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Interactive comment on "Technical note: AI-Track-tive: automated fission track recognition using computer vision (Artificial Intelligence)" *by* Simon Nachtergaele and Johan De Grave

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General Comments:

This short Technical Note briefly describes a potentially important development for the future of automated fission track analysis. The approach continues on from similar work started by Ray Donelick and colleagues using AI methods (eg Kumar, 2015, Goldschmidt Abstracts 2015:1712), which should be specifically cited in that context.

The approach is presented with very limited detail, both about the experimental conditions used and the way the software was trained and implemented, so I suggest the

paper needs significant expansion of exactly how the approach was implemented. For example, no information is given about sample preparation methods employed, the microscope configuration used for image capture (objectives, illumination etc), nor about the camera used. Indeed, very little information is given about the images themselves, other than that they are JPEGs. It is implied in line 55 that they may be 804 x 804 pixels in resolution, which seems improbably small (0.6 MP) for a modern digital camera.

It appears from Figs 2 and 3, although not stated, that a single transmitted light image was captured at each site and that these have been subsequently cropped or masked to a Region of Interest (ROI), thereby truncating some features, which seems to have caused problems for the track identification. Using a ROI is important, but it does not seem to be sensible to ignore information that is outside that area which would help in the identification of tracks that originated within it.

In lines 61-63 it is stated that the deep neural networks were trained, but no indication is given as to just how this was done, which I don't think is appropriate. I think a Technical Note should contain more technical details, which is presumably what most readers will be interested in. I am surprised that sufficient training could be achieved with only 15 images for apatite given the likely range of variables at play, and indeed this seems to be borne out by the relatively poor success rate. Were only isolated, non-overlapping, tracks used in the training? Perhaps a series of overlapping tracks could also be used in some way during the training, which might significantly improve the identification of such features which seem to be the major limitation of the system. I am also concerned at the very small sample size of about of 20 images used the test the system (line 115) which seems a very small sample to fully test its capabilities and limitations.

Not unreasonably, the authors want to present their own work in the best light, but I do take exception to their rather disparaging description of the image analysis system developed by our group in Melbourne as "The most successful attempt…" (line 25) and "Despite promising initial tests …" (line 33). Given that this system is in routine use in many laboratories around the world and has performance and efficiency levels, not only detecting and counting fission tracks but also in a range of automated measurements on them, that eclipse the relatively meagre results obtained in this paper (see Gleadow et al. 2015, EPSL 424, 95, and Gleadow et al. 2019, Chapter 4 in Malusa and Fitzgerald, for example). Also, none of the five examples in lines 35-37 where FastTracks supposedly "does not work well" remain an issue with the current system, with the exception of some shallow dipping tracks, which make up only about 2-3% of the total. Paradoxically, more technical details are given in lines 25-31 about the Melbourne system, than are provided about their own.

Clearly the authors have never seen nor used the Melbourne system and, because it has moved on from preliminary studies to successful routine application for comprehensive fission track analysis of real samples, they appear to think that its development somehow ceased ten years ago. The results of the AI system are compared to some very early data presented in 2012 by Enklemann et al. which grossly misrepresents the performance of our system today. Also most of the figures quoted depend on other unstated factors, like how many grains/areas were imaged, so the comparison is virtually meaningless. For comparison, our system actually takes about 15-17 minutes to capture images over 35-40 apatite grains, and these are not just a single image on each but comprehensive sets of about 18 Z-stacked transmitted light images and 5 reflected light images to flatten any surface relief. Automatically counting the fission tracks in these images sets takes about 20 seconds in FastTracks for this number of grains and manual review takes probably 10-20 minutes in most samples.

My point is not to suggest that the AI approach is not worth exploring, but rather that a new approach being presented today should be compared with the capabilities of other systems as they are today. But in reality, I think that the data presented in Table 3 and the discussion of it does not represent a valid comparison and contributes nothing of value to the paper. In my opinion this Table and the discussion of it should be deleted. I think that space should rather be devoted to a giving much more technical detail about what has been done in this study and a fuller discussion of the results obtained and

their limitations.

I think the paper would be acceptable for publication in Geochronology, but only after significant revision along the lines discussed above – to the satisfaction of the editor.

Some specific comments on the text:

Line 16: 'thermochronological' rather the 'geochronological'. 'Low-temperature' makes no sense if applied to 'geochronological'. Line 18: There were many papers that contributed to the 'discovery of the potential of apatite fission track dating for reconstructing thermal histories so I suggest you make this (e.g. Gleadow et al. 1986, . . ., . . .) – perhaps include some other key papers. Line 20: I suggest 'As of 2020, more than. . .' Line 69: 'Uranium-doped glass-covering mica' is a very clumsy expression – this should be reworded. Line 74: '. . .located with more than half. . .' Line 91: An interesting point here is that the coincidence mapping algorithm is extremely successful at resolving multiple track overlaps. That is one of its greatest strengths, and FastTracks would have no trouble in correctly identifying all of these overlaps in Figure 2. Line 118: I think that this should read 'essential' rather than 'highly recommended'. Line 136: this should say 'FastTracks' rather than 'TrackWorks', which does not contain the fission track recognition algorithms.

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