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The Cryosphere Discussions

# *Interactive comment on* "The case of a southern European glacier disappearing under recent warming that survived Roman and Medieval warm periods" *by* Ana Moreno et al.

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General

The authors provide results from a focused local study on remains of a glacier at Monte Perdido (MPG) in the Pyrenees. Their comprehensive analyses concern a question of quite fundamental relevance: are glaciers and, hence, the climate system now changing beyond natural, pre-industrial variability ranges? The main results of the analyses are that the maximum age of MPG can be constrained to the Roman period, and that no ice dating to the Little Ice Age remains present today. The results are interesting and certainly merit publication. They especially have the potential to encourage similar



studies in other regions of the world. Some parts need clarification and more precise presentation as explained below.

Sampling and age structure

Since the inferred age structure is central to the manuscript's main conclusions, it deserves a more clear and detailed presentation. The description of the ice sampling (e.g. on lines 151-163) is difficult to follow and should be clarified. The samples are obviously taken perpendicular to the stratigraphy along a profile at the surface of the stagnant, regularly layered ice patch. The assumption that this ice is cold and frozen to its bed may be reasonable, because this ice cannot warm up above 0°C in summertime but cool down far below 0°C in winter. This effect can explain the low flow velocities but not the ice stratigraphy, which must have been influenced by the active flow of the much larger glacier during the past millennia in question. This leads us to the following concrete questions that should be addressed in more detail:

(1) What exactly is the reasoning behind the inferred age structure of the remaining ice patch? Is it purely empirical from the dating or is it based on considerations of ice flow? The 14C dates are clustered in three different age groups, but the use of a linear interpolation needs better justification. In particular, why can the existence of further (presumably shorter) periods of hiatus or ice loss really be excluded from the presented evidence? Relatedly, if the only support for the hypothesis of a hiatus at 73 m is coming from a distinct dark layer (lines 233 ff., 302-303) – how does this interpretation of concentrated, impurity-rich dark layers fit with what is observed at the glacier surface today?

(2) How are distances along the surface profile transformed into values of (ice?) depths? Does "depth" relate to a former, thicker and less inclined ice body and, if yes, to which geometry/time exactly?

(3) What are "stratigraphic thicknesses" and how are they determined?

(4) If the ice is frozen to bedrock and stagnant, why did the authors find no evidence of neoglacial ice at the base and which process would have led to its removal?

(5) It also needs to be made more clear which part of the glacier was sampled (the lower portion?) and why the other (the upper portion?) was disregarded. Figure S2 should be included in the main text and supplemented with a zoom-in to the visual stratigraphy around the sampling sites for better visibility of the layering. Figure 4 suggests that neoglacial ice in the upper portion did not survive the Roman period, which is not supported by evidence in the manuscript. Are the authors assuming that this ice was removed by basal melting when the larger glacier was still warm-based, by thinning or ice flow? How does this align with the evidence for ice being frozen to bedrock now?

Radiometric and glacio-chemical analyses

The allocation of the samples to a position needs to be revised, at present much is left unclear to the reader. There seem to be two coordinates to consider: First, the position of the sampling site along the transect (MP1-100). Second, the depth below the surface / distance from bedrock. This information should be included in Table 1 to replace “sample depth (m from base)” – which is presumably referring to the distance from the glacier terminus? Again, a clear hypothesis should be stated why a systematic gradient in age of the samples in relation to their position on the glacier is expected? If the ice is stagnant, why is older ice expected closer to the terminus? The depth information should also be provided for the glacio-chemical datasets (especially Pb/Al and Hg of Figure 3).

The selection of 14C data for dating needs clarification, especially because a substantial number of samples is disregarded. The WIOC technique is state-of-the-art but only one WIOC sample is used to construct the chronology. Known difficulties with the interpretation of dating derived from macroscopic 14C, such as reservoir effects need to be addressed in more detail. Dark and dust-rich layers can be biased either through



incorporation of already “old” carbon (e.g. Saharan dust) or accumulate at the surface over a longer time period without ice formation. Regarding the pollen dating, which are presumably too old, the authors hypothesize that they originate from older ice which had melted and percolated through the ice. If this is true, how can such a process be excluded for the other radiocarbon dates?

Percolation of meltwater can also lead to redistribution of chemical impurities – would this be relevant at MPG and if not, why not? Along the same lines, it is important to give more attention to the glaciological settings of the site when interpreting the glacio-chemical records. Based on the presented hypothesis (Fig. 4), the MPG would have undergone substantial changes regarding its ice formation, possibly from a typical firnification process during cold periods to hiatus and melting during warm periods. An exposed glacier surface can lead to concentrated values of impurities, which would be more frequently the case in warm periods such as the roman or medieval period. In this sense, it is not clear that the heavy metals and their ratios should directly reflect any regional mining or smelting activities – this should either be removed or supplemented significantly by further discussion and justification. Notably, the connection between mining activities and heavy metal ice core records in the Alps was made at very high elevation locations (>4000 m asl) with a quasi-continuous snow sampling behavior.

Considering these points, the respective part of the manuscript dealing with the interpretation of the glacio-chemical analyses needs to be substantially revised and shortened. The main support for the conclusions of the manuscript provided by the impurity analysis is the absence of ice dating to the industrial period. This point has value for the manuscript. The relation to mining activities and chronological support through the comparison with the Marboré Lake record seems, at present, speculative.

Some minor technical comments can be found in the annotated file.

Wilfried Haeberli and Pascal Bohleber, 3 July 2020

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2020-107/tc-2020-107-RC2-supplement.pdf

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