. The model is presented, both the continuum mechanically based set of equations and boundary conditions as well as the numerical implementation. The set of equations are presented without further references and arguments for the choices of model parameters. It is clear that the model is run for Antarctic conditions (temperature conditions), but this is not mentioned. A little information on the choices of model parameters and possible effects would clarify (from the simplified temperature, and the chosen temperature regime).

Authors' answer: **We added some information about our global frame (our final goal is to work on a small dome in Antarctica), and we are more specific on the effects of temperature on viscosity.**

More details on how  $R$  is determined from the DEM using a scanning window are needed, for example - how is the fit done, - explain that  $R$  is not constant within the window. . . . How  $R$  is determined is a critical parameter, for example the size of the scanning window, and the details of how this is done should be clearly described.

Authors' answer: **More information is given concerning the determination of  $R$  during our twin experiment, especially what we expect from a small or a large window. In fact, the question is what is the typical distance which influences the local ice flow.**

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The effects when moving from 2.5D to 3D are complicated and result in surprising effects. It is surprising to see how the uncertainty of the radius of curvature for a small scanning window completely dominates in figure 5. It is very interesting to see the distribution of horizontal velocity fields for the non-isothermal case (figure 7). In 2.5D these variations may lead to spurious effects if used to model internal layers within the ice. A short paragraph should be included (introduction and/or conclusion) to mention this and thereby emphasize the significance of the results presented in the manuscript.

Authors' answer: **The issue of modelling the internal layers, which is indeed a possible goal of using such a model, is now addressed in the conclusion.**

The manuscript only considers 2.5D flow along straight lines. Sometimes 2.5D models are being used along curved flowlines, and neglecting the curvature of the flowline would add further to the uncertainties. It would be difficult to say something general about curving flow lines, so I do not suggest further studies, but the problem with curving flow lines should be mentioned in the manuscript.

Authors' answer: **This issue is now mentioned in the conclusion as well.**

I do not understand the comparison presented in section 4.4. The mass-only conservation model is not explained in detail, and does not add further to the conclusions. I am also suspicious about the boundary effects near  $x = 15000$  m. They are not discussed but clearly influences the solution. I suggest that this section is removed.

Authors' answer: **As both referees suggested to remove this section, we removed it.**

There are several examples of incorrect use of English (e.g. order of words in a sentence), and I suggest that the manuscript is carefully worked through to clarify the text. The structure of the manuscript is well planned, and overall the manuscript appears clear and with a logical flow. The figures are clear and well presented.

To conclude, I find that the manuscript is relevant and provides a needed insights into the applicability of 2.5D models. The results can help clarify the performance and

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limitations of these models, which has not been systematically done before. The results also demonstrate that full stress solutions are needed near domes and divides to fully represent the flow. I recommend that the manuscript is published with minor changes mentioned above, as well as a thorough correction of the use of English in the text.

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