Climate of the Past Discussions

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reviewer/editor comment on cp-2021-161

Alberto Reyes (Referee)

Referee comment on "Terrestrial records of glacial terminations V and IV and insights on deglacial mechanisms" by Fabrizio Marra et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-161-RC3, 2022

The manuscript by Marra et al. was reviewed formally by two anonymous referees, together with a community comment.

On the positive side, the community comment from Gianluca Marino notes that radiometric dates for intervals of rapid sea-level rise are valuable, particularly for Middle Pleistocene glacial terminations that are not as well studied as T1 and T2. Dr. Marino also noted the puzzling conclusion in the Marra manuscript regarding meltwater pulses during the glacial maxima preceding T5 and T4, and recommended better integration of the results and discussion in order enhance the readability of the manuscript.

The two anonymous reviewers were more critical. Both reviewers felt that important details of geological setting and—in particular—the stratigraphic/sedimentological framework were lacking. In turn, the reviewers argued that the lack of detail made it impossible for readers to consider alternative explanations and evaluate the suggested correlations within the basin. One of the reviewers also highlighted interpretive concerns with respect to the interpreting minimum/maximum/direct ages for sedimentary units based on the Ar-Ar dates and some inconsistencies between lithostratigraphic description of key sites between the Marra manuscript and the original publications. Finally, both reviewers felt the manuscript lacked polish. The author responses to the reviewer comments were largely dismissive; the authors requested an additional assessment, which I am providing as editor but also as a geoscientist with a background in Quaternary stratigraphy, geochronology, and paleoclimate.

Though the objective of the manuscript is indeed to shed light on global-scale sea-level rise during deglaciation, the story relies on outcrop-scale lithostratigraphy and correlation within a basin. As such, I don't think it's unreasonable to expect: (1) a decent level of lithostratigraphic description, including nature of contacts between units; (2) justification of correlations made across the basin; (3) justification of any deviations from published interpretations/description on those same sediments. Unfortunately I find myself agreeing with the two reviewers on these issues. The lithostratigraphic descriptions are not sufficient for readers to evaluate the significance of the coarse units with respect to a region deglaciation signal. The correlations of relatively thin coarse units across >10 km are similarly not well described/justified. Rev 1 points out some discrepancies between published accounts and description in this manuscript for key sediments at Isoletta and Ceprano boreholes, and I agree that these discrepancies are potentially non-trivial for interpreting the significance of the coarse horizons. I also agree that the sedimentation rate calculations don't seem appropriate here, given the mix of lacustrine and fluvial sedimentation and what appear from the figures to be some scour surfaces.

I have no doubt that the geochronology analyses are solid, but I do share some concerns with interpretation of these radiometric dates. Ages for several key samples (e.g. CE-1, CE-2) are based on clusters of only two young crystals; these are important dates which unpin much of the discussion. But then the reader is asked to just dismiss a coherent cluster of five too-young ages for sample BL-5 at San Giorgio al Siri, with little justification. I also can't agree with the assumption that the maximum ages based on single crystal ages should be regarded as direct ages, which is provided on lines 286-287 and p9/line19 of the supporting materials with limited justification. Of course I don't need to agree, but more justification should be provided when pushing detrital mineral chronologies so hard.

WIth all these uncertainties, the discussion section that relates the evolution of basin fill aggradation and erosion to glacial/interglacial sea-level change starts to feel overly ambitious, particularly when assessed at very optimistic temporal resolution against the insolation record. I do think the ultimate conclusion, made on lines 537-543 re: regional similarity of the aggradational successions, is pretty cool. And I think this approach is a clever way to indirectly date episodes of deglacial sea-level rise. But unfortunately it is very hard for a reader to tease out this story from the manuscript, and I don't think the conclusion is yet adequately supported by the data and observations.

There are, finally, some smaller points that are easily fixed but numerous enough that they detract significantly from the reader's ability to easily piece this story together. Some examples:

-It is not easy for readers to link locality names to individual dates across text, figures, and Table 1.

-Numerous references are presented as superscript numbers in tables and figures, but nowhere are the citations association with the superscript numbers

-Supplemental figure callout citations in the main text are commonly incorrect (e.g. line 317 Fig S1 seemingly should be S5; line 332 Fig S2 should be S6; line 340 no callout to Fig S7 for Isoletta)

-Inconsistencies in ages reported for the same sample at different points in manuscript (e.g. for BL-5 at San Giorgio al Liri, reported as 310+-12 at line 501, 300+-12 in Table 1, and <305 in Fig 5. Another: dates reported on Line 541 and 548 don't appear in Fig 8 in spite of a callout to that figure.

-The important Fig 6 is very hard to decipher.

The author responses suggest that these criticisms of the manuscript are not appropriate because the study is intended to tell a paleoclimate/deglaciation story, rather than a stratigraphic/sedimentological one. But I fundamentally disagree: the proxy here is the sedimentary record, and no matter how good the geochronology might be, readers clearly want to see better justification provided for this basin's sedimentary record of response to glacial/interglacial sea-level change.

Though I think the topic is important and there's potential here for a very interesting paper, at this point my recommendation is that the manuscript is not suitable for

publication in Climate of the Past in its present form.