We would like to thank Ewald Hejl for his constructive comments to improve the paper for the journal "Geochronology". Please see below our responses to all suggestions and comments.

Referee 1 (Ewald Hejl)

General comments:
This manuscript is built on high-quality data; it is well organized and conclusive. The topic is relevant for low-temperature chronology of basement rocks, sedimentary rocks, for geomorphology, and because of the very low closure temperature for fission tracks in monazite (Tc < 45°C) it can be also useful for paleoclimate research. Therefore, I strongly recommend this manuscript for publication. About 50% of text and figures can be published in the present form. Some paragraphs and sentences need minor revision. Argumentation is well-founded. I did not discover major shortcomings – even after very attentive reading.

My overall impression is very good.

Specific comments:
My main concern is that the wording "parallel to the c-axis" is a little bit misleading and not precise enough. In contrast to apatite and other minerals with one main symmetry axis (hexagonal, trigonal or tetragonal), monazite crystals are monoclinic and their c-axis is not a symmetry axis. Therefore, any surfaces parallel to this c-axis are not necessarily symmetrically equivalent. In fact, monazite's only symmetry axis is its b-axis. This axis produces a congruent position after a rotation of 180_. It is a binary symmetry axis. The faces with Miller indices (100) and (-100) are symmetrically equivalent. They cannot form a prism but only a pinacoid. The wording in line 13 ". . . parallel and perpendicular to (100) prismatic faces" must be replaced by ". . . parallel and perpendicular to (100) pinacoidal faces".

Just for better understanding: Monazite crystallizes in the crystal class monoclinic prismatic (2/m). All faces which are parallel to two axes form pinacoids and occur only two times on the crystal: (100) and (-100); (010) and (0-10); (001) and (00-1). Faces cutting the b-axis and one or two other axes form prisms. Each of them occurs four times on the crystal. (110) and its three symmetrically corresponding faces form a prism. This is also the case for (311), (011), and (111) and their symmetrically corresponding faces. All these prismatic forms comprise four symmetrically corresponding faces.

Track annealing was investigated on surfaces perpendicular to the crystallographic c-axis and on surfaces parallel to both b-axis and c-axis, i.e. on (100) pinacoidal faces. I suggest to replace the wording "parallel to c-axis" or "// c-axis" by either "parallel to band c-axes" or simply by "(100)" all over the manuscript. In the third column of Table 3 you could simply write (100) instead of // c-axis. Please note that "perpendicular to c-axis" is precise and correct. This should not be changed because a face with Miller indices (001) is not exactly perpendicular to the c-axis. The angle between a- and the c-axis is not 90_. Only // c-axis should be replaced by (100).

RESPONSE: We agree with this and will make the appropriate changes throughout the text.

Another aspect is the azimuth of implanted tracks. The sentence in lines 317 to 319 explains that the dip angle (30°) was constant but "there was limited control on the azimuth orientations" of implanted tracks. Eventually this should be stated earlier in the manuscript (2. Experimental methods) and not only in chapter 4 (Discussion). I understand quite well that handling of such small grains is very difficult and nerveracking and that a precise control on track azimuth orientation is almost impossible. We should bear in mind that some tracks on faces (100) can be almost perpendicular to c-axis and thus may have similar orientations than some of the tracks implanted on faces perpendicular to c-axis. This could be a reason for lower apparent anisotropy of annealing. I expect that anisotropy will become stronger with a better azimuth control.

RESPONSE: Yes, this is quite correct, handling of the small size of the grains can make controlling track azimuth very difficult. We will therefore add additional detail to the text explaining that we did our best to control track azimuth in 2. Experiments Methods.

I fully agree that the higher temperature limit of the MPAZ can be defined at $l/l0$ = 0,50 (lines 21 and 22; 540 and 541; but I wonder if this is understandable for readers which are not familiar with fission track dating.

The original PAZ concept was formulated by WAGNER (1972) under the name of "partial stability zone" (The geological interpretation of fission track ages, Amer. Nucl. Soc., 15, 117). At this time and for the next ca. 20 years it was generally assumed that radiation damage in latent fission tracks is erased from the track ends inwards (at least for apatite). A fresh apatite fission track has an etchable length of about 16 μm, and with progressive fading it becomes shorter but never fragmented. Thus, over the whole temperature range of the PAZ there should be a strict proportionality between etchable (confined) track length and areal track density on etched surfaces. The original PAZ concept has predicted that in the middle of the PAZ apatite fission tracks have a length of 8 μm (= 16/2) and that the observed areal density is exactly half of that produced by fresh tracks with a length of 16 μm. In the meantime we know that this assumption is wrong – at least for the confined track length.

There is strong evidence that apatite fission tracks have an original etchable length of about 16 μm and in course of annealing they shorten to a length of about 10 μm. Afterwards, they become fragmented and simply disappear. In fact, confined tracks with lengths < 8 μm are rarely observed because they mainly do not exist. Proportionality between the reductions of areal track density and the etchable track length does not describe the true behaviour of track annealing (HEJL, 1995, Chem. Geol. (Isotope Geoscience Section), 122, 259-269; and several other articles). This statement seems to be true also for monazite. I suggest some additional sentences for a better understanding of the (M)PAZ concept. Otherwise, readers could eventually not understand why 50 % length reduction correspond to almost complete erasure of tracks.

RESPONSE: Noted. We will include some additional sentences explaining how the closure temperature concept is applied to fission track annealing.

Technical corrections:

Line 244: “However, the same is not true for all of the control measurements” on unannealed samples.

RESPONSE: Will change this accordingly.

Line 297: Probably Figure 4 instead of Figure 2.

RESPONSE: Will change this accordingly.

Lines 550 and 551: “: : : the parallel model is slightly preferable”. This is in contradiction to line 575 (“: : : a fanning model is preferred”). Which model is better or preferred?

RESPONSE: Lines 550 and 551 state that based on the model statistics (coefficients of determination), the parallel model is slightly preferable, but the difference is negligible, as noted in Lines 574-575, leaving both models viable. By analogy with annealing studies in zircon and apatite that show fanning models best fit their respective datasets, we conclude that the fanning model is preferable for monazite. We will modify the text to make this conclusion more clear.

Figure 2: Can you eventually indicate the direction of the c-axis in this picture? Just leave it when the azimuth orientation is badly known.

RESPONSE: I will find the original image and see if I can identify the c-axis.