The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-8-AC4, 2018

The Cryosphere
Discussions

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

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Received and published: 23 July 2018

Dear Anonymous Referee #3,

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

This is an excellent manuscript and I highly recommend publication after a few minor changes. The authors present a well thought out modelling experiment which they combine (albeit in a qualitative manner) with extensive palaeo-glaciological data and cumulative work. I can see this work being extended into more extensive and rigorous work (RCM forcing, ice dynamic sensitivity, quantitative fitting to geomorphological record etc), but this is an important leap forward.

Thank you very much for these supportive words!

Abstract

p. 1, l. 2: "pioneer" should be "pioneering".

Thank you. Corrected.

p. 1, l. 16: I think the finding that you get asynchronous glaciation extents with a uniform climate offset is due to glacier hypsometry and setting should be stated here. i.e. that the timing of maximum glaciation and recession isn't purely a function of climate. This finding needs to be highlighted better in the abstract.

Thank you for pointing this out. We have reworked the last sentence in the abstract:

> *Finally, despite the uniform climate forcing, differences in glacier catchment hypsometry cause the Last Glacial Maximum advance to be modelled as a time-transgressive event, with some glaciers reaching their maximum as early as 27 ka, and others as late as 21 ka.*

Introduction

p. 2, l. 28: Ballantyne and Stone (2015) should be added to this list.

Thank you. This is relevant indeed. We have added the reference in the introduction and in the corresponding section of the discussion.

p. 2, l. 34: It should be stated that it could be a consequence of both glaioclimatic interactions and uncertainties in dating methods.

We have added “or both”.

p. 3, l. 2: These points serve the literature well to highlight gaps for future research. However, I would argue that you do not get very far here on 1 and 5 and do not completely solve the other 3 points. Your text reflects these shortcomings very well, for which you should be applauded. Though I think at this stage of the manuscript, your statement of intent, you should state that you do not claim to solve these questions, but rather push forward on all of them using your new approach of ice sheet modelling.

Thank you. In order to clarify our statement of intent, we added the following text:

> *Additional geological research will be needed to complete our knowledge. But here, we intend to explore these open questions from a new angle and [use the Parallel Ice Sheet Model ...].*

Section 2.6

p. 7: The spatial distribution of your modern climate variables (precip, temp) will be massively influenced by elevation. Though there is a lapse rate, does this pattern of high precip and low temp over mountains remain throughout the simulation despite ice surface topography, and if so, how does this influence your results?

The temperature lapse rate corrections dampen local high temperatures in ice-filled valleys. The precipitation pattern, on the other hand, remains the same throughout the simulation, and this results in a higher accumulation above the location of present-day mountains even as they are buried by ice in the model.

Although we expect that small-scale variations in surface mass balance are smoothed out by ice flow, the presence of an ice sheet may have redistributed precipitation on a larger scale, for instance from the inner ranges to the foothills, or inequally between north and south of the Alps, etc. However, global precipitation reductions appear to have little effect on the average ice thickness (Fig. 2b, compare dark and light curves). Nevertheless, the new text shortly mentions this caveat:

> *Besides [circulation changes], changes in the ice surface topography may have redistributed orographic precipitation.*

Our conclusions also highlight the need for a more realistic forcing.

> *Using [...] a more realistic climate forcing based on regional circulation model output [...] future modelling studies will certainly be able to quantify uncertainties associated with some of the above limitations.*

Section 3

p. 7, l. 14: You keep mentioning the number of processors. I find this information slightly irrelevant, and it will soon become outdated as processing speed and models increase (GPUs for example). The only way it will serve the community is if there is a full description of the computer set up. For example, it could be that the simulations took 4 days on 144 processors, but the processors were slow. Suggest removing these references.

We removed mentions of computing times and numbers of processors.

p. 10, l. 14: This is a more general point. You eventually choose the EPICA record, and for justifiable reasons based on comparison to reconstructed ice extent and timing. However, this is likely coincidence. EPICA is likely a complex record containing global and local antarctic influences upon climate. The "real" climate over the alps during glaciation is like decoupled from that of Antarctica to an extent. Therefore, the match you find is not an inference about climate, as different combinations of offsets may have made the same result (smoothed GRIP to remove some of the D-O scale noise?). You should make this explicit somewhere in the manuscript.

We agree. The following text was 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.*

p. 11, l. 2: First sentence needs reconsidering as it is slightly broken in its current form. Perhaps "Figure 3a shows the cumulative extent of glaciated area during MIS2".

We reworked the first sentence. It now reads:

> *During MIS 2, all six simulations yield comparative modelled maximum extents (Fig. 3a).*

p. 11, l. 10: Is it possible some ice is missing from the geological reconstruction in some instances? I guess some outlets are well constrained, whilst others areas could be "filled in" by this modelling experiment.

Yes, this is possible. Parts of the reconstructed LGM ice limits are uncertain due to the lack of moraines or dated evidence. However, without very detailed knowledge about palaeoclimate, and tight constraints on ice physics, the model won't replace observations. Actually, we are very confident that the model-data discrepancies in ice extent discussed in this passage are beyond uncertainties.

Section 4

p. 14, l. 15–31: I find this description of sites and timing of glacier extent compared to dates difficult to follow. I suggest a new figure to convey this important comparison: Have the reconstructed and modelled ice extents at key times for each of the mentioned glaciers on several smaller maps, including geochronological constraints.

This would be very useful but to our knowledge, no geological data have been made available that would allow such a comparison. The review by Wirsig et al. (2016, Fig. 5) which we refer to is the most up-to date compilation of LGM ages in and around the Alps, and deglacial outlines like there exists for the Eurasian and Laurentide ice sheets are unfortunately not available.

p. 17, l. 17: This finding is important and should be highlighted better in abstract and conclusion.

This finding was better highlighted in the abstract (see our previous comment). However, we feel it is already well expressed by the following bullet point in the conclusion:

> *The LGM (maximum) extent was a transient stage in which glaciers were out of balance with the contemporary climate. Its timing potentially varied across the range due to inherent glacier dynamics (Sect. 4.3).*

p. 17, l. 22: Is the model recreating possible surging? Or would this be an over-interpretation given the uncertainty in climate and physics. Seems to fit with the enthalpy model of Benn and others for surging. As reads, it suggests that these areas were possible palaeo-surges, please clarify.

Our simulations reproduce a thermodynamical instability akin to a surge. The internal energy (so-called enthalpy) conservation scheme used in PISM allows for variations in ice temperature and liquid water content (Aschwanden et al., 2012), which may result in thermodynamical oscillations (Pelt and Oerlemans, 2012; Feldmann and Levermann, 2017).

Unlike periodic surges (as described by, e.g., Sevestre and Benn, 2015), we interpret the model results as triggered by an initial perturbation of mass (and energy) caused by rapid climate cooling and inefficient mass transfer to lower elevations. We have rephrase the following sentence:

> *Several Alpine lobes surge and overshoot their balanced extent before thinning and receding towards the mountains as they warm towards thermodynamical equilibrium.*

p. 19, l. 9–12: On trimlines: I think you justify well why you haven't yet modelled the sensitivity to your trimline result - previous work backs this up. But, I think you should consider the following: Adding a plus/minus to your results to reflect the uncertainty. The importance of resolution - many trimlines will be below the resolution of your model, so perhaps aren't resolved enough for model-data comparison. They may have acted to deflect ice flow around mountain peaks for example. This resolution caveat should be mentioned. I would be surprised if all trimlines are subglacial transitions as this paper suggests - perhaps you need to directly challenge the geochronological community to find better trimline constraints (sub/supra) as a statement in this paper. Your mean value of 861 m is unrepresentative of your sample. Your sample is highly skewed, so a modal value (1050 m ish from Fig 6) is more appropriate. A similar finding with a similar approach has already been found for the British Isles, with the added constraint of GIA observations. I suggest referencing Kuchar et al. 2011 for this reason.

Thank you. This is a valid point. However we found an error in our figure: the data plotted on the histogram (Fig. 3b) were different from those in the scatter plot (Fig. 3a).

These data were raw interpolated ice thicknesses, i.e. they missed a correction for altitude shifts between our initial basal topography and the observed trimline elevations.

The corrected histogram is closer to symmetry. A slight skewness remains, but this is not really a result of our simulations. In fact the distribution of observations itself is skewed, with more trimlines observed at higher elevations, and there is a tendency for the differences between the modelled ice surface and trimlines to increase with altitude.

The modal value (now ca. 950 m) corresponds to a cluster of trimline locations around 2850 m in the Aletsch Glacier (south of Jungfrau) region where trimlines are well visible in the landscape. We prefer to stick with the mean because the number of trimlines mapped is not necessarily representative of their lenght, and may depend on other factors such as bedrock lithology and post-glacial erosion.

We now use the standard deviation (197 m) as a measure of variance, but not uncertainty, which would require more sensitivity tests. We also mention resolution as one of several caveats in the text:

> *The remaining discrepancies may relate to temporal migrations of the basal thermal boundary, an absence of sliding in warm-based areas, and levelling of small-scale topographic features in the 1 km horizontal grid.*

We added a reference to the study by Kuchar et al. (2012) although this approach is not doable for the Alps.

> *Unfortunately, validation through the bedrock uplift rate (cf. Kuchar et al., 2012) is not doable in the Alps due its lower values, active tectonics and uncertainties on geological properties (cf. Mey et al., 2016).*

A philosophical but important point is that your discussion throughout is written from the standpoint of the geochronological/geomorphological data and reconstructions as being “truth”. It should consider somewhere that perhaps data is missing as it is hard-won in the places it exists and interpretations may be slightly wrong. The model is also not the “truth” and there is probably a blurred line in-between upon which we can proceed.

Thank you. Indeed the quality and availability of geomorphological and geochronological data is very variable across the Alps, and long-standing interpretations are sometimes proven wrong (e.g., Monegato et al., 2007). Also, parts of the Alpine glacial geology community will certainly disagree with your comment and find our challenging of the trimline assumption quite provocative, given that it has been around for at least a century (Penck and Brückner, 1909).

p. 21, l. 11–19: Be really clear here that these are modelled, and perhaps not geologically recorded, advances of the ice sheet. If there is no data, it might be correct, might be just a modelled result.

Actually we meant geologically recorded asymmetry. Obviously, this needed clarification. This sentence now reads:

> *The observed asymmetric extent of ice north and south of the Alps can be explained by the modelled transient nature of the LGM extent without involving north-south gradients in temperature and precipitation change (Sect. 4.1).*

Finally, we thank you again very much for the time and effort you put into our manuscript.

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