

Clim. Past Discuss., referee comment RC2  
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## **Comment on cp-2021-161**

Anonymous Referee #2

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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 have carefully read the article entitled "Terrestrial records of glacial terminations V and IV and insights on deglacial mechanisms".

From a formal point of view, the overall presentation is well structured and clear, the language used by the authors is fluent and precise and the manuscript is easy to read. The references are up-to-date, including recent ones.

However, although it provides new chronological data on sedimentation events that may help reconstructing a palaeoclimatic interpretation in the basin, there is important stratigraphical and sedimentological information missing in order to back up the palaeoclimatic inferences presented in the article. Therefore, I think that the article should be accepted after major revisions have been undertaken.

In terms of the content, as I am not familiar with the geology of the region on which the study is focused nor with the references or previous geological work developed in the area, I have approached this revision purely as a stratigrapher and sedimentologist, looking at the formal aspects of the manuscript, at the type of data supplied by the authors and checking whether the discussion and conclusions they provide are soundly supported by the data they present and if they fit into the scope of the journal.

I will start with a general overview, to then land onto the specific comments addressing questions or issues that have arisen while reading the manuscript.

### **General overview**

Although the article shows a thorough chronological study, proper stratigraphical and sedimentological interpretation of the sections is missing in order to provide a solid background for their conclusions regarding the past climate in the studied area.

From my point of view, the basis for a proper chronostratigraphical framework in any basin requires a previous solid stratigraphical framework to correctly locate the beds into all the sequences, which needs a detailed correlation among the different sections, that in turn requires a characterisation of the facies and an interpretation of the different subenvironments that they represent. Without this, the reliability of the chronological frame is at risk.

From a sedimentological point of view, the article is lacking a proper study on the facies nature and their environmental (climatic) interpretation. They mention at the beginning three granulometric classes that they propose, which from a palaeoenvironmental point of view is too simplistic, but then they use an old classification by Devoto, 1965 when describing the stratigraphical sections, an old paper that is inaccessible to most readers. Apart from a very vague description of the three types of sediments that Devoto mentions, no real facies analysis is performed. Moreover, a detailed facies interpretation may provide valuable climatic information that has been disregarded in the present study.

Even when the authors have nine stratigraphical sections available, the correlation panel shown and the palaeogeographical reconstruction of the basin in different moments are too basic, so the reader does not know what kind of depositional environment is represented by each section nor is able to determine whether the correlation among different gravel beds has been properly carried out, which directly affects the dates that the authors present as the base of their work.

Regarding the linking of these continental gravels with sea-level changes, I personally think that without first establishing a proper stratigraphical frame for the continental area and without detailed correlations with other sections in coastal areas, it is too risky to interpret variations in potentially fluvial grain size as sea-level changes due to glacial terminations. Upstream sedimentology may be affected by tectonism, climate and base-level changes. If there was a lake damming the water at some point in the basin between the upstream facies (potentially related to glaciation/deglaciation events) and the mouth of the river (where sea level changes would have a direct record), the lake would act as a local base level, its fluctuations affecting the behaviour of the sediments upstream, and thus disconnecting their response to the sea level changes. The authors keep mentioning the word "lacustrine" without properly describing the facies or the subenvironments in which they appear, not considering whether there was a lake, several lakes, wetlands or ponds (something relevant regarding the potential existence of a local base level) or the type of river that deposits the coarser sediments (which could also be relevant for climatic inferences).

Apart from other potential issues related to the presence or not of glaciers in the source area, which is also something that should be properly addressed and discussed, a proper facies analysis must be done, the depositional environments should be properly

interpreted and their potential influence in the relation with the sea level changes should be considered as part of the discussion of the ages of the events dated, something that right now is missing in the article.

### **Specific comments**

pp.7, lines 164, Section 3.1. I don't see the connection of the three lacustrine facies described by Devoto (1965) and the three main granulometric classes proposed by the authors. There is no correspondence in grain size nor type of sediment. I think the authors should explain how this old facies description fits in their new sedimentary frame. In fact, when going further into the manuscript, the authors do use again these three lacustrine facies to characterise the sequences, but there is no clarification on how they correlate to the three main granulometric classes. I believe some kind of scheme or table should be provided, to show how these different classifications relate to each other, both in general and for each of the stratigraphical sections. This would be helpful information, especially taking into account that the three so-called "lacustrine facies" lack a proper sedimentological characterisation in the current article. Moreover, the three main granulometric classes are not used again throughout the manuscript, so I do not see the point of explaining them in the first place.

For a reader not familiar with the region, so many names of sections make it difficult to follow the text and to mentally situate each one, and figure 2 size does not help at all. This figure should occupy a full page (if it is the responsibility of the journal, I strongly ask them to rethink the size of this figure), the dots where each stratigraphical section is located should be easier to spot (and maybe represented differently if they are newly studied sections or re-analysed sections from the literature), and the names of the sections should be readable without needing to zoom in. If the reader is somewhere else in the manuscript and needs to go back to this figure to check these locations, it should not be so difficult as it is now.

pp.7, line 182. References are needed here about these previously produced datings

- 7, lines 183-184. The names of the sections that are no longer exposed should be mentioned here in the main text (I assume these would be the Cava Pompei, Colle Avarone, Isoletta, Lademagne and Ponte Corvo, shown in Supplementary Material #3) and the references from which these sections have been obtained as well, independently of this information being or not already in the supplementary data.

Regarding these supplementary data, the authors show no stratigraphical section of Ponte

Corvo, just a photograph in which no gravel layer can be seen. If this is all the information available on this outcrop, I do not see how this outcrop can be properly correlated to the other sections from a sedimentological point of view without proper information about the relative position and thickness of the supposedly present gravel beds with regards to the tephra layers. I think more information and a proper stratigraphical section of this outcrop is needed if these two datings are to be included in the general correlation scheme. Without it, I do not see the point in including this section in the present study.

pp.7, line 190. I am not sure that from a sedimentological point of view, the interpretation of the third granulometric class proposed by the authors is appropriate. They do characterise the facies regarding the energy of the environment producing them, but in terms of the depositional environment, a distinction should be made whether the sediment belongs to the floodplain of the river, to an alluvial flat (I have seen no mention of alluvial fans at all, but I would imagine there would be some coming down from the Apennines and potentially reaching the main valley from time to time) or to palustrine/lacustrine facies. This distinction may be useful for later climatic interpretations.

pp.13, line 310. How do the authors know that these lacustrine sediments have a relatively constant sedimentation rate? First of all, there is no discussion on the nature of these facies, so they could either be palustrine or lacustrine in nature. If they were palustrine, frequent pauses in sedimentation may occur, as palustrine facies are often dried up and then flooded again some time later, generating a discontinuous record that would not have a constant sedimentation rate.

Figure 4. I would think that "white carbonatic mud" is a too vague term. Is it a marl or a calcilutite? It does make a difference, as the lack of any proper limestone facies makes me think this is no lake, but maybe just a shallow pond (and this has implications for the sedimentation rates and the environmental interpretation, as well as for the way the authors use the term "lacustrine" throughout the article).

- 13, line 330 and pp.14, line 356. These sentence sounds as if the authors lack enough information about the sediments. This description sounds rather vague (I don't even think the word "travertine" is correct) and a rock that looks "something like a travertine" could also be a laminar calcrete. Given the lack of facies analysis in the article, I worry the authors may be mistaking one for another. Travertine deposition, following the current terminology (e.g. Arenas et al., 2010) requires the presence of a hot spring, which could happen, being in a volcanic zone, and would be independent of the climate. However, a calcareous tufa (also a laminated carbonate, see Arenas et al., 2010 for a further description) would usually imply warmer conditions of deposition, and they have in fact been used as indicators of interglacial conditions in temperate areas (e.g. Pedley et al., 1996). On the other hand, a calcrete requires an arid climate and no sedimentation, and may have local, or sometimes basin-scale implications in terms of the sedimentary processes taking place. My point is that these three possibilities would have different climatic implications. I think if the authors want to establish climatic patterns, they should be taking a closer look to the sediments and not just focusing on the gravel layers. If they have already done so, I strongly recommend them to include proper descriptions and photographs of the different facies, as it is

impossible to know if this has been done with the information provided in the current article.

Figures 4 and 5. The patterns used by the authors to characterise the different facies in the drawn sections are difficult to distinguish from each other. I would recommend to widen the pattern to make it easier to read and.

Figure 6a. I do not understand the criteria behind this correlation, when a proper sedimentological analysis is missing. Although in the figure caption the authors refer to the text for further explanations, I have not found them. I think this correlation is too simplistic to capture any nuances related to climate changes in the basin. The authors do not even refer to the three facies types of Devoto (1965), they just generalise and divide the sections into a lower, middle and upper lacustrine successions, without really providing information on how they have decided on this division and without providing the sedimentological features and palaeoenvironmental interpretation of these three successions, which could significantly alter the correlation among layers of different successions and therefore, the relative position of the dated beds, thus changing their chronostratigraphical scheme. Moreover, they add some dotted lines to correlate beds within those three successions, without a clear stratigraphical or correlational reason to it (for example, the darker blue dotted line connects the bottom of the Ceprano sequence with the apparent top of the same succession in Ponte Corvo section and then with a seemingly random point within the same lower lacustrine succession in the S. Giorgio a Liri sequence. I believe a detailed correlation scheme, based on sedimentological and stratigraphical evidence, clearly explained and shown, should be provided by the authors, in order to support the relative locations of the dated beds.

Figure 7. The authors show the sedimentary evolution of the basin, but if their intention is to propose climatic conclusions to their work, I believe this interpretation is too simplistic, and I don't mean to make the figure more complex. It is ok to visually simplify the figures and to generalise stratigraphical events to represent the different stages in the evolution of the basin. However, the information presented in the figure is incorrect.

First of all, the authors assign a 31cm/kyr sedimentation rate to the lower lacustrine succession. That is an unprecedented rate for these type of sediments and therefore should be well justified. Earlier in the text, they did give this same number for the sedimentation rate of the gravel beds (which would make sense), but I do not believe that this could also be the sedimentation rate for a lacustrine environment (e.g. a sedimentation rate up to 5cm/kyr was obtained for the depocentre of a fluvio-lacustrine basin in Southern Spain in Pla-Pueyo et al., 2011).

In stage C, they propose lacustrine to fluvial aggradation. They have not mentioned at all before what kind of facies changes support this evolution. Moreover, they have described all of the sequences as lacustrine, but then they have not properly discussed what parts are fluvial and what parts are lacustrine.

From stage C onwards the figure shows the sediments thinning towards the right side (which by the way, should be indicated as a cardinal point, there is no orientation to the figure) but I do not see what kind of feature could be forcing lacustrine sediments to end laterally in this manner if there is no erosion at that precise time (subsidence would not make lakes end laterally in that way, but would probably cause a change in the drainage pattern, preventing the water from forming lakes and maybe causing the sediments to move downwards, forming alluvial fans or deltas. I do not see the logic of this right slope.

In Stage G, the authors propose erosion and travertines progradation. As I understand, this is based on Devoto (1965), but due to the obvious difficulty of accessing such paper, and that this carbonate is supposed to bear cold fauna (Pentecost, 1995) I would strongly recommend the authors to provide photographs of hand samples and thin sections. If not, there is always the possibility of them being confused with a different type of carbonate with a different climatic meaning.

The information about the sedimentation rates in figure 7 is also contradictory to the one provided in pp.24, line580, where a sedimentation rate of 2.3mm/Kyr is assumed for the gravel aggradational sequence. It does not make sense that for a lacustrine sequence, the authors obtain a rate so large and for a gravel sequence, that supposedly deposited quite fast, the rate is below the one calculated for silts and clay in a fluvial environment (Pla-Pueyo et al., 2011). I strongly recommend the authors to recalculate all the sedimentation rates in the paper, not just for the gravel beds, but also to all the sedimentary intervals that appear in the different sequences, once they have made a detailed stratigraphical correlation, and to make sure that these rates are coherent with the type of sediment, the subsidence rate and the expected processes in the place for which the rate is calculated.

**Section 5.3.** I honestly don't think the authors have enough data nor are working in the right place to propose any triggering mechanisms for deglaciation, as this would have to do with whatever processes are taking place in the Apennines, and on a wider scale affecting all of the basins, not just the studied basin itself. I would totally avoid this section in the article, unless the authors are able to provide information of the same quality proving the existence of the same events in all of the affected basins and link it with proof from the Apennines themselves.

As the conclusions of the article are directly affected by all my other observations, I see no point in repeating myself.

### **Technical corrections**

The authors should agree on the term to refer to the Latin valley and stick to it throughout the manuscript, as sometimes it is spelled as Latina (for example, in figure 2 caption or in line 150) and others is spelled Latin.

**Figure 2.** First of all, the fact that the regional map is called B and the local map is called A is counterintuitive and leads to confusion. I would recommend the authors to change the labelling.

Secondly, and this may be a not for the editors rather than the authors, the size of the whole figure is too small and the legend is almost impossible to read, it requires so much zoom that when you are able to read it, you miss the image in the screen. I would recommend to use a full page if possible or at least, to increase the font of the legend to make it easier to read.

Third, following the legend, the colours used in figure 2A are confusing. On one hand, they are supposed to represent topographical height (which I am not sure is so relevant, I think a geological map would be more useful than a topographical one) while on the other hand, the same colours are used to represent the Meso-Caeonozoic limestones and flysch.