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Interactive comment on "Formulation, calibration and validation of the DAIS model (version 1), a simple Antarctic Ice Sheet model sensitive to variations of sea level and ocean subsurface temperature" *by* G. Shaffer

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General comments:

This paper presents a clear and concise description of a very simplified Antarctic ice sheet model, with calibration during the last glacial cycle, and testing vs. modern rates of ice loss. The circularly symmetric ice model follows an earlier design (Oerlemans, 2003-2005), extended with non-linear dependency of ice flow across grounding lines, and with long-term forcing of atmospheric, oceanic and sea-level variations.

It is impressive that the model is able to satisfy the 3 volume constraints in the calibration, and that the warming required for large-scale retreat is close to that in 3-D EAIS

models (pg. 1796). Nevertheless, the merging of the very distinct and non-zonal West Antarctic and East Antarctic Ice Sheets (WAIS, EAIS) and their heterogeneous coastlines into a single symmetric dome is a very simplified rendition, clearly too simplified for some applications. The paper does acknowledge this and briefly mentions some general areas for which the model may be useful (pg. 1806, lines 14-17; pg. 1807, lines 27-29; pg. 1808, lines 1-2). It would help to promote the model if more discussion can be added describing specific applications that the model could contribute to, for which 3-D models are not feasible.

Specific comments:

1. The real WAIS and EAIS are more like two separate ice sheets separated by the Transantarctics, than one single dome. Their horizontal scales (plateau to coastlines), and peripheral fractions and grounding-line depths of subglacial basins, are quite different. For problems involving marine WAIS collapse, it might be a better approximation to treat them as two separate symmetric domes, i.e., 2 independent instances of this model, with appropriately different bedrock profiles. Or perhaps half-domes, with a no-flow wall representing the Transantarctics in each.

This could be left to future work (not needed for this paper). Such a modification could address the shortcoming noted on pg. 1806, lines 18-20, that WAIS collapses cannot be addressed in detail by the model (even though they may be active during the Last Interglacial, one of the calibration periods in the paper).

2. The two-parameter space explored here is reasonable, involving two of the most important processes for glacial cycles: parameterized flux across the grounding line (gamma), and ocean melting under floating ice (alpha). As a suggestion for further work, the model would be very suitable for a Large-Ensemble approach (e.g., Hargreaves and Annan, Clim. Dyn. 2002; Applegate et al., The Cryo., 2012; Briggs et al., The Cryo., 2013), for instance using a Monte Carlo Markov Chain technique. This would systematically determine the importance of other model parameters, and pro-

vide pdf's of parameter ranges. Other important parameters could well be the slope the bedrock profile, and the constant yield stress at the ice-sheet base. This type of application is mentioned briefly on pg. 1808, lines 1-2, but could be discussed further and connections with previous work mentioned.

3. From the perspective of parameter space, the sequence of presentation in sections 3.1 and 3.2 almost seems reversed. Fig. 7, showing the full ranges of acceptable alpha and gamma, should drive the choices of the runs shown in Fig. 6. Regardless of the order of the figures, more time series should be shown as in Fig. 6, for alpha-gamma values both in the center of the cluster of red dots in Fig. 7, and also on the right-hand side of the cluster. The four chosen alpha-gamma values shown in the current Fig. 6 (black dots, Fig. 7) seem limited and off to one side.

4. In section 2.1, it is unclear if the coefficient beta (dependence of mass balance on elevation below the runoff line, Eqs. 4 and 7) represents summer melting via atmospheric lapse rate, or solely height-variations of precipitation (via orographic forcing? pg. 1796, lines 11-14). This point comes up much later (pg. 1808, lines 4-6), which suggest that there is no melting in the simulations in the paper. It would help to make this clearer in section 2.1.

5. The self-similar profiles of ice thickness seem to preclude the possibility of including a few-thousand-year bedrock lag due to asthenospheric relaxation. This may be a significant omission if bed depths in the grounding zone strongly affect ice fluxes (as discussed on pg. 1807). Is there a way to build a crude lag into the model?

6. Recent observational studies of sea level around the Last Interglacial (LIG) find a double-peak structure (O'Leary et al., Nat. Geosc., 2013; Kopp et al., Nature, 2009). This is not in the prescribed sea-level curve in Fig. 5a (black curve). Could it significantly influence the model's response around LIG?

Technical comments:

pg. 1793, line 8; pg. 1803, line 20: loose/loosing should be lose/losing.

pg. 1800, line 21: It would help to alert readers at this point that higher values of gamma will be considered in later sections. The value of 2 mentioned here is much lower than those in Schoof (2007) (as noted later).

pg. 1804, lines 12-14: The statement "includes a several meter SLE ice loss at melt water pulse 1A, forced by ocean subsurface warming and sea level rise across the pulse" is misleading (even though the next sentence says the result is consistent with a Northern Hemispheric source). The real MWP1A only lasted ~500 years, and the rise in the model curves during any 500-year window in Fig. 6b is at most ~0.5 m, not several meters.

pg. 1807, lines 22-25: The suggestion here seems correct, that the different response in the Pollard and DeConto (2009) model at 125 and 90 ka is due to the different ocean forcing. There, the January insolation component produced a very large positive peak at 90 ka, which is absent in the ocean temperature time series here (Fig. 5, blue curve, with a peak at 125 ka). This points to the need for further work on ocean forcing on these timescales. The reconstructed ocean time series in the Appendix still seems uncertain, but acceptable in the absence of better methods.

pg. 1810, lines 5-15. It would be interesting to add a sentence or two briefly describing the DCESS ocean model used in the ocean time-series reconstruction.

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