On Track

The Newsletter of the International Fission-track community

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One Copy for each lab. Please Share!

On Track is a biannual newsletter of the international fission-track community. It is published in the months of May and November. The views expressed in *On Track* are those of the authors and do not necessarily reflect those of the fission-track community or the editor of *On Track*.
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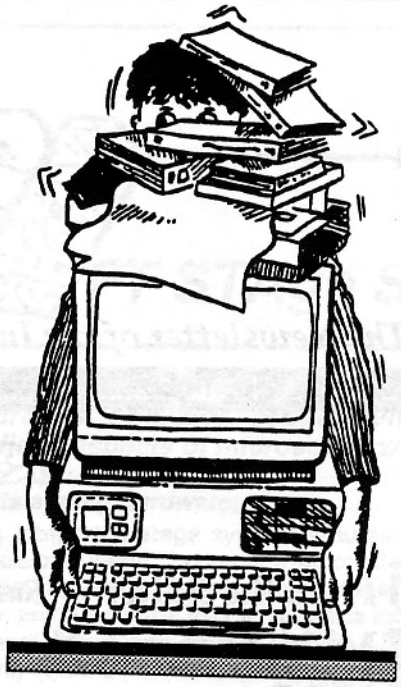


From the Editor

Welcome to the fifth issue of *On Track*. The rapid growth of *On Track* is an evidence in and of itself of the necessity of this publication. I think the fission-track community appreciates Dave Coyle who launched *On Track* in the December of 1990, and edited the first two issues. I would also like to thank Dumitru Trevor, the editor for the 3rd and 4th issues, who has kindly shared his experience with me in publishing this newsletter.

During the preparation of this issue, I have tried to think of ways to improve *On Track*, as the newsletter of the fission-track community. Ours is a scientific field and needs a serious and quality newsletter. (You have probably seen, as I have, many newsletters far more professional and in much better formats than *On Track*.) The question is: What is our expectation of *On Track*? Surely, this is a newsletter, not a refereed journal where research papers are published. But what does a newsletter actually mean? A few pages of fragmentary "gossip" ? Or a serious and quality publication (encompassing also gossip as informative news) to foster information among the fission-trackers and connect the fission-track

community by crossing the borders of countries and laboratories. If taken seriously by both the editor(s) and the community, I think *On Track* can contribute to the further development of the fission-track field. *On Track* can introduce new products and publications useful for fission-track research; it can be a networking tool among our community; it can provide a healthy ground for discussing important issues facing the fission-track field; researchers can present a summary of their field and lab work and experiences (both successes and failures) and give future directions for a specific problem, and so on.

Given the logic behind publishing *On Track* and its current growth, a day may come (not far in the future) when *On Track* will have to be published quarterly, be financed through subscription and donation, and be prepared by an editorial team (each person taking charge of one thing: soliciting articles and proof-reading, design and layout, advertisement, subscription, mailing, listing of papers and other publications, etc.) Perhaps the best way will be a group editorship by the members of a lab - not an individual or several persons at distant labs.

1993 marks the 30th anniversary of fission-track field (taking Price and Walker, 1963, in JGR, as the first paper). In the next issue of *On Track* (May 1993), I would like to devote a special section titled "Early Years of Fission-Track Dating." Fission-track peers and pioneers are requested to kindly contribute to this section telling us their stories and experiences in those early years. There are surely many untold stories behind published research papers of 1960s and 1970s. Please share them with the whole community and also for record in the history of the fission-track field.

Moreover, based on the mailing list which I have, I will try to prepare a directory of who's who in the fission-track field as an appendix to the next issue. For this purpose, a form is enclosed with the present issue. Please fill it in, and send to me. If you know some fission-trackers who may not have received this form, please send them copies of the form. Your cooperation is appreciated. It is hoped that by joint effort we will prepare a comprehensive fission-track directory in the coming year, and it will be updated once in a while (perhaps every four years on the occasion of the fission-track workshop).

As the editorship of On Track is rotated every two issues, the next issue (May 93) will be fortunately or unfortunately my last. Therefore, those interested in taking over the editorship for 1993-94 are invited to write me by the end of March 93. This is a good opportunity to become FAMOUS among the fission-track community through the SERVICE you provide for the fission-track field. But this is a voluntary position, and the editor is responsible, among other things, for generating funds to cover the publishing costs.

I would like to sincerely thank all contributors to this issue of *On Track* without whose contributions I could not be an editor! And finally a request: please help your newsletter remain a success and contribute in any manner you can: writing articles, reporting news and gossip, announcements, introducing new products and publications, or simply writing letters and stories of broad interest. Financial contribution is also appreciated and will be acknowledged.

Rasoul B. Sorkhabi

The 7th International Workshop on Fission-Track Thermochronology was held in the beautiful city of Philadelphia from July 13-17, 1992. Fission-track researchers from all over the world met in a friednly atmosphre, presented the results of their studies, and exchanged ideas. It was also fun with a happy end of a dinner prty on a boat. Our gratitude is due to to Dr. Goamma Omar and Dr. R. Giegengack of University of Pennsylvania for efficiently organizing the workshop. Rumor has it the next internationla fission-track workshop will be held somewhere in Europe.

Middle Age Spread

by Tracker

(Your man at the microscope,
with the geological time and inclination)

When Noah's Ark finally reached land, back in the mid-Holocene, there was a lengthy disembarkation procedure. The animals lined up, two by two, for Noah to complete their customs, immigration and quarantine documentation, record personal statistics and tell them to "Go forth and multiply". Presently there came two lowly creatures, appearing from under the coiled rope (they have one on every ship's deck, for practising the art of rope coiling), who said "We can't multiply, we are only adders."

God's instructions on this delicate matter, as on many others, had been somewhat lacking in detail and there was little Noah could do other than to keep them waiting under his table until he had finished with the other animals. Naturally this took some time, though the formalities were fairly straightforward in most cases. There was a problem with the giant pandas, who didn't seem to know which was which (their names didn't help) and whose future seemed uncertain. Eventually Noah sent them away, trusting to faith, and, wondering how far faith would take him, looked under his table for the two reptiles. They were both still there, along with numerous little ones, all looking very pleased with themselves. "I thought you said you were only adders", said Noah. "Ah yes," they replied, "but this is a log table."

Napier's invention

That awful story was told long ago by our school teacher in a desperate attempt to interest us in logarithms. I swear I have not repeated it until now, and as the average reader of *On Track* has probably never used a log table - at least for multiplication - there is little chance that it will go further. We had to use the common logs - the ones readily available though we knew that John Napier, in 1614, had suggested replacing 1, 2, 3, ... by 2.71828, 7.38906, 20.08554, ..., which he said was more natural.

Notwithstanding this obvious insight, Henry Briggs ten years later published the first great table: logarithms to base 10, to ten decimal places, of all the numbers from 1 to 100,000. Exactly three hundred years after that A.J. Thompson calculated them to twenty decimal places with the aid of a computer, who, as usual, was female and nameless. This was essentially an act of chauvinism: the French had threatened to publish fourteen figures and the Germans fifteen. They are now published in two volumes by the Department of Statistical Science, UCL, under the title Logarithmetica Britannica, price £20 o.n.o. Their weight is ideal for flattening posters, door stops, etc. and together with the three-and-a-half-foot-high statue of Napier behind the common room door, they are an inspiring reminder of the single most useful advance in Mathematics since before Noah was a sailor.

Manuel's Law

They also inspired other tables - an industry producing tables for the distribution of the correlation coefficient, tables for testing significance in a 2x2 table, symmetric functions (and allied tables), beta functions and gamma functions (which are incomplete). But it was Manuel I of Byzantium (1143--1180) who inspired a most wonderful result. Nine coins from his first coinage had the following silver contents, in parts per million:

5.9, 6.8, 6.4, 7.0, 6.6, 7.7, 7.2, 6.9 and 6.2.

You can see the coins in the Fitzwilliam Museum, Cambridge, UK, provided you promise not to subject them to fission track analysis. The mean and standard deviation of these measurements are 6.74 and 0.543, so the *relative* standard deviation is 0.543/6.74 = 0.0806. On the other hand, if we find the *logarithm* (the natural log) of each silver content and then calculate the ordinary standard deviation of these, it comes to 0.0806. The same.

Amazing though this is, it is apparently no accident, for the silver contents of seven coins from Manuel's fourth coinage,

5.3, 5.6, 5.1, 6.2, 5.5, 5.8, 5.8,

have a relative standard deviation of 0.0646 and their logarithms have a standard deviation of 0.0644. Practically the same. Moreover the mint precision has improved to below 6.5%, an achievement so commendable that one scarcely notices that the mean silver content has dropped to 5.61. The Byzantine government, in addition to originating and establishing the use of a standard monetary unit, were apparently well versed in other modern fiscal practices, including printing more money when in trouble and surreptitious devaluation - here by 17%, presumably without a formal announcement from the Bank of Constantinople.

But the really interesting thing about Manuel's Law is that it has nothing whatever to do with Byzantine coins. It simply states that the relative standard deviation of any set of measurements is approximately equal to the standard deviation of their logarithms. (Natural logs of course.)

To prove it, just take any set of numbers (any ten numbers between 80 and 120, say) and calculate their relative standard deviation and the standard deviation of their logs. Hey presto! Practically the same result. Not *exactly* the same, but close provided the numbers are positive (of course) and that their relative standard deviation is "small". Actually the word "small" here is highly misleading. The agreement is excellent even for a relative standard deviation as high as 0.50 (i.e. 50%). In practice one's relative standard deviations are usually well below 50%, so the approximation actually works where we want it to work. (Adherents of Murphy's law may notice an apparent contradiction, but the general form of Murphy's law implies that Murphy's law itself can go wrong.)

It won't have escaped you that the great value of Manuel's law is not so much in the calculation (few would regard calculating the s.d. of the logs as the easier option) but in the *concept*. That is the real power of mathematics.

Central ages

What is a central age and how is it calculated? Imagine a population of a large number of crystal grains. Imagine that each grain has a unique true value of ρ_s/ρ_i, the ratio of spontaneous to induced track density. For example, imagine that ρ_s could be measured exactly, without error, for each and every grain, that they could then all be irradiated together and ρ_i measured exactly in an external detector for each grain - and hence each ratio ρ_s/ρ_i could be determined exactly. We don't have to know what all these values are, but just that they exist in principle.

Now let μ and σ be the mean and standard deviation of their *logarithms*. (Why? Because that's natural.) Then the central age is the age corresponding to μ. That is, the age obtained by putting ρ_s/ρ_i equal to $\exp(\mu)$ in the fission track age equation. We call it the central age because it is in the centre of a distribution of ages. (Pedants might prefer to say that its logarithm is in the centre of a distribution of log-ages, but *anti-log central log-age* would never catch on, and in any case one should really say *age corresponding to the anti-log central log spontaneous-to-induced track density ratio*.) Moreover, by Manuel's law, σ, being the standard deviation of $\log(\rho_s/\rho_i)$, is the *relative* standard deviation of ρ_s/ρ_i. And because ρ_s/ρ_i is effectively proportional to the age, σ is the relative standard deviation of the population ages. We call σ the age dispersion. The central age and age dispersion *together* summarize the distribution of population ages.

In practice we just have a sample of grains from the population. If we could

determine ρ_s/ρ_i exactly for each sample grain, we could then *estimate* μ and σ (for example from the sample mean and s.d. of their logarithms). And if we had a large number of grains in the sample, these estimates would be rather precise. But we cannot determine ρ_s/ρ_i exactly, even for a sample grain - we can only *estimate* it from track counts. Nevertheless we can still estimate μ and σ, though to do so is more complicated because (a) there is an extra component of variation from the track counts (Poisson variation) and (b) this component differs between grains.

An efficient method (i.e. one that uses the data well) is to assume that the population values of $\log(\rho_s/\rho_i)$ follow a Normal distribution, and to use the method of maximum likelihood. It is not possible to write down the estimates explicitly. A simple algorithm is available on request and is included in a written (and submitted) version of Galbraith and Laslett (1992). The estimate of σ comes from the "extra-Poisson" variation - i.e. over and above the Poisson variation. To understand the estimate of m note that if σ is large and dominates the Poisson variation then each grain is weighted equally, while if σ is small the Poisson variation dominates and μ is estimated from the pooled track counts. In general the estimate of μ lies between these two values. Also, the precisions of the estimates of μ and σ will depend not only on the number of sample grains but also on how well each ρ_s/ρ_i is estimated - i.e. on the track counts.

Rufford well

When might the above model (for it is a model) be relevant? Suppose a number of contemporaneous apatite grains experienced a common thermal history severe enough to shorten tracks. And suppose that each grain had a different chlorine content, so that tracks would shorten by different amounts in different grains. Then the true values of ρ_s/ρ_i would vary between grains. Consequently the true "ages" (as given by the age equation) would vary between grains. The age dispersion tells us how much they vary and the central age tells us the central age.

For example if the age dispersion was 0.20 (20%) and the central age was 100 Ma, then about 95% of the population ages would be in the range

$$\exp(\log 100 \pm 2\times 0.20) = 100e^{\pm 0.40} = 67 \text{ to } 149 \text{ Ma.}$$

This is just one scenario and there may be others for which the model is applicable. Of course we don't know much about how chlorine contents vary - their distribution may be very irregular - or about the precise effect of amount of chlorine on track length reduction at various temperatures. So to assume that $\log(\rho_s/\rho_i)$ is Normal is something of an act of faith. Nevertheless the parameters μ and σ can provide a useful description of a sample that can serve as a summary and as a basis for comparison with other samples.

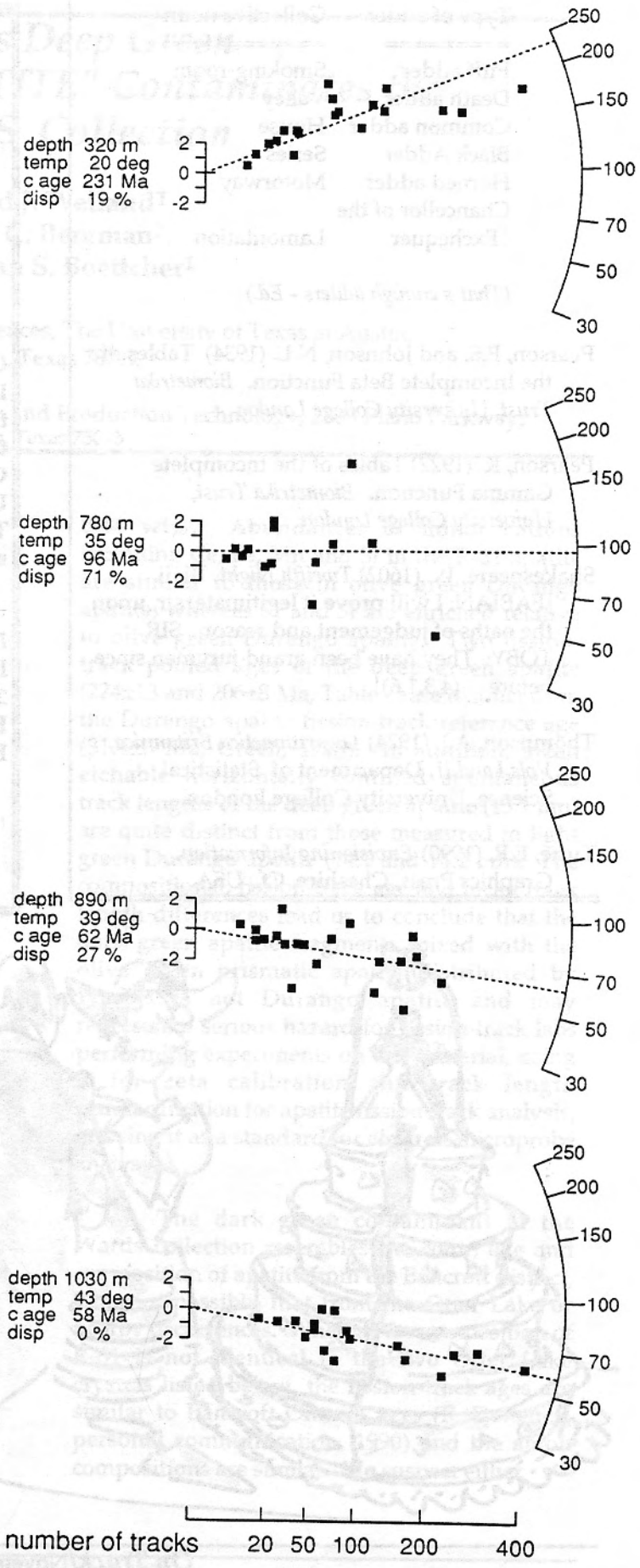
The graph shows radial plots of fission track ages for four samples from different depths in a well. The top sample, at depth 320 m and current temperature 20°C, has an estimated central age of 231 Ma and an age dispersion of 19%. There is evidence of a spread, suggesting that tracks in some grains have been shortened more than in others. In the next sample, at depth 780 m and temperature 35°C, some grains have ages comparable with the top sample while others are much younger. Their distribution is described by a lower central age of 96 Ma and a higher dispersion of 71%. In the third sample, at depth 890 m and temperature 39°C, many more of the ages are younger; the central age is now down to 62 Ma and the dispersion has gone down to 27%. For the bottom sample, at depth 1030 m and temperature 43°C, the single grain ages are consistent with each other and with the central age of 58 Ma. The age dispersion is now down to zero. (When the dispersion is zero the central age estimate coincides with the usual pooled age.) The inference here is that there has been sufficient heat to completely anneal all tracks formed before 58 Ma, since there is no evidence that tracks have been shortened by differing amounts, and that little or no shortening has taken place since that time.

Of course such data should be viewed in conjunction with length measurements. For the record, the numbers of confined tracks

measured, their means and standard deviations (in microns) were, from top to bottom, (94, 11.63, 2.03), (19, 11.02, 2.93), (94, 12.62,2.03), and (81, 12.17, 1.81).

This pattern - of the dispersion increasing and decreasing again as the central age decreases - is quite common and has, I am sorry to say, been referred to as middle age spread. It generally occurs, to differing degrees, in apatite samples in a sequence of increasing temperature - down a borehole, or over an area - and may be a useful indicator of such a sequence.

To see the pattern on the graph, an important feature is that the samples are plotted with respect to data frames having the same age scale (centred at the same place), and the same xy scales. Tufte calls this type of design "small multiples" and says "Constancy of design puts emphasis on changes in data, not changes in data frames." Such designs are useful for comparing data between samples, as opposed to within a sample, and are often a better alternative to superimposing several samples on the same data frame. So we return to multiplication.



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Type of adder	Collective noun
=========	==========
Puff adder	Smoking-room
Death adder	Valley
Common adder	House
Black Adder	Series
Horned adder	Motorway
Chancellor of the Exchequer	Lamontation

(That's enough adders - Ed.)

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Announcement

EIGHTH INTERNATIONAL CONFERENCE ON GEOCHRONOLOGY

ICOG8

BERKELEY 1994

The Eighth International Conference on Geochronology, Cosmochronology, and Isotope Geology will be held June 5-11, 1994 at the University of California, Berkeley, under the joint sponsorship of the Institute of Human Origins, the University of California, Stanford University, and the U.S. Geological Survey. The first circular will be mailed in a few months. To get on the mailing list, contact:

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A fissioning Cake!

Spurious Deep Green "DURANGO APATITE" Contaminates the WARD'S Collection

Richard J. Weiland[1], Steven C. Bergman[2], and Stefan S. Boettcher[1]

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Two types of apatite have been found in bags reported to be from the Cerro de Mercado (Iron Mountain) mining area, Durango, Mexico by Ward's Natural Science Establishment, Rochester, NY. These are found in collections at the ARCO Plano Research Lab, the University of Texas at Austin, and probably other as yet unidentified labs. Most of the apatite found in several bags purchased over the past few years from Ward's is the common euhedral, transparent, light olive green apatite (var. asparagus stone, similar to samples R-43, 58, 62, 68 in Table 1), from Durango, Mexico; however, a substantial proportion (10-40%) of deep green, transparent, anhedral to subhedral apatite fragments of larger crystals are also present (sample R-61, Tables 1 & 2). These two types differ in composition and apatite fission-track age and length systematics, indicating an unknown petrogenesis for Durango apatite, or more likely, the existence of a contaminant in the Ward's collection.

Compositional and fission-track data collected by the ARCO Plano Labs and the University of Texas at Austin are listed in Tables 1 and 2 respectively. Delayed neutron counting (DNC) of the deep green apatite (R-61) indicates U contents of 20-30 ppm (mean 25 ppm), approximately two to three times the concentration normally found in olive green Durango apatite (9-14 ppm U). Electron microprobe compositions of the dark green apatite indicate they are nearly end-member fluorapatites, with Cl<0.01 wt%, in contrast to typical Durango apatite with Cl contents of 0.36-0.44 wt%. Abundances of minor cations including Ce, Fe, Mn and Si in the R-61 apatite are similar to those in olive green Durango apatite, whereas Sr and Si are enriched relative to olive green Durango apatite. Two fission-track pooled ages of the deep green apatite (224±13 and 206±8 Ma, Table 2) are distinct from the Durango apatite fission-track reference age (31.4±3 Ma, Green, 1985). In addition, mean etchable horizontally confined spontaneous track lengths of the deep green apatite (13.1 μm) are quite distinct from those measured in light green Durango apatite (14.1 and 14.2 μm). The compositional, fission-track age and mean track length differences lead us to conclude that the dark green apatite fragments mixed with the olive green prismatic apatite distributed by Ward's is not Durango apatite and may represent a serious hazard for fission-track labs performing experiments on this material, using it for zeta calibration and track length standardization for apatite fission track analysis, or using it as a standard for electron microprobe analysis.

The dark green contaminant in the Wards collection resembles the color, age and composition of apatite from the Bancroft district, Ontario, possibly that from the Otter Lake or nearby occurrences. Whereas the composition of R-61 is not identical to the two Otter Lake crystals listed below, the fission-track ages are similar to Bancroft Ontario ages (R. Donelick, personal communication, 1990) and the anion compositions are similar. We suspect either

Table 1. Electron microprobe and DNC compositional data (ARCO Plano Labs)

sample no.	color	location	CaO (wt%)	P_2O_5 (wt%)	Cl (wt%)	F (wt%)	SrO (wt%)	Ce_2O_3 (wt%)	SiO_2 (wt%)	Na_2O (wt%)	MnO (wt%)	Fe_2O_3 (wt%)	U (ppm)
R-43	olive green	Durango, Mexico	55.5	41.0	0.44	3.45	0.05	0.43	0.24	0.19	0.01	0.01	9
R-58	olive green	Durango, Mexico	54.3	40.4	0.43	3.35	0.03	0.66	0.43	0.23	0.01	0.03	13
R-62	olive green	Durango, Mexico	55.1	40.7	0.36	3.42	0.06	0.67	0.42	0.23	0.01	0.04	12.3
R-68	olive green	Durango, Mexico	55.4	41.2	0.40	3.39	0.05	0.45	0.24	0.16	0	0.05	10
R-61	deep green	? (Bancroft, Ont.?)	54.0	40.2	<0.01	4.03	0.48	0.63	0.21	0.36	0.03	<0.01	25
R-63	deep green	Otter Lake, Ont.	54.9	38.3	0.06	4.01	0.21	0.85	1.04	0.08	0.02	0.01	134
R-215	deep green	Otter Lake, Ont.	54.7	40.7	<0.01	3.95	0.46	0.27	0.14	0.16	0.02	0.08	17

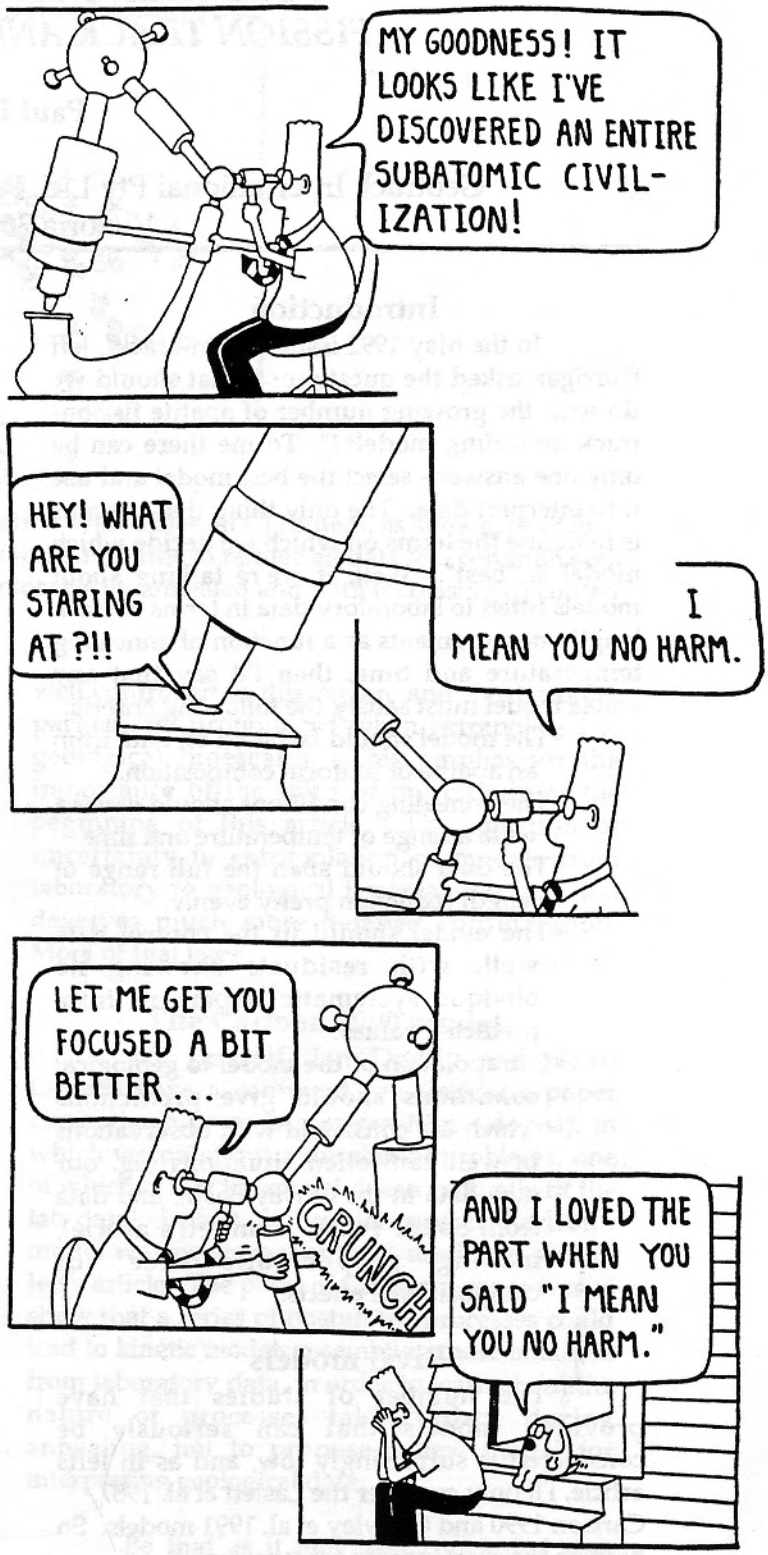
Notes: all analyses were performed on prismatic crystal plates using a JEOL733 superprobe (L. Liang, analyst); values represent means of 2-3 determinations using a 10 μm diameter rastered beam to minimize F & Cl migration, 15 kV potential and 15 nA beam current. U was analyzed by Delayed Neutron Counting. Sample R-61 is the contaminant in the Ward's "Durango" material, and is probably from the Bancroft arera, Canada. Compositions of four different light olive green Durango crystals (from various sources) and two different Otter Lake crystals (from R. Donelick & D. Doherty) are shown to illustrate the natural inter-crystal compositional heterogeneity.

Table 2. Fission-track age and length results (University of Texas at Austin)

analyst initials & zeta	sample no. & color	sample source & location	ρ_d ($\times 10^6$ tracks/cm^2) [N_d]	ρ_s ($\times 10^6$ tracks/cm^2) [N_s]	ρ_i ($\times 10^6$ tracks/cm^2) [N_i]	$P(\chi^2)$ (%) [r]	pooled F.T. Age ± 1σ (Ma) [N_g]	pooled U conc. (ppm)	mean track length ± 1σ (μm) [N_t]	length std. dev. (μm)
RJW 341 ± 6	DA/012 olive green	UT Barron Collection Durango, Mexico	1.667 [6179]	0.1135 [141]	1.156 [1436]	57 [0.230]	27.8 ± 2.8 [20]	11	14.2 ± 0.2 [70]	0.8
SSB 343 ± 4	DA/012 olive green	UT Barron Collection Durango, Mexico	1.659 [6025]	0.1146 [143]	1.036 [1293]	81 [0.075]	31.4 ± 2.8 [20]	10	14.1 ± 0.2 [70]	0.9
RJW 341 ± 6	R-61/012 deep green	Ward's via ARCO ? (Bancroft, Ont.?)	1.650 [6179]	2.133 [2623]	2.635 [3240]	81 [0.790]	224 ± 13 [20]	25	13.1 ± 0.4 [56]	1.4
SSB 343 ± 4	R-61/012 deep green	Ward's via ARCO ? (Bancroft, Ont.?)	1.558 [6025]	2.052 [1579]	2.622 [2018]	32 [0.609]	206 ± 8 [20]	26	13.1 ± 0.3 [57]	1.0

Notes: samples were irradiated at the Texas A&M Nuclear Science Center TRIGA reactor in the middle port of the D_2O box, Cd ratio = 118 for diluted Au (K. Crowley, personal communication, 1992), with ~1×10^{16} n/cm^2. Analyses were performed using the external detector method (Naeser and McKee, 1970) and the zeta calibration method (Hurford and Green, 1983) using the NBS SRM 612 dosimeter. Spontaneous tracks in samples [N_s] were counted on internal mineral surfaces etched in 1.5 M HNO_3 at 21 °C for 40 seconds (Crowley et al., 1989) at 1250x magnification under a dry objective. Induced fission tracks from samples and dosimeters [N_i and N_d] were counted on muscovite external detectors etched in 48% HF at 24 °C for 15 minutes (Crowley et al., 1989) with the same microscope conditions listed above. Pooled fission-track ages are reported, as the single grain ages pass the chi-squared test ($P(\chi^2) > 5\%$ and d.f. = N_g - 1) for a single population of ages (Galbraith, 1981); r is the correlation coefficient for N_s and N_i. Etchable horizontally confined fission-track lengths [N_t] are measured as suggested by Laslett et al. (1982) with the same microscope conditions listed above. Horizontally confined TINT densities in olive green Durango apatite were enhanced by vertical irradiation of a second grain mount at 9.5 mm distance for 30 minutes with a 50 μCi ^{252}Cf fission-fragment source (Donelick and Miller, 1991) at the ARCO Plano Labs.

Ward's obtained a mixed population from their ultimate source or somehow mistakenly mixed the two groups. We have informed John Fox, resident mineralogist at Ward's and he expressed shock and concern, assuring us that he would remove the dark green population from their existing stock. We advise all fission-track labs using Ward's Durango apatite to inspect their collection and separate the two populations of apatite according to shape and color, light olive green euhedral crystals vs. dark green chips.



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MODELLER'S CORNER

COMPARING KINETIC MODELS FOR FISSION TRACK ANNEALING IN APATITE

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Introduction

In the May 1992 issue of "On Track", Jeff Corrigan asked the question: "What should we do with the growing number of apatite fission-track annealing models?" To me there can be only one answer - select the best model and use it to interpret data. The only thing that remains is to define the terms on which we decide which model is "best". Well, if we're talking about models fitted to laboratory data in terms of track length measurements as a function of annealing temperature and time, then I'd say that any viable model must satisfy the following criteria:

- The model should be based on data from an apatite of uniform composition.
- The annealing conditions should cover a wide a range of temperature and time.
- The data should span the full range of length reduction pretty evenly.
- The model should fit the control data well, with residuals showing no obvious systematic departure from predicted values.
- Extrapolation of the model to geological conditions should give predictions which are consistent with observations in well controlled situations (eg. our own data in the Otway Basin, and data from South Texas, as in Jeff's article), making proper allowance for compositional effects.

Rival models

The number of studies that have provided models that can seriously be considered is surprisingly low, and as in Jeffs article, I'll only consider the Laslett et al. 1987, Carlson 1990 and Crowley et al. 1991 models. So how do the rival models measure up?

If you remember (or if you still have the last issue close at hand), Jeff concluded that of the three candidates, the Carlson '90 model using composite parameters gave the best approximation to the observed behaviour in the South Texas Wells (when renormalised to a lower L_o), and that the Laslett et al '87 model gave the worst agreement, with the Crowley et al '91 fluorapatite model in between.

Compositional effects

But let's look more closely at this, making full allowance of what we know about compositional controls on annealing rates. We know that the Cl-content of apatite exerts a strong control, with pure F-apatite most easily annealed and apatites richer in Cl annealing more slowly at a given temperature. In case anyone is still in any doubt about this, Figure 1 shows the results of some lab experiments we have done over the last four years. (One day we'll get round to publishing this stuff!)

The Laslett et al. 1987 model

The South Texas apatites have a spread in Cl contents up to 1.25 wt% Cl (thanks to Jeff Corrigan for that and other info), whereas the Laslett et al. (1987) model is based on Durango apatite with ~0.4 wt% Cl. Consider a sample from one of the South Texas wells which comes from a temperature at which the apatites with the same Cl content as Durango apatites are totally annealed. All those apatites with less Cl will also be totally annealed, and only those with more Cl will still contain tracks. The measured mean length will obviously be higher than the expected mean track length against length in Durango apatite (zero!) Generalising this, if we consider a plot of measured and predicted mean track length against temperature

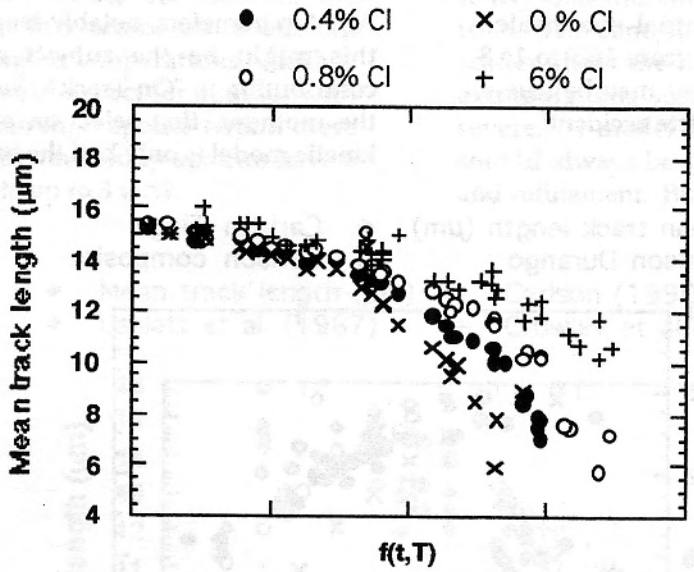

Figure 1: Mean track length in four apatites with different Cl content, as shown, plotted against a combined function of temperature and time to reduce all data to a common scale. At any temperature, fluorapatites are most easily annealed and with increasing Cl content the degree of annealing decreases.

in those wells, we would expect to find a model based on Durango composition to be close to the measured values at low-ish temperatures, and then the predictions should progressively underestimate the measured mean length, until the prediction reaches zero while measured mean length persists to higher temperatures. This is exactly what we see in Jeff's predictions from the Laslett et al '87 model (his Figures 2 & 3) for the South Texas data.

The Crowley et al. 1991 model

The predictions of the Crowley et al '91 model for fluor-apatite are actually higher than the predicted values for the South Texas wells at temperatures greater that ~80°C. Bearing in mind that this model should relate to the most easily annealed compositions within the samples, this can only mean one thing - that the model doesn't give a good representation of the true behaviour. It's much too retentive.

You might ask why this should be, when the model appears to fit the original control data well. I suspect its a result of the fact that the control data are dominated by mean lengths greater than ~13 µm, with only 3 measurements or less at shorter lengths for each annealing time. Therefore the model is not very well controlled in this region, and this becomes particularly pronounced when extrapolated to geological timescales. This emphasises the importance of the third of my criteria at the beginning of this article. The question of uncertainty in extrapolation of models from laboratory to geological timescales is one that deserves much more detailed consideration. More of that later.

The Carlson 1990 model

We (myself, Ian Duddy and Geoff Laslett) have a comment on Carlson's paper currently in press (American Mineralogist), in which we point out a number of problems, one of which is that the model doesn't actually fit the lab data! In fact, as I understand it, Carlson's model was never meant to be used as it was in Jeff's article. The point of Carlson's paper was to show that a series of postulated processes could lead to kinetic models resembling those obtained from laboratory data, in order to learn about the nature of processes taking place during annealing, not to propose a new model for interpreting geological data.

Be that as it may, Jeff concluded that Calson's model "can provide accurate predictions for apatite populations with Cl-

content distributions similar to those in the South Texas data set if the initial unannealed length of tracks ... is modified from 16.5 to 15.8 μm". While the basic statement may be true, I am convinced that it is a complete accident!

dealt with various aspects of the development of AFTA parameters, notably length bias. Perhaps this might be the subject of a subsequent contribution in "On Track". Suffice it to say, for the moment, that selection of an appropriate kinetic model is only half the battle.

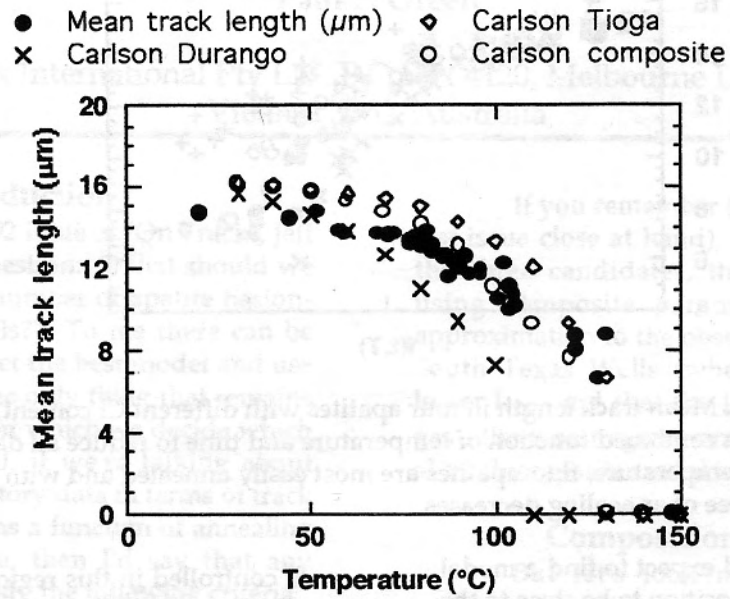

Figure 2: Mean track length in South Texas wells, from Jeff Corrigans article in the last issue, together with predicted values based from Carlsons (1990) models fitted to annealing data from Durango apatite and Tioga bentonite apatite, and a composite model based on both datasets. The composite model gives predictions that are close to the measured data, but the two models based on apatite of a single composition (which were combined to produce the composite models) give wildly different predictions.

I have many reasons for saying this, but the most basic is illustrated in Figure 2, which shows Jeff's South Texas data together with predictions from Carlson's "Composite Parameter" model plotted against temperature, but also showing the predictions of the models Carlson fitted to the two individual data sets which were combined into the composite model. I don't think any further comments are required, except to say that the apparent agreement of the composite model reminds me of Geoff Laslett's favourite statistical joke about having one foot in the fire and one foot in the fridge, but on average feeling quite comfortable!

The sharp-eyed amongst you will have noticed that my predictions for the composite model are not quite the same as Jeffs. This is probably due to differences in the way we have

Otway Basin

For anyone who was wondering, the Otway Basin data show more or less the same features as Jeff reported in the South Texas wells. Figure 3 shows a comparison of our Otway Basin data (from Green et al., 1989) together with predictions from the three models discussed by Jeff Corrigan. Values from Carlsons composite model are high at low temperatures but give a closer match to the data at higher temperatures. As with the South Texas data, Carlson's models for data from mono-compositional apatites give predictions for the Otway Basin samples which bracket the composite model as in Figure 2. The Crowley et al. model gives predictions which are close to the data at low temperatures but are too high at higher temperatures. The predictions from the

Laslett et al. (1987) model are close to the observed values at low temperature but fall below the data at higher temperatures, which is exactly what we would expect for apatites with the Cl content of Durango apatite within these samples, since the Otway Group apatites have a spread of Cl contents up to 3 wt%.

to say that this model is perfect. Of course it isn't. The control data show rather too much scatter about the fitted model, and in detail its extrapolations seem to be just a little bit too severe. A model is just that - a model, which should always be subjected to continual testing and refinement. But it's not at all a bad model,

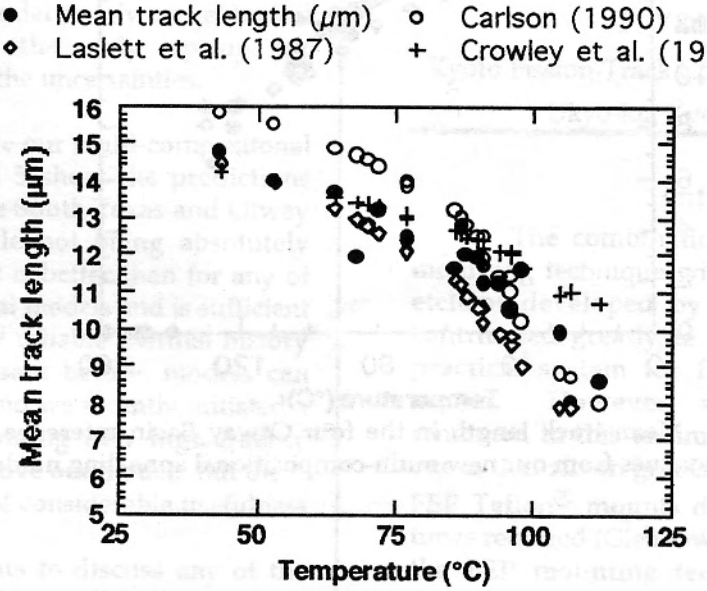

Figure 3: Mean track length in the four Otway Basin reference wells, together with predicted values from three models as shown (the same three models as discussed by Jeff Corrigan). Again, the Laslett et al. (1987) model seems to give the most accurate predictions, when allowance is made for compositional effects.

Which model?

The point I'm trying to make here is that although at first sight the agreement between Jeff Corrigan's data and predictions from Carlson's composite model might be taken as evidence that it's a good model, it cannot be used to interpret real data as it doesn't provide an accurate depiction of real annealing behaviour. Even if we use a mono-compositional model, we should always allow for the effects of compositional spread in interpreting data, and therefore we need a model which gives the most accurate predictions for the appropriate composition.

After applying the four criteria to the rival models, I believe that only the Laslett et al '87 model gets close to giving a good representation of the geological annealing behaviour of fission tracks in apatite. This isn't and most importantly, of those currently available, it seems to be closest to reality. It can certainly be improved upon, but until we or someone else comes up with a better one, I recommend it.

The future

Having said all that, you might be surprised to learn that we no longer use any of these models. Instead we use a new model which includes full allowance for the effects of Cl, and is based on both laboratory and geological control data. Because of the immense extrapolation inherent in application of any lab-based model to interpretation of geological data, such an approach will always be subject to large uncertainties. Take the Laslett et al. (1987) model, for instance. We had over 70 control data points covering times from 20 minutes to 500 days, and yet extrapolation to geological

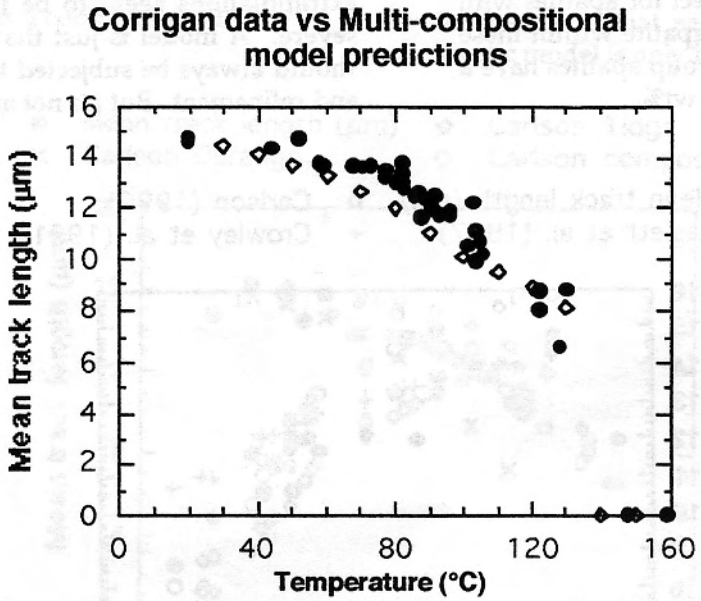

Figure 4: Mean track length in the four Otway Basin reference wells, together with predicted values from our new multi-compositional annealing model.

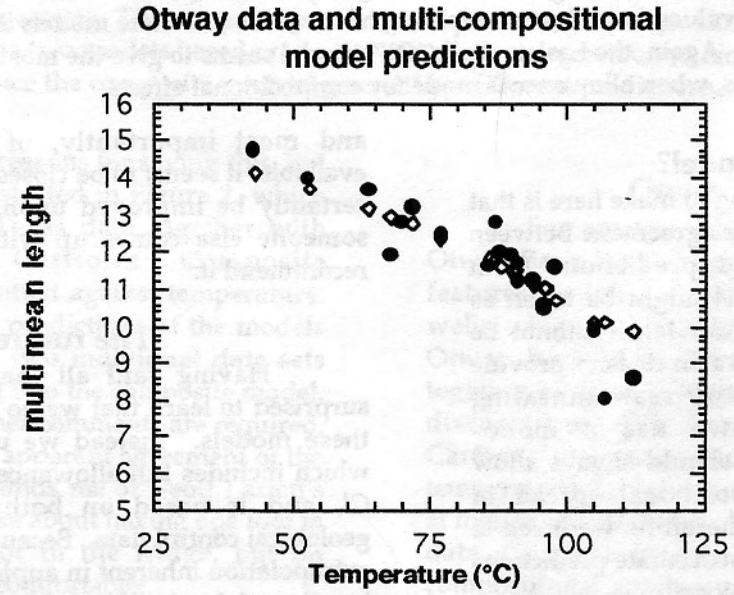

Figure 5: Mean track length in the four Otway Basin reference wells, together with predicted values from our new multi-compositional annealing model.

timescales still gave standard errors in predicted mean track length up to ~0.5 μm, equivalent to an uncertainty of ±10°C in paleotemperature estimation (Green et al. ,1989). This is a bit higher than we'd really like (although if you think about all the other uncertainties in thermal history analysis it's actually not at all bad!). Construction of a composite model based on both laboratory and geological data, or perhaps even constructing a model purely on geological data, seems to be the only option for significantly reducing the uncertainties.

To demonstrate our multi-compositonal model, Figures 4 and 5 show the predictions from the model for the South Texas and Otway Basin datasets. While not being absolutely perfect, the agreement is better than for any of the mono-compositional models and is sufficient to provide the basis of reliable thermal history interpretation. As I said before, models can always be improved, and we recently initiated a project aimed at obtaining new high quality geological data to improve our model. But the existing model is still of considerable usefulness as it stands.

If anyone wants to discuss any of the issues I've raised in this article, please get in touch. We're always interested in discussing an alternative point of view. Contact details:

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Phone: +61-3-344-7214;
Sorry, no Email!

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The PFA Sheet: An Improved Mounting Material For Zircon

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Introduction

The combination of the FEP Teflon® mounting technique with KOH:NaOH eutectic etchant developed by Gleadow *et al.* (1976) contributed greatly to the simplest and most practical system for fission track dating of zircon. However, some problems have remained in this technique. The most serious one is that zircon grains tend to fall out of their FEP Teflon® mounts during the long etching times required (Gleadow, 1980). In other words, the FEP mounting technique has a serious disadvantage that makes it inconvenient to apply to routine dating of young zircons; in general, zircon with less radiation damage requires longer etch time to reveal fission tracks optimally (Gleadow *et al*.,1976).

On the other hand, one of the authors, Danhara, had eagerly sought a new mounting material to replace FEP sheet for the purpose of dating young zircons. He eventually found PFA resin and used it successfully (Hayashida and Danhara,1985; Takemura and Danhara,1985). Fission track researchers in Japan then followed the use of PFA sheet (Tagami,1987), and the PFA mounting technique enabled researchers to study the etching characteristics of fission tracks of Plio-Pleistocene zircon with etching treatments at 230°C for up to 120h (Watanabe, 1988) and of thermally annealed zircon with no radiation damage remaining (Tagami *et al*.,1990).

At the 7th Fission Track Dating Workshop in Philadelphia this July, two of the authors, Iwano and Kasuya, were surprised to learn that many researchers, being unaware of the usefulness of PFA sheet, had trouble etching zircon mounted in FEP sheets. It appears that PFA is not so

popular among fission track researchers outside Japan. The aim of this article is to recommend the use of PFA sheet for fission track analyses of zircons.

The PFA Sheet

PFA is a fluorocarbon resin that consists of tetrafluoroethylene-perfluoroalkoxyethylene (Table 1). The PFA sheet we routinely use is the TOYOFLON® PFA, which is developed by TORAY INDUSTRIES INC., and is usable continuously in a wide temperature range from -253°C up to +260°C. The PFA sheet has the highest mechanical strength at high temperatures among all fluoride resins. The most important property, especially for prolonged etching, is that the maximum tolerable temperature for continuous use of the PFA is 60°C higher than that of the FEP (Table 1). We usually use KOH:NaOH etchant at 210°C-230°C for revealing fission tracks in zircon. This temperature range is higher than the maximum tolerable temperature for continuous use of the FEP, and lower than that of the PFA.

As an indicator of chemical resistance of PFA and FEP sheets against KOH:NaOH etchant (225°C), their bulk etch rates were measured and compared. The resulting bulk etch rates of the PFA and FEP sheets were 0.064 mm/h, 0.137 mm/h, respectively; the bulk etch rate of the FEP sheet was about 2.1 times that of the PFA sheet. In addition, PFA sheet had kept its transparency, which is important to observe fission tracks clearly, even after prolonged oropropylene etching; FEP, on the other hand, had lost its transparency at the same etching condition.

Mounting with PFA sheet is usually done on a hot plate at 330°C-340°C using a slide made of quartz glass (This is important!). If you use other kinds of glass slides, for example PYREX® glass, you will often have difficulty detaching the PFA sheet from the PYREX® glass slide.

EPILOG

A detailed paper regarding the PFA teflon will be submitted to *Nuclear Tracks and Radiation Measurements*. The PFA resin is manufactured by some companies. For example, the TOYOFLON® PFA is developed by the TORAY INDUSTRIES INC. We have heard that the DUPONT also manufactures PFA products.

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Table 1. Comparison between PFA and FEP resin

	PFA	*FEP*
Scientific Name	copolymer of tetrafluoroethylene-perfluoroalkoxyethylene	copolymer of ethylene-hexafluoropropylene
Chemical Structure	$-(CF_2-CF_2)_m-(CF_2-CF\cdot ORf)_n-$ (Rf: perfluoroalkyl)	$-(CF_2-CF_2)_m-(CF_2-CF\cdot CF_3)_n-$
Specific Gravity	2.12 ~ 2.17	2.12 ~ 2.17
Melting Point	302 ~ 310°C	255 ~ 280°C
Maximum Tolerable Temperature	260°C	200°C

The Roccastrada Rhyolite: A TestSample for FT Dating of Galsshards

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Istituto di Geocronologia e Geochimica Isotopica, CNR, Pisa, Italy

The Roccastrada rhyolite (Southern Tuscany, Italy) is composed of 60% very clear glass shards (size up to 0.2 mm, typically around 0.1 mm). Four samples from this rhyolite have been analysed by the Pisa fission track group, in order to check its suitability as a "test sample" for FT dating of glass shards.

The results have been presented and discussed during the 7th International Workshop on Fission-Track Thermochronology (13-17 July, 1992, Philadelphia, USA) (Bigazzi et al., 1992). The final paper, which reports the details of the analyses performed in Pisa, has been submitted for publication in the proceedings of the conference, a special issue of Nuclear Tracks and Radiation Measurements.

This glass has very good characteristics for FT dating. These are:

1) a relatively high spontaneous track density: $\rho_s \sim 20000$ tracks/cm^2. 500, or more,spontaneous tracks can be counted in a reasonably short time.

(2) fission tracks are safely identified: defects (such as microlites) that after etching could produce "spurious tracks" are in practice absent.

(3) the uranium content does not show inhomogeneities: induced track count distibutions are consistent with Poisson distributions.

(4) different temperatures (from 180°C up to 220°C, heating times up to 4 hours) were tested for plateau age determinations. A certain degree of alteration of the etched surface was observed only for temperatures $\geq$ 200°C. However, also after thermal treatments at 220°C track counting did not present special difficulties.

All of the four samples are affected by partial annealing of spontaneous tracks. Spontaneous to induced track mean size (the major axis of the etch pit) ratios between 0.82 and 0.86 have been measured. Apparent FT ages range from 1.74 Ma to 2.01 Ma, whereas size-corrected (Storzer and Wagner, 1969) and plateau ages (Storzer and Poupeau, 1973) are close to 2.5 Ma.

These peculiarities make the Roccastrada glass a good interlaboratory test sample. These are: (1) fresh samples in large quantities can be collected in a quarry that can be easily reached (from Pisa it takes about 2 hours), and (2) the size corrected and the plateau FT ages obtained in Pisa agree with Rb-Sr, K-Ar and Ar-Ar ages (Table 1).

Table 1 - The Roccastrada rhyolite. Comparison between FT size corrected (S.C.) and plateau (P) ages with Rb-Sr (whole rock-biotite), K-Ar and $^{39}Ar/^{40}Ar$ ages (from Bigazzi et al., 1992).

Sample	Rb-Sr (wr-bt)	K-Ar (glass)	$^{39}Ar/^{40}Ar$ (sanidine)	FT (glass) (S.C.)	(P)
R2V P26				2.44 ± 0.19	2.55 ± 0.07*
R2V P30				2.55 ± 0.24	2.50 ± 0.1
Roc 3	2.48 ± 0.04		2.446 ± 0.026	2.54 ± 0.24	
Roc 10		2.49 ± 0.03*	2.453 ± 0.027	2.48 ± 0.17*	
Roc 11	2.40 ± 0.04	2.65 ± 0.05*	2.435 ± 0.026	2.65 ± 0.23	2.53 ± 0.08*

*: weighted mean

The Roccastrada glass has been proposed at Philadelphia as a "test sample" for (1) track counting strategies and (2) apparent-age correction techniques applied to glass shards. It has not to be regarded as a putative "age standard", at least at present, for several reasons. Glass shards commonly present cmore difficulties than other phases for FT dating; thus such a sample is inadequate for routine work. In addition, as written above, its spontaneous tracks are affected by a moderate annealing rate. Several experimental results suggest that the corrected thermally lowered FT ages of glass sampìles should be, in the majority of cases, reliable formation ages. However, this hypothesis is not accepted by everybody in the FT community. Moreover, the results shown in Table 1 do not prove definitely that corrected FT ages of Roccastrada glass agree with other radiometric ages: due to the present status of fission track dating standardization, those FT ages refer to a given calibration system.

The Roccastrada glass will be distributed, upon request, to other laboratories. It should be useful for tests on etching conditions, counting methods, age correction techniques, track size measurements.

Several tests have been done in Pisa. The results suggest (1) to irradiate with fluences ≤ 10^{15} n / cm^2, (2) to use the population-subtraction method for spontaneous to induced track density ratio measurements and (3) to heat at temperatures ≤ 200°C for plateau-age determinations. Further recommendations will be mailed together with the sample.

At present only a small amount of the glass is available. If needed, the Roccastrada rhyolite will be sampled again. Those interested may contact me at the following address:

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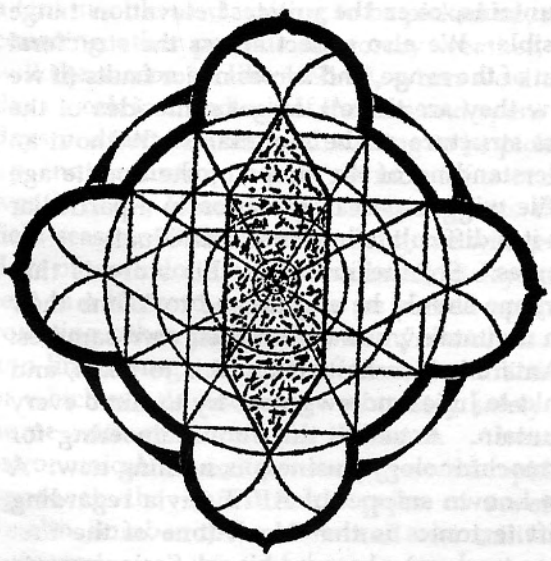

Fission-track Mandala by Edmund Stump

Ravings from the heart

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Editor's directive:
"Write whatever you like but within the undefined boundaries of fission track ing."

Our fine and esteemed editor, Rasoul has been soliciting articles for this edition of *OnTrack* for some time. Sharing an office with Rasoul means that he has not been amiss in getting me to think guilty thoughts about the fact I had yet to write something. I expect that Rasoul was expecting insightful scientific cogitation (never one of my strong points). However, we are off to Antarctica in less than a week for another season, and in these last few days of frantic preparation, clear, intelligent and coherent scientific thought is the last thing on one's mind. More important considerations take over: for example, have I packed my Walkman, sharpened my crampons, remembered to take the painkillers out of the fridge, and not burned my bridges. Still, a few things relevant to fission tracks have crossed my mind over the last few years, some serious, and some not so serious. This article was conceived at 4 am thanks to a dose of jet-lag and an autistic imagination, so please bear with the train of thought.

Many of us have, for years, with wild abandon, used the term apatite fission track analysis, which when shortened to AFTA rolled off the tongue with such panache that many people probably believed we knew what we were talking about. However now that AFTA is AFTA™ (the cheque is in the mail!) this is no longer possible without the following thought entering one's head "now if I were to become rich and famous (i.e., win the lottery and wed Elizabeth Taylor) I could maybe get sued if the oil business gets just a little bit worse! After many years of careful thought, I now like the term *apatite fission-track thermochronology*, or AFTT for short. Although this rolls off the tongue like the degassing of a sialic volcano or the upwind passing of a flatulent Kodiak bear I believe it solves the problem (with respect to

AFTA) of whether to abbreviate and become a pauper, or to be unimaginative and exceed the word count (with respect to *Geology*, *Nature* and *Science* articles !). I for one will sleep better at night knowing that AFTT is just three horizontal and one upwardly diagonal keyboard spaces away from achieving the international recognition it deserves.

Upon my arrival in the US in 1987 I was horrified to learn that human beings, if you can call graduate students human (maybe? After all, most of us have either been one, or is one...) were placed in offices without windows. I was mortified when I realized that I too would be placed into one of these cells. (*My consultant is now doing 3-5 years, with no chance of parole, in those very same confines.*) My recently acquired proto-ego as a brand-new post-doc plumented to the basement of despair. My years at Melbourne had been spent in a large (albeit smoky) sunny room and all microscope work we did was in front of a large window, and later in rooms where we could at least look outside. "You must relax the eyes when counting," we were told by our gurus Andy, Paul, and Ian, "so look out the window when the stage is going from the mount to the replica or you will get extremely tired!". It worked, too, as we toiled away, regularly counting 4-5 samples in a day. However, as I toiled away in the cell, seemingly as hard as I had in Melbourne, I would get only two samples a day counted and then crawl off home absolutely exhausted. The power structure here at ASU fortunately changed a couple of years later and we moved into a new building and now have a magnificent view out over the mountains east of Phoenix, toward the Superstition Mountains and the massif of Four Peaks. Clear and beautiful and topped with snow in the winter, this impels one to crank through lots of counts and length measurements so you can go skiing up at Flagstaff (in Arizona). Hot and burning in the summer, the shimmering air column reminds one to stay inside and not venture out into the 45°C temperatures. Either way, more work is done. Inspiration flows because of the scenery. Greater effectiveness is achieved because your eyes are rested and you do not get headaches. As a consequence, the pen flies, the keyboard rattles, and papers spew forth onto the pages of history. I seriously believe that not being able to relax one's eyes while counting makes you much more tired, less effective, less work is done and long term harm is probably done to the retina. So, if you are locked up in a cell with no rest for your eyes, then I suggest that changes should be made. Follow the French lead, dispense with the aristocracy, and remember the Bastille !!

A great deal of work and effort goes into the production of a fission track age and length distribution, not to mention a chemical analysis to determine the Cl-F ratio of at least a representative sample suite. A great deal of experience is required to be able to produce data, and therefore it is hoped that the data will be good. I have noticed, as I head into my twilight years, that the very best data comes from samples that one has collected personally. (We call this "litho-bonding"). The data produced from samples that other people collect is for some strange reason, never as good as one's own rocks. I believe that it is very important to collect your own samples to get a feel of what the rock is like, to see when the sample comes from in relation to the mountain, relevant geological structures, the car, or the nearest pub. Gone are the days when grabbing a few samples to generate a few fission track ages was good enough. We now try and collect samples in a systematic fashion towards solving certain problems (geological, geophysical, or maybe even personal...). In Antarctica we have collected vertical profiles from the front of the Transantarctic Mountains and the Ellsworth Mountains, over the greatest elevation range possible. We also collect across the structural grain of the range, and across major faults (if we know they are there*), to get some idea of the gross structure of the mountains. Without an understanding of the shape of the apatite age profile with respect to elevation in a particular area it is difficult to interpret individual samples. So, the moral of this story is that everyone should be encouraged to "climb their own mountain", and collect their own samples. In Antarctica we have taken this to heart, and thanks to Julie Andrews, now try to climb every mountain. Actually, this mountaineering for thermochronology business is nothing new. A little known snippet of AFTT trivia regarding uplift tectonics is that Noah (one of the first fission trackers), observed his ark fissioning into two portions (isoboats) beneath him as Mt. Ararat unexpectedly surfaced from below. (And we thought he had just run aground!!!).

Antarctica is a very serious place to do science and there are a lot of very serious people there. So much so that it is now the fashion to give one's project an acronym. Some examples: WAVE ("West Antarctic Volcano Expedition") - when funded again they became New WAVE; RISP ("Ross Ice Shelf Project); FORCE ("FOrd Ranges Crustal Experiment), or to really get convoluted, CASERTZ ("Corridor Airborne geophysics of the SouthEastern Ross sea Transect Zone - at least, that is what we think it means!). Now we are serious scientists too, and so after a great deal of debate, deliberation, discussion and dissatisfaction, a decision was made. Sophisticated Measurements of Uplift Tectonics was decreed too risqué. Others were deemed too costly (for example: "Application of Fission Tracks to Antarctica"). Instead, we became FILTH! (Fission track Investigations of Litho-Tectonic History). This upcoming season the debate rages. Should we be FILTH-2, or should we incorporate the Y in History and become FILTHY!? (Two months without a shower has nothing to do with it.). We would appreciate comments (and perhaps outside arbitration) when we return in late January, 1993 (atpgf@asuvm.inre.asu.edu and agtfr@asuvm.inre.asu.edu).

We close this piece of fine literature pondering how this season in Antarctica will proceed. Will it be like 1989-90 when I did 7 days fieldwork in 2 months and spent 14 days in a tent in a blizzard waiting to be picked up, not counting other periods of stormy weather, or will the season be like 1991-92, when we did 8 days work in 2 and a half months and spent 16 days in a tent in a blizzard waiting to be picked up, not counting other periods of stormy weather? A linear progression suggests this field-season will be 3 months long, we will get 9 days of work done, and we will wait 18 days in a tent in a blizzard waiting to be picked up, not counting other periods of stormy weather. A non-linear progression terrifies us, the infliction of which would encourage us to stay safely at home with our heads in the sand. Already astronomical, the cost per sample would rapidly approach the unjustifiable price of several Stealth Bomber toilet seats and one military spanner.

Lastly, as an AFTA™-thought: I encourage you to submit contributions to *OnTrack*, at the very least to avoid future contributions like this one.

Onwards, Fission Trackers

Onward, Drs. Green and Duddy
Marching off to count
May the tracks upon your stage
be just the right amount!
First and foremost apatite
whose tracks define the age
Go and generate the date
Become the ARCO sage!
Out upon the Arctic Slope
Deep into his quest
P.O.S. was heard to say
"I think I'm headed west!
Melbourne, sunny Melbourne,
where girls are young and sweet
No more dark and lonely nights
Perhaps a wife I'll meet!"
His beard was torn and tattered
His sledge was raised on high
In lands remote, John Murphy smote
Oh, how the chips did fly!
His samples dated in the lab
Arctic secrets yield
It's good he wore his Wellingtons
In such a fertile field!
In search of rifted margins
Omar headed east
But Moses was not with him
He could not find a piece!
Back in Philadelphia
He organized a meet
Fission trackers came from far
To gather at his feet
Far from Philadelphia
Through forests deep and black
Tracing down the Laramide
Giegengack's On Track!
But not with quartz exposure dates
Those nasty Isotopes!
He tried them in Antarctica
On tracks we place all hopes!
So gather up your separates
Make that polish fine
Let's all go irradiate
Then in the dark we'll shine!
Bless the Rock of Ages
Uranium is King!
To hell with argon spectra, FISSION
TRACKING IS THE THING!!!

By two FILTHY lads, gone south...

1992 AND FORTHCOMING FISSION TRACK PAPERS

Please send items for future listings to the editor. Send the reference or a photocopy of the first page of the paper. We especially want papers you may have seen by non-fission trackers that are of special interest to trackers. Papers in press are welcome; please include a guess of the month of publication. Requests for the reprints of papers listed here may be sent directly to the authors, not to the editor of *On Track*.

The editor would like to thank *Jeff Corrigan, Trevor Dumitru, Paul Fitzgerald, Paul Green, H. Iwanov,* and *Barry Kohn* for their help with this issue's list of papers.

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*Vol. 19, Nos. 1-4 (1991): Paricle Tracks in Solids, Proceedings of the 15th International Conference, Marburg, Germany, 3-7 Sept., 1990.

*Vol. 20, No. 2 (1992): Special issue on the 2nd All-Union Conference on Solid State Nuclear Track Detectors and Autoradiography, Odessa, [former] U.S.S.R., July 1989.

*The Proceedings of the 7th International Workshop on Fission-Track Thermochronology, Philadelphia, 1992 will be published in a special issue of *Nuclear Tracks and Radiation Measurments*, hopefully in 1993.

Recent Books

Fisison-Track Dating
By *Gunther Wagner & Peter Van Den Haute*
Kluwer Academic Publishers, Dordrecht, 1992
(See the ad on page 28)

Heavy Minerals in Colour
By *M.A. Mange and H.F.W. Maurer* Chapman& Hall, London, 1992, U$80.

Low Temperature Thermochronology: Techniques and Applications
Edited by *Zentilli, M. & P.H. Reynolds*,
Mineralogical Association of Canada, (MAC) Short Course Handbook, Vol. 20, May 1992
MAC, Cityview 78087, Nepean Ontario K2G-5W2, Canada.

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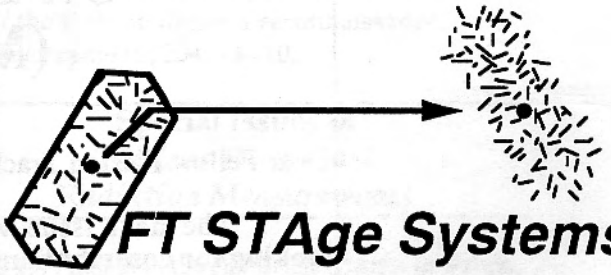

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