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The Cryosphere Discussions

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

Anonymous Referee #3

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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.

Comments:

Abstract

P1, L2: "pioneer" should be "pioneering" P1, L16: 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.

Introduction

P2, L28: Ballantyne and Stone (2015) should be added to this list. P2, L34: It should be stated that it could be a consequence of both glaioclimatic interactions and uncertainties in dating methods. P3, L2: 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.

Section 2.6 P7: 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?

Section 3: P7, L14: 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 I find this information slightly irrelevant, and it will soon become outdated as processing speed and models increase (GPUs for example).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.

P10, L14: 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.

P11, L2: 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".

P11, L10: 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.

Section 4

P14, L15 - 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.

P17, L17: This finding is important and should be highlighted better in abstract and conclusion.

P17, L22: 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.

P19, L9-12: 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.

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 inbetween upon which we can proceed.

P21, L11-19: 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.

Additional references: Ballantyne, C.K. and Stone, J.O., 2015. Trimlines, blockfields and the vertical extent of the last ice sheet in southern Ireland. Boreas, 44(2), pp.277-287. Kuchar, J., Milne, G., Hubbard, A., Patton, H., Bradley, S., Shennan, I. and Edwards, R., 2012. Evaluation of a numerical model of the British–Irish ice sheet using relative seaâĂŘlevel data: implications for the interpretation of trimline observations. Journal of Quaternary Science, 27(6), pp.597-605.

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