Reply on RC2

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Author 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-AC2, 2021

Reviewer 2:

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.

One of the main concerns of Reviewer 2 is that our conclusions are derived from published bedrock data and that the implications of our new detrital data are unclear. Our work is based on a compilation of the existing cooling data, which include bedrock and detrital data, and we base our conclusions on both datasets. We believe the detrital data are complementary to the bedrock data. In the Northern Apennines, bedrock data are unevenly distributed, even in areas with no limitations related to rock types. Because of the uneven and potentially biased spatial data distribution, the cooling record at the scale of the orogen could be only partially sampled by bedrock data. The detrital data are thus useful to check against these potential biases. In fact, detrital data by a previous study on the Adriatic side of the Apennines revealed the presence of a much younger thermochronologic signal than the one recorded by the bedrock samples alone (Malusa' and Balestrieri, 2012). Although detrital data can also be biased as a result of spatial variability in erosion rate combined with variability in apatite fertility, they ensure sampling at the scale of a drainage basin. In this context, they can also be useful to spot a missing record if compared with bedrock data. Finally, our new detrital data indicate that exhumation in some areas of the Northern Apennines could have started earlier than suggested thus far, as they show a large population centered at 13 Ma. This is discussed in section 4.1. In the light of the reviewer's concerns, we partly modified the introduction to clarify our approach and to highlight the importance of the combination of detrital and bedrock data.

With regard to the reviewer's suggestion to split the manuscript into two manuscripts, we think that splitting the manuscript would be detrimental to our work. We collected our new data purposefully to complement the existing ones, to ensure the largest possible spatial sampling of the thermochronologic record in the Northern Apennines, and to propose a new orogenic model that builds on existing models. Instead of splitting the manuscript, we revised our introduction in order to explain better our goals and approaches, and to justify the need to integrate data and orogenic models.

Another concern of reviewer 2 is that our manuscript is too long, but it is unclear which parts should be shortened, since much of our text is devoted to discussing our assumptions, and much of the discussion is devoted to constraining the thermal boundary conditions for our erosion rate analysis. We nevertheless hope that the improvements we have made to the manuscript address the comments that reviewer 2 has provided.

Finally, reviewer 2 elaborates on most of the points and improvements to the manuscript in the specific comments below, where we address them in detail. To summarize, we have added references where requested in the figure captions, added text to the methodology clarifying our model assumptions, justify our treatment of the initial geothermal gradient, and address the concern regarding the strike-slip component and slab rollback rates.

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).

Since we refer to the entire Apenninic wedge here, we amend our statement to say that development of the wedge began during the Eocene and cite the Lustrino et al. 2009 study.

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)

We have clarified in Lines (54-56) that the Central Alps provided the sediment and use the suggested references.

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

We have changed all the references for "Tertiary" in the text to "Cenozoic".

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.

We found two typos in the spelling of "turbidite" and have fixed these in the legend. We have also added the sources for the cooling ages shown in Figure 1 to the caption and clarify that the ages shown in Figure 1 are only those that are <10 Ma.

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

We have added references for the vitrinite data and cooling ages in the figure 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.

We have changed the text to say:

"The first evidence for emergent topography in the Northern Apennines is documented in the early Pliocene by lacustrine deposits in an intermontane extensional basin located within the Magra River catchment (Fig. 3) (Bertoldi, 1988; Balestrieri et al., 2003). These deposits are overlain by late Pliocene alluvial conglomerates that contain metamorphic pebbles sourced from the Alpi Apuane metamorphic dome (white, hatched area in Fig.

3a), indicating that the Alpi Apuane was emergent at this time (Fellin et al., 2007)."

Line 82: "recorded by Pleistocene surface uplift of rocks at the drainage divide".

Surface uplift or rock uplift? This sentence should be amended.

We agree that this sentence is unclear. We have changed it to say: *"The onset of topographic relief then migrated eastward (Abbate et al., 1999; Thomson et al., 2010; Carlini et al., 2013), recorded by increased rock uplift rates at the drainage divide during the Pleistocene (Balestrieri et al., 2003)…"*

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.?

We have included references for the published detrital AFT data in the caption. Due to space limitations within the figure panels, we state in the caption that all ages are given in Ma and that "n." refers to the number of dated grains.

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 Fissiontrack thermochronology and its application to geology (pp. 25-48). Springer, Cham.

We respectfully disagree with the reviewer that a reference is needed, since these are standard techniques that have been established for more than 50 years.

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

For each sample, we counted all countable grains, including zero track grains. Countable grains specifically refer to any grains that expose a section parallel to the c axis, independently of whether it has zero or more spontaneous tracks. We added text to this effect on lines 134-135

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)

We have increased the size of the panels in Figure 3b for readability, and have added the range of stratigraphic ages for the rocks and sediments that each basin drains. The colors in the legend correspond to the colors shown on the geologic map in Figure 1.

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?

At the beginning of the Section 2.2: Erosion rate analysis section (now lines 150-155), we list the number of samples that we use in the study from each theromochronometer, for the purposes of the AGE2EDOT erosion rate analysis. We believe this is the most appropriate place for the information, since this is part of our methods. We also provide a short review of the main interpretations from published thermochronologic studies in Section 1.2 as part of our introduction to the field area. The title of this section "1.2 Thermochronology data compilation" is perhaps misleading, since it is not part of the Methods, so we have removed the title for section 1.2 and include these lines within section 1.1, under the new title "Geologic and thermo-tectonic evolution".

We have moved Tables 1-4 to a new Supplementary Materials document. We are not clear on reviewer 2's question "What kind of ages are indicated in those tables?" Each table is organized by the thermochronometer used to calculate the sample cooling age (e.g. bedrock AFT or detrital AFT). In each table, we provide the cooling age and errors, in addition to sample information and the original manuscript reference for the sample.

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 me 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.

The literature on the Northern Apennines provides evidence for cooling histories (age-elevation profiles in several locations) that are not monotonical (e.g., Thomson et al. 2010; Balestrieri et al., 1996). Besides age-elevation profiles, the other way to check for non-monotonic cooling ages is to do a paired-ages analysis with AGE2EDOT. For AFT samples, track lengths provide constraints on non-monotonic cooling paths, but these can be modeled on single samples and to derive cooling histories only. This precludes the inversion of a large dataset and also does not allow taking into account the effect of heat advection to derive exhumation rates along a cooling path.

In this manuscript, we adopted two methods for calculating erosion rates through time. In the first method, we calculate time-integrated erosion rates for individual samples, where we cannot check for non-monotonic cooling ages. In the second method, we adopt the paired ages approach detailed in our manuscript (Lines 207-231). Regardless of the method that we use, we find the same overall results, namely that erosion rates have decreased through time on the Ligurian side and increased through time on the Adriatic side.

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.

Constraining the thermal initial conditions is an important part of this manuscript and for the erosion rates analysis, and we discuss this topic at length. In Lines 183-196, we introduce our approach for constraining the geothermal gradient. We evaluated thermal conditions in two ways. In the first method, we assume initial conditions, namely an initial geothermal gradient of 25 C/km. This value should represent the geothermal gradient before the onset of erosion, so we chose a low geothermal gradient in the range of typical values for a foreland basin. In the second method, we impose only a final geothermal gradient that we derive from measured heat fluxes (della Vedova et al. 2001; Pauselli et al., 2019). This methodology is detailed in lines 180-194, and we dedicate an entire section (Section 4.2) to discussing possible uncertainties in these thermal conditions and how they may have affected the calculated erosion rates.

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).

The initial geothermal gradient of 25 C/km is only one of the methods that we use to constrain the geothermal gradient. In the AGE2EDOT inversion, the initial geothermal should represent the geothermal gradient before the onset of erosion, which in our case was fixed at 10 Ma. Thus, the initial geothermal gradient does not necessarily relate to the current position of the sample in the frontal or rear part of the wedge, but it should reflect the possible geothermal gradient within the wedge before and until the onset of erosion and heat advection. As we discuss in the text, there is little evidence of erosion, therefore heat advection, in the Northern Apennines before 10 Ma ago, and this justifies the assumption of a relatively low initial geothermal gradient. Because we are aware that this assumption is intrinsically simplistic, we also used the alternative approach of imposing a final geothermal gradient (described in Lines 187-196) that we derive from the observed

heat flux. We then compare how these different approaches affect our results, which we discussed at length in section 3.2.

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.

Given that there is no erosion while a sedimentary rock is buried in the foreland, heat advection only starts as the accretionary wedge emerges above sea level. Thus, the time of accretion of foreland sediments constrains only the maximum age for onset of erosion. As we introduce in section 1.1 and discuss in section 4.1, there is little evidence of erosion before 10 Ma in the Northern Apennines. Thus, for all the samples from Cenozoic foredeep units with depositional ages much older than 10 Ma, an onset time for erosion at 10 Ma is likely a reasonable assumption. For the Cenozoic foredeep units that were deposited in the middle-late Miocene, this assumption may not hold. However, among the dated rocks compiled here, the youngest ones are the foredeep sediments of the Marnoso Arenacea. The depositional age of this unit can extend until the late Miocene. However, except for possibly a couple of samples, most ages from the Marnoso Arenacea come from portions of this unit that likely have depositional ages significantly older than or close to 10 Ma. Given that accretion in the Northern Apennines occurs very shortly after deposition (e.g., Argnani and Ricci Lucchi, 2001), it is likely that accretion and erosion started close to 10 Ma, even for the youngest dated Marnoso Arenacea rocks. Assuming a later onset of erosion, for instance 8 Ma, would result in a slightly higher erosion rate. Finally, we do present the case where the initiation of erosion may have been earlier (Lines 453-456), but note that changing the initiation age would proportionally decrease all erosion rates in the Northern Apennines in response to a longer period of erosion.

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?

Reviewer 2 mentions this point in a previous comment, which we address above.

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.

Reviewer 2 mentions a major strike-slip component here and in the summary as an important point that we have not considered. Reviewer 2 has not provided any reference studies that discuss this strike-slip component, and it's also not clear to us which faults they are referring to in the Northern Apennines. Without more details on this point, we are not able to address whether such a fault or such movement would affect our 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 ?

As suggested by Reviewer 2, we reran our model with slab rollback rates of 20 km/Ma. Increasing the slab rollback rate to 20 km/Ma significantly alters the material paths, so that no particles accreted below ~2.5 km reach the surface, and particles accreted at depths of < ~1.5 km are not reset for any thermochronometer. Thus, only a very narrow range of rock particles are reset for AHe and AFT, and no rock particles are reset for ZHe. The reset AHe ages range from 3.7 Ma near the prowedge drainage divide to 4.5 Ma near the Tyrrhenian coast. AFT ages are only reset for the Ligurian side and range from 6.2 to 7.1 Ma. While these ages are reasonable, the high rate of slab rollback does not produce a realistic pattern of cooling ages across the orogen.
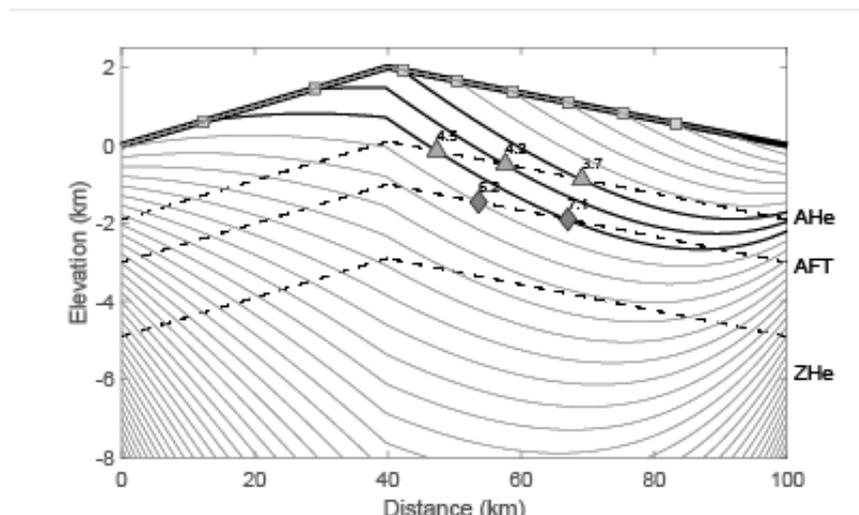
The Malusà et al. (2015) "Contrasting styles of (U)HP rock exhumation along the Cenozoic Adria-Europe plate boundary (Western Alps, Calabria, Corsica)" study mentioned by Reviewer 2 includes a transect through Corsica and the Northern Apennines, where slab rollback and hinge retreat rates are estimated (see Figure 10). We could find no quantitative estimates from the paper beyond Figure 10, where it is difficult to confirm whether the rate of rollback suggested by reviewer 2 is consistent with this study. Rosenbaum and Piana Agostinetti (2015) constrain slab rollback to be a maximum of 10.6

km/Ma in the Sillaro area at the southern extent of our field area, based on the age of sedimentary basins on the Ligurian side of the orogen, and rates derived from tomographic data are similar (Faccenna et al. 2014). While we agree that our estimates of slab rollback could extend to ~11 km/Ma, to account for the southern portion of the study area, 20 km/Ma is higher than the estimates from this study and from the independent methods for constraining the slab rollback rate given in the literature.

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

We have placed these tables in a new Supplementary Materials document.

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.

While we have admittedly not performed a fertility analysis in this study, the catchments we sampled on the Ligurian side drain the same lithologies as the Adriatic catchments included in the Malusà et al., (2016) study. This suggests that the detrital samples on the Ligurian side should also be representative of eroded bedrock. We added a sentence (Lines 438-441) to convey this point:

"Since the Ligurian catchments sampled in this study generally expose the same lithologies as the Adriatic catchments studied by Malusà et al. (2016), this suggests that detrital samples on the Ligurian side are also representative of eroded bedrock."

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