Solid Earth Discussions

Solid Earth Discuss., referee comment RC2
https://doi.org/10.5194/se-2021-129-RC2, 2022

Comment on se-2021-129

Simon Large (Referee)

Referee comment on "Whole-rock and zircon evidence for evolution of the Late Jurassic high-Sr□/□Y Zhoujiapuzi granite, Liaodong Peninsula, North China Craton" by Renyu Zeng et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-129-RC2, 2022

The manuscript by Zeng et al. Presents interesting and seemingly robust new data on the Zhoujiapuzi granite. Based on new bulk-rock geochemical data and zircon U-Pb-Hf isotope and trace element data the authors argue for the granitic melts inherited their geochemical signature from the partial melting of a metamorphosed granite. Zircons are further interpreted to record a two-stage crystallisation model. The manuscript reads well is organised and the main interpretation. In my opinion there are a couple of points that would help the authors in robustly making the point for their favoured interpretation and that I would consider necessary to address.

- I am not convinced by the geochronological data being able to discriminate between two different zircon populations. The geochronology part would benefit from a more careful data treatment. Sometimes individual dates are presented without uncertainties even when trying to resolve a difference of a few Myr (~1-2%), which is below the resolution of individual data-points. should not be the case. I am not familiar with the approach of only illustrating 1s uncertainties as in most studies 2s is used. This would further highlight the overlapping nature of all analysed Jurassic zircons. The weighted means have very low uncertainties (<1%), which to me is maybe a slight overinterpretation of the data. Horstwood et al., 2016 is a good reference for robust data treatment for geochronological data. I would think that with a more conservative approach the temporal differences would fade away. Also, I am surprised that apparently all cores are older than the rims even though they typically fall within analytical uncertainty. I have rarely seen this in an in-situ data-set. Having said that as far as I can tell the data looks high quality. However, it is essential to report standard analyses and their reproducibility. I am sure the authors just forgot to add it but without those standard data the analytical data are not useable. The same applies for the zircon trace element data and Hf isotope data.

- The exclusion of other possible explanations appears slightly simplistic at times. An example would be referring to the small amount of silicic melt generated from differentiation of mafic magma but not addressing the same point about the partial melting of meta-granitoids (The Jurassic intrusions are much more exposed than the Lioaji granites). I believe going into more detail in the individual discussion points would help. Also, comparing the data to other studies and describing similarities or differences might help. If more than one model cannot be fully excluded it is fair to say that as well. Not every data-set can identify the one and only solution.

- I struggle with the current temperature/fO2 discussion. It might benefit from being a bit more in-depth in the main manuscript. See Loader et al., 2022; Loucks et al., Schiller and Finger and others.

Please also see comments on the specific lines in the manuscript.

Line 22: The abstract changes rapidly from being descriptive to the conclusion part. Just saying “Interpretation of the elemental and isotopic data suggests” does not illustrate how the conclusions were derived. It would be good if a few lines were added highlighting how the conclusions were derived.

Line 38: It would help the reader if the reason for this interpretation by most authors was briefly explained.

Line 41: In granitic rocks in general or those in the NCC?

128-132: The relatively broard interpretation here comes very early in the manuscript purely based on CL images. Some Zircons illustrate CL patterns of a dark core surrounded by a lighter domain and again a dark rim. This is not discussed in the text so far. Are the dark cores considered to be inherited or do these zoning systematics suggest a more dynamic system than just early and late crystallisation?

152: I would say the dates overlap within uncertainty.

153: I couldn’t find the grain with supposedly inverse zoning of the U-Pb dates.

166: The assumptions to determine TDM2 need to be described in more detail.

189: What is the definition here of “same magmatism”? Magma reservoir, plumbing system, trans crustal mush, magma chamber? There are very different models about the architecture of magmatic systems, it would be good to be precise here. Especially, as different models use different assumptions on the potential timescales of the magmatic systems.

190: And what about antecrysts (Miller et al., 2007)?

196: What would be the reference to use those activities for that mineral assemblage? It needs to be argued why it I valid to use the same activities for both zircon generations. Schiller and Finger could be a good reference here. Also Gualda and Ghiorso. They also highlight the variation of aTiO2 within individual systems. It might be worth to propagate that uncertainty onto the uncertainty of the temperatures.

198: Could a zircon crystallisation temperature of 498C maybe suggest that not all calculated data are valid?

199: Please explain the relevance of the correlation between U and Ti.

200ff: Maybe see Loader et al., 2022 for a thorough description different potential sources for the Ce anomaly.

204: Again the role of U needs to be discussed in more detail. It is always referred to but the petrogenetic reasons for it are not explained.

205: I would rather say it suggests. Imply is a very strong wording, which I personally would not be comfortable with in this case.

207: I doubt that Breiter is the original reference for this. Claiborne et al., 2006 might be better.

220 ff. This assumes that the magmatic history was very simple over long timescales: just cooling and differentiation over at least 3 Myr with a change in locus in between. Maybe refer to studies that suggest something similar. At the moment most studies suggest far more complex and dynamic magmatic systems.

227: Magmatic rock dating = geochronology?

228: Only in this example it appears to be two stages. It could be more and it could be

different for any other magmatic system.

226-237: In situ geochronology typically really struggles to resolve different magmatic events within single magmatic systems. Especially, at the age range investigated in this study. Ii would suggest referring to CA-ID-TIMS work here to make a more robust point.

254-256: This assumes that the WR geochemistry is equivalent to the melt chemistry the first zircon crystallised from. It would be good to outline why this assumption is valid.

256-257: I can not follow this point. Please clarify. A zircon crystallisation temperature does not automatically mean that the zircon dissolves immediately at that temperature.

Also, what is an initial temperature? It should also be addressed that typically zircon is considered to crystallise late during magma evolution.

288 ff: As mentioned above the temperature argument is currently not very strong. Zircons typically do not record the high-temperature magmatic stage.

295 – 316: I find the argumentation against differentiation of basaltic magma slightly selective. SiO_2 is not the greatest proxy for melt differentiation if only granites are exposed. The resolved SiO_2 window is very small. Maybe the variation seen in the granites is just a matter of slightly different accumulation of minerals in different parts of the pluton? Also, major and trace elements do not necessary show similar signatures (see Klaver et al., 2017). The last point about the volumes is typically dealt with by mafic

magmas composing le lower arc crust while more differentiated ones migrated upwards. I think a more thorough comparison with other studies might be beneficial in really discarding this model. Especially, work by Jagoutz and others on the Kohistan arc.

346-347: Not being familiar with the study of Kamei it is unclear for what reason they argue for a partial melting origin. Can the same line of argumentation be used in this study?

347-348: Previously it was argued that only very little felsic melt can be generated by the differentiation of basaltic melt. But isn't the same true for the partial melting of a granite? In fig. 1 the Liaoji granites are far less exposed than the Jurassic granites. How would this be reconciled in this model?

366-368: The diagram 13b) seems to illustrate fractionation paths. Wouldn't this rather favour a differentiation origin?

368-369: Alternatively, crystallisation of amphibole would result in the opposite fractionation path. The presence of plagioclase does not imply that it was the main fractionation phase as the in-situ plagioclase has no fractionation effect on the WR-chemistry.

410: It reads like they granite crystallised over 4 Myr. I am sceptical that the data can resolve this. Also, did all zircons crystallise in-situ at the emplacement level? Previously, and in the second point it is argued that the early zircons might have formed deeper in the crust.

412ff: I struggle with the current temperature/fO2 discussion. It might benefit from being a bit more in-depth in the main manuscript. bulk-rock geochemistry per definition gives only one value. It would be surprising to resolve multiple stages from it.

Fig 4. What is the reason for the Tm-anomaly in some of the literature data? I can't think of many petrological reasons and it might be the result of sample contamination? Potentially by a flux if the data was generated from XRF beads. In any case, if there is no explanation for it might be better to discard that data.

Fig. 11d) Were all zircons analysed in the same counting mode? Systematic uncertainties could arise from analysing zircon with hugely varying U contents. It would be good to exclude this and describe how the data was analysed and whether a correction was applied.

Fig. 12: The range in SiO2 is very limited and the arrows often seem to rely on the single slightly lower SiO2 value. Is there maybe any more mafic data that could be illustrated?

Fig. 13b) the average composition of the Munihue granite does not seem to fall into the centre of the 6 clustering individual data points. I assume it requires the data point at ~35 ppm Y, which massively pulls the average to higher Y contents. It would be good to argue why the high Y data point is not an outlier. Especially, as it also falls outside the range of other Jurassic granites.

Fig. 14a) The preferred interpretation in this manuscript is partial melting of a granitic source. This figure does not support this point very much.