Reviewer #1 Olivier Lacombe

The manuscript by Smeraglia et al. is concise, well written and easy to read. It deals with U-Pb dating of thrusts and strike-slip/tear faults in the Jura Mountains with inference on the absolute timing and sequence of deformation within this FTB. Overall, the paper brings very useful absolute ages for the appraisal of the Jura tectonics, and more broadly of the Alpine foreland. The conclusions are overall supported by the data. I therefore recommend publication after minor to moderate revision.

We thank the reviewer for appreciating our work. We hope that the revised version of the manuscript satisfy the Reviewer. Below, the detailed answers to the comments.

Specific comments

- I am wondering about the most recent age 0.7 +/- 4.5 Ma obtained for the Pratz tear fault and its true significance. I would be much more cautions with this age, and even I would possibly discard it from the interpretation. **Ok, done. We removed that age. See also answer to reviewer #2.**

If I well understand, all the ages are calculated assuming secular equilibrium in the U-series decay chain. As fluids are generally characterized by excess in ^{234}U with respect to ^{238}U, resulting in an excess of radiogenic ^{206}Pb, the calculated ages should be considered as maximum ages (eg, Walker et al., 2006; Roberts et al., 2020). The magnitude of the offset ages due to initial $^{234}U/^{238}U$ disequilibrium can be significant, and the true age could be younger by several hundreds of thousands years. This may significantly impact the already very young age obtained from the Pratz tear fault. In all cases, I would invite the authors to calculate the new younger age that would be obtained for an initial $^{(234}U/^{238}U)$ activity ratio of 2 for comparison, and clearly state that the ages obtained assuming an initial ($^{234}U/^{238}U$) ratio of 1 are thus regarded as maximum ages. **Yes, in case of the young age of age 0.7 +/- 4.5 Ma from the Pratz tear fault, an initial $^{(234}U/^{238}U)$ activity ratio of 2 would result in an age of around 0.4 Ma. However, we removed that age anyway for a different reason (see above). The younger age we report now is 3.9 +/- 2.9 Ma. Assuming an activity ratio of 2 for that sample would result in a true age of around 3.5 +/- 2.9 Ma. Given the low precision of the younger ages we report (4.8 +/- 1.7 Ma, 5.7 +/- 4.7 Ma), we regard the effect as neglectable and not really relevant for our conclusions. For the older ages, the effect is not significant. Also, as we do not have any data to constrain the initial ($^{234}U/^{238}U$) activity ratio we think it is better to not make assumptions and instead clearly state that no disequilibrium correction was applied (see Methods and Supplementary material).**

-I would suggest adding some critical discussion of the number of age results, methodology and interpretations. I am thinking of possible late fluid infiltration and calcite recrystallization yielding young (reset) ages. Petrographic and elemental analyses are important requirements for interpreting calcite U-Pb data. To that respect, the paper by Roberts et al Geoscience Frontiers 2021 should be considered together with Beaudoin et al., Geology 2018; Hoareau et al., EPSL 2021, both of which reported reset ages related to late (hydrothermal) fluid circulation. Although the ages obtained are internally and regionally consistent in terms of tectonic evolution, the authors must be aware of possible limitations of their approach. **We agree with the reviewer and we highlighted in the manuscript that we are aware about the possible limitations of the approach. In particular, we stated that "although U-Pb dating was**

performed on crystals with no indication of later open-system alteration based on CL-microscopy, possible late fluid infiltration and calcite recrystallization cannot be excluded as previously suggested by other studies (Beaudoin et al., 2018; Hoareau et al., 2021; Roberts et al., 2020, 2021). Also, potentially older or younger deformation phases recorded by other veins and slickenfibers not sampled and analyzed here may have been missed. Accordingly, the presented ages should be regarded as minimum ages for the onset of deformation at the studied faults or as maximum ages for its termination." See lines 221-226.

-L62: Mosar (EPSL, 1999) and Lacombe and Mouthereau (Tectonics, 2002) have also provided evidence of ongoing shortening in the Jura. For a fair acknowledgement of previous work, these papers should be referred to. **Ok, added in the references.**

-L93: Maybe I am wrong, but to my knowledge, only some of those tear faults are seismogenic. **Ok, corrected.**

-Fig. 3+caption: please check labelling and description, it seems some undesired repetitions occur (caption of Fig. 3). **Ok, corrected.**

-L134-139: The interpretation of the Eocene ages obtained from shear veins along the NNE-SSW striking Vue des Alpes left-lateral strike-slip fault should be discussed in a more precise and substantiated way. **We have discussed this age in a more precise and substantiated way. Please see lines 237-247.**

First, It is unclear to me how a left-lateral movement along a subvertical strike-slip fault (presumably sigma 1 and sigma S3 being horizontal) may be kinematically compatible with forebulging. Forebulge/foredeep have been widely documented at being associated with extensional structures, either longitudinal or transversal, as recently summarized in Tavani et al (Earth Sc Rev, 2015). The simplistic sketch of Figure 5 and the text do not convincingly support the statement that such strike-slip faulting is related to bulging. **We agree with the reviewer and we modified the interpretation. We propose that the Ypresian-Lutetian tectonic activity can be related to the late Mesozoic-Eocene far field tectonic shortening in the European plate foreland due to the advancing Alpine orogen (Mazurek et al., 2006; Timar-Geng et al., 2006). See lines 237-238.**

Second, it is mainly based on a subjective (gut) feeling that the authors discard the interpretation of this strike-slip fault having moved under a 'pyrenean' ~ N-S oriented compression. There has been a wealth of papers that supported the likely transmission of pyrenean stresses very far into the Pyrenean foreland, in the Paris Basin (eg, Lacombe et al., Tectonophysics, 1990; Lacombe and Mouthereau, C. R. Acad Sc Paris, 1999; Lacombe and Obert, C. R. Acad Sc Paris 2000), in eastern France just in front of the Jura (Lacombe et al., Tectonics, 1993), and even in UK (Hibsch et al., tectonophysics, 1995) where Pyrenean veins were dated by U-Pb on calcite (Parrish et al., J Geol Soc London, 2018). Such efficient transmission is highly dependent on plate rheology and the amount of plate coupling (Lacombe and Mouthereau, Tectonics, 2002; Dezes et al., tectonophysics, 2004; Lacombe and Bellahsen, GeolMag, 2016; Dielforder et al., G3, 2019). Tectonic stresses have been shown to be transmitted more than one thousand of km away from the orogenic front (Craddock et al., Tectonics, 1993; Beaudoin and Lacombe, JSG, 2018), so the 650km limit does not hold. I may understand this is not the core of the manuscript, but I would recommend that the authors be more cautions in their interpretation and avoid unsubstantiated conclusions. At least, this point which is seemingly under debate would deserve a short paragraph with a true discussion (and not only author's feeling) with appropriate references to be added in the manuscript.

Yes, we agree with the reviewer and we expanded the discussion on the Pyrenean origin of the Ypresian-Lutetian tectonic activity. In addition, we suggest that further studies are necessary to better constrain the origin of pre-Miocene fault activity in the European foreland. Please see lines 237-247.

-The sketch of figure 5, even if simplified to carry the first-order message, neglects evidence for post-4 Ma basement-involved shortening and thick-skinned tectonics beneath the Jura (Mosar, Lacombe and Mouthereau, Lacombe and Bellahsen, Ustaszewski and Schmid, …). I would suggest modifying the section to include such evidence together with supportive references. **Ok, modified the figure. See also lines 87-90.**

-Like the 'pyrenean' compression, the Oligocene extension related to ECRIS has been described in the Jura (Lacombe et al, C. R. Acad Sc Paris, 1993; Homberg et al., 2002) but is properly ignored in the evolutionary model proposed by the authors. Of course, if the authors have not sampled any normal fault, they could not date them. But this event should be considered in the regional tectonic evolution in Fig.5, which should not report only 'dated' tectonic phases at the risk of misleading readers unfamiliar with regional geology. **We thank the reviewer for this comment. However, we prefer not to add an additional panel to Fig. 5 related to the minor extensional phase that occurred in the Jura area.**

- Why not attempting at deriving a rate of Alpine thrust propagation in the Jura to be compared with analogue and numerical models of fold-and-thrust belts above a weak evaporite décollement? This would broaden the perspectives of the manuscript I think. **Unfortunately, we didn't find papers showing the rate of thrust propagation of analogue and numerical models of fold-and-thrust belts. We appreciate if the reviewer could suggest us a few papers dealing with this issue.**

Sorry for sometimes referring to papers I co-authored, but I guess I was contacted as potential reviewer because of my earlier work on both the area and the topic. I do hope that these comments will help the authors improve their manuscript. I am looking forward to seeing the revised version of this nice piece of work published in SE.

We thank the reviewer for his suggestions and we believed that they improved the paper and broaden its perspectives.

Olivier Lacombe

Reviewer #2 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 affected 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 one 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 or 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. **We agree with the reviewer and we stated that because of the lack of radiogenic $^{207}Pb/^{206}Pb$ compositions to anchor the extrapolation to a lower-intercept concordia date, the U-Pb ages on the samples DA2, BUS1, PR1-A, PR2-2 show larger errors than those on thrust faults and therefore, the inferences on strike-slip fault activity must be taken with caution. However, we note that most of the isochrons referred to by the reviewer show a tight data array and therefore yield realistic intercept ages. Please, see lines 191-192.**

The youngest dates overlap 0 Ma, and should be removed entirely, and all reference to them. **Ok, done. See also answer to reviewer #1.**

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.

We agree with the reviewer and we added microphotographs (Under optical and cathodoluminescence microscope) of all samples yielding reliable age data in the supplementary material. See Figs. S1-3. In addition, we better described the petrographic data at lines 148-156 and we stated that the U-Pb datings have been performed on calcite crystals showing either a homogenous color or undisturbed growth zonings (except the sample BUS1 sowing multiple sealing and re-opening processes) under cathodoluminescence light in order to avoid the mixing of calcites precipitated in different time intervals and/or as results of recrystallization processes. Please see lines 107-110. In case of sample BUS1, there are clearly different calcite phases observable. Sample BUS1 clearly shows multiple calcite phases with potentially different ages (Fig. 4h). However, the Tera-Wasserburg diagram shows a single age trend with a low MSWD of 0.82 (Fig. 6d). This would not be observed in a sample that experienced crystallization at significantly different times. Therefore, sample BUS1 reflects calcite precipitation within a time interval smaller than what would result in multiple age trends. Please, see lines 227-231.

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 slickenfibers, and Beaudoin et al., 2018 Geology for mention of young ages, for example). **We agree with the reviewer and we discussed this point at lines 221-226, 273-275, and we added the necessary references. See also answer to Reviewer #1.**

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."). **We expanded the section regarding the Pyrenean orogeny, as also suggested by Reviewer #1. Please see lines 237-247.**

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.

We agree with the reviewer and we added a section including structural data from all of the studied faults in a new figure (see the new Fig. 2). Please, see lines 116-131. Then, we discussed the structural data in terms of regional tectonic evolution and how they fit with previous structural data from the Jura FTB and new U-Pb ages from this study. Please, see lines 248-251 and 261-266.

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 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. **We added the structural data from all the studied faults (please see answer above) and we modified point 1 and stated "(1) field structural analyses and fault rock sampling …". Please, see lines 100.**

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. **We agree with the reviewer and we added in the supplementary material a figure with vein microstructures from the different fault. Please see answer above and the new Fig. S1-3.**

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.

We thank the reviewer for appreciating our work and we agree with his comments. We hope that the revised version of the manuscript satisfy the Reviewer. Thanks for your help on our manuscript.

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. **We stated in the text that ages from strike-slip faults are affected by large errors and so they must be interpreted with caution. Please see lines 191-192. In addition, we discussed this data with more caution. Please see lines 271-273.**

Sample BUS-1 – same as above. The slope is controlled by the most radiogenic data point only. Removing the most radiogenic data point shows how sensitive the slope is. **Yes, we agree. We stated in the text that ages from strike-slip faults are affected by large uncertainties and so they must be treated and considered with caution. Se answer above. In addition we explained the uncertainty of sample BUS-1 at lines 227-231.**

Sample PR-V2 is a very poorly defined regression, and therefore not a robust date. **Please, see answer above.**

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. **We agree. We estimated the maximal effect by separating the two populations. As the resulting ages are significantly different, we decided to remove the age.**

Samples

I see no information on the locations of samples such as GPS. **Ok, added in the supplementary material.**

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**. From field data is not possible to highlight such re-activation. We discovered it only from U-Pb dating.**

Why not include the concordia plots as a figure? **Ok, we added in the main text. See the new Figs. 5 and 6.**

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. **We added the figures of the age discussed in the text. Now only the outliers are in the supplementary material.**

Tables – All OK, but sample information needed. **We added the coordinates of each sample, as previously suggested.**

Line 20 – The subsequent text is divided into thee rather than four categories. **Ok, corrected.**

Line 22 – 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. **We modified this part following also the comment of Reviewer #2.**

Line 21 – list in reverse chronological order – here and in all instances. **We prefer to list the ages from the oldest to youngest.**

Line 21 – 0.7 +/- 4.2 – definitely not worth including! **We deleted it.** 5.7 +/- 4.7 is pretty suspect too. **We stated that the U-Pb ages of strike-slip faults show larger errors than those of thrust faults. Therefore, the inferences on strike-slip fault activity must be taken with caution. Please, see answer above.**

Line 28-30 – 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. **Ok, corrected.**

Line 42 – 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. **We agree with the reviewer and we stated that "this issue is addressed by dating syn-tectonic sediments and, more recently, better constrained through dating of fault activity with K-Ar, $^{40}Ar/^{39}Ar$, and U-Pb methods."**

Line 43 – U-Th carbonate geochronology can also be applied to carbonate-hosted faults. **We added the U-Th method.**

Line 45 – 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. **We agree with the reviewer and we deleted the term "carbonate faults".**

Line 50 – I would remove "To fill this gap". **Ok, deleted.**

Line 51 – were other faults sampled and dated but unsuccessfully? Information on this is always appreciated and useful to others to comment on and ideally discuss. **No, all the sampled faults has been dated. However, we were only able to date 14 samples out of 19. We added this information at line 175.**

Line 55 – As above, I find this conclusion rather obvious and unnecessary. **Ok, deleted.**

Line 66 – I'm sure some older more seminal papers could be cited about the Jura. **Ok, added.**

Line 100 – Shear veins do not all have slickenfibers, and I would not classify all slickfibres as occurring in shear veins. So the terms should be used with clarity. **We corrected the term and we used only slickenfibers throughout the manuscript.**

Line 204 – Although these primary features represent tectonic slip, they can be overprinted by later fluid-rock interaction. This caveat should always be in mind. **Yes, see answer above.**

Line 206 – speed of crystal growth in this case is perhaps better stated in relevance to the vein opening rate. **Ok, corrected.**

Line 208 – see also many works by Janos Urai, Christoph Hilgers, Paul Bons. **Yes, added in the reference and text.**

Line 207 – I would add a sentence about the caveat above. **Ok, added. See also lines 221-226 and the answer above.**

Line 230 – 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. **We agree with the reviewer and we modified the section regarding the comparison between Pyrenean-Alpine orogeny following also the suggestion of Reviewer #1. In addition we added structural data collected in the field and discussed this data with previous literature on Jura tectonics. See answer above and lines 239-257, 248-251 and lines 261-266.**

See above comment on this imprecise age. **We highlighted that ages of strike-slip faulting are imprecise and should be taken with caution. See answer above.**

Remove all reference to this non-age. **We deleted this 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. **We agree with the reviewer that extensional veins could not be directly associated with fault slip, while slickenfibers yes. Therefore, we changed this part, warning the reader of a possible late event of fluid-flow/alteration not directly linked to fault slip during the Pliocene. See lines 273-275.**