This manuscript by Mas e Braga et al. explores the effect of nunataks on ice surface elevation during periods of ice thinning. This is important because the best available records of past ice sheet thinning are from cosmogenic exposure ages of glacial erratics collected on the slopes of nunataks. The findings of their idealized ice sheet model simulations suggest that samples collected upstream and downstream of a nunatak would produce different surface exposure ages (up to 14 kyr difference) due to an increase in ice surface elevation upstream and decrease in elevation downstream. Coarsening model resolution leads to an underestimation of this surface steepening effect. The study is well-written and presented, and should be of interest to readers of The Cryosphere. However, I hope the authors can address the following comments and questions. This review is divided into general and specific comments.

General comments:

1. Choice of model: The study addresses the issue of nunataks acting as a barrier to ice flow using an SSA model, with the authors correctly noting using a full Stokes model would be infeasible considering the long timescales involved (i.e. 20 kyr). I agree with Reviewers 1 and 2 that this warrants a robust discussion of model limitations given the 3D flow regime and steep gradients that would be present near nunataks. In addition, I think it would be useful for the authors to draw on the results of model intercomparison projects (e.g. MISMIP+, Cornford et al., 2020) to discuss whether other types of models would behave similarly or not. I'd note that SSA models are atypical for paleo-applications, with the majority of such studies using hybrid models and, more recently, higher order models, so a discussion of the model dependency of the findings in this study would be helpful.

2. Context of findings: The authors assert throughout the paper that "ice sheet models overestimate ice surface elevations and underestimate the pace of ice sheet melt contributing to sea level rise compared to empirical reconstructions." I find this to be an oversimplification for a number of reasons. There are many examples of ice sheet models that fit empirical constraints during glacial and interglacial periods (e.g. Whitehouse et al., 2012; Golledge et al., 2013; Goezler et al., 2016; Whitehouse et al., 2017; Clark et al., 2020), as well as the rate of mass loss during deglaciations and through the Holocene (e.g. Gomez et al., 2013; Lecavalier et al., 2014; Tigchelaar et al., 2018; Cuzzone et al., 2019; Briner et al., 2020; Albrecht et al., 2020a). While two studies are cited with records from the Transantarctic Mountains (Lines 72-73), even models analysed within those studies show similar rates of ice thinning as observed in the records. I agree with the authors that data-model mismatches occur in both Greenland and Antarctica, particularly with respect to the timing of regional ice thinning, though I'm not sure that the findings here definitively rule out other possible explanations, such as uncertainty in climate forcing (e.g. Lowry et al., 2019; Albrecht et al., 2020a), or uncertainty in the surface exposure ages themselves (e.g. Jones et al., 2019). It would be more accurate to simply point out that many previous paleo-ice sheet model studies, which use coarse resolution, do not resolve site-specific features and this could contribute to observed timing mismatches between surface exposure records and model simulations.

3. Influence of GIA: I am surprised that glacial isostatic adjustment was not considered in the simulations given the timescale. The calculation of surface exposure ages is highly dependent on the elevation, and correcting for GIA-driven elevation changes in non-trivial (Jones et al., 2019). Ice sheet models themselves are highly sensitive to GIA because it impacts bed elevation (e.g., Gomez et al., 2013; Kingslake et al., 2018; Colleoni et al., 2018; Albrecht et al., 2020b). While this study uses idealized experiments, in reality the basal topography

would evolve through time in response to changes in ice loading. The authors do provide a useful discussion on limitations in the simplified SMB forcing for the idealized experiments (Lines 320-334), but a similar discussion with respect to GIA is currently lacking.

Specific comments:

Line 16-17: See above general comment on context of findings.

Fig 2: Is there a scale for the slices? The colours correspond to upstream and downstream, correct? The point is to show that samples are taken in nearly all directions?

Line 136: citation? Does SMB only change in one direction?

Line 160: Any sensitivity tests for this value? 5-6% K^{-1} is appropriate for EAIS (Frieler et al. 2015). WAIS is less straightforward (Fudge et al. 2018).

Fig 4: Could you make the inset larger? It is difficult to see the coloured circles that correspond to panels c and d.

No technical corrections noted.

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