

Interactive comment on “Past Ice-Sheet Behaviour: Retreat Scenarios and Changing Controls in the Ross Sea, Antarctica” by A. R. Halberstadt et al.

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Received and published: 8 March 2016

Halberstadt et al - Cryosphere

The submitted manuscript presents new and legacy marine geophysical data from the Ross Sea, Antarctica, and uses this to reconstruct the pattern of flow both at the LGM and during its retreat. The paper is very well-written and well illustrated, with clear conclusions that are robust with respect to the data presented. I have no problem in recommending that the paper be accepted with only a few minor edits.

My only gripe really is that there has been quite a lot of Ross Sea work published recently, and not all of it is acknowledged here. This is unfortunate, because the papers

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I'm thinking of lend considerable support to the interpretations presented by the present authors and so would nicely bolster their arguments. In particular it would be good to acknowledge McKay et al., 2016 (Geology), who came to very similar conclusions based on different data.

Clearly I have a bias in this regard, but I think it would be good if the efforts of the modelling community were also acknowledged. It is often stated in introductions to 'empirical' studies that the new data will help 'constrain numerical models', indeed, the current authors do this in the very first sentence of the Abstract. But what is the point of modellers using these geological data, if the models they produce are then disregarded? Maybe sometimes the modelling can help with the geological interpretations, rather than the other way around.

There are of course many modelling papers out there, but I know for a fact that Golledge et al., 2012, 2013, and 2014 all mention that retreat most likely started first in the deeper parts of the outer Ross Sea, and that the pattern of retreat was a product of incoming fluxes from both EAIS and WAIS, and was highly dependent on the location of bedrock highs. To illustrate my point, I'm uploading a figure showing the modelled grounding-line positions from the simulations published in McKay et al 2016, overlain on Figure 7 of the submitted paper. Personally I see a considerable amount of agreement there, which is gratifying because it means the models are getting something right!

Anyway, I'm not insisting that the authors have to cite all these papers, but it would be nice to 'close the loop' in a sense and recognise that sometimes synergies between modellers and empiricists can allow a convergence of views that together really show how flawed the 'swinging gate' model is.

Other than this, I can't really find fault with the paper, so I commend the authors for doing a great job pulling the data together and hope to see this published soon.

N R Golledge 8th March 2016

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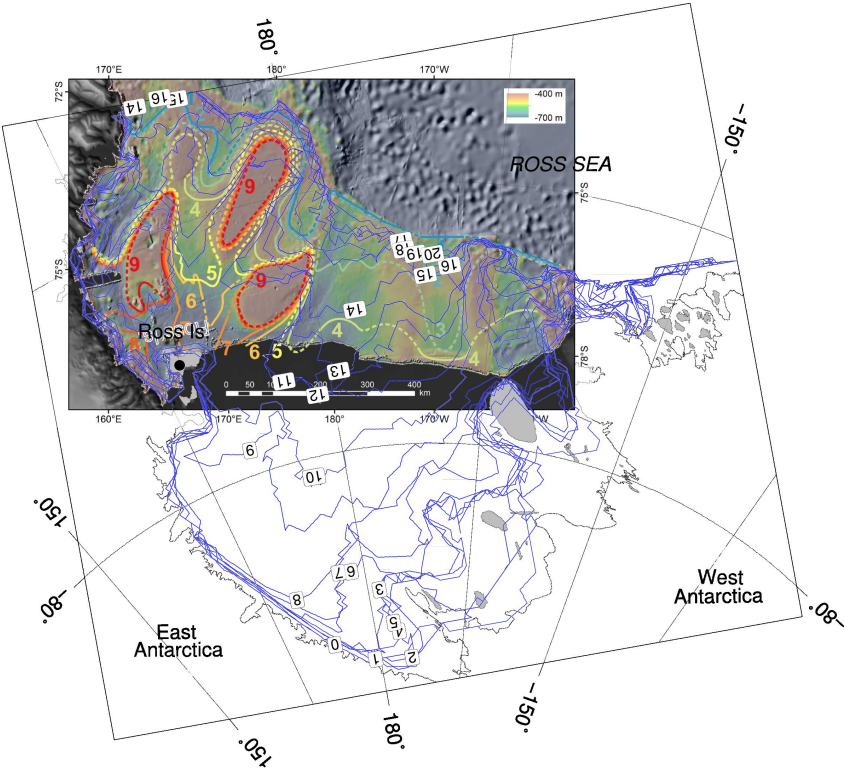


Fig. 1. Modelled GLs overlain of Fig. 7

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