

I would like to thank both reviewers for their constructive comments on this manuscript and also the authors for posting their responses to the reviewers' comments.

The manuscript investigates the chronology and controls on ice thickness change along David Glacier, East Antarctica, during the last deglaciation via a combination of surface exposure dating and glacier flowline modelling.

The reviewers highlight that the study is a useful contribution to our understanding of the controls on outlet glacier behaviour which will be of interest to both glacial geologists and numerical modellers. However, both reviewers request additional detail on the modelling setup and justification of parameter choices. They also request a clearer explanation of how the dating and modelling components of the study inform each other and I recommend that you address this latter point within the opening section of the article to provide motivation for your two-pronged approach. It may also be useful to review previous studies that have carried out data-model comparisons in a similar setting.

A second aspect that is raised by both reviewers is the approach used to compare the field data and modelling results. Comparisons with previously published model output will be moved to earlier in the manuscript, which will improve the logical flow of the article. However, the reviewers also request that you state more clearly that the modelling carried out here comprises a sensitivity study, and that the experiments are not designed to replicate the chronological details of glacier retreat. When comparing results it would also be useful to acknowledge any factors that may explain differences between model output and data (e.g. unmodelled processes) or between modelling studies.

One reviewer queries the validity of your final conclusions. I encourage you to focus on the specific findings of your study, i.e. your identification of evidence for prolonged, widespread Holocene thinning along David Glacier, and the likely controls on that thinning. Any extrapolation to a different setting should be carefully justified. In general, please ensure that statements are supported by evidence.

Individually, the reviewers raise a number of additional points that require clarification. The authors provide a response to each of these points and indicate that edits will be made to the manuscript to address them. In addition to the issues highlighted by the reviewers, I note the following:

- the phrase 'ice surface elevation' usually refers to 'elevation above sea level', but of course, sea level changes through time. Referring to 'ice thickness' can navigate this tricky issue.
- when you refer to the W12 and ICE-6G models I think that you are referring to the ice sheet reconstructions created for the purposes of glacial isostatic adjustment (GIA) modelling, not the actual GIA models (which include predictions of bed deformation, sea-level change etc.)
- regarding the top panel of Fig. 6B: (i) if the melt rate is prescribed why does it vary over time, and (ii) how should the left y-axis be interpreted; is lateral buttressing prescribed or inferred?
- I encourage you to consider including Figures A3 and A4 (modelling results) in the main text to support statements in the Discussion and Conclusions relating to the role of buttressing or the combined role of buttressing and ice shelf thinning in controlling glacier dynamics

Overall, both reviews are positive, and I invite the authors to submit a revised version of the manuscript that addresses all points raised during this review process. When responding to more substantial points, please document the details of any changes implemented.

Kind regards,

Pippa Whitehouse