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Interactive comment on "A high-resolution bedrock map for the Antarctic Peninsula" *by* M. Huss and D. Farinotti

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Abstract: I do not think that the model used to derive a gridded bed topography for the Antarctic Peninsula region is adequate.

As an important motivation to perform this study, the authors state: "Thus, for advancing the development of ice flow models used for predicting the future response of Antarctic Peninsula glaciers to climate change and/or ice shelf break-up, a high-resolution bedrock topography is required as an essential geometric constraint". I wonder what can be the scientific strategy behind this statement. If models are going to be used that are more complete than the one used here (e.g. including normal stresses, thermodynamics, more sophisticated sliding law), what could be the outcome? That a model does not perform well because it produces an ice thickness distribution which

differs from the one generated by a more primitive model ? I really do not understand the point.

The method proposed here (using a SIA model to estimate thickness from surface elevation) has been used since a long time (e.g. Budd and Allison, 1975; Kruss and Smith, 1982; Oerlemans, 1997). Some kind of acknowledgement would have been appropriate.

For the ambitious goal set in this paper, the model is just not adequate. Too many rigorous assumptions and calibration factors are needed to derive an ice-thickness distribution. In an environment like the Antarctic Peninsula, a great deal of variation in ice thickness is related to thermodynamics and changes in basal conditions. The ad-hoc assumptions used to capture a part of this (e.g. whether sliding is possible or not) by 'global' calibration factors do not lend confidence to the resulting product.

The limitations of the method described in this manuscript motivated Van Pelt et al (2013) to use a 3-dimensional thermomechanically coupled model with normal stresses and a more sophisticated sliding law. By means of an inverse method, in which the model is run in a time-dependent mode again and again until convergence, an ice thickness distribution can be derived that does justice to the effect of many more physical processes than included in the approach of Huss and Farinotti. I therefore strongly suggest that the authors repeat their work with a more sophisticated model. Several open-source models are now available that can readily be used. Using a SIA model without any thermodynamics for the ambitious goal defined here is really out of date.

References:

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