Solid Earth Discussions

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# Comment on se-2021-2

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Referee comment on "U-Pb dating of middle Eocene-middle Pleistocene multiple tectonic pulses in the Alpine foreland" by Luca Smeraglia et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-2-RC1, 2021

General comments

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.

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 cautionous with this age, and even I would possibly discard it from the interpretation

If I well understand, all the ages are calculated assuming secular equilibrium in the U-series decay chain. As fluids are generally characterized by an 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.

-I would suggest to add 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.

-L64 : 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.

-L67 : Maybe I am wrong, but to my knowledge, only some of those tear faults are seismogenic.

-§4.1 and Fig.2+caption : please check labelling and description, it seems some undesired repetitions occur (caption of Fig.2).

-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.

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.

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 cautionous 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.

-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 to modify the section to include such evidence together with supportive references.

-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.

- 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 decollement ? This would broaden the perspectives of the manuscript I think.

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

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