

Interactive comment on “Using ice-flow models to evaluate potential sites of million year-old ice in Antarctica” by B. Van Liefferinge and F. Pattyn

Anonymous Referee #2

Received and published: 10 July 2013

This paper from van Liefferinge and Pattyn discusses the use of two ice-flow models to evaluate potential sites for obtaining the oldest ice in Antarctica. The motivation for the study is well made and clearly links into wider discussions the ice-coring community is having at the moment with regard to why it would be optimal to find ice across the Mid-Pleistocene Transition (MPT). The paper is well structured, guiding the reader through the main issues. A model that neglects horizontal advection is used as a starting point to explore potential sites using three “best estimates” of geothermal heat flux as one of the key inputs. Later (Section 4), thermomechanical modelling is introduced, using an updated form of Pattyn’s (2010) model, and in Section 5 the inputs are tuned using known distributions of subglacial lakes to calibrate basal conditions. The two approaches produce consistent results, namely that apparently the best sites for targeting the oldest ice appear to be close to existing ice cores. Perhaps the best promise

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lies around Dome A/Ridge B, but there remains uncertainty from this whether one can reach the MPT even there.

I find the paper well conceived, a useful exercise, and excellently written. As a result my comments are minor. I would like to see some discussion of the few points I raise below, but otherwise my comments are largely restricted to minor grammatical corrections, appended in the attached supplement.

Fig. 2, 4, 5, 6, 7: I think these would all benefit from an annotation of where the key sites actually are, e.g. Dome Fuji, Argus, Ridge B etc. Okay, many readers will probably be familiar, but nevertheless some readers are probably not.

Fig. 3 and text on p.2866: Can we have just a little more clarity on what exactly it is that defines where the black rectangle has been drawn? From this I think the limits are, essentially, arbitrary, in that ΔG (x axis) needs to be positive and $s.d.G$ (y axis) needs to be “smaller rather than bigger”. If they are arbitrary, make this explicit; otherwise define.

p.2865 Mostly in the paper all symbols in the equations are well elucidated in the text, but I don’t find here an explanation for vH nor z' . One can of course source these from Pattyn (2010) but still, in an otherwise standalone work, for completeness these would best be written out here too.

Throughout the manuscript including in the reference list, Purucker (2013) should be spelt with ‘ck’ and Fox Maule should not be hyphenated, i.e. Fox Maule NOT Fox-Maule.

p.2870 We are promised a discussion on the choice of sigma values but I didn’t actually then come across this. Can this be more explicitly included? Do these choices affect the similarities in results between the two different models?

p.2875, lines 12-16. The presentation of these data as a supplement or in an appropriate repository will certainly be a major addition to the paper as well as a very useful

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resource for the community. As a statement to appear in the CP paper on its publication, however, this sentence needs to be revisited in the light of what the authors can make available at the time of publication, i.e. don't publish the final manuscript saying these "will be" available – publish saying what is available already.

Overall I congratulate the authors for a clearly written and useful paper.

Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/9/C1421/2013/cpd-9-C1421-2013-supplement.pdf>

Interactive comment on Clim. Past Discuss., 9, 2859, 2013.