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Discussions

Interactive comment on "Modelling last glacial cycle ice dynamics in the Alps" *by* Julien Seguinot et al.

Anonymous Referee #1

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This paper is a landmark advance in modelling European Alpine ice cover, applying a high-resolution (1 km) ice model to the entire Alps through the last glacial cycle, for the first time to my knowledge. Results are compared with diverse geological data, and several important findings are presented, including time-transgressive ice marginal extents at LGM. The climate forcing is simple, applying uniform perturbations to modern observed datasets, which leads to some uncertainty in the results, but does not detract from them too much given the advances made in the ice modelling alone.

The introduction gives an elegant summary of Alpine glacial science since the 1700's, including many historical references. The paper is well organized, with well-chosen sensitivities described first that calibrate the climate forcing, followed by detailed analysis of one best-fit high-resolution (1 km) simulation through the last 120 kyrs. Detailed

comparisons to a variety of geological data are made, constituting a thorough assessment of model performance. An impressive animation of the whole cycle is included as supplementary material.

Specific comments:

pg. 4, lines 9-10: Can the physical basis of englacial water fraction and sensitivity of results be summarized briefly? This is not a usual component in ice-sheet models. Is the cap value ("capped at 0.01") well constrained, and does it have a significant effect on results?

pg. 4, lines 19-20: The sub-glacial hydrologic component should be described more (even if exactly as in Bueler and van Pelt, 2015). Is basal water transported horizontally down the hydropotential gradient? This is usually a highly uncertain component of ice-sheet models, but can have a large effect on results through its influence on basal sliding, and basal frozen vs. thawed areas, which is relevant to section 4.4 regarding trimlines.

Somewhat related: Little information is given on the choices of basal sliding parameter values in Table 1. This could be discussed briefly. Presumably no inversion or optimization was performed for these values beforehand, and they do vary spatially. Are they appropriate for Alpine bedrock overall?

pg. 7, line 7: Are there any data to support this atmospheric lapse rate value (6 K km-1), and do other values have the potential to significantly affect ice temperatures? In particular, could they change the basal areas of frozen/unfrozen ice and so the comparisons with trimlines in section 4.4?

Climate forcing:

The method of spatially uniform shifts to modern climate forcing is common in paleomodeling of large ice sheets, and in my opinion is acceptable as a starting point in this work, with coupling to regional climate models (RCMs) left to follow-on work. There are good discussions on possible shortcomings of this method, for instance as a cause of anomalous east-west marginal ice extents at LGM (pg. 11, line 6-8). However, I suggest changing the sentence on pg. 9, line 5, which mentions some RCMs applied to LGM Europe, but also says "...over the Alps during the last glacial cycle, of which little is known apart from the LGM". There are several other RCM modeling studies over Europe during the last 120 kyrs, e.g., for MIS Stage 3, Kjellstrom et al., Boreas, 2010; Barron and Pollard, Quat. Res., 2002; Alfano et al., Quat. Res., 2002; and for 6 ka, Strandberg et al., Clim. Past, 2014. Perhaps there is little useful material there for the Alps, but such papers exist.

The past climate variations are prescribed following 3 quite distal core records, and the most distal (EPICA) is chosen as yielding the best fit to Alpine glacial evidence. The basis for preferring EPICA seems reasonable (matching some higher-frequency amplitudes of ice variability, section 3.3). However, this agreement is in a sense co-incidental, in that there is no direct meteorological link between Antarctic and Alpine regional climate variations. Are there any proximal proxy records of Alpine climate at all, perhaps lacustrian varves, that could be used to assess the EPICA-based shifts in air temperatures and precipitation, even over limited periods of the last 120 kyrs?

A PDD scheme is used based only on seasonal air temperatures. van de Berg et al. (Nature Geosc., 2011) showed that for long-term variations including the Eemian, orbital changes in insolation are important and should be considered explicitly. This could be particularly relevant here, because the EPICA core does not reflect changes in insolation over the Alps. In further work, an insolation-change term (summer, local) could be combined with EPICA in the climate temperature paleo-forcing.

Trimlines:

The point is well taken that trimlines do not necessarily indicate past ice surface elevations, but the upper limit of temperate ice with cold ice above (pg. 18, line 5 to pg. 19, line 4). It is an important point, because model LGM ice surfaces are far above most

trimline elevations (as noted and in Fig. 6a,b). The pertinent results are shown in Fig. 6c, and I agree there is good support for the cold vs. temperate (basal) ice hypothesis.

It might help general readers to spell out the interpretation even more in the text. That is, as I understand it, the observed trimlines should coincide with the boundaries between model areas of frozen vs. temperate beds, so the dots in Fig. 6c should all lie on the borders between the hatched and white areas. The sentence on pg. 19, lines 20-21, is confusing in this regard. Incidentally, it would also help to add the word "basal" to the last sentence of the Fig. 6c caption: "...experienced temperate basal ice for ...".

One reason for the remaining discrepancies in Fig. 6c could be temporal variations in the model boundaries, that are aggregated in time by the "< 1 kyr" criterion for the hatching and the grouping of all trimline data. To go into this in more detail, in principle Fig. 6c could be expanded to show the model basal frozen-temperate boundaries at particular times (21.5, 22.5, etc, ka), with only the dots for each time period superimposed. But that may not be worth it unless there are large temporal variations in the model boundaries.

A slight concern is that the majority of the trimline data seems to be orange dots i.e., older that 27 ka in the timescale of Fig. 6c. The period for the model hatching extends back only to 29 ka. Hopefully, most of the orange-dotted data are within that period and are not older than 29 ka(?).

The text could briefly mention (and hopefully rule out) the issue of very fine-scale topographic features on which the trimlines are located, not resolved by the 1-km model topography. If data sites are on small-scale highs or lows significantly different from their ∼km-scale surroundings, that could contribute to the discrepancies in Fig. 6c.

Technical comments:

pg. 3, line 2: Perhaps change "lead" to "led", "to which" to "to what".

Fig. 1 caption, line 4: Perhaps change "estimated" to estimate", or "estimated of" to "estimated".

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