Reviewer 2 comments

General reviewer comments:

In the Paper titled “Miocene basement exhumation in the central Alps recorded by detrital garnet geochemistry in foreland basin deposits” Stutenbecker et al. use a relatively new provenance tool to infer a minimum peak age of the exhumation of the External Alpine Massifs and their consequent exposure as a surface lithologies. Their major outcomes highlight the possibility that portions of the external massifs have been exhumed and eroded since ~14 Ma. This could be regarded as a potential novel find and I think that is a good starting point to speculate on the models of exhumation of the External Basement Massifs in the Alps. However, in my opinion, their work has a few new data to convince the audience that the onset of External Massifs Rocks has been driven during the mid-Miocene by high denudation coupled with crustal delamination and buoyancy-driven vertical uplift. They use this model as a key to interpreting their detrital data. This is, due to the lack of data is a bit redundantly stressed and needs to be reformulated. I, therefore, suggest the authors reworking the structure of their paper focussing in describing the previously proposed model with more objectivity with respect to their new data.

I have tried to highlight two major points of weakness of this manuscript which I think the author might want to improve:

First the paucity of new data, the authors present results from only three samples (and additional previously published data) comparing the chemistry of the garnet with the source rocks information (3 additional samples). This is a good pilot approach but needs more constraints, possibly expanding the area of investigation to different fan deposits in the foreland to gain confidence in drawing interpretation for the onset of exhumation and erosion of the External Massifs Units. Furthermore, I find that the authors lack while interpreting/presenting their detrital datasets of a correct acknowledgment and discussion of works that focussed on the present-day evolution of detrital thermochron/petrographic proxies in the Alps. I think that would be useful to compare other proxies available in the literature with garnet chemical composition. What other analytical detrital/in-situ methods describe?

Second, the authors seem supporting “a priori” the model of “buoyancy-driven vertical displacement” associated with slab dynamics and erosional unloading, as a prerequisite to interpret their dataset (e.g. Herwegh et al., 2017; Nibourel et al., 2018). Those models and other proposed interpretations could, in my opinion, be described in more detail in the introduction, whereas in the discussion the authors reconcile their data with the geometric interpretation of Nibourel et al. (2018). This is an interesting ongoing discussion and might be expanded (e.g. Herman et al., 2013, Herwegh et al., 2017, Schildgen et al., 2018). I would suggest redrawing your discussion by inserting yours and available literature data in a more precise metamorphic, tectonic and erosional patterns context. The latest, in my opinion, would require a bit of discussion on how the foreland deposits might have been biased by e.g. river patterns reorganization during Miocene to present-day time, heterogeneous erosional patterns along strike, glacial processes, etc. Those processes are important for the evolution of the detrital record and need to be accounted while interpreting provenance data.

It would be really helpful to show a compilation of different available datasets as a map view tracking External Massifs source units and their contribution in the Molasse sedimentary deposits. How does the hinterland info’s are correlated with the detrital ones? A Map would greatly help the reader to track source hinterland and detrital provenance, the author cuould benefit by using their previous

work e.g. Stutenbecker et al. (2017). An effort has been done in Figure. 2. However, there is not a correspondence between the legend and metamorphic grade indicated in the map. This map might be redrawn as a simplified map highlighting the information that is essential to understand the authors' discussion.

Overall, the paper reads well but there are a few changes required. I have noticed a few interferences between results description and discussion, this might be changed. The English language is good, although I might not be the best example of scrutinizer on this topic, I, therefore, suggest a native English colleague reading the manuscript once.

Comments byline:

25. “Tectonic processes influence” I find "influence" a bit week, maybe change with "regulate" or "drive" the evolution of mountain chains.

34. Please be more specific, what you mean for highest erosion rates in the Alps in (mm/yr) or as you mention in line 43 km/Myr.

61. New provenance studies that used detrital thermochronology multi-proxy approach to constrain exhumation rates and its spatial variability has been recently used in the Alps (e.g. Carrapa et al., 2016; Tectonics; Gemignani et al., 2017. Tectonics) and need to be acknowledged.

72-75. Additional information to what. Does the author mean to previously published papers? Such as for instance Stutenbecker et al. (2017). Tectonic forcing of the Molasse basin or in the hinterland? Please be more specific.

82-84. Reference is needed

105. architectural elements are capital, column, architrave, etc. Do the authors mean tectonic units or litho-tectonic units?

119-120: It would be useful if the author could refer to a temporal frame when invoking for timing and rates comparing it with other’s colleague works. This will help the reader to follow the argumentation in chronologic order.

106-142. What is the relationship of this description of the potential source rocks with the garnet composition? This is important for a clear understanding of the relationship between hinterland source units and syn-sedimentary sequences in the foreland. I think would be worth to expand this description with a map or figure showing potential source in the hinterland and their present-day distribution in the foreland units.

143. The Napf fan

It is the first time that this fan is mentioned in the text. This information is missing in section 1 and should be introduced before in the text.

208. Fertility is a specific definition applied to detrital sediments. Please make sure you properly introduce this concept and acknowledge the promotors of this new definition.

213. What is the effect that you might obtain by using pestle and mortar on the round-shaped grains of garnets? There is not a less invasive mineral separation technique?

228-229. This might be related to an incorrect mineral separation approach and mislead to biased interpretation of the data. How could you check for consistency of the data? In other words, how fractures might bias your chemical analysis? Please explain.

229. Could the authors specify the amount of “randomly selected grains”?

246. figure 4 is confusing because the authors use black and white tones to indicate a different aspect of the different ternary plots. This could be improved by using a colored version of the figure with a color-coded legend.

272-275. Here, you are discussing the data. Please objectively describe the data.

295-297. Here, you are presenting results. Please reformulate this sentence.

348-354. The authors describe their data but what is lacking, in my opinion, is a clear discussion of what is the importance of those data for interpreting the evolution of the External Basement Massifs. In particular, I think that would be really interesting to insert this new preliminary finding i.d. the External Massifs Units reached the surface at ~14 Ma as constrained by Grn chemical composition, in relation with the thermokinematic model of low-temperature chronometers arguing for a sustained increase of denudation during the Pliocene. This has been the focus of a recent debate in literature see e.g. Schildgen et al. 2018 vs. Fox et al. 2015, 2016, Herman et al., 2013, etc., and I think it is important to discuss it.

363-364. What is the present-day evolution of the detrital provenance/thermochronological signal? Which units constitute the present-day major erosional contributions in the Alpine river patterns? I think that might be useful for the authors to acknowledge recent studies that worked on tracking source rocks information with detrital thermochronologic evolution of modern river sands in the Alpine river patterns. There are several works that investigate these processes in a different portion of the Alps and should be, in my opinion, acknowledged (Bernet et al., 2009, Carrapa et al., 2004, Gemignani et al., 2017; Resentini et al., 2012).

365. “Very young”, how young <2 Ma, <5 Ma, <10 Ma, <30 Ma?

370-393. At this point, it is clear that the compositional change of the garnets in the youngest ~14 Ma foreland deposits with respect to the older ~19 Ma interval (where Grn yield a different composition = different provenance) has been interpreted by the authors as the lower temporal limit for the surficial exposure of the External Basement Massifs units. Using this new observation they argue for “important implication for the tectonic evolution of the orogen” (Lines 375-376). Furthermore, the authors support the geometric restoration of the central Alps (Aar Massif-Helvetic nappes) as proposed by Nibourel et al., 2018, where ~7-8 km of basement rocks have been exhumed and eroded since ~14 Ma lead by “lithospheric mantle roll back” associated with “crustal delamination” and “buoyancy-driven vertical exhumation coupled with surface erosion” of the External Basement Massifs (e.g. Herwegh et al., 2017). This point in the discussion is clear and well expressed, however, I think that you should describe also the other proposed model in the introduction, and, lately, data on hands, describe why your data support this proposed hypothesis. This is, in my opinion, a bit lacking in the text and would require some improvements.