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Climate of the Past Discussions

Interactive comment on "Modelled glacier equilibrium line altitudes during the mid-Holocene in the southern mid-latitudes" *by* C. Bravo et al.

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Author's response to Anonymous Referee #2

We appreciate the review of Anonymous Referee #2, and to address the comments made we will introduce changes in the manuscript in order to address the reviewer's concerns. Next, we answered point by point the general comments (enumerated) of the Referee #2:

1) By approaching the task, the authors make clear on several occasions that, given the limited knowledge about Mid Holocene conditions, only differences between a reference (PI) and the target period (MH) can be addressed. It remains, thus, unclear why the major proportion of the paper (including 6 of the 10 Figures) deals with ab-

solute values of temperature, precipitation, and ELA. Among many other expendable portions, e.g. the discussion about biases in the absolute values is not needed.

The aim of showing absolute values of the PMIP2 models and comparing them with CRU and weather stations is to assess the climate models results that we use as an input of the mass balance models, in two aspects: the seasonal cycle and the longitudinal gradient, as these are important to give credibility to the mass balance calculations. We agree that dedicating 4 figures to this end is too much. We are happy in eliminating all 4 figures (figs. 2-5) and probably add to a supplementary material. Results of those figures and conclusions taken can be explained without the figures. With the conclusions from figures 2-5 in mind, we can discuss the limitations of the mass balance models results, especially those associate to the ELA gradient between the west and east due to the poor representation of temperature and precipitation gradients in some regions (Page 618, line 17-18). Also we show the results of the absolute ELA (just for the PI) to indicate that the spatial distributions of our results represent the expected latitudinal ELA gradient and in a more limited way the west-east gradient (Figures 8 and 9). This will be explicitly stated in the new version of the manuscript.

2) It remains obscure (probably because of the dominating discussion on absolute values) how recently observed conditions can hold to validate the PI as a reference period.

Unfortunately PMIP2 no 20th century simulations were carried out by the groups. So the only reference period that can be used in this study is the pre-industrial control simulations. We think that it is expected that pre-industrial and present-day climate followed the same spatial and temporal pattern. The only reason to compare the pre-industrial PMIP2 simulation and the the20th century climate data, is to verify that these patterns agree between these two databases To address the issue of comparing Pre-Industrial simulations with 20th century data we will make those comparisons with 1901-2004 CRU data, as a proxy for more anthropogenically unperturbed conditions.

3) The key for explaining any climate forcing is the dating of the targeted proxies (glacier extends in this case). It seems that the problems associated with dating are large for glacier extends during the Mid Holocene in the southern mid latitudes. The authors must examine and clarify if this enables them to retrieve conclusions from their exercise.

As we mentioned in the paper (page 618, lines 14-17): ". . .and the fact that we are comparing glacier fluctuations spread throughout the mid-Holocene with a precise time-slice, namely 6 ka BP, in addition to the already discussed uncertainties in the timing of glacial fluctuations." There are several sites in the Southern Alps where it is clear from precisely dated moraines (using 10Be) that mid Holocene glacier extents were larger than those during the preindustrial reference climate. For example, Kaplan et al., Schaefer et al. and Putnam et al. all show that glaciers were larger than present between 8-6000 years ago compared to several hundred years ago (during the LIA). Probably in Patagonia there are more uncertainties in the timing of glacial fluctuation at MH. However we argue that this is sufficient knowledge to undertake a first-order modeling study that attempts to understand the driver of these different glacier extents. We are going to take this point with extremely precaution because we understand that the data discussed in the paper does not allow us to state that Neoglacial advances happened around 6 ka but it can help in determining if the climatic conditions of the MH would permit to have glaciers larger than PI, and this could be part of the discussions around the timing and causes of glacier advances at 8-6 ka.

4) The use of ELA as the crucial variable requires a much clearer definition of the ELA the authors use.

This is a pretty unclear comment. We do not know in what sense the referee wants us to explain and define the ELA. A reference it would be useful for us. Our definition of the ELA (Page 606, lines 23-26) it seems to be appropriate, especially as we are talking about temperate glaciers (where internal accumulation and superimposed ice are negligible). But perhaps the comment is more about why we use the ELA as a

'crucial variable'? And the obvious answer to that is that the ELA, as calculated by a degree-day model, is appropriate variable to consider for a regional study, where we are not considering individual glaciers and their specific responses to climatic variations. Rather, we are interested in translating the output of the PMIP2 models into a signal that the glaciers respond to. In a possible new version of the manuscript we attempt to emphasize the definition of the ELA.

5) A much clearer discussion is needed on how ELA changes are related to glacier extends under different landscape geometries in order to compare modeled ELA differences with reconstructed glacier extends.

The reviewer rise an important problem. However, this problem is not relevant for this research. We did not aimed to quantify the impact of the ELA change in the glacial extent. Indeed, we use the ELA as our proxy of glacier change, because it is independent of the topography, however we mentioned in the manuscript that the value of change could impact glacier extent under different characteristics, as the hypsometry (page 618, lines 21-28).

6) It remains unclear how PMIP output enters the mass balance model (any downscaling procedure applied?) as well as on how reference levels are defined to which the vertical gradients of temperature and precipitation are applied.

We will clarify this point in the text. No downscaling procedure was applied considering the regional view that we have as a main aim. We just resize the PMIP2 models output with linear interpolation. About the levels we indicate in the paper that we apply the mass balance model in an altitudinal range defined from 0 to 4000 m asl with a step of 20 m (page 607, lines 10-13)

7) What is the final significance of the differences in ELA (Figure 10)? What is the uncertainty of the obtained ELA differences? In a revised manuscript we will change the question stated in the introduction as objective of the paper. Instead of stating the orbital control on glacier extends we will talk about climate controls. In the conclusions

we can address the orbital part again. About the second question the reviewer rise an important point. In the first version of the manuscript, we did not address the uncertainty and statistical significance of our results properly. In the new version we carried out a sensitivity test, in order to quantify the significance of our results.

8) The paper needs, by addressing the above mentioned and some more issues, a major re-organization, a re-structuring, and a re-writing before a proper review is possible.

The reviewer's comments helped us to recognize that the main goal of the paper was a bit disconnected with our analysis. In the new version of the manuscript we restructure the document, in function of our new main goal: Determine if the climate conditions during the MH would permit to have glaciers larger than today.

We will restructure the manuscript considerably. The main points are the following:

1. We will change the question stated in the introduction as objective of the paper. Instead of stating the orbital control on glacier extends we will talk about climate controls. In the conclusions we can address the orbital part again.

2. We will leave out figure 2-5 and leave those as supplementary material if possible.

3. We will further assess uncertainty of the ELA results by doing sensitivity experiments to the degree day factor.

References

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Interactive comment on Clim. Past Discuss., 11, 603, 2015.