

Clim. Past Discuss., referee comment RC1  
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## Comment on cp-2021-161

Anonymous Referee #1

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Referee comment on "Terrestrial records of glacial terminations V and IV and insights on deglacial mechanisms" by Fabrizio Marra et al., Clim. Past Discuss.,  
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I read with interest the manuscript "Terrestrial records of glacial terminations V and IV and insights deglacial mechanism" by Marra and co-authors. This work shows some advances in the knowledge of the Liri basin but in my opinion it lacks of alternative interpretations of the sedimentary data and consequently of the sedimentary evolution. The interpretation seems to be equalize to a single hypothesis, the most unlikely, in order to be suitable for the Journal. I think that the work has to be rejected; it needs to be completely reconsidered and then submitted to another journal focusing not only on climate but also on geodynamics and basin analysis.

I list the reasons why I made this decision as follow.

- In the title the Authors clearly stated that the discussion will be about deglacial mechanism during terminations. But, first of all, where are the glaciated areas? No figures show them, no mentions in the geological background. In the Liri catchment, according to Giraudi (2011) evidence of glaciations can be ascribed to the LGM, older glaciations are lacking, or not yet studied. Anyhow, potential glaciers could be inferred in the Ernici, Meta, and Serralunga mounts. These are characterized by karstic environments, so the potential meltwater could have been captured by karstic circulation (for the argument see Zebre and Stepisnik, 2014 *ESPL*). Moreover, the supposed meltwater streams, before reaching the study area, could have been deposited in upstream basins, like Sora and Atina. For example, the Pescosolido alluvial fan. So, for having deglaciation, it needs glaciers, and these are not reported. This question can alone show the weakness of the work.

- Geological background. The Liri catchment is characterized by several mountain basins that can act as sediment trap. The geological background is focused only in the Latin Valley, skipping the innermost sectors towards N and NE. A better framework of the tectono-geological and geomorphological evolution of the catchment, including Roveto valley, is basic for understanding the sedimentary evolution of the drainage, and the possible presence of meltwater streams. This information can also help to understand the presence of lacustrine basins and their fading into fluvial environments. In Muttoni et al (2009 *EPSL*, fig 1) the presence of damming ridges were supposed. Considering the presence of the wide lacustrine basin of Sora just upstream, a break of the threshold (see Carrara 1991 *Il Quaternario*) could have completely renewed also the downstream sectors between Ceprano and the confluence with the Garigliano river. After the junction, the river flows through the southern sector of the Volsci range at the boundary with the Roccamonfina volcano, this before arriving at sea. I wonder if such volcanic structure took a part in the upstream basins' evolution. Active faults are also documented (Bidittu et al. 2012). Moreover, tectonic structures reported in figure 2, especially faults A and B are not reported in the recent geological map (Ceccano Sheet) at the scale 1:50.000 (Centamore and Dramis, 2015) ([http://www.isprambiente.gov.it/Media/carg/402\\_CECCANO/Foglio.html](http://www.isprambiente.gov.it/Media/carg/402_CECCANO/Foglio.html)). How the sedimentary evolution described in chapter 5.1 and figures 6 and 7 can be presented with supposed fault?
  
- Sediment characterization. The sedimentology is related to previous works. The reinterpretation of the geological sections needs to be justified. For example, Isoletta section showed in the supporting material (figure S7) shows a layer of gravels that in Pereira et al (2018 *QSR*) are not reported but only sand layers, with volcanoclastic facies in the lower part. The gravels drilled in Ceprano cores, at -34-38 m, are described in Muttoni et al (2009 *EPSL*) as "subangular" and ascribed to an alluvial fan. Are these related to the Liri? Or to a stream flowing down the nearby Volsci mounts? Any provenance analysis? For example, in Devoto (1965 *GR*) there is a rough description of the gravel succession in Ceprano and S. Giorgio al Liri, pointing to a mix of volcanoclastic clasts and carbonates, with abundant quartz in the sands. The correlation across the basin of gravel units, actually very thin, must take into consideration facies and provenance. They can be simply different sedimentary episodes not strictly related. Also the development of volcanoes in the Volsci can have contributed in some way to geomorphological modifications, including the availability of volcanoclastic sediments, as reported by Pereira et al. (2018 *QSR*).

- The 8 new datings provided in this work are reported in different way in the text. Especially CE-1 and CE-2 have to be considered as *post quem* ages as reported in some figures but not in the table, in figure 8 or in the text at lines 293 and 295. The assumption at line 295-297 is not acceptable because they take into consideration just a single grain and there is no reason to think to the possibility that also younger crystals can be found in such reworked sediments. In this perspective, CE-1 indicates that the age of the gravel GH1 can be 452 ka to about 400 ka, the sedimentation of such thin gravel bed can be instantaneous geologically speaking, in respect to the overlying lacustrine deposits. CE-2 is again *post quem* and may be younger. The age of GH-2 is based on two *post quem* datings (Fig.6) and at Isoletta the original description (Pereira et al. 2018 *QSR*) shows sand and not gravels. The sedimentation rate of figure 4 has no mean because it is referred to 2 points that are *post quem* ages, the sedimentation rate reported in Muttoni et al (2009 *EPSL*) can be taken into consideration, but the new ages suggest a re-discussion. I also ask why the information given by pollen analysis of the Ceprano 1 core reported in Manzi et al. (2010 *JHE*) is not discussed for matching to your climatic discussion. The Pignataro Interamna upper lacustrine is tentatively ascribed to the fluvial unit upstream, but no chronology is provided for such correlation. In this way the chronology is too poorly detailed for very tight climate discussion.
  
- Alternative driver mechanisms. In such complex structural and geomorphological sector of the Apennines, the only hypothesis considered for depicting the evolution of the Latin valley (Fig. 7) is the most unlikely: the deglaciation from unknown glaciers and the relationship with sea-level changes of a coastline located far away and separated from the basin by a range and the Roccamonfina Volcano. The presence of gravel layers cutting lacustrine deposits (GH1) can be also determined alternatively by the failure of a dam or a threshold, and this can be ascribed to fault activity, threshold erosion, river piracy, etc. The gravel progradation can be ascribed to upstream reorganization of the catchment due to uplift, drainage changes, volcanic activity, etc. The abundance of travertine units (see Carrara 1991 *Il Quaternario*) could have also played a role. The presence of sand and gravels in a fluvio-lacustrine environment, such as the middle and upper succession can be simply related to channel avulsions. It is possible that all these drivers can be discarded, but it would be important to know why. The relationship to sea-level changes is related also to the Tiber valley (also too largely described in the setting), but I personally think that the geological/geomorphological framework is quite different. Anyhow for a robust correlation to sea-level changes and discussing about climate cycles, and also about depositional sequences, the availability of coastal successions in connection to the continental would be basic. No mention is provided about the coastal succession and they connection to those of the Latin valley. Here the separation from the coast area by a range with an active volcano hamper any consideration. Let's why I think that chapter 5.3 is, in this perspective, a nonsense, especially the recognizing of early phases of se-level rising.

- Introducing figures need to be more informative (like in Pereira et al., 2018 *QSR* for example), especially figure 2 that would include all the information for understanding the complexity of the area. Figure 6 is key for understanding the correlations, some logs (also those provided in the supplemental data) are different from the original ones. What are the dashed lines? Basal surfaces? What represents the blue one? Why some of them cross the travertine in Pontecorvo? The Pignataro Interamna upper lacustrine correlation with fluvial unit of Isoletta looks a bit forced. Figure 7 is quite rough and many steps are unclear (also following the text).

best wishes