Dear fellow fission-tracker:

On Track is currently free because the founding editors felt this necessary to successfully launch the newsletter and because of the hassles of collecting money from a dozen or more countries. Cost have been met mostly from the editors' often meager research funds (many thanks to Steve Bergman & ARCO for the grant that was looted for this issue). Because of this, we generally only mail one copy per lab and ask that you share or copy it. **Please send back this coupon so we know where to send your lab's copy.** In addition, if you have easy access to U.S. currency (like an old $10 note lying around from your last trip), donations (totally optional) towards production and mailing cost are much appreciated. But please don't bother to donate if it means you have to trot down to the bank and waste half an hour getting a foreign currency check.

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Return to (cut out for mailing label if you want):
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On Track

THE NEWSLETTER OF THE INTERNATIONAL FISSION-TRACK COMMUNITY

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A NEW COMPUTER-AUTOMATED MICROSCOPE STAGE SYSTEM FOR FISSION-TRACK ANALYSIS

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In the the external detector method of fission track dating, mineral grains are mounted in epoxy or teflon, ground and polished to expose an internal surface, then etched to reveal their spontaneous tracks. An external detector (generally mica) is then sealed in contact with the mineral grains and the sample-mica sandwich is irradiated. Irradiation induces fission of ^{235}U in the grains, forming new tracks. Some of the new tracks in the grains extend into the adjacent mica and form tracks there. The mica is then detached from the grains and etch to reveal these new induced tracks. The number of tracks in the mica provides an assay of the U content of each individual grain in the sample, which is necessary for calculation of the fission track age.

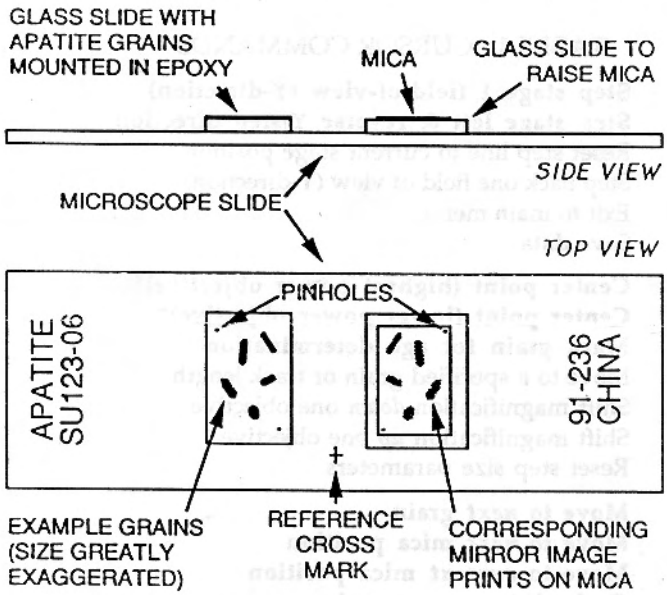


Fig. 1: Sketch of microscope slide prepared for fission track analysis by external detector method. The induced-track prints on the mica are a mirror image of the grains in the sample. The software handles the mirror image matching and also adjusts for any misalignment when the grain mount and mica are glued to the microscope slide. Pinholes are made through the mica-grain mount sandwich before detaching the mica; these serve as coarse alignment guides. Grains and their prints are then used to refine the alignment.

This procedure requires the repeated matching of individual sand-size grains with their respective induced track images ("prints") on the mica (Figure 1). The tediousness of the matching process led the Melbourne fission track group to initiate the development of a computer-automated microscope stage in the mid-1980's. This stage in current available as a commercial product from Autoscan Proprietary Limited. Most fission track labs also use computer-automated digitizing tablets and microscope drawing tubes for measuring track lengths.

When the new fission track lab at Stanford was being designed, I decided to develop a new integrated stage-digitizer system based on a Kinetek-brand computer-automated stage. This is a high precision X-Y stage that has an excellent reputation for accuracy, reliability, and rapid factory service, at a price substantially less than the Autoscan stage. Mark Cloos and Leslie White of the University of Texas originally evaluated this stage for fission track purposes and have been very happy with the one they purchased.

The new integrated system includes (Figure 2):

- a Kinetek stage
- a Wacom digitizing tablet
- a 2000-line computer program I wrote that handles the specifics of slide scanning, mica print locating, and track length measurement
- a Zeiss Axioskop microscope with drawing tube
- an Apple Macintosh computer

Kinetek Stage

The manufacturer's specifications say the Kinetek stage has a positional accuracy of ±10 µm, a repeatability of 3 µm, and a positional resolution of 0.1 µm. In actual use, I've found the stage will generally hit the center of the mica print within about ±5-10 µm, which is more than adequate for fission track purposes. The stage is very fast, moving between grain and mica image in about 2 seconds. I'm very impressed with the general quality and operation of the stage and consider it substantially better than the half dozen Autoscan

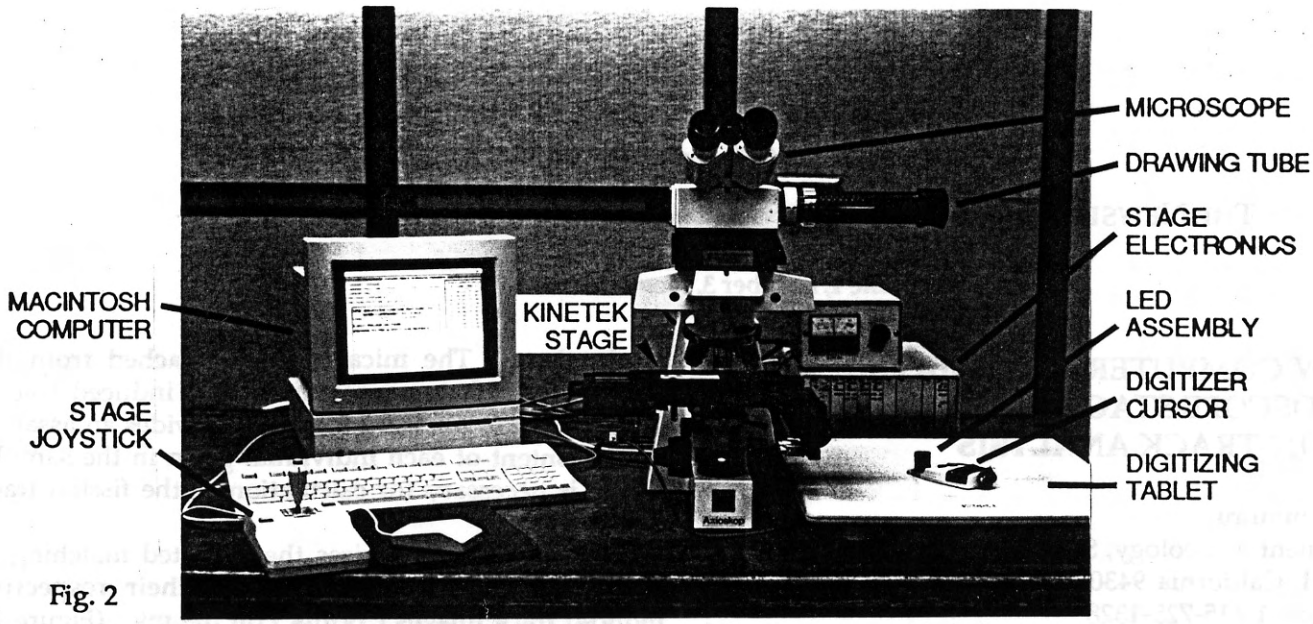


Fig. 2

stages I've used. Kinetek has sold several hundred of the stages, primarily to the semiconductor industry. They offer a one-year factory warranty and advertise that 48-hour turnaround is often possible on repairs. I think the fact that they've sold hundreds of stages for industrial use is the best testament to the quality of their product.

Wacom Tablet

The Wacom digitizing tablet has been rated very highly in computer magazines. On our microscope, it's able to measure track lengths with a reproducibility of ±0.15 μm. The digitizing cursor is cordless and does not require a power source, communicating with the tablet by receiving and then retransmitting a radio signal from the tablet. I've fabricated a simple high-intensity light emitting diode and battery assembly that fits to the cursor without needing a power cord. The LED is projected into the microscope field of view though a drawing tube, where it's superimposed on the microscope image of the sample as a well-focussed red dot. By moving the cursor over the tablet, the dot can be moved to any desired point in the field of view (this will be familiar to anyone who has measured track lengths with a digitizing system). As well as it's use in measuring track lengths, the tablet constantly transmits the cursor position to the computer and the cursor is used to drive the stage around (see below).

Software

The software permit measuring track lengths and determining ages at anytime within the same program, records the positions of all grains and length measurements for easy reexamination, and records track orientations to the grain c-axis as well as lengths. It incorporates many features of user-friendly Macintosh user interface such as windows, buttons, and text entry fields.

I tried to designed the software taking careful account of the actions involved in working at the microscope, in order to make the software operation as natural and intuitive as possible. In particular, the system runs so the operator seldom needs to look away from the eyepieces to the computer screen or keyboard. Most of the stage actions are controlled by the digitizing table and its cursor rather than the computer keyboard or a joystick (Table 1). The cursor has four buttons which are easily distinguished by feel so there is no need to look up from the scope. For example, to record a grain position for dating, you press 2-3 (cursor

TABLE 1: CURSOR COMMANDS

1-1	**Step stage 1 field-of-view (Y-direction)**
1-2-1	**Step stage left & reverse Y-step direction**
1-2-2	Reset step line to current stage position
1-2-4	Step back one field of view (Y-direction)
1-3-1	Exit to main menu
1-3-2	Save data
2-1	**Center point (highest power objective)***
2-2	**Center point (lower power objective)***
2-3	**Mark grain for age determination**
2-4-1	Move to a specified grain or track length
2-4-2	Shift magnification *down* one objective
2-4-3	Shift magnification *up* one objective
2-4-4	Reset step size parameters
3-1	**Move to *next* grain**
3-2	**Move to *next* mica position**
3-3	**Move to *current* mica position**
3-4	**Go back to *current* grain**
4-1	**Measure a track-in-track***
4-2	**Measure a track-in-cleavage***
4-3	**Measure crack width and orientation***
4-4	**Mark grain C-axis for length measurement***

*These commands use the position of the cursor as well as the buttons pressed. Other commands use only the buttons pressed so the cursor need not be in a particular position.

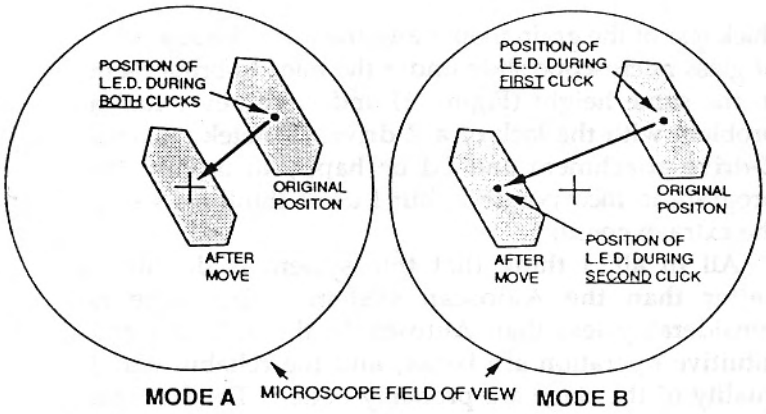


Fig. 3: Schematic illustration of the centering routines available from the digitizer cursor. In Mode A, you place the LED in the center of a grain (as projected via the microscope drawing tube) and click 2-2 *without moving the cursor*. The point on the grain where the LED was placed is automatically moved to the exact center of the field of view. In Mode B, you move the cursor between the two clicks. The grain is automatically moved from the LED position of the first click to the LED position of the second. These routines operate with any magnification and are used for a variety of purposes ranging from roughly centering grains under low power to precisely adjusting the positions of grains to a fraction of a micron before counting tracks. With these routines, tedious centering via the joystick is virtually eliminated.

button 2 then button 3) and the computer records the position. To then move to the mica print, you press 3-3 and the stage moves to the print. If you want to center a grain or other object, you move the cursor so the LED is superimposed on the object and press 2-2 and the stage automatically centers the object in a small fraction of a second (Figure 3). Manual driving of the stage with a joystick is always available, but I've found using the digitizing tablet to tell the stage exactly what to center much more natural. Twenty-one of the most commonly used commands are available in this way through the cursor (Table 1), while other less frequently used commands are available from the keyboard.

The system also includes a carefully designed stepping routine that steps over the slide one field of view at a time in a regular grid pattern, as when searching for grains to date or lengths to measure (Figure 4). Typically you press 1-1, 1-1, 1-1 . . . until you find a grain of interest, the stage stepping along one field of view at a time. Then you plop the LED on that grain and click 2-2 to center it, then you date it or measure the length. When finished, press 1-1 and the stage returns automatically to the original step line. Thus you don't waste time looking at the same grains twice if you end up on the wrong line.

Figure 5 shows some examples of the information displayed on the computer screen during stage operation.

It's For Sale (Sort of Cheap)!

The system is currently in full operation and working beautifully. I've recently put a lot of extra effort into cleaning up the software to make it robust and user-friendly, and I'm happy to offer the system for sale to other labs. The tentative price of the system for sale in the U.S. to university labs is about $15,000 (including stage, digitizing tablet, software, and miscellaneous items, but excluding computer, microscope, and microscope drawing tube). This breaks down into the $13,063 cost of the stage, $600 for the digitizing tablet, and $1337 for the software, marketing costs, LED assembly, manual, full technical support, etc. (profits after costs go toward our starving research effort). I haven't seen a recent price on the Autoscan system but extrapolating from an old 1987 quote I'd guess it's US$25,000-30,000. For non-U.S. sales, I'd be happy to figure out a price after investigating the minor formalities needed to export the system, and I'm also happy to negotiate a price for commercial labs.

The software has been tested on a Macintosh Classic ($1100 in the U.S. with a hard disc or $800 without) and a Mac II and should also work on an old Mac Plus. Our system is installed on a Zeiss microscope, and Kinetek also makes stages for Leitz, Nikon, Olympus, and Reinhardt scopes, and could probably fit a stage to

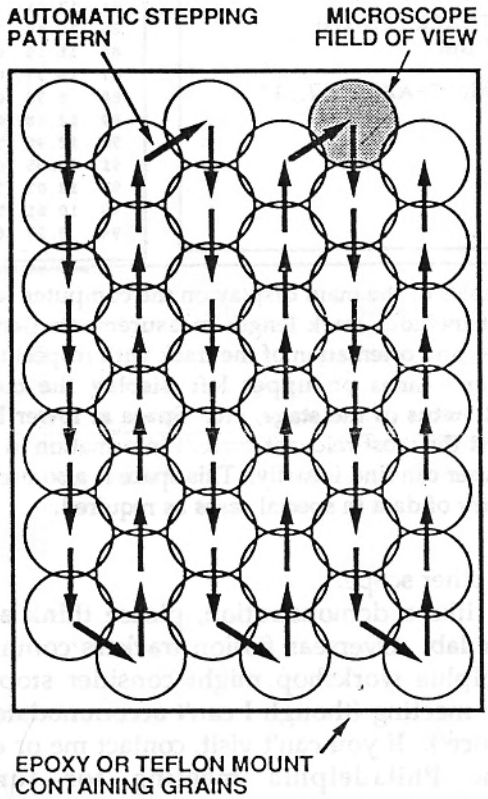


Fig. 4: Schematic illustration of the automatic stepping routine in the software. By pressing 1-1, the stage automatically steps one field-of-view at a time. If you move off the step line to measure a length or date a grain, a press of 1-1 will bring the stage automatically back on the step line to continuing scanning for the next length or grain.

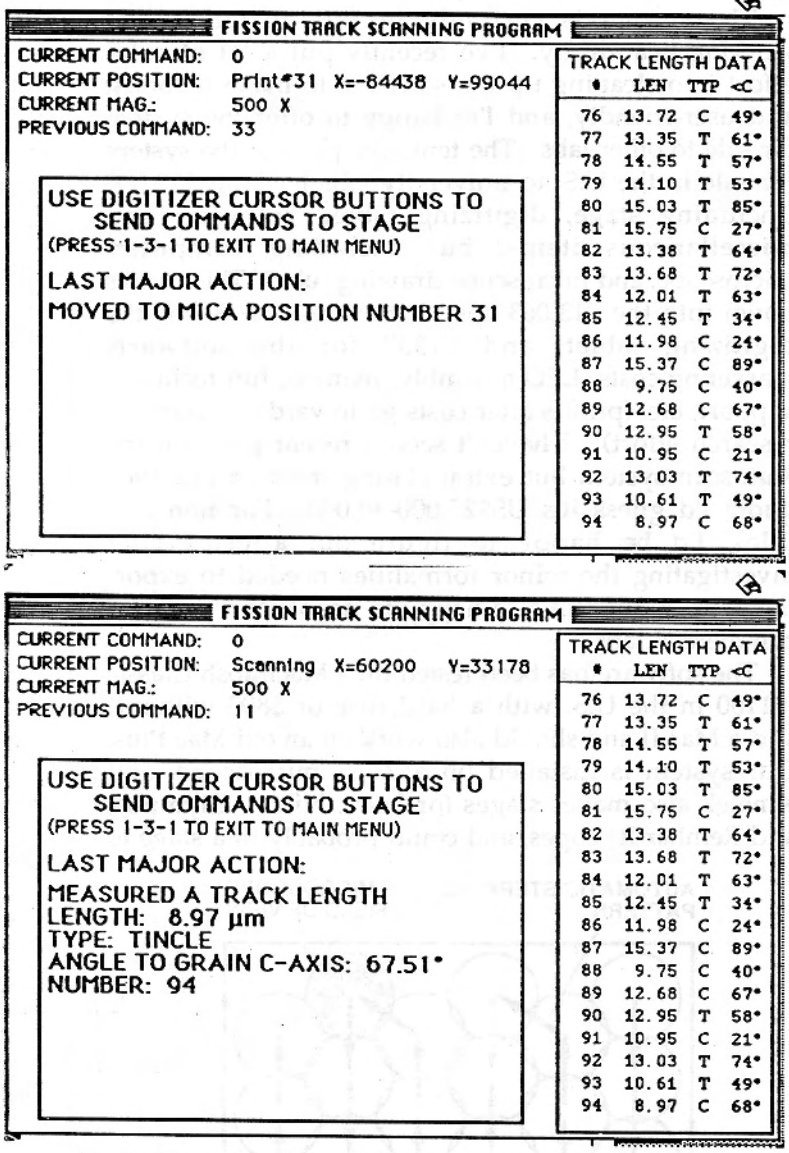


Fig. 5: Examples of the main display on the computer screen. Box on right includes track length measurements (lengths, Tint or tinCle, and orientation of the track with respect to the grains c-axis). Lines on upper left display the current position and status of the stage, etc. Space at lower left is used to report the most relevant current information in large print so the user can find it easily. This space is also used for keyboard entry of data in special cases as required.

almost any other scope.

If you'd like a demonstration, please think about visiting our lab. Overseas fission trackers coming to the Philadelphia workshop might consider stopping by after the meeting (though I can't accommodate 100 people at once!). If you can't visit, contact me or catch me at the Philadelphia meeting for further information. I'll try to bring the system to Philadelphia to demonstrate, but the difficulties of shipping a microscope safely might preclude this.

The only real limitation of the system compared to Autoscan's is the lack a Z-drive for automatic adjustment of focus to compensate for the differing thickness of the grain mount and the mica. I put a piece of glass microscope slide under the mica to bring them to the same height (Figure 1) and have never had a problem with the lack of a Z-drive. Kinetek makes a Z-drive attachment and I'd be happy to modify the program to incorporate it, but I don't think it's worth the extra expense.

All in all, I think that this system is definitely better than the Autoscan system. The price is considerably less than Autoscan's, the software and intuitive operation are better, and the reliability and quality of the stage are probably better. The fact that several hundreds of the Kinetek stages are in operation versus a few dozen(?) Autoscans is a real plus, as the Kinetek stage has a critical mass of user and service experience behind it.

IT'S ALL A PLOT

by *"Tracker"*

(Your man at the microscope with the tinted glasses and the geological watch - it tells the time in Ma, digital version only $2.)

My famous namesake, J.K. Galbraith, wrote "The Age of Uncertainty", a lofty enterprise, well suited, no doubt, to the whimsical world of economics, where time is money and the only certainties are death and taxes. But in the hard world of rock and acid, of hot times and revealing cleavages - down, so to speak, at the apatite face - it is the uncertainty of ages that is of greater concern. And not only the uncertainty but also the *variation*.

Fission track age

What is a fission track age? You need to distinguish between a *true age* and an *estimate*. An estimate is what you get from observed data. A true age is what your estimate is an estimate of: the value, if you like, that the age equation *would* deliver if zeta, rho-d, rho-s and rho-i were measured perfectly, with zero error (and let's not worry here about what we mean by error). This true age may not have a true meaning: it may correspond to the time since your sample cooled to below some temperature, or it may be something more ethereal - a reflection of a meandering thermal history, reminiscent perhaps of Mae West's invitation - full of promise but almost certainly later.

For a sample of grains the age estimates will vary even when the true ages are the same. And when the true ages differ, the spread of the estimates will be greater than the spread of true ages.

Chi-square test

Some years ago I was disconcerted to see a headline in the London Times:

Chi-Chi dying

The thought that this noble statistic might be falling into disuse, implausible though it seemed, was alarming indeed. One could well imagine how the report would go:

"The world of fission track dating was rocked to its foundations today when it was revealed ..."

which would be echoed in other newspapers under headlines like "Test frequency fails to reach expectation" (Guardian), "Only 5% reported use of statistic" (Financial Times), "Dating agency drops age test" (Daily Mirror) and "Chi-square out for the count" (Sun Sport). But it turned out to be a story about London Zoo's giant panda, though in its own way no less tragic, I suppose. Today it is London Zoo itself that is dying.

What does the chi-square test do? It tells you whether your sample age estimates are consistent with a single true age, or whether there is evidence of a spread in true ages. If your data are consistent with a single true age, this does not mean that there really is a single true age, but merely that there is little evidence to the contrary. And if there is evidence of a spread, it doesn't directly tell you how much spread. To put it another way, the upper chi-square tail probability (the P-value) measures the amount of *evidence*, not the amount of spread.

Of course if there is a large spread (of true ages) and you have moderately large counts you will get a large chi-square statistic. But you may also get a large chi-square statistic, or small P-value, even when there isn't much spread because you have large counts - you have a lot of evidence that there is *some* spread.

On the other hand, it often happens that your data "pass" the chi-square test even when there really is a spread, just because there isn't much evidence - usually because the spontaneous track counts are small. Your data can be consistent with a single true age and at the same time be consistent with a spread of true ages, and if the counts are very small there could be quite a large spread.

So what do you conclude? If you are dating an age standard or determining your zeta, then you know your sample age estimates should agree and you would expect your data to "pass". (I use the common jargon, though I prefer to quote the P-value and avoid the pass/fail language - after all, the message from $P = 0.04$ and $P = 0.06$ is essentially the same.) If it does, and you have sufficient data that you would have found a spread if there was one, then you have increased confidence in your result. If it "fails" you would look to your experimental procedure for an explanation.

But if you are analysing a field apatite sample with unknown thermal history, you may suspect that some track annealing has taken place and that there is a spread in true ages. So even if your data pass the chi-square test you would not *automatically* assume that there is no spread. Especially if you have small counts. You will normally consider length measurements and related samples and view all the evidence in its wider context. And if your data "fail" the chi-square test it could be due to genuine variation.

At this point I wish to *scotch*, once and for all, a pernicious rumour that heretofore has been largely contained beneath the earth's crust, even if, to pursue the metaphor, it has not been deeply buried, but of late seems to have been gaining credibility in certain quarters - I refer of course to the scurrilous notion that there is nothing new about the chi-square test - some have even said it is as old as the hills. I wish to state categorically that there is absolutely no truth in this totally unfounded allegation - indeed it was as recently as 1900 that Karl Pearson, founder of the UCL Statistics Department, and almost certainly a genius, for R.A. Fisher disliked him intensely, first derived the chi-square test for assessing the agreement between observed and expected frequencies. Fisher's animosity - indeed it was mutual - was greatly reinforced in 1922 when he pointed out that Pearson had got his degrees of freedom wrong - and although Fisher provided the essential mathematics it was only in 1945 that David Finney, renowned in statistical circles for his work on selecting insecticide doses that kill bugs half dead, explicitly (and intentionally) applied it to pairs of counts from independent Poisson distributions. Even then it wasn't used for ages.

Radial plots

"A man and his *hobby-horse* ..." quoth Tristram Shandy, though it would be difficult to find a man with such a hobby-horse as to compare with that of his Uncle Toby: yet will I follow his example who "would use no other argument to prove his *hobby-horse* was a *hobby-horse* indeed, but by getting upon his back and riding him about - leaving the world, after that, to determine the point as it thought fit."

So "what the devil does the plot signify, except to bring in fine things?" - the question could hardly be better put. If you have two estimates, one very precise and the other not, you place more credence in the precise one - you do not weight them equally. A radial plot displays your single grain ages so they can be seen and compared *at face value*. It automatically gives them the right weights in accordance with their precisions. If you plot just the ages, ignoring their differing precisions, it is practically impossible to compare them sensibly.

In a general sense there are two reasons why age estimates vary: because they differ from the true ages

(there is *uncertainty*) and because the true ages themselves vary (there is *variation*). In a radial plot you can display the uncertainty, so it is easier to assess the variation. You can judge whether single grain ages look homogeneous, and check your view if necessary by the chi-square test. And you can go further and assess visually the spread of ages, comparing it with possible explanations or possible models. It is a tool for assessing your data, and it is a tool for showing it to others. But let us climb upon our hobby-horse and ride him about.

Some examples

The plot of age estimates from the Barrabool Hills, Otway Ranges - only a stone's throw, so to speak, from that delightful restaurant in Apollo Bay (hours extended to 9:30 p.m. on Fridays) - illustrates apatite data with a clean thermal history. These grains were cooled at about 120 Ma and have not experienced significant heat since. The estimates look homogeneous on the graph and the chi-square test confirms that there is no evidence to the contrary. (The chi-square statistic is 10.0 with 19 degrees of freedom). We have plenty of data and the picture seems clear - if there really was a spread we feel we would see more sign of it.

The Flaxmans-1 data, also from the Otway Ranges, are from a depth of 2.6 km and a current temperature of around 92°C. Tracks have been annealed by varying amounts and the *true* ages vary between grains. There is clear heterogeneity in the graph, where the estimates vary from 0 to 120 Ma, and its pattern invites further study. The heterogeneity is so obvious that a formal test is unnecessary. The chi-square statistic is 241.4 with 29 degrees of freedom; it is large partly because there is a wide spread and partly because there is a lot of information. Furthermore these two sets of data reinforce each other.

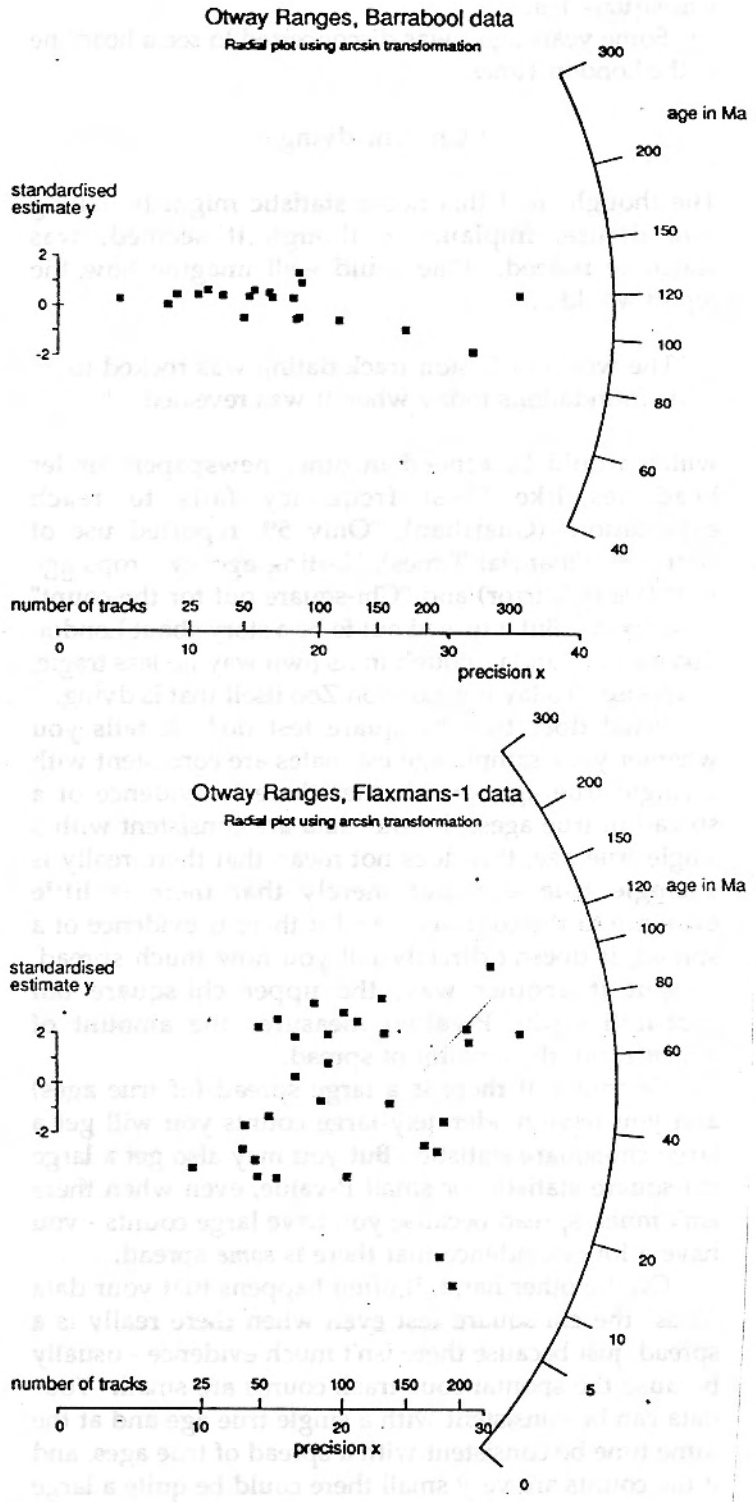
Before the next plot, try these exercises:

Radex and bar

Draw a straight line on a strip of overhead transparency: this is your *radial extrapolator*. Place it on the graph so it passes through the origin and through a point. Read off the age on the circular scale.

Make a copy of the ±2 y-scale also on transparency (this is your two-sigma error bar - the same bar applies to each point). Center the bar, parallel to the y scale, on a point; hold one end of your radex at the origin and move the other end up and down as much as possible so that it always intersects the bar. The resulting interval on the age scale is the two-sigma age confidence interval for that grain. Repeat this for other points. See how you get a shorter interval from a more precise estimate. Optimists can do one-sigma intervals in similar style.

What about the pooled age and its standard error? For this you might need more space, a bar top will do



nicely, and an extended radex. Simply plot the pooled age on your graph (probably off the page), centre your error bar *there* and repeat the previous exercise. (In the above graphs the pooled age is at $y = 0$ and $x =$ twice the square root of the total number of tracks - spontaneous plus induced - for all grains. This ignores error in zeta and rho-d but you can add that roughly in your head.)

To compare several estimates together, place the line so that it goes through the origin and through the middle of the points. Move the centre of the error bar

along the line, keeping the bar parallel to the y scale; see if you can catch all the points. Adjust the line and repeat if necessary. If so, the estimates are roughly consistent with each other. There will be times when it isn't obvious whether or not they are; you can check formally using the chi-square test. You could do this with all the points, or with a prescribed subset. With two lines you could see if the estimates are consistent with a two-component mixture.

Turn the graph so the y scale is horizontal. Imagine you are standing at the origin looking out to the age scale. Slowly tip the paper away from you so you are eventually looking right along a line from the origin through the points. Tip the paper back and forth a few times. See how many points you can cover visually with the y error bar. Also if the points with high precision tend to fan out more than those with low precision that is evidence of variation in true ages. This is dynamic graphics. Now lean on the bar ...

Down to Earth

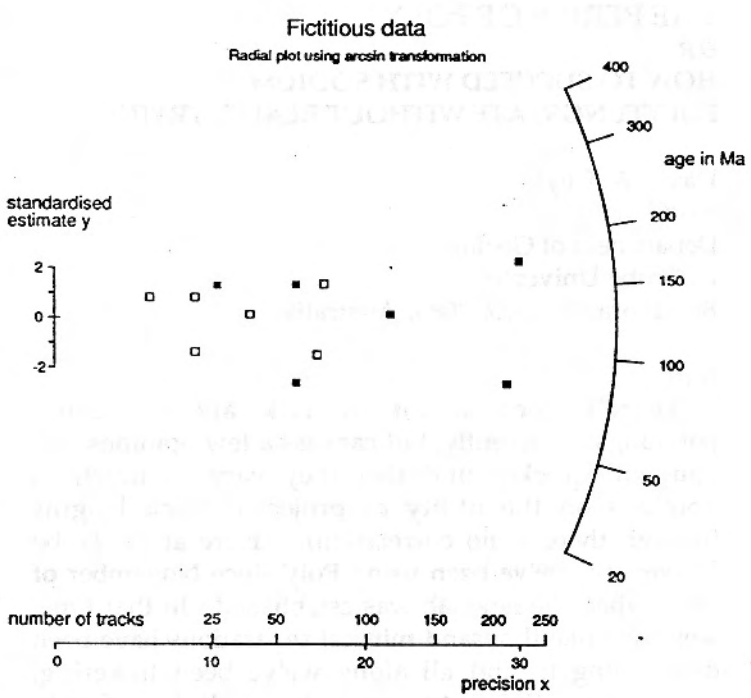
The last graph shows two sets of data I collected myself. In the first set, plotted as open squares, the counts are (2,18), (3,6), (6,14), (9,30), (11,60) and (22,53). (I confess I made them up. I used only 6 grains so the graph wouldn't get too cluttered.) They look reasonably homogeneous. The chi-square statistic is 7.0 with 5 degrees of freedom, giving a P-value of 0.22 - not much evidence here of any heterogeneity.

To get the second set, plotted as closed squares, I multiplied every count in the first set by 3. The ratios Ns/Ni are exactly the same as before - and therefore every age estimate is exactly the same as before. (Each point has moved outwards along its radius.) The mean age is the same for the two sets, as is the standard deviation or indeed any calculation done on just the ages. But the second set don't look homogeneous - they scatter too much in the y direction. And the chi-square statistic is 21.1 with 5 degrees of freedom, a P-value of less than 0.001.

There are two messages here. One is that the precisions carry information and methods that ignore this must be wrong in principle. Any calculation using just the ages would give the same answer for both these sets of data. But in the second set the precisions are higher; there is more information.

The other message is the fallacy of assuming that a hypothesis is true just because data are consistent with it. We may have data something like the first set, but had we counted bigger areas per grain we might have got something like the second set. Of course this fallacy is inherent in all scientific inference and is well known, but it is still a common source of misjudgment.

Sometimes there are many grains with very few or even no spontaneous tracks. What about these? Can we put them on a radial plot? What happens to the chi-square test? Who wants to know? For answers to these



and other mysteries we must visit Alaska, in the next episode.

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THE PERILS OF POLY
OR
HOW TO SUCCEED WITH SODIUM POLYTUNGSTATE WITHOUT REALLY TRYING

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Intro

There's been a lot of talk about sodium polytungstate recently, but canvass a few opinions and you will quickly find that they vary as much as opinions on the utility of projected track lengths (though there is no correlation). Here at La Trobe University, we've been using Poly since November of 1988, when the new lab was established. In that time, well over one thousand mineral separations have been done using it, and all along we've been tinkering, constantly refining the procedure. It is a firmly entrenched part of our system, and I think that most people, having now used it, would never want to go back to TBE. I'm presenting here a pretty comprehensive review, and so I think that this article will have a little something for everybody, users and non-users.

"So what *Is* Sodium Poly, and why should I use it?"

Poly completely replaces TBE or bromoform for mineral separation. Sodium polytungstate, $3Na_2WO_4{\cdot}9WO_3{\cdot}H_2O$, is an inorganic sodium salt and, like all salts of sodium, is soluble in water. Thus you can mix up a solution of Poly with an SG of anything between 1 and 3.1, at which point Poly will crystallize. But much more important than this is what Poly isn't: *toxic*. Unlike the halogenated lower alkanes (TBE et al.), Poly has no known toxic effects (Actually, that's not quite true. The LD50 (lethal dose to kill 50% of a population) for another tungsten compound, disodium tungstate, is 1190 mg/kg body weight (in rats). This translates into about 60 grams for a typical 50 kg human in other words, less toxic than table salt). I won't dwell on this issue, but it is the main reason for switching to Poly.

Of course, Poly's properties also mean that when used properly, it will be significantly less expensive to use over the long run.

- You no longer need special laboratory space. You can do away with fume cupboards, toxic waste collection, protective clothing and emergency breathing apparatus. You won't need to evacuate the building if you spill a litre of it on the floor!
- You can do away with expensive solvents. All washing up is done with distilled/deionised water. If you're really lucky, your department will have distilled water lines plumbed into the building (as we do, courtesy of the chemistry department).
- Do away with those expensive lab grade filter papers! Whatman filter papers have an abysmally low wet strength, and so we've switched to coffee filters. They're an order of magnitude stronger, cheaper and faster. Greenies take note: you can also get unbleached coffee filters (on which the clear apatite grains are more visible).
- Your recovery system comes from K-Mart. To evaporate the washings back up to density, we simply bought two Teflon coated electric frying pans. When set to about 70°C, and placed into a fume cupboard with the front panel open only to the top of the pans (to increase the airflow), we get evaporation rates of greater than 250 ml/hr per pan. Some people actually boil the Poly, but then you run the risk of getting a brick and also, if you don't catch it in time you can drive the water out of the Poly molecule itself (in which case you're stuffed, mate!).

One last point I'd like to make here is that one of the other reasons why I argued that we should switch to Poly at La Trobe is that I have personal knowledge of two labs where both TBE and DIM are verboten. You can see the writing on the wall can't you: if there is a low-, or better yet non-toxic alternative, the day will come when you too will be *required* to use it. Better to start now and get familiar with something at your own pace, than to one day have a new and strange system thrust upon you (a bit like Autoscan's version 6).

Reality intrudes

Of course, all this does not mean that you can simply lock TBE into a cupboard, replace it with Poly and continue on as before. This misconception is probably what has led to all the negative opinions of Poly. You have to remember that Poly is a salt, and when dissolved in water you've got an ionic solution. It is reactive, and if you don't take precautions, you are definitely going to run into trouble. The main culprit are those crazy Ca^{++}. The basic problem is that Poly really loves these guys, and will drop sodium in an instant if there are any around. This is bad news for you, because calcium-poly is insoluble, and will precipitate, clog your filter papers, and make you long for the good ol' days of mutagens and low liability cover.

The second property of Poly which you must take into account is its higher viscosity when mixed to an SG in excess of about 2.7. This can increase settling times, but it's only a significant problem if the grains are very small, or if there are only a very small proportion of heavies in which case you run into entrainment problems.

Luckily, there are ways of dealing with these problems, which require observing the Four C's:

Clean
Concentrate
Carbonate
Clay

Clean

This principle applies to every step in the separation procedure. Unfortunately, we have observed here that because Poly is so forgiving, people tend to take advantage of her: slopping things around, letting the liquid get dirty, not washing the fines out of the sample first, and so on. And then they complain that the separations aren't working! If you keep your lab and the samples clean, you'll eliminate at least 90% of the aggravation that some people experience. Points to note:

- WASH THE SAMPLE THOROUGHLY The point of this is to get rid of all the really fine particles, and especially clays, which are released when the sample is crushed. Their fine size means that they can remain in suspension in the Poly for a long time. Clays are a particular problem, because they will flocculate in the Poly, and clog the filter paper. You can't imagine how aggravating this can be until you've experienced it. I suggest that if, after repeated rinsing, you still have visible fines in the sample, you may consider dropping the sample into an ultrasonic cleaner for ten minutes or so.

- KEEP THE LIQUID CLEAN Because you're now using coffee filters, the really fine particles which do remain with the sample will wash through. They can go on accumulating, sample after sample, until a) you can't see what's going on in the separating funnel, and b) they flocculate, leading to well, you know by now. The solution to this is to keep a few #2 Whatman filters (or equivalent) around to clean up the liquid. I've found that the best procedure is:

  Let the washings stand in large (litre or greater) graduated cylinders overnight. Most of the fines will settle out here. Filter the washings through a #2 before starting the recovery.

  When the SG of the Poly being recovered is up to about 2.5 or so (that is, before it begins to get viscous), give it another filtration. You don't need to measure the density, you'll develop an eye for it. Here it's called the 'slosh factor.' Slosh it around in the pan, and if it looks like high quality Canadian 100% natural maple syrup, then it's about right. If it looks like cheap, imitation Aunt Jemima Butter Flavoured syrup, then you've gone too far. The liquid should now be crystal clear, and will only need a short time longer in the pan to get it right up to your desired density.

Concentrate

I'm not referring to your state of mind, although a bit of that is required too. By concentrate, I mean you should concentrate your heavies using a suitable device such as a Wilfley or Gemeni table (I really like the Gemeni table, because if you design your feed mechanism right, you can divert the sample down only one side of the table, which makes using it practical for even relatively small samples). It's best to concentrate the sample before you clean it, because this step will automatically take out many of the fines for you. Also be sure to get out all the ferromags, and as much of the other magnetic crud as possible by running the sample through the vertical Frantz.

Think about it. You can have either 1000 grains trying to separate from ten million lights, in a litre of liquid and having to settle through fifteen or twenty centimetres, or you can have that same thousand grains trying to separate from another thousand lights, in one cup of liquid, over a distance of five or six centimetres. It also means that you don't need as much liquid, can crowd more funnels onto a bench, the settling times are drastically reduced, fewer washings means shorter recovery times, and you're out of the lab that much quicker.

Oh, and don't forget to use vacuum filtration. If you rely on gravity, you can be waiting an awfully long time.

Carbonate

Carbonates are the main source of those nasty calcium ions mentioned earlier. Big, sparry calcite isn't too much of a problem, but thin carbonate crusts on grains, such as from a calcite-cemented sediment, can cause trouble. There are two things that you should do. First, check to be sure that your liquid isn't acidic. We have a problem here, in that our distilled water has a pH of about 5.5 (how it can still be called distilled, I don't know). We neutralize it with a small pellet of NaOH in about 30 litres of water; the extra sodium ions won't get in the way, but don't go overboard: something in the 7–8 pH range is best. Second, if you know that your sample has calcite in it, let it soak in acetic acid overnight (after you concentrated it). This won't hurt the apatite, and because you're dealing with small grains, longer soaking really isn't necessary (see also Dennis Arne's article in the last issue). Use acid which has a higher strength than vinegar. If you have conodont people in your lab, beg some acid from them, otherwise glacial acetic acid from a darkroom supply shop will do the trick.

Clay

This was the biggest headache. No amount of washing ever removes all of the clays in some rocks, simply because they can be in the cracks of individual grains. The Poly seems to be able to strip some of the

finest clay particles out of the grains, and with some sediments (especially those that have muscovite in them), and weathered igneous rocks, quite a lot of really fine white material appears in the liquid. We've XRD'd it, and it isn't one of the commoner things like sericite, although it most certainly has some clay component. We suspect that the Poly is reacting with the finest clay particles and forming a new hybrid, whose only purpose on this earth seems to be clogging filter papers. It really doesn't matter what exactly is happening, as long as we know that it is clay minerals which are responsible.

Help came from our micropaleontologist, Mark Warne, who told us that you can strip the clay minerals out of a sample by soaking it in hydrogen peroxide. There are a few cautions that you must observe, however. Don't use a metal container. Don't fill a beaker more than about 1/3 full of sample, because the reaction gives off oxygen, and the sample can bubble over. You don't need a fume cupboard, because there's nothing wrong with adding a little extra O_2 to your lab, but do wear gloves. Don't let your blonde girlfriend know that you're using peroxide: it'll all mysteriously disappear. We're still experimenting to determine what concentrations are best to use, how long to let it sit, and so on. In the meantime, just let it soak until it stops bubbling. Depending on sample size and the amount of clay in it, and the strength of the peroxide you're using, that can be anywhere from half an hour, to two or three days (a week end---plan ahead). If you're really in a hurry, then the thing to do is add concentrated peroxide to dry sample. This can give you a pretty violent reaction, so be sure you have a tray to catch the overflow!

Epilog

I recently had the opportunity to put all these principles into practise on a few samples from the Wemmetsweiler-Nord well from the Saar-Nahe basin of Germany. I had separated a few samples over a year ago, before we got so sophisticated. They were without doubt the most troublesome samples I had ever dealt with. This last batch, however, separated beautifully. No precipitation, no strange white scum, the Poly remained crystal clear, and the time I spent in the lab was less than half of what it had been for the same number of samples one year previously. In the end, the best advice that I can give you is to know your sample. If you're separating granites which have been blasted out of the ground by geochemists looking for that perfect specimen, then you'll be able to skip some of the steps I've outlined. On the other hand, if you've got graywackes, altered tuffs, shales, or diamicts, then you want to be really careful. You can separate all rocks according to one recipe, but that would involve a waste of effort for most of the samples. The suggestions I've made here can make all the difference between struggling with mineral separations, and really being able to appreciate the significant advantages that Poly has to offer, but in the end, success or failure rests with your experience and dedication to the task. In addition to bulk separations, there are some more sophisticated uses for Poly. I'll describe the coolest one in the next issue.

Sodium polytungstate is manufactured only by the SOMETU company of Germany. Depending upon where you are, the price can be significantly higher than that for TBE or bromoform, but in the long term the total operating costs even out, or even swing in favour of Poly, depending upon the initial price difference. The price you pay will fluctuate with the exchange rates, but the most recent price I have got from Sometu is 240DM/kg for powder. They also offer a discount for large orders, but the price does not include shipping or any taxes payable at the point of entry. I have heard that there is a U.S. reseller who is selling Poly at a significantly higher price, so I suggest that before you buy, check the price direct from the manufacturer. Their address is:

SOMETU
Falkenried 4
D--1000 Berlin 33 Germany
FAX: 030/831 61 75
Telex: 0185 443
Phone: 030/831 19 50

FORELAND BASIN DISCOVERED BY FISSION TRACKING

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New Zealand is well known amongst the global geoscience community for at least three features: the Taupo Volcanic Zone, the Alpine Fault, and the number of fission trackers it has produced (Barry Kohn, Diane Seward, Nancy Naeser, Rex Galbraith, Paul Fitzgerald and myself)! I originate from Rotorua, the centre of the thermal wonderland, but ever since I was an undergraduate student I have had an enormous fascination for the Alpine Fault. Upon becoming a postgraduate student I soon realised that there was global interest in this fault and that a key to getting manuscripts into international journals was to have "Alpine Fault" somewhere in the title, or at least prominently in the abstract. The Alpine Fault is of course one of the great transcurrent faults of the world, and it is chiefly noted for the 480 km of dextral offset

along it. (Incidentally, displacement on it started in the late Oligocene, 2 m.y. after initiation of the San Andreas Fault System (SAFS), it is linked directly with the SAFS via the East Pacific Rise, and it has been proposed that they may have a common tectonic origin - see Kamp (1991)).

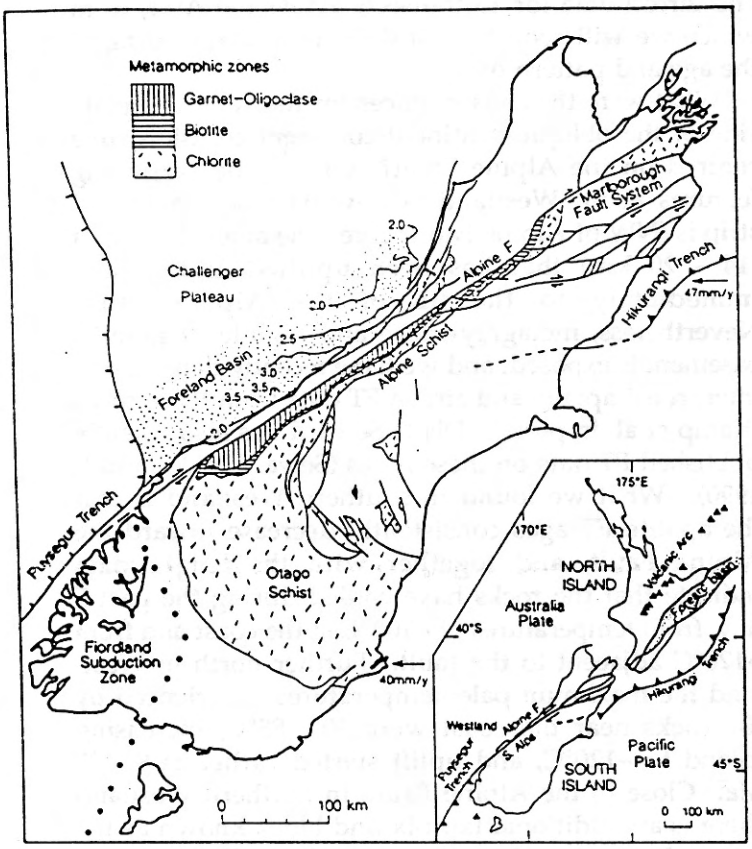
However, it may be that for too long we have focussed on the magnitude of the horizontal offset on the Alpine Fault, and related problems of its age and seeming disappearance north of South Island (Kamp, 1987), to the exclusion of all of the consequences of the vertical displacement across the fault. Originally, it was the vertical displacement on the fault, evident both geomorphically and from the juxtaposition of amphibolite grade schist (SE side) with metagreywacke (NW side), that drew P.G. Morgan's attention in 1908. Much later, in 1948, Harold Wellman established the case for 480 km of horizontal offset on the fault from the displaced ends of Permian-Jurassic basement terranes. Through the 1970's and 80's the role of the Alpine Fault as a sector of the modern Australia-Pacific plate boundary, its late Oligocene age of initiation, and its tectonic evolution in the context of SW Pacific relative plate motions became firmly established (see Kamp 1986 and references therein). Discrepancies remain to be resolved however between predications based on sea floor spreading data (Stock and Molnar, 1987) about the age and amount of horizontal displacement on the Alpine Fault, and the observations based on the onshore geology; the seafloor data would have displacement starting 8 to 10 m.y. earlier (35 Ma) and accommodating 300 km more horizontal movement than is evident on the ground (Kamp and Fitzgerald, 1987).

The geophysically based predictions of Stock and Molnar (1987) and the onshore geological record do agree that the compressive component of displacement on the Alpine Fault started after anomaly 5 (9.8 Ma). Our zircon fission track data (Kamp et al., 1989) show unequivocally that in central South Island uplift was underway by 7 Ma, although most of the 14-20 km of rock uplift immediately SE of the Alpine Fault in the Mt Cook region has occurred in the last 1 to 2 m.y. Although conventional K-Ar dating has been undertaken on the schists SE of the Alpine Fault (e.g. Adams and Gabities, 1985), interpretation of the data is equivocal due to excess argon problems in the zone one would expect to have reset ages.

In terms of viewing the consequences of the late Cenozoic oblique convergence across the Alpine fault, attention to date has been focussed almost exclusively on the SE (Pacific plate) side of the fault. This has probably been the case because that is where the geomorphic evidence of uplift (Southern Alps) is most obvious, and where the highest grade rocks crop out. This is why we were first attracted to apply FT analysis in the Southern Alps. Following on from the work of Paul Green and myself, Mark Tippett (Ph.D. student, University of Waikato) has derived apatite and zircon data for 13 transects across the Alps, from which we will soon have a definite understanding of the age and pattern of uplift.

What were the consequences for the Australia plate side of the oblique continent-continent collision zone centred on the Alpine Fault? One of the surprising features about Westland is how narrow the coastal strip is (NW of Alpine Fault) given the amount of crust (14 - 20 km) that has been uplifted and eroded immediately to the SE of the Alpine Fault. Nevertheless, metagreywacke and high-level granitic basement is exposed, and we have recently derived and interpreted apatite and zircon FT data from these rocks (Kamp et al. in press). Diane Seward has also recently published FT data on these rocks (Seward and Nathan, 1990). What we found in southern Westland is that the apatite FT ages consistently decrease toward the Alpine Fault, and together with the length data indicate that the rocks have cooled during the past 5 m.y. from temperatures of ~100°C at the coast and from >120°C adjacent to the fault. Further north in Westland the maximum paleotemperatures experienced by the rocks near the coast were 80 - 85°C, increasing inland to ~120°C, and uplift started earlier at 8 - 10 Ma. Close to the Alpine Fault, in northern Westland there is an additional fault-bound block known as the Fraser Formation that appears to be uplifted Alpine Fault zone, and these rocks have suffered much greater amounts of late Cenozoic cooling.

The question arising from these FT data is the nature of the material that was eroded. Fortunately, there are enough outliers of late Cretaceous-Oligocene and mid to late Miocene cover rock sequences onshore to be able to be confident that the 3.5 - 5 km of section eroded was not chiefly basement. We think that it involved mainly the erosion of a very thick mid to late Miocene succession, that is, the inversion of part of a sedimentary basin. From industry seismic, Nathan et al. (1986) mapped the distribution and thickness of a number of seismic units beneath the shelf and slope offshore from Westland. In Fig. 1 we show their estimates of the depth (km) to the top of a prominent late Oligocene reflector. Importantly, the thickness of this Neogene section increases towards the coastline, where it is exposed in a few places as a giant truncated monocline. In the south the Miocene section truncated at the coast is about 3.6 km thick, which compares well with the ~3.8 km of uplift and erosion estimated from FT data for samples at the coast. The significance of the FT data is that they indicate that prior to inversion, the mid to late Miocene basin deepened towards the Alpine fault. One of the key features of a foreland basin is that the sedimentary section increases in thickness towards the mountain belt, which is the source of the lithospheric flexure, and that typically, as a result of continued thrusting, the inner margin of the basin becomes



deformed and inverted, as is the case onshore in Westland.

In standing back from the FT details, it has always been known that the major convergent mountain ranges of the world usually have an associated foreland basin. This association has however not been drawn previously for the South Island, probably because the foreland basin in this case is out of sight offshore in a poorly explored basin, and to date our attention has been focussed chiefly on the magnitude, age and location of the horizontal displacement on the Alpine Fault, and the spectacular amount of uplift in the Southern Alps.

A lesson that can be more widely learned from this experience, the identification of an important tectonic element that puts a new perspective on the character of the oblique continent-continent collision in South Island, is that the derivation and interpretation of FT data can make key discoveries, particularly if it is integrated with other geologic data. Of course, many of you will have similar claims. I think it is important to impress upon our postgraduate students the need to be confident in the use FT analysis to make new interpretations, even if they may appear at first to be outrageous, and not limit interpretations to confirming other geologists assertions. This assumes that ones confidence does not overstep ones technical and interpretative ability.

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FROM THE EDITOR & CALL FOR CONTRIBUTIONS

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Welcome to the second year of *On Track*, the semi-annual newsletter of the international fission-track community. I've taken over the editorship for this year and I think we should all congratulate and thank Dave Coyle for his outstanding efforts in launching and edited *On Track* for its first year. Great job, Dave!! The main purpose of *On Track* is to foster informal communication among the fission track community. So if you have anything to contribute, please send it in. We welcome virtually anything of interest, such as descriptions of new lab techniques, reviews of useful products, news and gossip, raving editorials about what all the other labs are doing wrong, meeting announcements, job openings, cartoons, commercial ads, and descriptions of what you are doing in your research. ***On Track* is (currently) free but to cut costs we generally only mail one copy per lab, so please pass it around to your coworkers.**

Certainly the editor's most difficult job is getting people to contribute. So if you have anything of interest, *please* send it in. That bears repeating: if you have anything of interest, *please* send it in. The next issue will come out about 1 June 1992. If you want to contribute, please scribble me the briefest of notes with a guess of the title and the length and get it to me by 1 April (on 2 April I get on the phone and call around if I don't have enough titles). Then send me the final text by 1 May at the latest. When I get your title I'll send details of formats, discs, etc.

The plan is for the editorship of *On Track* to rotate every year, so the next issue will be my last. If anyone out there would like to take over the editorship, let me know by March so we can put the details in the next issue. Otherwise I may have to appoint someone whether they like it or not. I'd strongly encourage some of the fission track graduate students out there to volunteer as editors (we could have more than one editor). It's a great excuse to call and write all the other fission trackers in the world and become recognized among your worldwide peers (fame, that's why I did it!).

MEETING ANNOUNCEMENTS

7TH FISSION TRACK WORKSHOP PHILADELPHIA '92

Gomaa I. Omar and Robert Giegengack, Co-convenors

The Geology Department of the University of Pennsylvania will host the 7th International Workshop on Fission-Track Thermochronology during the week of 13-17 July, 1992, on the campus of the University of Pennsylvania. A second circular with details of the meeting will be sent out before the end of 1991. Anyone interested in participating who has not yet received an invitation should write us directly at:

7th International Fission-Track Workshop
Department of Geology, Box Lambda Zeta
University of Pennsylvania
240 South 33rd Street
Philadelphia, Pennsylvania 19104-6316 U.S.A.

We are planning a busy week in Philadelphia, the Cradle of American Democracy, with its abundance of cultural and recreational opportunities.

We look forward to welcoming the fraternity/sorority of fission-track scientists to Philadelphia in July 1992.

INTERLABORATORY STANDARDS FOR THE 1992 WORKSHOP

Don Miller will be distributing two or three samples for analysis prior to the Philadelphia workshop (apatites for age and track length measurements). Plans are to distribute these samples in December 1991 and/or January 1992 to those laboratories requesting an aliquot. The samples available to date are new samples which have not been circulated before. If you wish to receive these samples please contact:

Don Miller
Department of Earth and Environmental Sciences
Rensselaer Polytechnic Institute
Troy, New York 12180-3590, U.S.A.
Don's office 518-276-8523; Fax 518-276-8627

GAC/MAC WOLFVILLE, NOVA SCOTIA, 1992

"Mineralogy and Crystallography in Low-Temperature Thermochronology" is scheduled as a special session at the 1992 Geological/Mineralogical Association of Canada annual meeting in Wolfville, Nova Scotia, May 25-27, 1992. The session organizers (Casey Ravenhurst, Fax (518) 276-6003 and Ray Donelick, Tel (713) 550-9593) solicit papers from researchers interested in mineralogical or crystallographic controls on the kinetics of diffusion of daughter products in low-temperature thermochronologic systems. In recent years, important mineralogical and crystallographic controls have been identified in the K-feldspar $^{40}Ar/^{39}Ar$ system and the apatite fission track system. This information has advanced our understanding of diffusion at laboratory timescales, thereby improving our ability to study tectonic unroofing, provenance of sediments, and hydrocarbon generation and fluid flow in sedimentary basins at geological timescales. It is anticipated that the *deadline for abstracts will be January 15, 1992*, but the organizers would appreciate being contacted by interested individuals before that date.

Before the meeting (23-24 May), the Department of Geology, Dalhousie University will host a short course entitled: "Low-Temperature Thermochronology: Techniques and Applications". The course is intended for the non-specialist and previous experience in geochronology is not necessary.

For a circular for the GAC/MAC meeting, contact:
Aubrey Fricker, Secretary of the Organizing Committee
Atlantic Geoscience Centre (Geol. Surv. of Canada)
Bedford Institute of Oceanography
P.O. Box 1006, Dartmouth, Nova Scotia B2Y 4A2
Tel (902) 426-6759 Fax (902) 426-4465

For information on the short course, contact:
Marcos Zentilli or Peter Reynolds
Department of Geology, Dalhousie University
Halifax, Nova Scotia B3H 3J5
Tel (902) 494-2358 Fax (902) 494-6889

EIGHTH INTERNATIONAL CONFERENCE ON GEOCHRONOLOGY
ICOG-8
BERKELEY 1994

The Eighth International Conference on Geochronology, Cosmochronology, and Isotope Geology will be held June 5-11, 1994 at the University of California at Berkeley, under the joint sponsorship of the Institute for Human Origins, the University of California, Stanford University, and the U.S. Geological Survey. The organizing committee is trying to assemble a complete mailing list of possible participants. A first questionnaire was sent out in August 1991. If you didn't receive it, you're probably not yet on the mailing list. The current *On Track* mailing list will be forwarded to the ICOG-8 committee, so if you received this issue of *On Track* correctly addressed to you personally, you'll be on the list. If you're reading someone else's copy or if you have friends or colleagues who should be on the list, please mail their names, addresses, telephone numbers, primary interests (e.g. geochronology–fission tracks), and fax numbers and e-mail addresses (if available) to:

Dr. Garniss H. Curtis
Chairman, ICOG-8
Institute of Human Origins
2453 Ridge Road
Berkeley, California 94709 U.S.A.
Fax 1-510-845-9453
Telephone 1-510-845-4003

or send it by e-mail to "trevor@pangea.stanford.edu". (If you send it by e-mail you'll be sent a confirmation by e-mail. If you don't receive it, your e-mail may be lost so please send your message again by post.)

RECENT AND FORTHCOMING 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.)

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SHORT TRACKS

Something in the water in Melbourne? A remarkable number of babies were born or due in Melbourne this year. Dennis and Sue Arne (Geotrack) started it off by giving birth to Matthew Thomas Arne on 19 April. Matthew's hobbies reportedly are eating, sleeping, playing (much like his parents) and crying. Paul Green and Sue Marshallsea (Geotrack) followed with Hariett Elizabeth Marshallsea Green on 5 September. Seems Harriet was 11 days early and caught the parents a little by surprise! Paul and Andrea O'Sullivan (Geotrack and La Trobe) are expecting in mid-November, Kevin and Kathy Hill (La Trobe and Monash) in late December, and Mike Krochmal and wife (Autoscan) in February. All babies and parents are reported doing well but productivity in Melbourne is presumably at an all time low. Congratulations to all!!

Changing of the guard at Texas. Jeff Corrigan and Leslie White have finished up their Ph.D.'s at the University of Texas at Austin, Jeff on modeling of fission track annealing and thermal history analysis of the Texas Gulf coast and the Bengal fan, and Leslie on the thermal and uplift history of the western Transverse Ranges northwest of Los Angeles. Jeff's now working with Steve Bergman at ARCO Research in Dallas and Leslie has started with Exxon in Houston. Alas, both seem to be moving away from fission track work, Jeff into synthetic basin modeling among other things and Leslie into seismic stratigraphy. Ray Donelick's now a part-time post-doc at UT, and a couple of new Ph.D. students are doing fission track work. Rich Weiland is working on the fold-and-thrust belt in Irian Jaya and Stefan Boettcher is working of Basin and Range and thermal history problems in the Dome Rock Range in Arizona.

Cherry Lewis, late of Tony Hurford's lab, has reportedly joined Geotrack–Europe, continuing

Geotrack's breakneck expansion onto yet another continent. Rumour has it that Rod Brown may take a position with Tony in London after finishing up his Ph.D. at La Trobe.

Brian Patrick of the University of California, Santa Barbara is setting up a new fission track lab. Brian is mainly a metamorphic petrologist but with a lot of interests in unroofing histories. Ann Blythe, formerly a PhD student at Cornell, has a post-doc to do most of the work! Phil Gans, late of Stanford, has a new faculty position at the UCSB. Phil is mostly an argon rat and will be setting up a new argon lab, but maybe he'll find time to keep his hands in his very interesting fission track work (done in collaboration with Rod Brown of La Trobe).

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Ph.D. from Melbourne (1989), postdoc at La Trobe, currently postdoc at Stanford; designed, set up, and managed new Stanford fission track lab; good computer programmer; credible zeta; likes Stanford very much but is ever on the look out for a good permanent job!
T. Dumitru, Stanford Geology, Stanford CA 94305.