

Solid Earth Discuss., referee comment RC2
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Comment on se-2021-96

Anonymous Referee #2

Referee comment on "Exhumation and erosion of the Northern Apennines, Italy: new insights from low-temperature thermochronometers" by Erica D. Erlanger et al., Solid Earth Discuss., <https://doi.org/10.5194/se-2021-96-RC2>, 2021

This manuscript presents a compilation of published low-temperature thermochronology data from the Ligurian and Adriatic side of the Northern Apennines that are analyzed to infer the evolution of erosion rates on the pro side and retro side of the wedge. The authors also present new detrital AFT data from modern-sand samples from the Ligurian side. However, most of their conclusions derive from published bedrock data. I think that the manuscript requires a major re-organization before further consideration and should be shortened considerably for the sake of clarity. My first impression is that what is presented here should be divided into two separate manuscripts: a first manuscript that analyzes the compiled bedrock ages and illustrates the main conclusions shown in this work, and a second manuscript that presents the new data on detrital samples from the Ligurian side. However, it is not clear to me what are the main implications revealed by this new detrital data set.

As far as this submission is concerned, improvements are required in the description of the tectonic setting and associated references, and in the description of the methodological approach. The authors should better describe the strategy they have adopted to check that their results are not compromised by cooling histories that are not monotonical. They should also better discuss the assumptions they have made concerning their modelling approach. For example, they have adopted the same initial geothermal gradient for samples located in the frontal and in the rear part of the orogen, which is difficult to understand given the different tectonic settings, contractional in the frontal part vs extensional in the rear part. Their model assumes that erosion initiated over the entire region at 10 Ma, despite compiled ages derive from samples belonging to tectonic units that were accreted at different stages of the orogeny. Also, the evolution of the Northern Apennines in the past 10 Ma includes a major strike-slip component that was not considered by the authors, and slab rollback rates were probably higher. I think all these points should be addressed before further consideration of the manuscript. Below are more specific comments that I hope will help improving this work.

Specific comments

Lines 44-45: "Development of the Apenninic wedge began at ~30 Ma, due to convergence and southwest-directed subduction of the Adriatic microplate beneath Eurasia."
This statement is incorrect if referred to the Apennines generally, as the development of the Apenninic wedge in the south started in the Eocene at the latest (see Lustrino et al. 2009 – Tectonics).

Lines 45-46: "From the late Oligocene, sediments supplied largely by the Alps were deposited as turbidite sequences into a series of northward-migrating foredeep basins (Macigno, Cervarola, and Marnoso-Arenacea Basins)".
Here the authors should be more precise: sediments were supplied largely by the Central Alps (see, e.g., Garzanti and Malusa 2008 – EPSL; Malusa et al 2015 – Geology)

Line 49: "Tertiary foredeep deposits". Change to Cenozoic, Tertiary is an obsolete term

Figure 1: There is a typo in the keys on the top-right. In the caption, it should be made clear that thermochronological ages are from the literature, and the original papers should be also quoted.

Figure 2: Vitrinite reflectance and cooling age data are from the literature. References should be explicitly indicated in the caption.

Lines 77-80: "The first evidence for emergent topography in the Northern Apennines is documented in the Early Pliocene, both by lacustrine deposits in an intermontane extensional basin located within the Magra River catchment (Fig. 3) (Bertoldi, 1988; Balestrieri et al., 2003), and by the exhumation of the Alpi Apuane metamorphic dome (white, hatched area in Fig. 3a) to the surface (Fellin et al., 2007)."
The second part of this sentence is conceptually wrong, because topography and exhumation are not directly related.

Line 82: "recorded by Pleistocene surface uplift of rocks at the drainage divide".
Surface uplift or rock uplift? This sentence should be amended.

Figure 3: Reference for published detrital AFT data should be explicitly indicated in the caption. Measurement units (Ma?) are missing in the diagrams. What is the meaning of n.?

Lines 110-112: "Bulk samples were sieved, and heavy minerals were separated using standard techniques, involving the use of the Wilfley table, heavy liquids, and the Frantz magnetic separator."
References are needed here, for example: Kohn, B., Chung, L., & Gleadow, A. (2019).

Fission-track analysis: field collection, sample preparation and data acquisition. In *Fission-track thermochronology and its application to geology* (pp. 25-48). Springer, Cham.

Lines 123-124: "We determined age populations for detrital samples based on dominant age peaks identified with the Binomfit program (Brandon, 2002), which is well suited for AFT data with low spontaneous track density."

Which strategy was employed not to miss zero-track grains? Please, provide details on this point

Lines 124-126: "In order to estimate the degree of resetting of the detrital age populations relative to the Apenninic orogenic event, we compared the detrital cooling ages with minimum depositional ages of the Tertiary foredeep units exposed in the drainage areas (Fig. 1)."

The range of stratigraphic ages in the drainage should be indicated also in the plots of Fig. 3b (e.g., by horizontal bars)

Lines 128-130: "We compiled ages from new and existing detrital AFT samples (23), bedrock AFT samples (139), AHe samples (135), and ZHe samples (26) (Tables 1–4) (Abbate et al., 1994; Balestrieri et al., 1996; Ventura et al., 2001; Zattin et al., 2002; Balestrieri et al., 2003; Fellin et al., 2007; Thomson et al., 2010; Malusà and Balestrieri, 2012; Carlini et al., 2013)."

Only at this stage do the authors present their methodological approach, despite the results based on such a compilation are already presented in a previous section of the manuscript.

Tables 1 to 4 should be placed in the Supplementary Material. Which kind of ages is indicated in those tables?

Lines 149-154: "We converted ages to erosion rates using a half-space cooling model and a closure temperature concept (Willett and Brandon, 2013). This model has the advantage of including an accurate representation of the transience associated with whole lithosphere geotherms. Reset ages were converted to erosion rates using the closure temperature concept (Dodson, 1979), with closure temperatures specific to each thermochronometer, although this is a simplification of diffusional daughter product loss that neglects effects associated with complex cooling histories. For monotonic cooling histories, the measured age of the sample is represented by the time needed for a rock to move from the closure depth to the surface (e.g. Reiners and Brandon, 2006)."

The authors should be more specific about this point and describe the strategy they have adopted to check that their results are not compromised by cooling histories that are potentially not monotonical.

Line 159-160: "In addition, the thermal initial and boundary conditions, as well as thermal parameters, must be specified for each sample site"

How did the authors evaluate the initial thermal conditions? Please, better explain to the

reader also underlining potential pitfalls.

Table 5: "initial geothermal gradient: 25°C/km"

The authors adopted the same initial geothermal gradient for samples located in the frontal and in the rear part of the orogen. This choice is particularly difficult to understand, given the remarkably different tectonic settings (contractional in the frontal part vs extensional in the rear part).

Lines 190-191: "The modelling procedure described above was applied to all ages, assuming that erosion initiated over the entire region at 10 Ma."

This is another assumption that is difficult to understand, because samples belong to tectonic units that were accreted during different and well-constrained time intervals.

Lines 191-197: "The resulting erosion rate applies from the onset of exhumation at 10 Ma to the present and reflects the time-averaged erosion rate constrained to pass through the closure temperature at the age and with a cooling rate commensurate with the average erosion rate. Thus, this method is limited to a single, average erosion rate. However, changes in exhumation rates through time in the Northern Apennines are supported by several lines of evidence, particularly by age-elevation transects (AETs). In fact, AETs from the existing literature illustrate differences along the age-elevation slope for a single thermochronometer (as in Balestrieri et al., 1999) or among age-elevation slopes for multiple thermochronometers (as in Thomson et al., 2010)."

As previously stated, how the authors can be confident that their results are not compromised, given that there is evidence that cooling histories are not monotonical?

Lines 230-231: "The kinematic model presented here approximates the Northern Apennines as a doubly tapering, asymmetric wedge, given the geometric parameters illustrated in Fig. 5."

Note that the evolution of the Northern Apennines in the past 10 Ma includes a major strike-slip component not considered by the authors. This point should be discussed in detail also evaluating the impact on the model results.

Line 248: "Slab rollback rates are on the order of 6–10 km/My in this region of the Apennines"

What is the impact if slab rollback rates are much higher (ca 20 km/Ma), as shown for example by Malusa, Faccenna et al 2015 ?

Tables 8 to 11 should be placed in the Supplementary Material.

Lines 414-415: "Fertility analysis of sediment from sampled Adriatic catchments also confirm that the detrital samples are representative of the eroded bedrock (Malusà et al., 2016)"

However, no fertility analysis was performed by the authors on their samples draining the Ligurian side, which implies that this argument cannot be used to support their conclusion.