Solid Earth Discussions

Solid Earth Discuss., referee comment RC2
https://doi.org/10.5194/se-2021-2-RC2, 2021


# Comment on se-2021-2

Nick Roberts (Referee)

---

Referee comment on "U–Pb dating of middle Eocene–Pliocene multiple tectonic pulses in the Alpine foreland" by Luca Smeraglia et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-2-RC2, 2021

---

Review of Smeraglia et al

Nick Roberts

Summary

This paper presents U-Pb dates from several calcite veins taken from faults across the Jura mountains. The dates are used to constrain the timing of various phases of deformation that have effected this fold and thrust belt, with implications for the Alpine orogeny made at the end. The number of samples and number of dates is fine for making some general observations about the timing of brittle deformation. I would consider the dataset big enough to make any grand claims about the phases of deformation and when they may have switched on or off. For the oldest phase, we have two pretty good dates from on fault. The second phase is dated by a handful of good to OK dates from around four faults. With this quantity of data, we do not get a handle of the longevity of periodicity of these faults during these phases. Which would really give us tight constraints on the relationships between faulting in the Jura and what is going on across the orogen. The third phase is only dated by a handful of very poor dates which are not robust. Thus, this last phase is questionable. If the authors are going to include this data, they need to make it clear that the dates are not robust, and that the observations based on the dates are therefore speculative. The youngest dates overlap 0 Ma, and should be removed entirely, and all reference to them.

As with all studies using this method, there is a lot of reliance on the assumption of calcite growth syn-tectonically, or at least soon after slip. The authors present some petrographic data to help with this assumption. The figures are useful, but I would suggest that more information could be provided in the supp files, such as images for ALL samples, and bigger images that are easier to understand. The in-text images are more useful as a summary. They do not really convince me that no alteration of recrystallization as happened. Because the younger ages are themselves suspect, perhaps a discussion of the possibility of post-slip fluid-flow or alteration is no necessary. However, a note on the caveats with this method is always prudent for readers that are not entirely familiar with this method is prudent (which is most readers of course). In others studies, there has been evidence of post-slip fluid-flow and/or alteration (see Roberts et al., 2020 GSF for recrystallization of slickenfibres, and Beaudoin et al., 2018 Geology for mention of young ages, for example).

The paper is very short. It deals with the introduction, geological background, discussion and implications, very briefly. Writing concisely is of course generally of benefit, however, in this case, the conclusions seem rather vague (i.e. foreland deformation is prolonged and related to collision), or based on conjecture (i.e. ". However, we suggest that the Pyrenean orogen, located ~650 km in the SW, was likely too distant to have any effect on the Jura area."). There is very little background and supporting information provided for the tectonics. In any study fault chronology needs thorough linkage with structural work, kinematics etc. if any new insight is to be gained about propagation of faulting, origins of faulting etc.

The states the following methods (1) geological field mapping and fault rock sampling from four major thrusts (From SE to NW: Montlebon, Buron, Fuans, and Arguel thrusts) and three NNE-SSW tear faults (Vue des Alpes, Pratz, and Buron) moving from the internal (most deformed) to the external (less deformed) parts of the Jura FTB (Fig. 1); (2) microstructural analyses 75 with optical microscope and cathodoluminescence to unravel different phases of calcite precipitation.

However, we see no results from point (1) in terms of mapping and what is presumably structural work. For (2), we see images with annotation of veins, but no microstructural work in terms of kinematics, or petrographic work in terms of different calcite generations.

In summary, the paper could benefit from a more thorough presentation of the background data that support the chronology, but are also critical to interpretation of the chronology. It could also benefit from a more thorough description of what aspects are speculative, what parts are based on robust versus non-robust data, and more background evidence to support any new conclusions based on the data.

Results

Sample DA2 – 3.9 +/- 2.9 Ma this is a short array, which is barely indistinguishable from sub-horizontal. Should be treated with caution.

Sample BUS-1 – same as above. The slope is controlled by the most radiogenic datapoint only. Removing the most radiogenic datapoint shows how sensitive the slope is.

Sample PR-V2 is a very poorly defined regression, and therefore not a robust date.

Sample PR-V1-B has a very high MSWD – and clearly is not a single population. Variation at the common lead end of the array indicates mixed lead sources in this sample. So the spots are clearly hitting different components. The age could therefore be erroneous.

Samples

I see no information on the locations of samples such as GPS.

Figures

Figure 2. These field images do not provide enough detail to be used as evidence for tear fault reactivation of orogenic faults, this line of evidence requires more data presentation.

Why not include the concordia plots as a figure?

Data and Methods

In the supp files, for the plots with outliers, the outliers are not clearly indicated. It also not clear that those ages presented are the raw data with the outliers included.

Tables – All OK, but sample information needed.

Line 17 – The subsequent text is divided into thee rather than four categories.

Line 18 – I don't really follow how uplift relating to the great Alpine convergence is pre-orogenic, rather than just part of the orogeny. But I suppose this is semantics.

Line 18 – list in reverse chronological order – here and in all instances.

Line 21 – 0.7 +/- 4.2 – definitely not worth including! 5.7 +/- 4.7 is pretty suspect too.

Line 23 – I would remove this statement and write something more specific to the study area. We know that the convergence history is long-lived from decades of work, and we also know the foreland deformation is linked to the continental collision. These are not conclusions from this study.

Line 34 – This sentence sums up a broad research area rather too briefly for me. The timing and provenance of sediments in foreland basins are still massive research areas in places such as the Andes and the Alps, and this sentence reads a bit like those studies are surpassed by dating of faults. Which is not the case, as they are all relevant and important tools.

Line 36 – U-Th carbonate geochronology can also be applied to carbonate-hosted faults.

Line 39 – I am not sure what the relevance of just discussing carbonate faults is, many fold and thrust belts will be dominated by other lithologies, but could still be dated with this method.

Line 41 – I would remove "To fill this gap".

Line 41 – were other faults sampled and dated but unsuccessfully? Information on this is always appreciated and useful to others to comment on and ideally discuss.

Line 44 – As above, I find this conclusion rather obvious and unnecessary.

Line 52 – I'm sure some older more seminal papers could be cited about the Jura.

Line 122 – Shear veins do not all have slickenfibres, and I would not classify all slickfibres as occurring in shear veins. So the terms should be used with clarity.

Line 122 – Although these primary features represent tectonic slip, they can be overprinted by later fluid-rock interaction. This caveat should always be in mind.

Line 123 – speed of crystal growth in this case is perhaps better stated in relevance to the vein opening rate.

Line 125 – see also many works by Janos Urai, Christoph Hilgers, Paul Bons.

Line 127 – I would add a sentence about the caveat above.

Line 134-139 – To me, with a lack of detailed Pyrenean-Alpine geological knowledge, this interpretation would seem like conjecture. I do not really see a clear line of reasoning why one hypothesis is favoured over another. The only proposal of the current study is summed up in a tectonic cartoon - but anything can be drawn on a cartoon. The link between the faults and what is going on regionally should include data on kinematics, structural geometries etc, with literature data of course.

Line 146 – see above comment on this imprecise age.

Line 149 – Remove all reference to this non-age.

Line 154 – This young age for extensional reactivation is entirely possible, but to me, is the most likely case where one should be looking to try and see whether the age reflects post-slip fluid-flow/alteration, rather than relating to slip itself.