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Discussions

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

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Dear Anonymous Referee #1,

Thank you very much for your detailed review of our manuscript.

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The introduction gives an elegant summary of Alpine glacial science since the 1700's, including many historical references.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.

Thank you very much for these elogious and supportive comments!

Specific comments

p. 4, l. 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?

Thank you for bringing this up. Laboratory experiments have demonstrated that the rheology of temperate, polycrystalline ice depends on its content in liquid water (Cuffey and Paterson, 2010, p. 65–66). Unfortunately, the only measurements available to date (Duval, 1977), used to quantify the effect of liquid water on ice softness, the creep parameter A in Glen's flow law (Lliboutry and Duval, 1985), only extend to fractions of liquid water content between 0 and 0.8%. They show a three-fold increase of ice softness over this range (Duval, 1977, Fig. 1).

Ice sheet models have previously ignored this effect, but it has now become a typical component of polythermal models such as SICOPOLIS (Greve, 1997), COMICE (Rückamp et al., 2010), PISM (Aschwanden et al., 2012), ISSM (Seroussi et al., 2013), and TIM-FD^3 (Kleiner and Humbert, 2014).

However water fractions between 1 and 5 % have repeatedly been observed in temperate glaciers (Murray et al., 2000, 2007; Bradford and Harper, 2005; Bradford et al., 2009), and also occur in model results (e.g., Blatter and Greve, 2015), but it is not known whether ice viscosity continues to decrease substancially for values above 0.8 %. Previous modelling studies have commonly assumed constant ice viscosity above 1 %. This arbitrary threshold is not constrained at all, and the urgent need for new ice deformation experiments has already been pointed out (Kleiner et al., 2015).

The sensitivity of our results to the 1 % threshold was not tested. However, in our model results, liquid water fractions above 1 % typically only occur within the basal temperate layer of the fastest-moving glaciers where ice movement is largely dominated by basal sliding. We suspect that increased ice deformation in these regions is negligible in comparaison to uncertainties related to basal sliding and, more importantly, to climate forcing.

Thus, we prefer to avoid including the above discussion in the manuscript, but have reworked the sentence on water content:

> *[Ice softness] increases with liquid water fractions up to 0.01 (Duval, 1977; Lliboutry and Duval, 1985; Cuffey and Paterson, 2010, p. 65–66), an arbitrary threshold above which new ice deformation measurements are critically needed (Kleiner et al., 2015).*

The uncertainty to unknown rheology of water-rich temperate ice was also mentioned in the conclusions:

> *In the absence of ice deformation measurements, a constant rheology was used for temperate ice containing more than 1 % of liquid water.*

p. 4, l. 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.

We refer to Bueler and van Pelt (2015) as their paper contain the most up-to-date description of PISM till effective pressure physics used in our simulations (Bueler and van Pelt, 2015, Eqs. 18, 23, and 24). However, subglacial water is not routed down the hydropotential gradient. We have clarified this:

> *Effective pressure is related to the ice overburden stress and the modelled amount of subglacial water, using a formula derived from laboratory experiments with till extracted from the base of Ice Stream B in West Antarctica (Table 1; Tulaczyk et al., 2000; Bueler and van Pelt, 2015, Eqs 23 and 24). Basal meltwater is accumulated locally without transportation. When the till becomes saturated, additional meltwater assumed to drain off instantaneously outside the glacier margins, i.e. it is removed from the system in an accountable way.*

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?

Although we refrain from repeating parameter values given in Table 1 in the main text, the following text was added in the methods:

> *[a constant basal friction angle] corresponding to the average of available measurements (Cuffey and Paterson, 2010, p. 268). [...] Other parameters*

(Table 1) follow simulations of the Greenland ice sheet (Aschwanden et al., 2013), or benchmarks when other data is missing (Bueler and van Pelt, 2015).

Inversion of specific basal sliding parameters for past Alpine glaciers is difficult because the altitude and age of maximum ice surface elevation is discussed (cf. introduction and discussion on trimlines), and also depends on the even more uncertain regional climate history. Therefore, basal sliding was also mentioned as one of the major sources of uncertainty in the conclusions.

The till deformation model used here does not hold for sliding over bedrock surfaces. On the other hand, the constant friction angle used is representative of wet till but weaker basal conditions may have applied over saturated lake sediments where they occured.

p. 7, l. 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?

In the European Alps, monthly temperature lapse rates vary between approximately 4 K km^{-1} in winter and 7 K km^{-1} in summer, and annual temperature lapse rates vary spatially between 5.4 and 5.8 K km^{-1} (Rolland, 2003). A reference to the study by Rolland (2003) was added in Table 1, and as we have now explicited, our constant value of 6 K km^{-1} is thus:

slightly above average but more representative of summer months when surface melt occurs (Rolland, 2003, Fig. 3).

Although no sensitivity tests were conducted, we argue here that the effect of atmospheric temperature lapse rate variations on ice temperature is negligible. First, seasonal variations do not penetrate ice or bedrock below a few metres. Second, even above 2 km of ice, spatial variations of 0.4 K km^{-1} would translate into surface temperature variations of only 0.8 K. But during the Last Glacial Maximum, ice surface temperatures in the trimline region are typically between 15 and 20 K below freezing. Thus the effect on the temperature gradient would be small. Actually, the effect would even nearly disappear near the glacier base, where the temperature gradient is much steeper and primarily controlled by geothermal heat flux and shear heating.

Climate forcing

The method of spatially uniform shifts to modern climate forcing is common in paleo-modeling 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.

Thank you. We did not know about references on MIS 3 and have developped our sentence to better reflect the current state of knowledge on palaeo-precipitation:

Palaeoclimate proxies indicate slightly reduced LGM precipitation in western Europe with anomalies diminishing eastwards (Wu et al., 2007). Re-

gional circulation models indicate generally dryer conditions during MIS 3 (Barron and Pollard, 2002; Kjellström et al., 2010) but more precipitation south of the Alps during MIS 2 (Strandberg et al., 2011; Ludwig et al., 2016).

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 coincidental, 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 short review of available proximal proxy records was added here:

Only few regional proxy records exist that extend over periods when the Alps were glaciated (Heiri et al., 2014). These include lake sediment records in north and west of the Alps (de Beaulieu and Reille, 1992; Wohlfarth et al., 2008; Duprat-Oualid et al., 2017, e.g.,), and cave speleothems in the Eastern (e.g., Spötl and Mangini, 2002; Boch et al., 2011) and Western (Luetscher et al., 2015) Alps. Due to the scarcity of vegetation north of the Alps during glacial periods, varying sources for moisture advection, and the limited duration of the records, quantitative palaeoclimatic interpretation will require combining multiple proxies in space and time, and comparing them against regional circulation model output (Heiri et al., 2014).

In the review paper by Heiri et al. (2014), the latter has been specifically identified as a neeeded future development, but to our knowledge, no quantitative reconstruction has

been made available since then. The following word of caution was also added in the conclusions:

This suprising result is likely coincidental as there is no direct link between European and Antarctic climate. This highlights the need for more quantitative reconstructions of European palaeoclimate.

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.

We agree. This is one of many possible improvements that could be tested upon the climate forcing used here. In the conclusions we now advocate for:

a more realistic climate forcing based on regional circulation model output or including the effect of long-term term changes in incoming solar radiation.

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.

Thank you for emphasizing this point.

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 p. 19, l. 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 ...".

We realise that Fig. 6 is confusing. We had chosen the 1 ka limit because temperate basal ice tends to occur above the trimlines during short periods of warmer climate. To avoid confusion, Fig. 6c has been simplified to show the basal thermal boundary at the age of maximum ice thickness. The last sentence in the caption now reads:

> *Hatches mark the LGM cold-based ares (basal temperature above 1×10^{-3} K below freezing at the age of maximum ice thickness).*

Unfortunately this also results in a worse fit to trimline locations, as was noted in the main text:

> *In the upper Rhone Valley, observed trimlines are often located near the LGM cold-temperate basal thermal transition, or in cold-based areas (Fig. 6c).*

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.

Fig. 6c has been simplified to show the basal thermal boundary at the age of maximum ice thickness. Although this results in a time-transgressive picture, it means that the frozen areas indicated on the new figure are contemporaneous with the plotted ages and surface topography contours, and data shown on panels a and b.

Although the model output allows to study the migration of the basal thermal boundary over time into more detail, the basal velocity, which also controls erosion, should probably be considered as well. We leave this for future studies. The following sentence was added in the main text:

> *The remaining discrepancies may relate to temporal migrations of the basal thermal boundary, an absence of sliding in warm-based areas [...]*

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(?).

In fact, much of the mountainous areas reach maximum ice thickness during early MIS 2 when ice is colder and stiffer. This is also visible on Fig. 5. Some areas even reach maximum ice thickness during MIS 4 when the bedrock is slightly less depressed. There is certainly very much scope to discuss the age of the trimlines, which may differ significantly from that of the maximum ice extent on the lowland, but we are not aware of any data that could be used for validation here. However, frozen areas plotted on Fig. 6c are now consistent with the modelled ages used in panels a and c.

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.

This is a valid point which we can unfortunately not rule out. Despite PISM's enthalpy scheme, which ensures a seamless transition between cold and temperate ice physics, resolving the basal thermal boundary on the valley sides is delicate, because it implies resolving a steep (vertical) enthalpy gradient over a steep bedrock slope. This requires both high vertical and high horizontal grid resolutions. This limitation was mentioned in the section on trimlines:

> *[The remaining discrepancies may relate to] levelling of small-scale topographic features in the 1 km horizontal grid. They call for more detailed comparisons [...]*

Technical comments

p. 3, l. 2: Perhaps change "lead" to "led", "to which" to "to what".

Done.

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

Done. We thank you very much again for the time and effort you put into our manuscript.

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