On Track

The Newsletter of the International Fission-Track Community

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☞ *Special Issue:*

30 Years of Fission-Track Dating

Edited by *Rasoul B. Sorkhabi*
Arizona State University

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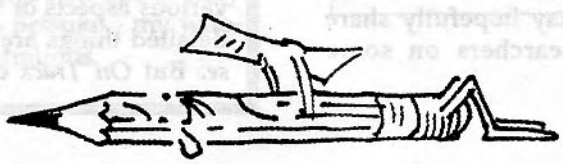

Editor's Notes

As promised in the last issue of *On Track*, the present issue is special one celebrating the 30th anniversary of fission-track dating. In 1963, P. Buford Price and Robert M. Walker, physicists at the General Electric Laboratory in Schenectady, New York, published a paper in *Journal of Geophysical Research* in which they described observation and counting of spontaneous fission tracks in mica, the first age equation for fission-track dating, and implications of their results for dating other minerals. Three physicists are usually recognized as the founding fathers of our field: Robert L. Fleischer, P. Buford Price, and Robert M. Walker (cited alphabetically as agreed by the authors; see the preface in *Nuclear Tracks in Solids*, 1975). Fission-track dating, which grew out of the Solid State Nuclear Track Detectors (SSNTD), has witnessed remarkable advances in the past three decades. These advances were made possible due to pioneering efforts of many committed individuals and some strong research groups, which means that future advances in our field will depend on the strength of research groups (strength in many aspects: ideas, instruments, finance) and facilitating the way for new talent, young energy, and new words. All these can be better achieved in an atmosphere of friendship, cooperation, support and respect for each other, resolving our problems with scientific spirit and constructive criticism.

In preparing this issue of *On Track*, apart from my general request in the editorial note of the November 92 issue, I also sent out about two dozens of letters to some peers and pioneers of the FT field, inviting them to contribute to this special issue. (There may still be some persons I have been unable to contact due to my ignorance *not* negligence. I hope they will accept my sincere apology for this.) The contributions that I received have been printed in this issue. Those who were unable to send a piece may hopefully share their stories with their fellow-researchers on some other occasions.

FT dating is increasingly finding its place in university text books of geology and geochronology. In *Nuclear Tracks in Solids* published in 1975, Fleischer, Price and Walker cited 274 papers in the chapter on fission-track dating, while Gunther Wagner and Peter van den Haute in *Fission-Track Dating* (1992) cite about 700 papers (a figure I heard from Dr. Wagner at the Philadelphia Workshop). These developments are exciting and promising.

History of fission-track dating may be divisible into three decades and three "fission-track generations" - i.e. those of 1963-1972; 1973-1982; and 1983-1992. And starting with this year, a fourth generation is rising. FT dating as we know today is (and has to be) different from that of the 1960s because it has developed over all those years. But the differences between the techniques of each period should *not* be translated into my-daddy-can-beat-your-daddy-type of debate. "Generation gaps" are natural but they should be filled with respect and mutual understanding. That a person from the third FT generation has initiated the idea for this special issue of *On Track* hopefully expresses our appreciation for all those scientists who have paved the way for us.

In our field (as in any other scientific field), there are many problems to be solved, and many questions to be answered satisfactorily. This is of course natural. Problem-solving is the nature of any science. However, I have a feeling (I repeat *a feeling* that I will be glad to be proven as an illusion) that some problems have also become subjective dilemmas among our community. For example, contradictory comments (most probably from fission-trackers) are given to proposals (not necessarily mine). Mr. X proposes a geological study using apatite; it is criticized as to why zircon was not included. Mr. X resubmits it including zircon this time, it is again criticized but on the account that closure temperature in zircon is not precisely known and it will be a waste of tax-payers money to do it. Proposals are also killed for using or not using this or that thermal history model, for microprobe analyzing or not analyzing of this or that element in apatite, and so on and so forth. Perhaps our problems should be defined better - their nature, limits, and implications or irrelevance for the objectives of a certain study. Problems can be solved scientifically; constructive criticism should be welcome. Perhaps we had better accelerate friendly communications between labs, groups, and individuals, and come up with a clear and healthier ground to continue fission-track research more efficiently.

Toward this end, I would like to see more pages of *On Track* be devoted to single topics presenting various aspects or views of a particular problem. Many detailed things are usually not mentioned in papers *per se*. But *On Track* can provide such an opportunity. I

hope that future editors of our newsletter will pay attention to this suggestion.

There is a Japanese saying that flowers on hills other than yours look more reddish. Faced with some uncertainties, one may feel that things are more rosy in other dating techniques. But I don't think it is true. If one pinpoints the assumptions and uncertainties in other dating techniques, one realizes that fission-track dating is as established as any of them - of course with its own scope, limitations, applications and its unique characteristics.

On the 50th or 100th anniversary of FT dating, our future colleagues will probably know much more than we do now about the FT field. But they will appreciate our efforts more if today we continue fission-track studies with scientific spirit and friendship.

With this issue of *On Track* my term of editorship ends. And I am both happy and unhappy about this: Happy because I will be able to devote more of my time to research and other obligations; moreover, I am certain that the next editor of *On Track*, Dennis Arne of the Dalhousie University, will do a better job. Unhappy because I will miss talking to you, writing to you, reading your letters and manuscripts. It has been a pleasure to serve as the editor of *On Track*. Over the past one year, I have tried my best to produce a useful, informative, and interesting newsletter for our community. And now the torch is passed to Dennis. Let's cooperate with him to upgrade *On Track* and keep it a success. I would like to take this opportunity to thank those who have contributed to this and previous issues of *On Track*, and I hope you will enjoy reading them. ❑

Rasoul Sorkhabi

Acknowledgment

I would like to thank Prof. Edmund Stump, Chairman of Geology Department at Arizona State University, for his generous support of *On Track* during my editorship. Funding the printing cost of this and last issue of *On Track* was kindly made by him, as well as the correction of the English of certain parts of the articles. Weekly meetings of our fission-track group - comprising of Paul Fitzgerald, Tim Redfield, Annia Fayon, Ed Stump, and myself - have been fun and fruitful in reading new papers and discussing problems and developments. Amy Slaven kindly (and I hope cheerfully as she always is) typed several articles of this issue. Sue Selkirk has always assisted me with the computer work of figures and photos, including some of those in *On Track*. Conversations with Trevor Dumitru have been helpful in preparing and upgrading the newsletter. And the peace of mind that one usually need for this kind of work, I owe to Setsuko - my wife- who has also contributed some illustrations.

CALL FOR CONTRIBUTIONS FOR *On Track* issue 7 (November 1993)

Dear Fellow Fission Tracker:

The next issue will come out in November 1993 and **we're looking for contributions.**

We welcome contributions of virtually any kind, including 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, and descriptions of what you are doing in your research. If you want to contribute, send me the final text to arrive by the **NOV. 1, 1993 DEADLINE** at the latest (if it's a substantial article, let me know the title and length ASAP). It is hoped to actually have the issue ready for printing by 15 November, so please really try to make the deadline!

If possible, please send both one paper copy of your contribution and a **Macintosh DISC** (3.5 inch size) with the text saved in Microsoft Word. If you can't send a Mac disc, send an IBM compatible disc (3.5 inch size) in Word, or WordPerfect.

On Track includes a **LIST OF RECENT AND FORTHCOMING FISSION TRACK PAPERS**. If you know of a paper that was published recently or that is in press and should appear in the near future, please send it in. Just send your contributions on paper or send a copy of the first page of the paper.

On Track is also happy to run **ADVERTISEMENTS**. There's no charge for line ads by non-commercial entities (e.g., universities). Please contact the editor for advertising rates for commercial ads (e.g., by equipment dealers).

On Track will remain free, at least for the near future. However, to save costs we generally mail only one copy per lab so please be sure to pass *On Track* around your lab. Personal copies and back issues of *On Track* are U$2 per issue. Donation is welcome.

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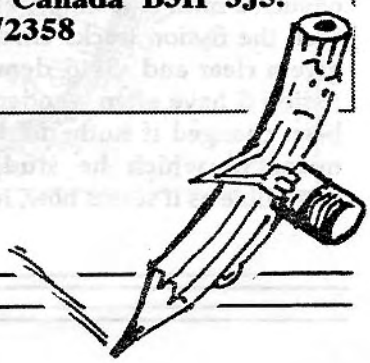

Maxims for Success in Doing Research: Lessons from the Early Days of Track Studies in Minerals

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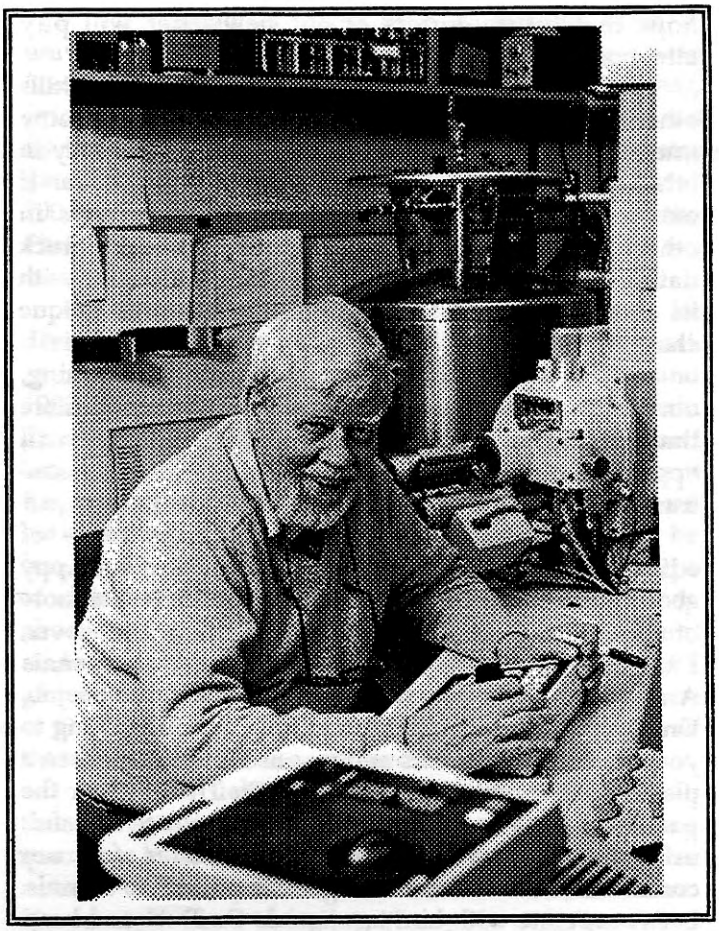
In August 1992, I gave an invited talk on the discovery of tracks in meteorites at a symposium on the History of Meteoritics. The afternoon session where the talk was presented was one of many at the International Geological Congress in Kyoto, Japan. Since work on tracks in meteorites is inextricably intertwined with the discovery of the fission track dating method, I was delighted to discover that there was a morning session devoted to fission track dating being held in the same (small) room where I was later to present my talk! I therefore seized the opportunity to advertise my meteorite talk and was delighted to see many of the new, as well as old, practitioners of the art of fission track dating at the afternoon session.

The talk itself used some of the more amusing anecdotes in the early history of the field to illustrate and develop some general rules for how to succeed in research. No one should be surprised to learn that the first rule is to be lucky. An equally important one is to choose your co–workers wisely!

Another rule that I proposed says, "It is often better to *do* an experiment than to discuss why the experiment is not worth doing." The anecdote I chose to illustrate this rule concerned the initial discovery of fossil fission tracks in the transmission electron microscope and the subsequent, and later, discovery of fossil fission tracks in the optical microscope.

Readers should appreciate the fact that fossil fission tracks were sought for by Buford Price and myself not for their intrinsic interest but rather as a means to the end of demonstrating that cosmic ray tracks in lunar rocks would be stable over long periods of time (if indeed they could be formed in the first place—which was by no means certain at the time). I was aware of the phenomenon of pleochroic haloes and obtained some samples of mica containing the appropriate radioactive inclusions from a geologist at Cal Tech during a visit to the west coast.

Everyone now knows that if one takes a sample of such a mica and simply etches it for a sufficient time, then the fission tracks emanating from the inclusions give a clear and vivid demonstration of spontaneous fission (I have often wondered how history might have been changed if Rutherford had happened to etch the micas in which he studied pleochroic haloes). Incredible as it seems now, it was *not* obvious that we



should etch the samples for a long enough time to see tracks in the optical microscope. You see at that time we had seen tracks only in the transmission electron microscope!

With the self–imposed ground rule that it was necessary to use a transmission electron microscope to see tracks, finding the first fossil tracks proved to be a formidable task. What had to be done was to find a pleochroic halo, carefully remove the radioactive inclusion, then cleave the mica in proximity to the hole to a thickness of approximately 1000 Å, lightly etch in acid and finally locate the etched region in the electron microscope.

This task was well beyond my feeble experimental talents and Bill Johnston of the GE research staff (later our boss) was recruited to help. Johnston had a reputation as a superb experimentalist acquired, in part, for his elegant studies of dislocations where he had used etch pits as a method of following dislocation motions in crystals in response to applied stress. For all that I know, and now that I think about it—very likely—it was probably the prior work of Johnston and his colleague Jack Gilman on dislocation etch pits that led Price and myself to the idea of track etching in the first place! I really don't remember how Bill got involved in this work since I know that Buford Price could certainly have pulled it off. Probably

Buford was busy working on some other aspect of tracks and we simply sought out Bill to get more accomplished per unit time.

Whatever the reason, Bill succeeded in finding the first fossil fission tracks in the electron microscope. The results were published in *Nature* (volume 196, pp. 732–734, 1962). This article was the first to propose the possibility of using spontaneous fission tracks as a geochronometer and could be considered as the real discovery paper, not the later more theoretical paper that I wrote while on sabbatical at the University of Paris. The discovery of optically visible tracks is also alluded to in a "note added in proof" in the *Nature* paper.

However, the issue is not clear since the results reported in the *Nature* paper clearly did not constitute a working, practical dating scheme—no one in their right mind would attempt to actually date something by cleaving mica in the vicinity of radioactive inclusions. Indeed, Bill Johnston was so scornful of the idea that he actually withdrew his name from the article and he appears only as an acknowledgment, "for helpful suggestions," at the end of the paper.

Now to get finally to the point. During the time Bill was doing his work I remember vividly a long and heated discussion with him in which I suggested that we etch for a longer time and then look in the optical microscope to see if we could see fission tracks. Bill was the world's expert on dislocation etch pits and had spent countless hours etching crystals and studying etch figures in the optical microscope. As a consequence, he was able to convince me that the experiment was not worth doing, that the very best we could accomplish would be to reveal a multitude of background pits from ever–present dislocations! The argument went on for far longer than it would have taken to simply do the experiment. In fact it was not tried then, and tracks were not seen in the optical microscope until some months later and then almost by accident—but that is another anecdote with its own lesson. The moral is clear: IT IS OFTEN BETTER TO *DO* AN EXPERIMENT THAN TO DISCUSS WHY THE EXPERIMENT IS NOT WORTH DOING. There is another lesson as well: one can simply know too much (that isn't so) for ones own good.

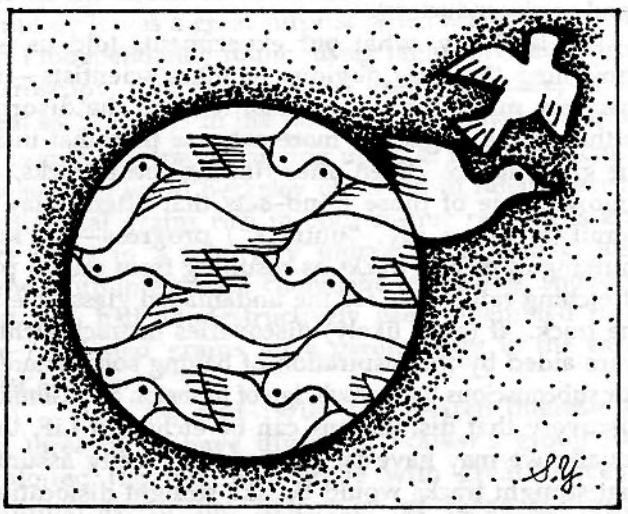
There is another anecdote that repeats the lesson at my expense. While I was in Paris on sabbatical leave, I got a phone call from Bob Fleischer in which he told me that he had succeeded in revealing etched tracks in glass. I told him forcefully that he must be mistaken. After all, glass was a totally disordered substance to begin with and the passage of a charged particle could do nothing to change the state of disorder. Bob had the right response—he simply said, "try it." I did and soon called him back to apologize for my misplaced skepticism.

The full set of rules proposed at Kyoto for how to achieve success in research are as follows:

1) One must be lucky
2) A little bit of ignorance and a lot of naiveté can be very useful.
3) It is often better to do an experiment than to discuss why the experiment is not worth doing.
4) Where possible, quantitative estimates of probable results should be made. However, this rule should not interfere with rule three above.
5) Be acutely aware of the miserable fact that you tend to see only what you are looking for.
6) Choose your co–workers wisely.

Although I illustrated these rules with anecdotes from the early days of track research, I could equally well have chosen different stories from other parts of my career to make the same points. And, although one might have hoped differently, I must confess that the accumulated wisdom represented by these rules doesn't help one whit in deciding what to do tomorrow on the problem at hand. I must also tell you with great sadness that I have found that I, for one, can never really learn the rules—I have to continually relearn them through new experiences.

I would be remiss if I failed to point out that the rules, or at least the ideas inherent in the rules, apply equally to groups as well as to individuals. Take rule 3, for example, as it applies to the deliberations of review panels. I never cease to be amazed at the prescience of panels who *know* that a new line of investigation is doomed to failure or will be substantially without interest *before* an experiment or new idea can be tried. Such panels clearly save the government a lot of money by rejecting proposals that would otherwise open up entirely new fields of research. Given the way research funding is apparently headed this may be a useful social activity. ❑



Remembrances of Tracks Past

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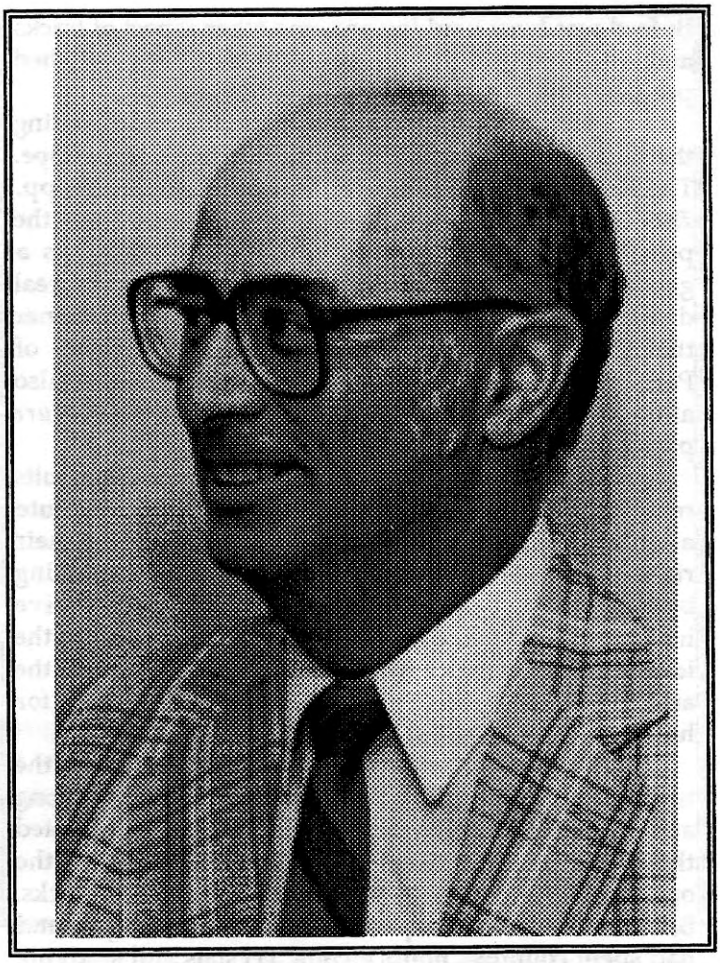


My beginnings with nuclear tracks, and Buford Price and Bob Walker, were a rare treat. It was a truly unique time when things went right repeatedly and a time for learning about what for me were fascinating new areas in which I had never imagined being a participant. There was also the special charm of etched tracks, which at first viewing were simplicity personified—but whose subtleties and reactions to their environment even now continue to provide new adventures and undiminished intellectual stimulation.

Perhaps nine of ten ideas in science go nowhere—because if they are good ideas, other workers turn out to have recognized them first. And other ideas perish because our models and nature's reality just don't agree. In contrast, the recognition that tracks exist and can easily be revealed suddenly provided qualitatively new tools for visualizing and measuring nuclear reactions, nuclide distributions of solids, and doses of neutrons and heavy nuclei. The detectors—the tools—were themselves of immense variety; documenting what substances did and did not register tracks was interesting for its own sake and vital to constructing models of how tracks form.

In spite of Walker's gracious comments, my finding that tracks can form in glasses and plastics was not primarily due to my dogged, bullheaded willingness to ignore our initial assumptions that tracks in crystals were glass-like. The fact is that Price had smuggled in from Berkeley a weak Cf-252 source. Its convenience made testing for tracks almost easier than debating them. And, as just noted, to understand how tracks form we needed to know not only where they do form (insulators), but also where they don't (metals and good semi-conductors).

Basically, what our experiments told us was something that was obvious to glass scientists—that there are many degrees of disorder, and the disorder within tracks in glass is more extreme than that inside the glass matrix. Even after finding these tracks, we followed one of those mind-sets that often delay or derail (or as we say, "untrack") progress—we kept thinking of etched tracks as resulting from only a pair of etching rates—one for the undamaged glass, one for the track. If (as is likely) discoveries in track etching were aided by the inspiration of having somewhere in our subconscious the knowledge of Johnston and Gilman's discovery that dislocations can be etched in LiF, then equally we may have tacitly and incorrectly assumed that straight tracks would be like straight dislocations in having constant energy per unit length. Happily, in time we realized that etching rates parallel the extent of the damage that is produced by ionization. And that effect led in turn to a major new facet in the use of tracks—identification of the nuclear species of individual particles, for example, cosmic rays or particles that are released by nuclear reactions.

For me the ultimate fascination that drew me into the earth sciences has been the finding of natural tracks—their relative simplicity on earth and then their diversity in extraterrestrial materials. In particular, the identification by Price and Walker of U-238 spontaneous fission tracks in micas and their proposal for the use of fission tracks in dating led to my entry into some of the initial tests of track dating and the disputatious world of tektites. Tektites are simply read recorders of fission tracks, and the observed ages that agreed with K-Ar results gave us confidence that track dating was indeed timely. The rapid recognition that tracks can exist in many minerals and observations of their array of temperatures for track fading told us that thermal effects in the past would be measurable. But we hardly visualized how that possibility would come to fruition.

Here was another case of our slowness to recognize the importance of a clue. We did notice and

remark that sometimes after annealing a tektite, the tracks were altered and less crisp in appearance. But the decisive, important step was Storzer and Wagner's. They recognized that track diameters were lowered, and showed experimentally that the shrinkage led to track density and track diameters obeying a relation that allows spuriously lowered track ages to be corrected to give consistent ages. And that was the start of the road to the marvelous quantitative interpretations of thermal histories that now abound.

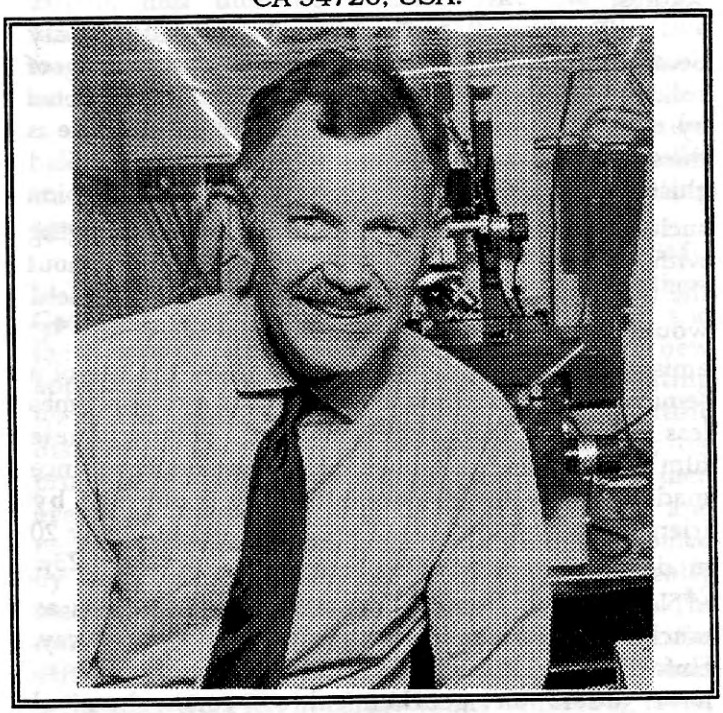
I mentioned that tektites have been the subject of much wrangling. This situation became apparent to me early on at the first scientific meeting at which I presented dating results, the second International Symposium on Tektites at Pittsburgh in 1963. Here, people with chemistry–related backgrounds (mostly) favored terrestrial origins, and workers with physics or aerodynamics training (mostly) leaned toward the moon. The bizarre and memorable low point of this affair was a poll at the end of the meeting in which the moon defeated the earth 21 to 18. That margin would not be convincing at a football game; it expresses the state of unanimity that existed. This subject remains as one of unresolved, polarized disputes.

Mostly the striking new results of early track work breezed through journal review systems. One exception was our work with Louis Leakey on dating of Bed I at Olduvai Gorge. As the cover photograph in *Science* (where it was finally published, 2 April 1965) shows, tracks in the pumice that was used were hardly obvious even to those familiar with tracks in glass; and major corrections were necessary to allow for empty space where tracks were not to be observed. At Leakey's urging we submitted the article to *Nature*. We thought that the article might encounter questions about track identification and track–density corrections, but what I suspect we brushed into was a reaction from an archaeologist who intuitively disagreed with the (to him) great K–Ar age of 1.75 My and our confirmation of it. I remind the reader that Price, Walker, and I were at that time part of a wide ranging unit at General Electric that went by the not very descriptive name Metallurgy and Ceramics Department. Without explanation the editor returned the manuscript and suggested that we submit the article to a metallurgy journal. We decided that, as far as that editor knew, *Science* was such a journal.

The collaboration that the three of us have had has been a fruitful adventure. Yet we are pleased to note that we benefited from many associates and contributors. A recent list showed 86 co–authors of papers on nuclear tracks and specific acknowledgments of assistance from 228 people. We owe them an immense debt. ❑

On Beyond Fission Track Dating

P. Buford Price
Dept. of Physics, University of California, Berkeley, CA 94720, USA.



Rather than reminisce about our early work at General Electric, I thought it would be interesting to write about a few of the later studies with particle tracks. By 1969 Walker and I had moved to St. Louis and to Berkeley, just when samples from the first Apollo mission were about to become available. For the next three years Fleischer and we saw each other regularly at Lunar Science Conferences, where we reported on the astonishingly rich fossil record of ancient radiation in lunar rocks and dust. Ironically, transmission electron microscopy, used in the initial discovery of fossil fission tracks, played an important role in the much later analysis of extra–terrestrial samples. It was a great surprise when Michel Maurette and I independently found, using high–voltage electron microscopy, that micron–sized grains from depths as great as 10 meters in the lunar soil contained densities of cosmic–ray tracks ranging up to at least 10^{11} cm^{-2}. Phenomena we at Berkeley observed in lunar breccias (individual grains rich in both cosmic ray tracks and rare gases in a relatively unirradiated matrix) also showed up in gas–rich chondritic meteorites, showing that they had nondestructively been assembled from pre–irradiated grains on a parent body in the early solar system.

Since the mid–1970s the research interests of the three of us have diverged: Walker's group has exploited the ion microprobe with great success, culminating in the discovery of large isotopic

anomalies in meteorites; Fleischer and his colleagues have used alpha–sensitive plastic track detectors to trace radon and its movement in soil; and I have used track–recording solids mainly in nuclear and cosmic ray studies.

Readers of *On Track* may be interested in my occasional use of the track–recording properties of solids to search for hypothetical phenomena predicted by nuclear or particle physicists. My first example is the study of a new mode of spontaneous decay called cluster radioactivity, in which a monoenergetic ion such as ^{14}C tunnels out of a heavy nucleus, emerging with a kinetic energy of a few tens of MeV. Without the use of nuclear track–recording solids, the field would have died after the 1984 discovery of ^{14}C emission by ^{223}Ra, which was found by the use of a semiconductor telescope to occur about a billion times less frequently than alpha emission. Polycarbonate film detectors and phosphate glass detectors have since made possible the discovery (by my group and by friendly competitors at Dubna in Russia) of nearly 20 modes of decay, including emission of ^{14}C, ^{22}O, ^{23}F, ^{24}Ne, ^{28}Mg, ^{32}Si and ^{34}Si, with branching ratios as much as 1016 times lower than that for alpha decay. Unfortunately, these decay modes are far too rare to have observable geochemical or cosmochemical consequences.

My other examples deal with searches for new kinds of fossil tracks in natural micas. Gauge theories of unification of the fundamental forces led in the 1980s to the prediction that magnetic monopoles with mass as large as 10^{16} proton masses should exist and might be raining down on earth with speeds of a few hundred km sec^{-1}. These so–called supermassive monopoles would produce almost no ionization, would probably penetrate most if not all of the earth, and would thus have escaped detection by previous techniques. Although we found no monopole tracks in mica, the search had interesting consequences. Our limit on the flux was low enough to rule out the hypothesis that monopoles with certain properties comprise a significant contribution to the dark matter of the universe. Along the way, we discovered that, in addition to natural tracks of spontaneous fission and of recoiling nuclei emitted in alpha decay, a third type of fossil track exists in micas. These tracks are caused by nuclear reactions of some of the alpha particles with aluminum and silicon nuclei in the mica. Because of the Coulomb barrier, only the alphas of highest energy in the uranium and thorium decay chain can undergo reactions. The tracks, which have a range of ~1 micron, are produced by compound nuclei of alpha + Al or Si. They can be seen by any of you who take the trouble to partially cleave a mica crystal, etch it, reassemble it, and then scan it while focusing on the partially cleaved layer. The fission tracks are 20 μm long when the lengths of the two portions are added, whereas the alpha–interaction tracks are about 1 μm long.

My last example is our current search for WIMPs (= Weakly Interacting Massive Particles), believed to make up the dark matter that comprises more than 90% of the universe. WIMPS, being neutral, are invisible and can be detected only when they collide elastically with an atom in a solid. Their mass is believed to be between about 10 and 10^4 proton masses, their velocity is a few hundred km sec^{-1}, and they will penetrate a few tens of km of rock. Being weakly interacting, they collide very rarely, and when they do, the recoiling atom travels only a few tens or hundreds of atom distances. At first glance, such a particle would appear impossible to detect.

Stored tracks in mica may provide the key, provided we can distinguish a WIMP recoil from an ordinary alpha–recoil track, which also travels only a hundred atom distances or so. Prospects would be hopeless if it were not for the recently invented technique of Atomic Force Microscopy, which enables us to measure the depths of etched recoil tracks with an accuracy of about one Angstrom. Our accelerator calibrations indicate that we may be able to distinguish recoils of atoms in the U and Th decay chain from recoils of intermediate–mass atoms such as iron in the mica structure. A 1-cm^2 crystal of mica with an age of 10^9 years has advantages over the best electronic detectors in the search for WIMP recoils, and we are giving this search our best shot.

I now invite the reader to share with me in thinking of applications of the AFM technique to the study of fossil tracks in minerals. We know from an electron microscopic study carried out in Bob Walker's laboratory in 1969 that alpha–recoil tracks can be seen (but not quantitatively measured) in albite, so AFM applications need not be restricted to mica. Probably the greatest uncertainty is whether a section of a track–recording mineral can be polished with a mean roughness smaller than a few tens of nanometers without erasing the alpha–recoil tracks. If so, one might use the spatial resolution of AFM to study alpha–recoil tracks in micron–sized polycrystalline samples such as apatite–bearing teeth or bones and might be able to date young archaeological materials and young volcanics.

Last week, while I was working with my post–doc at the AFM, a graduate student from the Department of Soil Science who was watching us asked me my name. We introduced ourselves and she told me about her work with fission track dating. I asked her if she knew who invented the technique. She said "No." My post–doc and I laughed. The moral of that story is that, with students around, a professor has to keep hustling to maintain his self–respect. ❑

Early Recollections of Fission–Track Dating

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My first encounter with fission–track dating occurred in the spring of 1964, while I was finishing my master's degree at Dartmouth College. Henry Faul, my advisor to be at Southern Methodist University, wrote to me and said, "You are going to spend the summer at the General Electric Research Laboratory in Schenectady learning fission–track dating." Not knowing anything about fission–track dating, I went to the fountain of all knowledge, my professors, and asked, "What is fission–track dating?" It seems as if they knew about as much as I did, nothing or a little less. I soon learned of two abstracts presented at the 1963 Geological Society of America Meeting in New York City. One was presented by Buford Price, along with co-authors Bob Fleischer, Don Miller and Ernie Symes, on "Fossil charged–particle tracks and mineral ages." The following talk was presented by Bob Fleischer with Buford Price on "Tektite ages by fission track dating." I must confess that I attended this meeting, but I did not attend the aforementioned talks. Henry did attend the talks by Price and Fleischer, and he recognized the potential. Henry later recounted to me an incident that occurred at the Price and Fleischer talks that would continue to occur with fission–track presentations for many years to come. The speaker before Buford Price was Pat Hurley (MIT) talking on Rb–Sr ages from the Sierra Nevada Batholith. The room was full for Hurley's talk, but Henry was one of the few left in the room when the fission–track talks started. Looking at the program, competing with Buford was a talk by Heezen and Tharp on "Ocean ridges, transcurrent faults and continental displacements." That was heavy competition for a new dating technique.

I spent the summer of 1964 working at GE with Bob Fleischer. It was a wonderful experience. By the end of that summer I knew that there were a lot of potential applications to the geological sciences for fission–track dating. That fall I started my Ph.D. program at Southern Methodist. Henry was an incredibly bright individual, but there are certain advantages when the student knows more about his thesis topic than his advisor. This was a case of the poorly sighted student leading the almost blind professor. There was a lot to do—specifically, techniques had to be developed that permitted the routine dating of the small crystals that geologists routinely encounter in rocks. Splitting a small crystal with a chisel (á la Buford Price) did not appeal to me. It was amazing to watch Buford sitting at a stereoscope with a small zircon (100–200 μm long), a chisel, and a small box with a hole in it. The zircon was in the bottom of the box, the chisel went through the hole in the box and rested on the long axis of the zircon, and then he would strike the chisel. Miraculously, he always seemed to recover the two fragments, which were then etched in hot phosphoric acid. There had to be a better way!

At this same time I was developing techniques for apatite and sphene, Gunther Wagner was on a parallel track at Heidelberg developing techniques for dating apatite and glass.

I went directly to the Geological Survey from SMU in 1967. The USGS was an exciting place to be then. Geologists were interested in fission–track dating, and there was no shortage of new projects and new applications. The early days of fission–track dating were fun and exciting. There were scientific disagreements among the various laboratories; the principal one was over the decay constant. Even then everyone recognized that the decay constant and neutron dosimetry tied together, and the ages obtained by the various laboratories were geologically meaningful even if different constants were used. The neutron dose calibration compensated for the differences in decay constant. This was a friendly time; differences were left in the laboratory or meeting room, and after a heated discussion everyone went out for a beer.

Musings on the present and the future

Unfortunately, there still aren't many hard–money fission–track positions in this country or any other country, and it seems that as a fission–track community we are making it very difficult for each other to obtain grant money. Having been involved in the review process for many years, I have seen, particularly in recent years, many excellent proposals that have been short–circuited by other fission trackers. In talking to many of you privately, I am bothered by the fact that it appears that some reviewers of fission–track proposals to NSF and PRF denigrate other researchers needlessly. This hurts the entire fission–track field, not just the person who wrote the proposal. All of the major laboratories in this country are turning out quality data. Their techniques may be different, but one thing that I have learned over the years is that there are many ways to determine a fission–track age. The use of oil versus dry objectives is a good example of a non–issue that has been blown entirely out of proportion. There are, of course, a few procedures that are incorrect for theoretical reasons, but most work very well, and using ZETA, many differences in technique are compensated for in the calibration. If you question someone's techniques, it had better be for sound reasons and not just because they do things differently than you do. Stop slamming each other and start working together. The researcher you trash will probably

return the favor. If the proposal is scientifically sound, support it on the science. Fission–track proposals aren't competing against each other, they are competing against everything else. United we can survive; divided there won't ever be any new hard–money fission–track positions.

There are a lot of us running around and collecting samples now. I strongly suggest that you do more communicating among yourselves before starting a project to avoid duplication of effort. If someone else is/was working in the area, don't ignore their work. Use it as a foundation and cite it. ❑

Memories from Early Years of Fission-Track Dating

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Thirty years of FT dating! For me, it means that: (1) If I redetermine the age of a sample analyzed 30 years ago, it should yield a 30-year older age although I will not be able to detect it; (2) If I compare a photograph of mine taken in 1963 with my present appearance, the elapsed 30 years will be evident.

Whereas (1) is related to FT statistics, (2) certainly is a different reality, as 30 years account for about 60 per cent of my life, and I cannot obtain FT ages younger than 100 a with an uncertainty of less than 60 percent. (Well, Gunther Wagner was able to do it on his uranium-loaded test tube of 1942!)

I was a student in 1964 when I was assigned to write a thesis at the Institute of Physics, University of Pisa. I was sent to Professor Cesar M.G. Lattes, then a famous particle physicist and an author of celebrated papers on mesons. He was very enthusiastic about the papers published in that year by R.L. Fleischer, P.B. Price, and R.M. Walker on the new Solid State Nuclear Track Detectors: the idea that minerals could store tracks produced by nuclear events that occurred during the early history of the solar system was very interesting for him, who was patiently waiting for long time expositions in a cosmic ray laboratory on Chacaltaya mountain in the Bolivian Andes, near La Paz.

He was able to communicate his enthusiasm to me in a few minutes. Cesar Lattes, a professor at Sao Paulo University (Brazil), was in Italy with a one year grant, and wanted to mount a track laboratory at Pisa. We started with muscovite purchased in an electric store. As the rooms for our laboratory were not ready yet, we revealed our first tracks by using a bowl of HF acid fluctuating in hot water in the wash basin of the bathroom in his house. We were so excited when in a few minutes we could observe our first fossil tracks under an old Italian Koritzka microscope! No doubt, we had breathed HF fumes and our temperature control was rather poor, but we had in our sight the record of nuclear events that occurred in far past times.

The next step was to irradiate our sample. Five research reactors were operating in Italy at that time (at present only one is active!). We performed the first irradiation in a reactor near Turin. Just 15 days later, we manipulated our micas and revealed our first induced tracks in a Rhodesian mica, and using the neutron fluence reported by the staff of the reactor, we computed our first FT age, which agreed reasonably with the K-Ar and Rb-Sr ages determined at the Institute of Nuclear Geology, University of Pisa. Thus, we thought (rather optimistically) that the FT age was concordant with the mean of K-Ar and Rb-Sr ages. We had the first age standard. It seemed to be so easy ... we had to just solve some minor problems.

Were the tracks preserved without any alteration over geological time? If not, what was the effect of such changes on the computed age? Was actually the "Italian" decay constant of uranium-238 measured by Emilio Segre in 1952 accurate? But these "details" required many years of research, and they have not been resolved satisfactorily yet.

In those days, I did not understand what the tracks wanted to communicate to me. No donut, they contained a lot of information about their history, but I persisted in trying to cancel this information, and to force them to correct their erroneous etching efficiency and to disclose the "true" formation age of the rock. I did not want a thermometer; I wanted a clock!

I met Robert M. Walker during a conference in Florence in 1966, and I showed to him my results of track length measurement and age corrections in muscovite. He was so encouraging to me. Some months later, I met Don Miller, who was visiting our lab with his family. In those days, contacts among fission trackers were sporadic: We were relegated to small sections of large conferences. In addition, traveling was not as usual as nowadays. I remember my first exciting meetings in the early 70s with G. Wagner and D. Storzer (both long-haired at that time), J. Burchart, D.E. Fisher, B.S. Carpenter, U.K. Hack, G.M. Reimer, S.A. Durrani, G. Poupeau, P. Pellas, and C.W. Naeser. It was in a small bar called "The Refectory" in Snowmass-at-Aspen (Colorado) that some of the mentioned colleagues probably remember (especially those who did not drink too much) and in a certain night of August 78 that the present FT community was shaped. That night (during the fourth ICOG), a small group of fission-trackers discussed the necessity of organizing special meetings - hence the International Fission-Track Dating Workshop.

To conclude my reflections on the past three decades of FT dating, I would like to greet our young colleagues (I remember Robert Walker saying during

the Fifth ICOG in Nikko in 1982 that he was very impressed by the new generation of FT researchers.) They did not experience those stirring pioneering years of FT dating and rather found an established community with its celebrated peers; nevertheless, they are so enthusiastic and very sharp in understanding where the problems lie. ❑

Dating MORB by Fission Tracks: The Sands of Time

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When I discovered, twenty-five years ago, that oceanic basalts trapped their preeruptive radiogenic Ar-40 to an extent that overwhelmed the posteruptive stuff, it meant that normal K/Ar ages on such samples were not possible [Fisher et al., Earth Plan. Sci. Lett. 5: 95-100, 1968.]. I looked around frantically for another way to date these samples, and luckily, in the very next volume of *Science* the FPW group published the first study of fission track dating of oceanic glasses [Fleischer et al., *Science* 161, 339, 1968]. I read it quickly, and remembered a talk given by Buford Price a couple of years earlier when I was at Cornell, the gist of which was that anyone could measure fission tracks if he had a simple microscope, a bottle of HF, and a little patience.

I had the microscope and the HF, and a very little patience, which was soon sorely tried. The first thing I did was spill a tiny drop of HF on my finger. It itched a little, and I washed it off. The next morning it was festering, and I went to the infirmary. "Uh huh," the doctor explained when I told him what I had done. "We'll keep an eye on it."

"What does that mean?" I asked.

"You come in every day and we'll clean it out and see how deep the hole grows. If it reaches the bone we'll have to take off the finger. If it stops before then, you'll be okay."

I thought he was kidding, but he wasn't. Luckily the hole stopped growing in less than a week, and the finger remained.

My temper did not. The first group of "tracks" I measured in the oceanic glasses turned out to be bubbles, the second group consisted of dislocations. I still remember my joy when I found my first real fossil track (after visiting Schenectady and learning the tricks of the trade from Bob Fleischer). It was a beautiful thing which I still sometimes dream about. It was also one of the last tracks I ever saw in a MORB glass.

It turned out that typical MORB (Mid-Ocean Ridge Basalts) had uranium concentrations in the ppb region, which meant that they would produce a measurable density of tracks in roughly 20 Myr [Fisher, *Nature* 221: 549, 1969], while the age region of interest in MORB genesis turned out to be <1 Myr.

This is the first consideration that dissuaded people from fission track dating of MORB. It cannot be avoided by going to longer counting times or larger samples, since track-like non-track artifacts in the glasses provide a baseline of spurious counts; as the true density of spontaneous tracks decreases, these artifacts come to dominate the track count and lead to unavoidably erroneous ages. Early fission track dating of oceanic glasses gave some apparently reliable results [Fleischer et al., 1968; Aumento, *Rad. Effects* 11: 193; 1971], but the combination of low U contents and low ages, together with the possibility of track annealing, led to a pessimistic forecast for the utilization of this technique, and in fact virtually to its abandonment.

These problems, however, are not necessarily present in all oceanic basalts. There are a few samples of interest with ages up to tens of millions of years which still contain fresh glass. In addition, several samples have alkalic geochemical tendencies, with uranium contents enriched over the normal MORB concentrations by orders of magnitude. And although most ocean basalts have cooled too rapidly to allow growth of large crystals, some samples do contain minerals with U contents higher than the groundmass average, and some of these have excellent properties for fission track dating. Obviously, these are the materials of choice for FT dating. For example, apatite from Kodiak Seamount and Giacomini Guyot was fission track dated by Naeser [*Science* 182: 579; 1973]. The most-used minerals are zircon, sphene, and apatite, and occasionally in seamounts these may be obtained in large enough sizes, i.e., >100 microns. Although these minerals do not often occur in such sizes in seamounts, sphene crystals up to two millimeters large in some have been found (D. Clague, private communication); it may also be possible to separate apatite and perhaps zircon from the groundmass.

There remains the problem of track annealing. The first studies of fission track dating in MORB samples found that this was not serious: the first paper [Fleischer et al., 1968] indicated that significant track fading at ocean temperatures would not be seen in less than ten billion years. Similar track annealing characteristics were determined for other MORB by Aumento [1971]. I found much greater annealing but still less than 20% loss at 65 Myr [*JGR* 90: 1801; 1985].

But other work indicated a more serious problem. Melson [*EOS* 1011; 1973] found ages lower than the estimated age of basement and of overlying sediments, leading him to suggest that "the calculated ages are subject to revision." MacDougall [*EOS* 987; 1973] found that annealing should be substantial for ages >1 Myr, even at ambient ocean water

temperatures. It is now known that glasses of different chemistries will have different degrees of resistance to track annealing [Lakatos et al, *EPSL* 14: 128; 1972]. Therefore each sample must should be analyzed for track annealing properties before a dating experiment is attempted.

Although there are not many oceanic basalts which have the characteristics necessary for successful FT dating, the set of such samples is not totally empty. For example, there are a series of seamounts from offshore Southern California which might be used to test predictions of a model describing the tectonic history of this region proposed by Lonsdale [D. Clague, private communication]. The samples are from seamounts on crustal segments estimated by Lonsdale to range from 18 to 30 Myr. The K_2O concentrations in these samples range up to ~3%, and so there should be enough uranium to give a sufficient spontaneous fission track density.

There is little reason to expect that these samples may be reliably dated by variations of the K/Ar technique, since they are submarine erupted and the glass rims are therefore likely to contain inherited or excess radiogenic Ar, and the crystalline rocks are altered. The age determinations would be important evidence bearing on the proposed history of the region and on the geophysical model described.

BUT.... The fission track dating of MORB is not fundable. It was proposed twice to NSF and rejected both times, though the reviewers' reports were favorable. The reason is simple, and echoes Bob Walker's conclusion: the funding panels simply "know" that some research will fail. And meanwhile the problem of determining the ages of MORB continues. According to a recent Workshop convened by JOI to discuss the dating of MORB:

"Ideally, one would like to have at least two techniques which could be used independently to date MORB glass...(but)...At present there are no techniques available for reliably dating young MORB..."

Oh well. Way back a hundred years or so, nearly as soon as I found my first fossil tracks in MORB I ran out excitedly and wrote presciently [Fisher, *New Scientist*, 10/23/69]:

"In order to test sea-floor spreading we must date the lava flows which form the actual sea-floor bottom....The problem of dating them correctly remains, and fission-track analysis seems to be the most likely method."

But you know how it is:

"Wise men come, ever promising
The secrets of MORB to know;
Wise men come, and over the sands,
The flatulent sands
Of time
They go..."❑

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Fission-Track Dating

by **Günther Wagner**, *Forschungsstelle Archäometrie der Heidelberger Akademie der Wissenschaften am Max-Planck-Institute für Kernphysik, Heidelberg, Germany; and* **Peter Van den haute**, *Geologisch Instituut, Rijks Universiteit Gent, Belgium*

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Fission-Track Dating is the first text to deal entirely with fission track dating. The book covers all aspects of uranium fission-tracks and their use as a tool for dating terrestrial materials. Topics discussed include: the origin of fission tracks, track etching and fading, dating techniques, practical procedures, and a look at the most recent applications in geology and archaeology.

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Poisson Probabilities and Age Limits

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In "A Message from Alaska" (*On Track*, May 1992), "Tracker" wrote an amusing article in which the meaning of single grain ages when there are no fossil tracks (N_S=0) was discussed. Tracker has shown that N_S=0 is information useful for estimating confidence intervals (and then upper limits) for an FT age. Also, single grain "zero ages" can be put in a radial plot.

The problem of zero (or small non-zero) counts is common specially when very young material is analyzed (for example in Quaternary research or archaeometry). In addition, "very young sample" is frequently combined with "very small sample", for example in case of tephras from deep holes or of archaeological materials.

How much useful can an age affected by a large experimental error - or an upper (lower) age limit - be depends on the problems related with a given sample. Oftentimes the question is: "Are the FT data consistent with this hypothesis ?", instead of "What is the age of this sample?". For example, sometimes archaeologists want to know whether a given handmade object is original or fake. They are not interested in the precision of the FT age, because they know very well the age of that object, *if* it is original. Therefore, FT dating is requested in this case as "authenticity test". Usually, the sample is a small piece of glass, with an age of few centuries or a few millennia (obviously, it one cannot destroy an archaeological find to prove its authenticity: usually a small shard is available for analyses) and as additional problem, with a small uranium content. In this case "N_S very small" is the rule. On the contrary, the induced track density is generally determined on a separate split: usually $N_I \gg N_S$.

As is common in glass, the uranium content does not show inhomogeneities (this can be tested by checking the induced track distribution). In this case, Poisson probabilities can be helpful to indicate whether FT results support or do not support a given hypothesis.

Let me illustrate this with an example. Let's suppose that we observe 1 spontaneous track (N_S=1) in 1 cm^2 (ρ_S = 1 track/cm^2) of a Roman glass, and, after irradiation with a fluence of 10^{15} n/cm^2, an induced track density ρ_I=15000 tracks/cm^2. Is the glass original? The Poisson probability of finding "at least 1 track" can be computed by:

$$P(N_S \text{ at least } 1) = 1 - P(N_S=0) = 1 - e^{-N}$$

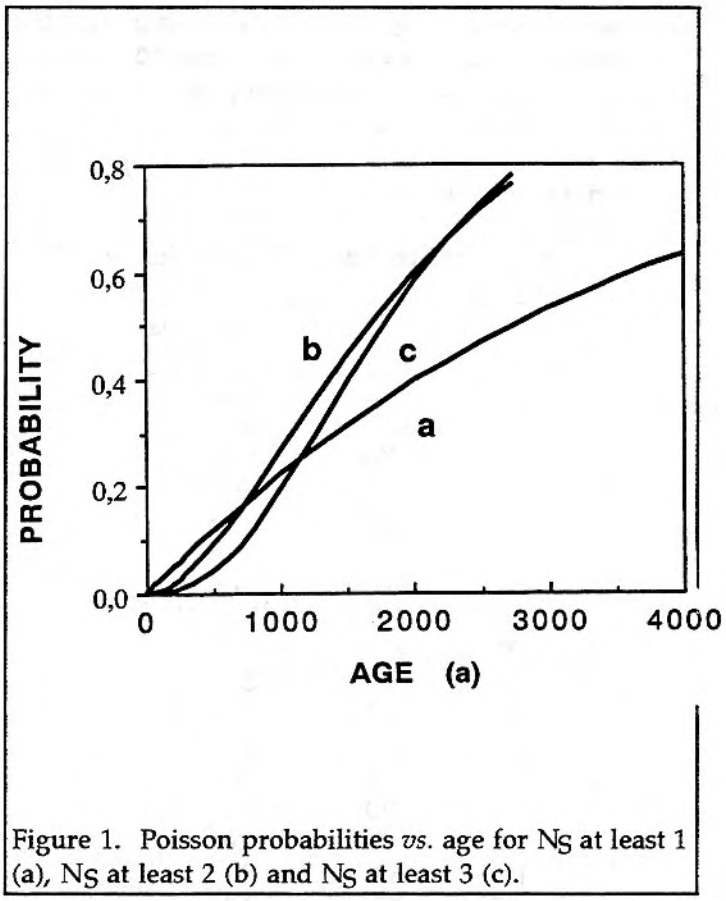

Figure 1. Poisson probabilities *vs.* age for N_S at least 1 (a), N_S at least 2 (b) and N_S at least 3 (c).

for several N values - N is the variable spontaneous track density ρ_S. Moreover, each ρ_S value corresponds to a certain ρ_S/ρ_I ratio, and therefore to the age T. Consequently, a curve P(N_S at least 1) *vs.* T can be plotted (Fig. 1, curve a). The probability of finding at least 1 track in 1 cm^2 is lower than 5% for 200 a, lower than 2.5% for 100 a, and finally lower than 1% for 40 a. It means that very probably the glass is older than 10 years, and consequently original. However, "very probably" does not means "certainly".

If 2 or more tracks are observed, the curves Poisson probability *vs.* age become more significant. The curves b and c in Fig. 1 have been plotted assuming N_S=2 for 4 cm^2 and N_S=3 for 6 cm^2, respectively. (In these cases, ρ_S/ρ_I corresponds to an age of ~2000 a, i.e. a Roman age.) The following equations were used:

$$P(N_S \text{ at least } 2) = 1 - P(N_S = 0 \text{ or } 1)$$
$$P(N_S \text{ at least } 3) = 1 - P(N_S = 0 \text{ or } 1 \text{ or } 2)$$

For curves b and c the probability becomes < 1% for ages lower than 150 a and 300 a, respectively. Thus the hypothesis "not original" can be reasonably excluded.

This example shows a real case. Only 1 track was observed after repolishing the same surface four times. The above curves were used in order to make a

decision. N_S=2 or N_S=3 could discriminate the hypotheses of "~2000 a old" or "less than 10 a old" in better way; however, considering the remaining available surface (at most 1 cm^2 more) and the time needed for further repolishing and counting phases, it was decided to stop the analysis.

Table 1 - 95% confidence limit for the age

N_S	N_I	Age - Ma*	Age - Ma$^+$
0	1	863.9	752.0
0	2	441.0	387.0
0	3	290.2	260.5
0	4	214.9	196.4
0	6	140.7	131.6
0	8	104.3	98.9
0	10	82.8	79.3
0	15	54.6	53.0
0	20	40.7	39.8
0	30	27.0	26.5
0	40	20.2	19.9
0	50	16.1	15.9
0	60	13.4	13.3
0	80	10.0	10.0
0	100	8.0	8.0
0	150	5.3	5.3
0	200	4.0	4.0
0	300	2.7	2.7
0	500	1.6	1.6
0	1000	0.8	0.8

* from Tracker (1992); $^+$: this work.

What about upper age limits? For N_S=0, assuming $P(N_S=0) = e^{-N} = .05$, the corresponding N (2.9957) can be easily computed. By the age equation, I obtained the values shown in Table 1 for some N_S and N_I counts considered by Tracker. The upper age limits computed by Poisson probabilities fail for large ages - let say, larger than ~ 100 Ma.

Table 2 presents the upper limits (95% and 99%) computed using Poisson probabilities for the above example (ρ_I=15000 tracks/cm^2 for a fluence of 10^{15} n/cm^2) for different areas with 0 tracks and at most 1 track. The Poisson probabilities $P(N_S=0)$ and $P(N_S=1)$ for an age of 2000 a and an age of 10 a are also shown. By these data, the surface needed for discriminating the two hypotheses "~ 2000 a old" and "less than 10 a old" - can be estimated.

In this example, ~ 10 cm^2 of surface with no tracks are needed to reasonably exclude the hypothesis "original".

Table 2 - Age limits and Poisson probabilities for different areas.

Area	N_S=0 95%	N_S=0 99%	N_S=0 or 1 95%	N_S=0 or 1 99%	$P(N_S=0)$ 2000 a %	$P(N_S=0)$ 10 a %	$P(N_S=1)$ 2000 a %	$P(N_S=1)$ 10 a %
1 cm^2	11900	18400	18900	26700	60.0	99.7	30.7	0.25
2 cm^2	6000	9200	9500	13400	36.7	99.5	36.8	0.50
3 cm^2	4000	6100	6300	8900	22.2	99.2	33.4	0.75
4 cm^2	3000	4600	4700	6700	13.5	99.0	27.0	1.0
5 cm^2	2400	3700	3800	5300	8.2	98.8	20.4	1.2
6 cm^2	2000	3100	3160	4500	4.9	98.5	14.9	1.5
10 cm^2	1200	1800	1900	2700	0.70	97.5	3.3	2.4
15 cm^2	800	1200	1300	1800	0.05	96.3	0.41	3.6

In conclusion, the above calculations based on Poisson probabilities provide a tool for weighing the pros and cons of analyses in case of very low ρ_S densities. The parameters to be considered for judging the convenience of further work on a given sample are: availability of surface, time needed for counting, tracks already counted and further surface needed for discriminating different hypotheses. ❑

On The Thermochronological Significance Of Semi-Track Lengths

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In the past decade, length distributions of subhorizontal confined tracks in apatite have become an important tool for thermal history analysis. The lengths of tracks intersecting a polished plane (so-called surface tracks or semi-tracks) have also been used (MÄRK et al., 1973; WAGNER & HEJL, 1991; GRIVET et al., 1993) but the consistency of these approaches remains still controversial.

When a population of latent tracks with identical lengths is etched on an internal surface of a thick crystal, the projected lengths of the revealed surface tracks yield a well known triangular frequency distribution (DAKOWSKI, 1978). In order to predict such a frequency distribution for a population of latent tracks with varying lengths, each subpopulation must be weighed according to its areal density on the etched surface. This areal density P [cm^{-2}] is proportional to the spatial density N_f [cm^{-3}] of the subpopulation and to the etchable length L [cm] of its individual tracks.

$$P = N_f L K \quad (1)$$

Because of the influence of the bulk etching rate the constant K can be different from the theoretical value of 1/2 which is valid for internal surfaces

(WAGNER & VAN DEN HAUTE, 1992). According to equation (1), the new models for the analysis of projected track-length distributions (WAGNER & HEJL, 1991; GRIVET et al., 1993) are based on the assumption that the areal density of any sub-population of tracks (with the same age and thermal history) is proportional to the etchable length of the corresponding latent tracks in space. This assumption seems to be self-evident, but is it true? As we will see, the term "etchable length of a latent volume-track" is not precise enough and can give rise to confusion.

Recent investigations of CARLSON (1990) and PAUL & FITZGERALD (1992) have shown that track fragmentation plays an important role in the annealing process. The kinetic model of CARLSON (1990) postulates a radial shrinkage of the damaged zone surrounding the path of the fission fragments, in response to the elimination of crystalline defects by short-range atomic repositioning. The initial disrupted zone of a latent fission-track is approximated by a central cylindrical part and regions of conical taper at both ends (Fig. 1, stage 1). As a consequence of the radial shrinkage, the track shortens systematically from its tips inwards to about two thirds of its original length (stages 2 and 3). Further annealing often produces unetchable gaps and results in a random segmentation (stages 4 and 5). Finally, the track fragments disappear or become so small that they cannot be identified by etching and optical observation (stage 6). From stage 4 to stage 6, the term "etchable track-length" would be misleading. Therefore, we should distinguish between an etchable fragment-length (EFL) and the etchable bulk-length (EBL) of a latent track.

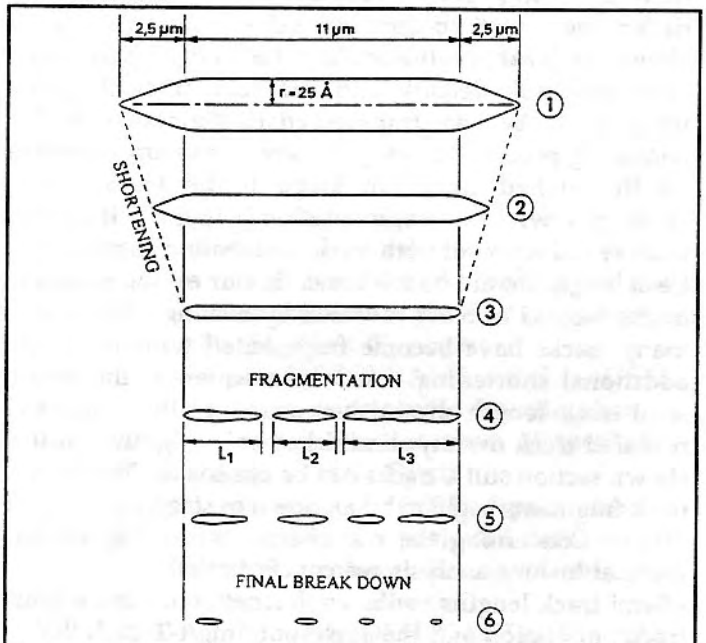

Figure 1. Shrinkage of the damaged zone of a fission-track in the course of annealing. The vertical (radial) scale is enlarged 400 times compared to the horizontal (longitudinal) scale.

For the example of Fig. 1 (stage 4), we can write

$$EBL = L_1 + L_2 + L_3 \qquad (2)$$

and

$$\text{mean EFL} = (L_1 + L_2 + L_3)/3 \qquad (3)$$

For any population of latent tracks in space with n original tracks and f etchable fragments of varying length L_i we define

$$\text{mean EBL} = \overline{EBL} = \frac{\sum_{i=1}^{i=f} L_i}{n} \qquad (4)$$

and

$$\text{mean EFL} = \overline{EFL} = \frac{\sum_{i=1}^{i=f} L_i}{n} \qquad (5)$$

For a population of fresh or slightly shortened tracks $n = f$ and $\overline{EBL} = \overline{EFL}$. After the onset of fragmentation f will exceed n. From $f > n$ follows that $\overline{EFL} < \overline{EBL}$, the latter being proportional to the areal track density. As a consequence, the relative decrease of the mean EFL will exceed the relative decrease of the areal track density. L_o being the initial track length and P_o the initial areal density of the population, we can write

$$(L_o - \overline{EFL})/L_o > (P_o - N_f\overline{EBL}.K)/P_o \qquad (6)$$

Therefore the curves for the growing of the apparent cut-off ages A2, B2 and C2 given in WAGNER and HEJL (1991) and in WAGNER and VAN DEN HAUTE (1992) are not realistic. For the corresponding t-T-paths they yield an overestimation of the expected ages, i.e. an overestimation of the effective track-retention temperatures.

In order to solve this problem, it would be desirable to know the relationship between the mean EFL and the mean EBL for any stage of annealing. Unfortunately, a simple algorithm is not available and the frequency distribution of etchable fragment lengths in a given volume cannot be evaluated directly. For many reasons, which have been discussed in detail by GREEN (1988), the measured length distributions of subhorizontal confined tracks etched either through host tracks or fissures are biased towards the longer tracks - especially in highly annealed samples. The probability distribution of these track lengths does neither reflect the relative frequencies of EBLs nor the relative frequencies of EFLs when the areal reduction of

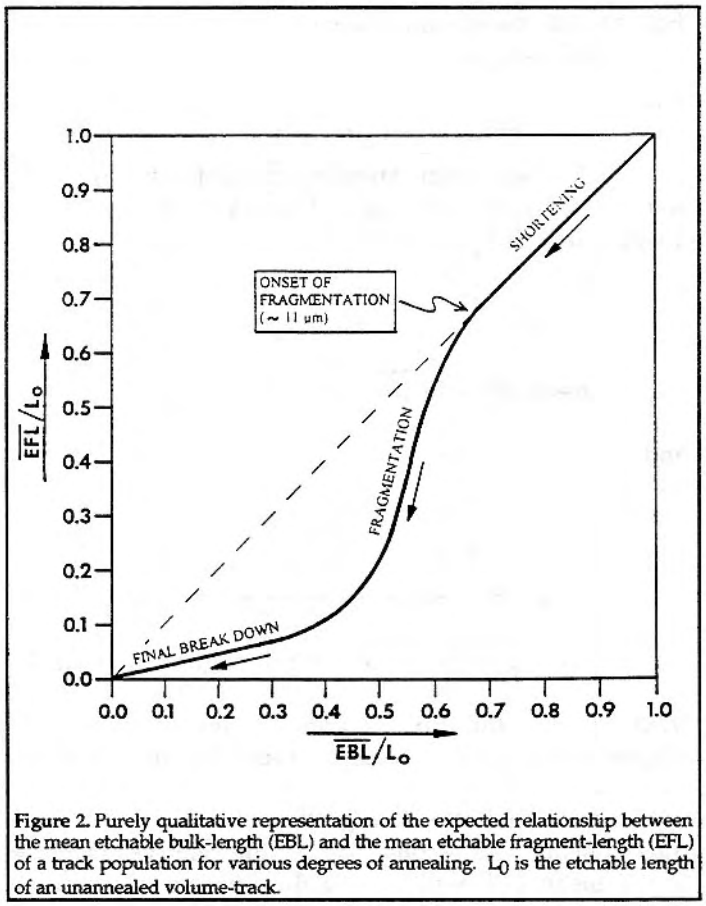

Figure 2. Purely qualitative representation of the expected relationship between the mean etchable bulk-length (EBL) and the mean etchable fragment-length (EFL) of a track population for various degrees of annealing. L_0 is the etchable length of an unannealed volume-track.

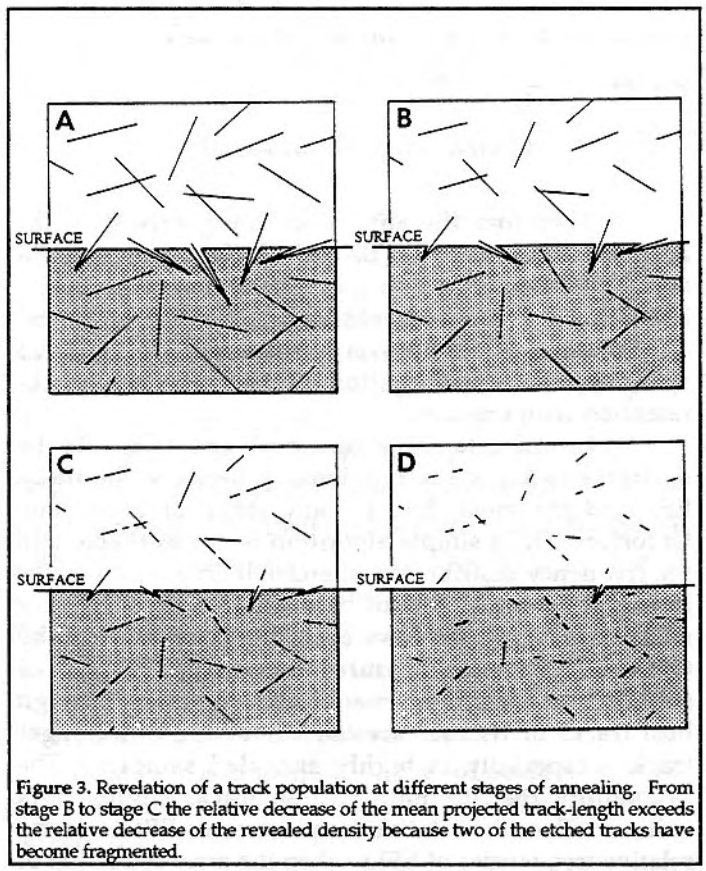

Figure 3. Revelation of a track population at different stages of annealing. From stage B to stage C the relative decrease of the mean projected track-length exceeds the relative decrease of the revealed density because two of the etched tracks have become fragmented.

the track density exceeds 30%. Be as it may, we can try to imagine qualitatively what could happen with the mean EFL and the mean EBL in the course of annealing. The 3-stage model of Fig. 2 is highly speculative; it is not based on measurements. A population of fresh tracks plots in the right upper corner and evolves in the direction of the arrows when it becomes progressively annealed. If we consider a sample that has cooled continuously through the whole partial annealing zone, the older subpopulations of tracks plot on the segments 3 and 2 ("final break down" and "fragmentation") while the young sub-populations of tracks plot on the segment 1 ("shortening"). The conventional interpretation of cut-off ages (WAGNER & HEJL, 1991) can be true only for cut-off lengths corresponding to the stage of simple shortening.

Let us imagine a two-dimensional universe containing some (of course) two-dimensional apatites. Such a universe has the main advantage that its microscopic civilizations cannot be crunched by a clumsy three-dimensional fission-track man, because they cannot be more than flat (remember the terrible accident in the last issue of *On Track*, page 11). Another advantage is that the two-dimensional geometry of track registration on an etched line (not plane) is easier to show by drawings. In the two-dimensional case, the high angles of incidence (for example between 80 and 90°) have the same probability than the low angles of incidence (between 0 and 10). Please note that this probability distribution for the angles of incidence is not the same as for an etched plane in a tree-dimensional space because a track has more possibilities of being parallel to a plane than of being perpendicular to it. Beside this difference, our two-dimensional example of Fig. 3 shows at least qualitatively what happens for the revealed track density and the semi-track lengths when tracks become fragmented in the course of the annealing process. In stage A five tracks are revealed on the etched line. In stage B the tracks have shortened without fragmentation. In principle, the relative reduction of both track density and mean semi-track length should be the same. In our example, only 4 tracks instead of 5 are revealed by etching. In stage C many tracks have become fragmented without much additional shortening. As a consequence, the mean semi-track length diminishes dramatically while the revealed track density diminishes only slightly. In the shown section still 4 tracks can be observed. Finally the track fragments begin to break down in stage D.

Concerning the use of semi-track lengths for thermal history analysis we conclude that:

- Semi-track lengths contain information on the whole track population and the corresponding t-T-path that a sample has experienced within the partial annealing zone. They are particularly promising for the analysis

of short tracks which cannot be properly revealed as subhorizontal confined tracks.
- In any deductive model calculation, a clear distinction between etchable fragment lengths and etchable bulk lengths of latent tracks has to be made in order to avoid confusion.
- Recently published models for the interpretation of semi-track lengths do not consider the important process of track fragmentation. Therefore, they yield an unrealistic prediction of time-dependent areal track accumulation and are not recommended for uncritical application.
- For any future investigation concerning the relationship between semi-track length and areal track densities, a more empirical approach is highly desirable.

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Some Thoughts on "Correcting" Fission Track Ages in Apatite

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One topic of discussion in many fission track labs likely to be controversial is that of "correcting" fission track ages in apatite using the relationship between mean track length shortening and fission track age reduction in apatite defined by Green (1988). Not only is there some uncertainty regarding when such corrections are appropriate, there is some confusion as to what value to "correct" to. The present discourse is offered as an attempt to either clarify the approach, or to confuse the reader further, and owes much to discussions with Paul Green and Ian Duddy over the years.

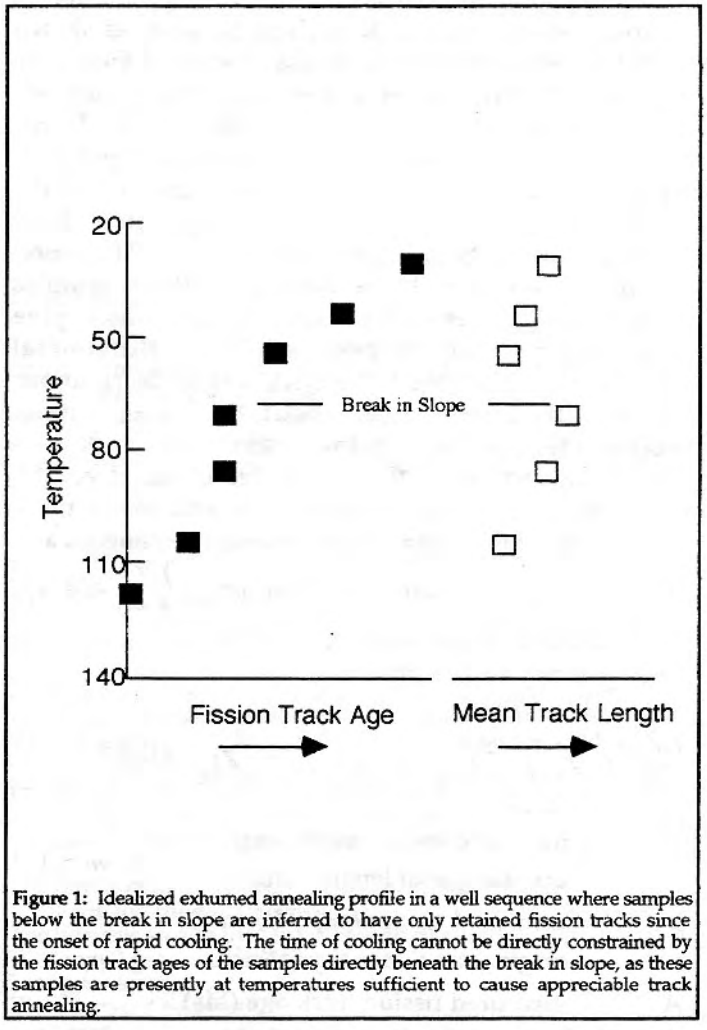

Figure 1: Idealized exhumed annealing profile in a well sequence where samples below the break in slope are inferred to have only retained fission tracks since the onset of rapid cooling. The time of cooling cannot be directly constrained by the fission track ages of the samples directly beneath the break in slope, as these samples are presently at temperatures sufficient to cause appreciable track annealing.

Ultimately, most of us would prefer to work with apatite samples giving mean lengths greater than 14 μm, thereby reducing the amount of effort required to

interpret thermal histories. Unfortunately, in well profiles from sedimentary basins, those samples in which fission tracks were totally annealed prior to some particular cooling event may presently reside at temperatures sufficient to cause appreciable track annealing. If we examine a classic exhumed annealing profile (Figure 1), the best estimate for the time of cooling is obtained for those samples directly beneath the break in slope. What we would like to know for these samples that have cooled from the total track annealing zone, is when track retention began. Provided these samples cooled quickly to their present day temperature or below (allowing for possible re-heating), the vast majority of tracks accumulated since the onset of track retention should be uniformly shortened in response to their present day temperature, ignoring for the time being the possible effects of composition in different apatite grains. Of course, if cooling was gradual, our classic break in slope would not exist.

Caution must be used in applying a correction to samples beneath the break in slope because, as shown by the two deepest samples, tracks in some of these may be severely annealed at present day temperatures. These samples can be identified by their greater degree of age reduction, as well as by a significant spread in fission track ages between individual grains (due to the effects of composition on annealing rates) and relatively broad track length distributions. Therefore, correction should only be applied to those samples beneath a well-defined break in slope which give pooled ages (i.e. no obvious effects of differential annealing) and in which the track length distributions are roughly symmetrical about the mean, albeit shortened to some degree below 14 μm.

Correction of these samples is based on the relationship between track density and mean track length defined by Green (1988) for both spontaneous and induced tracks in apatite, such that for $L/L_O \geq 0.65$, the relationship between mean length and track density is approximately 1:1

Therefore,

$$L/L_O = \rho/\rho_O = A/A_O \qquad for\ L/L_O \geq 0.65$$

Where

L = measured mean length (μm)
L_O = original mean length (μm)
ρ = measured track density (t/cm^2)
ρ_O = original track density (t/cm^2)
A = measured fission track age (Ma)
A_O = original fission track age (Ma)

Another significant conclusion from Green (1988) is that spontaneous and induced tracks in apatite appear to behave in a similar fashion. However, this leads to the question as to whether to correct to an average mean length of ~14.5 μm, representative of mean lengths commonly measured for spontaneous tracks in age standards, or whether to correct to a mean length of ~16 μm, representative of mean lengths measured for induced tracks. What this means in terms of age standards is that apatite fission track ages determined by absolute methods should *not* be concordant with the age determined by more stable radiometric techniques. If it is accepted that fission tracks start with a length similar to that observed in induced tracks, that is ~16 μm, then even fission track ages for age standards calculated by the absolute method require length correction.

For example, if we take the case of Durango apatite and use the mean spontaneous and induced track lengths from Green (1988), we can calculate what the "absolute" apatite fission track age should be.

$14.24/15.91 = A/31.4$ and $A = 28.1$ Ma

A similar exercise can be carried out for Fish Canyon Tuff (25.8 Ma) and Mt. Dromedary (90.5 Ma), indicating a systematic difference of ~-10%.

However, one of the many advantages to the zeta calibration method is that, since the apatite fission track age has been arbitrarily set concordant with the "true" age of the standard, a fission track age calculated using the zeta approach already contains an intrinsic length correction. Therefore, to correct a measured fission track age to an induced mean track length of ~16 μm would actually result in an over-estimation of the "true" age of the unknown by ~10%. Thus as long as the zeta calibration method is adopted, unknown fission track ages should only be corrected to a mean length of ~14.5 μm, where appropriate.

To correct fission track ages determined by the zeta approach to the induced mean length of ~16 μm would require a reduction in either the zeta (see discussion in Green 1988) or the fission track age of the standard by ~10%. While this approach is (excuse me) more "correct" in an absolute sense, it would give the same result as correcting to 14.5 μm where the fission track ages of standards are assumed to be concordant to the "true" age of the standard (i.e. the conventional zeta calibration approach), and therefore would seem to be a lot of trouble to go to in order to get to the same point, the onset of track retention. Furthermore, since mean lengths measured for both spontaneous and induced tracks for age standards vary slightly between analysts, such a "length correction" would have to be done on an individual basis to be rigorous.

Thus, for corrected ages, the following approach is recommended.

$$T_C = \frac{T_M}{L/L_O}$$ Where

T_C = the corrected fission track age
T_M = the measured pooled fission track age
L = the observed mean track length
L_O = the original mean track length (~14.5 μm)

Note that a fission track age "corrected" in this fashion is not only subject to the uncertainties typically associated with measured ages, it is also subject to the uncertainty associated with the mean track length determination. The resultant value should therefore be recognized for the approximation that it is.

As examples, this approach has been used in the Canning Basin, Australia (Arne et al. 1989) and in the Taranaki Basin, New Zealand (Kamp and Green 1990).

For those samples that have undergone more complex cooling histories or which were re-heated at some time following the initial onset of track retention, the correction procedure described above is not strictly valid. In these instances an estimate of the time that track retention began requires predictive modeling of the observed fission track parameters. Such procedures will also necessarily result in a range of estimates for the onset of track retention. Given these uncertainties, the only wise thing to do is collect vertical profiles in the Transantarctic Mountains!

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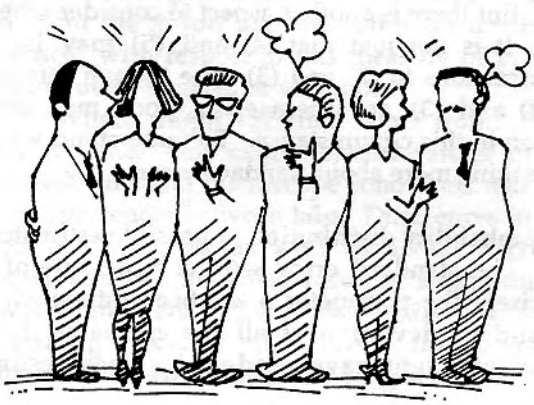


Trials and Errors

by *Tracker*
(Your man at the microscope, with the Fish Canyon T-shirt and the Durango jeans)

Recent disclosures concerning bears and Byzantine coins have prompted several letters from readers:

Grand Canyon Hotel,
Denver, Colorado.

Dear Tracker,
My wife and I have been trying to reduce our errors for ages, but we find that the older we get the larger they become. Is this inevitable?
Yours, CW.
P.S. Nancy says that our *relative* errors have gone down a bit, though.

Oasis Bar,
Surfers' Beach, California.

Dear Tracker,
I have discovered that I can reduce my standard errors by dating younger things. Unfortunately, this seems to lead to large relative errors. Yesterday I dated a groovy piece and got an age of 0.00003 Ma with a relative standard error of 4000%. Is this a record?
Yours, Trevor.

Windsor Castle,
Windsor, Victoria.

Dear Tracker,
In your otherwise admirable explanation of Manuel's Law (*On Track* 2, ii) it can't have escaped your attention that there is one feature that does not ring true. Not only was the silver content reduced in the later minting, but also the relative standard deviation went down. As every analyst knows, when the true value goes down the variation goes down but the *relative* variation tends to go up. What is going on? I think we should be told.
Carry on, G.

The Hat Shop,
Grizzly Mountain, Alaska

Sir,
In a recent issue of your tracking magazine you allude to bears that leave no tracks in the absence of snow, a conjecture whose authenticity is questionable to say the least. As one with no little experience of the heavier thick-furred plantigrade quadrupeds I submit that evidence of their proximity can usually be inferred from a rudimentary knowledge of the geological environment in a variety of climatic conditions. To quote a bard from an earlier age:
"...Farewell!
The day frowns more and more: thou art like to have
A lullaby too rough. I never saw
The heavens so dim by day. A savage clamour!
Well may I get aboard! This is the chase:
I am gone forever.
[*Exit, pursued by a bear.*]"
Furthermore the giant panda is never seen in these parts--what sort of tracker are you?
I remain, yours etc., D. Crockett.

The last correspondent is clearly receiving *On Track* in error, probably as a free offer, and is best ignored. The first three, though, have put their collective finger on a useful point. A relative standard error, being a ratio of the standard error to the value of the quantity being estimated, will tend, other things being equal, to be small when the true quantity is large and large when the true quantity is small. Of course this is not the full story. In some experimental procedures the absolute standard error is naturally dependent on the value of the quantity being estimated while the relative standard error is not, or not strongly so at least. This is the case for fission track ages, as observed by CW.

A fiscal mystery

Our correspondent G is referring to the following data from the reign of Manuel I:

Silver content x (pph)

	mean	s.d.	s.d./mean
First coinage	6.74	0.543	0.081
Fourth coinage	5.61	0.363	0.065

pointing out that while the mean and standard deviation both went down, so did the relative standard deviation, from 8.1% to 6.5%. How did this happen? My theory is this: Manuel needed to produce more coins but did not wish to use more silver than before, so what did he do? He added more alloy. Let us look not at the silver content of each coin but at its *alloy* content—the number of parts alloy per part of silver:

Alloy content 100/x

	mean	s.d.	s.d/mean
First coinage	14.91	1.20	0.081
Fourth coinage	17.88	1.15	0.064

Now the mean alloy content has gone up, the s.d. is much the same (surely evidence that we are on track) and the relative standard deviation has gone down, in accordance with G's desire.

Indeed the relative s.d.'s are practically the same as before—and a moment's thought tells you that this must be so: by Manuel's Law the relative s.d. equals (to a close approximation) the s.d. of the logs; and furthermore the s.d. of log(100/x) must equal the s.d. of log(x). Manuel's Law is powerful indeed.

A Poisson situation

But let us take a simpler situation: estimating a track density. Consider a count N that is a random quantity with expected value $A\rho$, where A is known (for example a known area of crystal surface) and ρ is an unknown density to be estimated. Suppose N has a Poisson distribution. (Today we will leave aside the question of why we suppose this—contrary to popular belief it has little to do with deaths from horse kicks.) Then the variance of N is $A\rho$ equal to the mean. The standard deviation is therefore

$$\sqrt{A\rho}$$

and the relative standard deviation (or coefficient of variation) is

$$\sqrt{A\rho}/A\rho = 1\sqrt{A\rho}$$

Suppose we observe N = r and we wish to estimate r. The natural estimate is

$$\rho = r/A \qquad (1)$$

which has standard error

$$se(\rho) = \sqrt{A\rho}/A = \sqrt{\rho}/A \qquad (2)$$

and relative standard error

$$se(\rho)/\rho = 1/\sqrt{A\rho} \qquad (3)$$

Notice that if the density ρ is fixed and the area A is large then both the standard error (2) and relative standard error (3) will be small. If the expected count $A\rho$ is large then the relative standard error (3) will be small, though the absolute standard error (2) may not be. However, if ρ is small the standard error (2) will be small but the relative standard error (3) will be large—simply because it is relative to something small. In the limit as ρ tends to zero, the absolute standard error tends to zero but the relative standard error tends to infinity. One consequence of this is that it may not matter if a relative standard error is large if the quantity being estimated is small. To see how good your estimate is, you may need to express its precision more directly—possibly as a confidence interval.

An approximate standard error

Now (2) and (3) depend on r which is unknown, so we can't calculate them. (And if we knew r we wouldn't want to calculate them as we wouldn't need to estimate r, an irony familiar to statistics students long before Yossarian's famous Catch 22.) In order to get an idea of the precision it is standard practice to substitute the estimate r/A in place of ρ, to get approximate values.

$$se(\rho) \approx \sqrt{r}/A \qquad (4)$$

and

$$se(\rho)/\rho \approx 1\sqrt{r} \qquad (5)$$

Remember that (4) and (5) are approximate. Furthermore they are likely to be poor approximations when r is small. In particular it often happens that r = 0 even though ρ is not zero.

But there is another aspect to consider when r is small. It is not just that (4) and (5) may be poor approximations to (2) and (3), true though this is, but also (2) and (3) are themselves poor measures of precision in this circumstance. To understand why, we need to think more about standard errors.

A probability distribution of possible estimates

The standard error se(ρ) is a measure of how imprecisely the parameter ρ has been estimated. It is the standard deviation of all the estimates that the experiment might have produced. These estimates

have a probability distribution derived from the Poisson distribution—i.e. the distribution of N/A, where N has a Poisson distribution. If the standard error is small, the distribution of estimates is closely concentrated around the true value; but the usefulness of a standard error as a measure of precision also depends on the shape of this distribution.

If $A\rho$ is large enough, the distribution of possible estimates is approximately Normal with mean ρ and standard deviation se(ρ). Then, for example, $\rho \pm 2$se(ρ) is an approximate 95% confidence interval for ρ.

But if $A\rho$ is not large, the distribution of possible estimates is *not* approximately Normal. Then se(ρ) is not a particularly useful measure of precision, and $\rho \pm 2$se(ρ) does not provide an approximate 95% confidence interval—indeed it may provide nonsense. This applies to (2), and therefore to (3), when r is small. There are better things to do in these circumstances, such as transforming to a different scale or using confidence intervals directly.

A mean length

What about trials? Before the 1992 Philadelphia Workshop, aliquots of apatite samples were sent to several labs for analysis, both for dating and for making length measurements; and in several of the labs the samples were analysed by more than one analyst. For the present purpose let us regard this simple as an exercise in estimating the *mean length* of tracks in the original apatite sample. And let us suppose that everyone is trying to estimate the same thing. In practice, people measured lengths of horizontal confined tracks according to their normal procedure, uncorrected for length biased sampling.

The first graph is a radial plot of mean length estimates by 20 analysts from 7 labs for one of the apatite samples. For each analyst, the estimate is the mean of the length measurements made and the standard error is their standard deviation divided by the square root of the number of tracks. Thus if an analyst measured 100 tracks having mean length 14 microns and standard deviation 1 micron, the standard error used on the radial plot would be 0.1 microns (and the point would plot close to that by analyst 11). It is clear from the graph that the estimates are not all in agreement with respect to this measure of precision—there are other sources of variation.

Where might variation come from? You will easily tell me. For example, differences in etchant used, etch time and microscope conditions will result in length differences between labs. Differences in the way tracks are selected, as well as actual differences between sub-samples, may result in length differences between analysts. Even two different analysts measuring exactly the same tracks may get systematically different mean lengths. (One person I know claims to have better eyesight and can see the track ends better than another.) It would be possible to estimate the sizes of systematic and random differences from suitably designated studies—and this could be a worthwhile exercise.

Within and Between

Looking at the Sample 1 graph more closely, you can see that differences between analysts from the same lab are not so great as those from different labs. The "within lab" variation is less than the "between lab" variation. In fact within some labs (2, 6 and possibly 1 and 3) there is little evidence of extra variation between analysts. Treating all variation as random I have estimated the between lab standard deviation to be 0.35 microns and the between analyst (within lab) standard deviation to be 0.19 microns. These represent extra components of variation: for example, mean lengths measured by different analysts in the same lab, each having a sub-sample standard error of 0.1 microns, will vary with a standard deviation of

$$\sqrt{0.19^2 + 0.10^2} = 0.21 \textit{ microns}$$

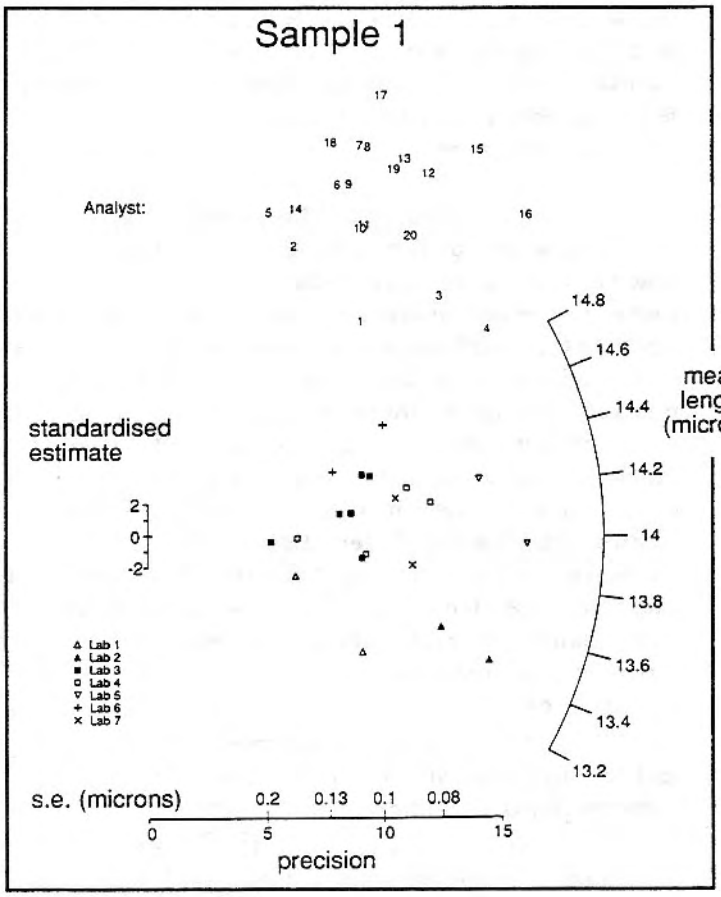
and for such analysts in different labs they would vary with standard deviation

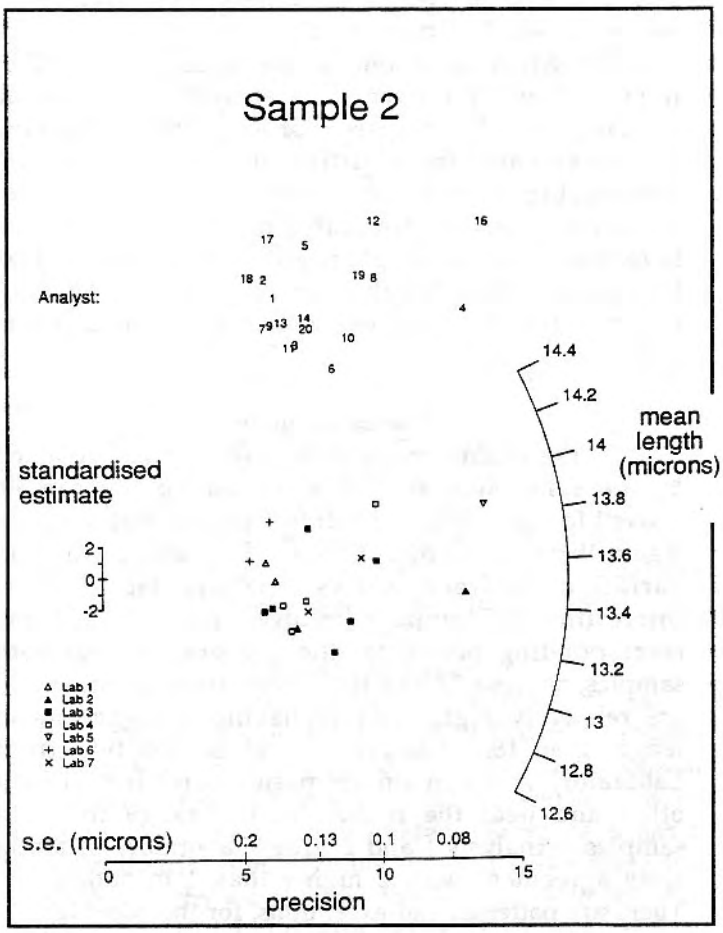
$$\sqrt{0.35^2 + 0.19^2 + 0.10^2} = 0.41 \textit{ microns}$$

The practical importance of these extra components of variation will depend on the context. For example, if the reduction in length is of interest, rather than the absolute length (as in some annealing studies) it will be important either for the same analyst to measure all the samples to be compared, or to be able to combine measurements from different analysts without introducing systematic biases or too much extra variation. Furthermore, differences between analysts in *reduction* in mean length might be much smaller than for absolute mean length. These things are unknown, but are relevant to studies that use data from different labs.

A second sample

The second graph shows the estimates obtained by the same analysts for a second apatite sample (except for analyst 15 who didn't analyse this sample). Again there is variation between labs and some extra variation between analysts within labs. It is interesting to compare relative positions of the corresponding points for the two graphs. For both samples, analysts 17 and 18 (the two from Laboratory 6) are relatively high, with 17 having a higher mean length than 18. Analysts 19 and 20 (the two from Laboratory 7) are in similar positions relative to each other and near the middle of the range for both samples. Analysts 1 and 2 (from Laboratory 1) are in close agreement with 2 higher than 1 in both cases. There are patterns, and exceptions, for the other labs





also. It seems likely that there are both systematic and random effects between analysts, and between labs.

An Anova

There are two sorts of people. Ordinary folk like you and me, who derive simple pleasure from hydrothermal intrusions in a sedimentary basin or an alpine fault in a parauthochthonous terrane. But to these other people, beauty is analysis of variance. Here is what they would do with the data from the two graphs (leaving out Lab 5 for simplicity):

Source of variation	d.f.	Mean square
Samples	1	2.8274
Labs	5	0.3765
Samples X Labs	5	0.1794
Analysts (within labs)	12	0.0994
Residual	12	0.0706

Naturally d.f. stands for degrees of freedom and mean square stands for variance. To read this table start at the bottom. The residual mean square 0.0706 represents the variance between estimates having accounted for all systematic differences between samples, labs and analysts. If there were no such differences, all the other mean squares would roughly equal this one. The residual standard deviation (the square root of 0.0706) is 0.27 microns. Actually part of this is known (the part corresponding to the s.e.'s in the graphs) and if we take away this part (a less than trivial exercise) the remaining part comes to 0.21 microns. This represents the standard deviation of *random* differences between analysts (within labs) and agrees well with the figure of 0.19 microns found earlier.

The mean square for Analysts (within Labs), 0.0994, is a bit larger than 0.0706. This extra bit is due to *systematic* differences between analysts in the same lab. The other three mean squares are even larger. The Samples mean square reflects the fact that the mean lengths for the two apatites are different (13.5 microns as opposed to 14.0 microns). If they were more different it would be even larger. The Labs mean square is also large, reflecting systematic differences between labs. And the Samples x Labs mean square reflects the fact that differences between labs are different for the two samples. Beauty indeed.

A laboratory difference

In the absence of more information, the figure of about 0.2 microns for the extra within lab standard deviation may have implications for current laboratory practice. Differences between labs, on the other hand, are usually relatively large and not so easily regarded as being random. Naturally a laboratory will tend to trust its own results and discount others if they are

different. As a laboratory representative once remarked (in a slightly different context): "There are three sorts of laboratories: those that can count, and those that can't."

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OmniTrack

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OmniTrack is a computer program developed at the University of Texas at Austin to coordinate the use of a digitizing tablet and motorized microscope stage for fission track thermochronology analysis. Its basic goal is to enable the user to collect fission track data in an easy and efficient way, perform standard calculations and produce standard graphics. Interpretation of the data is left up to the investigator. It is now available to anyone who wants a copy for free. This article is intended to acquaint any interested parties with the program and hardware facilities at the University of Texas at Austin and what they can do.

OmniTrack defines a coordinate system for each sample, and records the positions of each measurement taken, enabling simple to return to the exact site. This makes double-checking easy, and is particularly valuable for training. It also simplifies later microprobe analysis of grains where tracks are measured or counted. OmniTrack also automates driving between the grain mount and detector plate when the external detector method of fission track analysis is being used. It implements an automated scan of the sample, allowing examination in a thorough and systematic way. It makes a number of calculations, including single grain ages, a number of standard "combined ages," and the chi-squared test, and produces some of the standard histograms as printouts or PICT files in virtually publication-ready form.

THE PROGRAM

OmniTrack was written for the Apple Macintosh computer, and with the basic philosophy of Macintosh programs in mind -- it has a graphical interface, making it easy to learn, versatile, and powerful. Furthermore, the user dictates the course of action at every turn instead of hard-wired program logic. It is written in Think-C, and is largely ANSI-standard conformant (with the exception of Mac-specific function calls). It is compatible with any Macintosh and any version of the operating system, although its screen window will not fit on the old-style small Mac screens -- anything larger will do.

The basic purpose of the program and documentation is to provide the user with as much power and flexibility as possible while being simultaneously idiot-proof (with absolutely no disrespect intended for any users, past, present or future). It has been in use at the University of Texas at Austin since the fall of 1989, although it has been enhanced and updated for virtually its entire life. Users have included Jeff Corrigan, Leslie White (now Wahrmund), Ray Donelick, Rich Weiland and Stefan Boettcher, and the program is basically the sum of all of their requests and suggestions, plus ideas of my own. Among the main things I learned was that each user tends to want to do things in his or her own way, and thus the program has been written to a significant extent to be customizable according to the whims and habits of each individual.

The program has had a number of names through its history. At the 7th International Workshop on Fission-Track Thermochronology in June 1992 in Philadelphia it was introduced as "MacTrack", but at that conference it was learned that there was already another program by that name. The current name is OmniTrack, and unless another program by that name is called to my attention, it will remain that way.

An example of the main program window is shown below in figure 1. Counting data and track length data are stored in on-screen scrolling lists, which allow the user to access any of the data at any time.

UT FISSION TRACK LAB HARDWARE

The fission track lab at the University of Texas consists of the following hardware: A Zeiss Axioskop binocular microscope with transmitted and reflected light and a drawing tube, a Macintosh IIx computer

with a Radius 2-page display monitor and an extended keyboard, a Houston Instruments Model DT 114 Hipad^{tm} digitizing board with a 4-button cursor, a Kinetek LEP motorized microscope stage with 3" by 3" travel with X and Y motors and MAC 1000 system control unit, and some parts made at UT (the stage slide holder, radiation shield and a light for the digitizer cursor).

Anyone wanting to set up a fission track lab modeled after this one will have some difficulties, however, as the Hipad^{tm} digitizing board listed above is no longer made. This in turn leads to problems with the OmniTrack software, since it is written specifically for that digitizer. Fortunately, the program was written with this eventuality in mind, and the source code is relatively easy to modify to allow it to utilize virtually any digitizer which can be connected to a Macintosh, so long as it has a 4-button cursor. Likewise, the code which interacts with the stage is written in such a way that modifying it to drive a different stage is straightforward.

FEATURES

Process automation: The program uses the Kinetek stage to automate as much of the fission track analysis process as possible, including translation from sample to detector and back and recording all coordinates of everything measured. The Kinetek stage is very durable and reliable, and its optical positioning system ensures virtually no coordinate drift.

Keyboard redundancy: Since fission-trackers tend not to want to look at computer screens too much, OmniTrack has keyboard redundancy, where the user can assign any key (known as "hot keys") on the Mac keyboard to execute any of the functions regularly used during fission track analysis. The key definition procedure is simple, as illustrated in Figure 2.

User-specific customizing: The program is to a large extent customizable to every different person who uses it. This includes the "hot keys", the user's dating parameters (zeta, etc.), the preferred report and histogram formats, the timing of when the computer gives an audible warning or signal (i.e. beeps), digitizer calibration and preferred digitizer setting, and some other things. All of this data is stored in a "User" file, which is maintained automatically. This ensures that in a lab where several people are using the program, each one can always find the program "just as he/she left it", instead of subtly reconfigured in some unknown way.

Automatic scanning: An automatic scan function allows the user to define a complete scan path for looking at a sample. To use it, the user specifies the starting and ending points, the microscope magnification, and the desired stage speed. After this information is entered, the computer calculates and maintains a scan path, which is calculated so that all points in the scan area will fall within the square enclosed by the circular field of view. The program allows both incremental (step by step) and continuous scanning, and freely allows switching between them. Backwards scanning is likewise implemented, as well as allowing users to pick up where they left off in the event that they stop the scan and drive the stage off of the scan path.

Macintosh "look and feel": OmniTrack is written for the Apple Macintosh computer, and is true to the Macintosh standards for "look and feel." This basically means two things. First, it utilizes a graphical interface, making it both intuitive and powerful, and most importantly allowing the user to choose which action to do at any time, rather than forcing the data collection process along some pre-defined, rigid one-way path, such as might be the case with older "flow-chart" based programs. Second, it follows Macintosh conventions on where commands are, what they are called, what the graphical items mean, and what they look like. This should make learning and fully utilizing the program much easier for anyone with any Macintosh experience.

Implements both detector and population methods: OmniTrack is programmed to support both detector and population methods. However, the population method has never been used at UT, so this aspect of the program is a bit less tested.

Can collect c-axis, TINT/TINCLE data: The user can collect c-axis and/or TINT/TINCLE data, if desired. The user can also not collect it, if that is desired.

4 digitizer settings: The digitizing board has a 4-key cursor, and there are 4 settings to choose from which assign meaning to each button, depending on what the user wants to measure and how he/she wants to measure it.

Calculations: The program automatically calculates a variety of numbers, including single grain age; density; Uranium contents; pooled, mean, and weighted mean ages; the Chi-squared test; and fiducial mark quality.

Output: OmniTrack produces printouts where the user specifies which data is to be included in the table, in a format used in some publications. In addition, data from the program can be dumped into text files which are spreadsheet-readable, including parallel columns.

Graphics: The program also produces standard and generalized histograms of grain ages (including probability density curves) and track lengths. These can be printed or saved to PICT files, which can in turn be enhanced in standard graphics programs.

Watch-dog routines: The program keeps an eye on the data pathways coming from the stage and digitizing pad, and reports any glitch and deals with

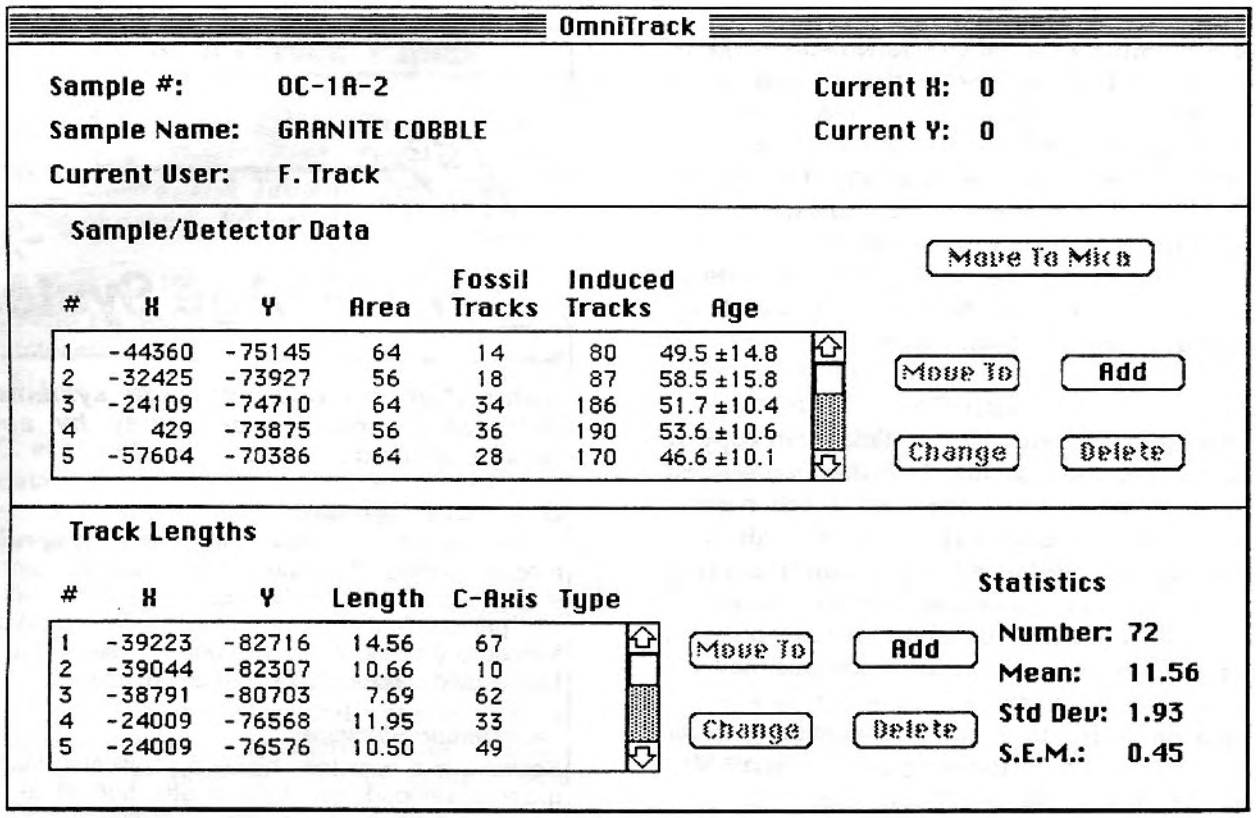


Figure 1: The main program window

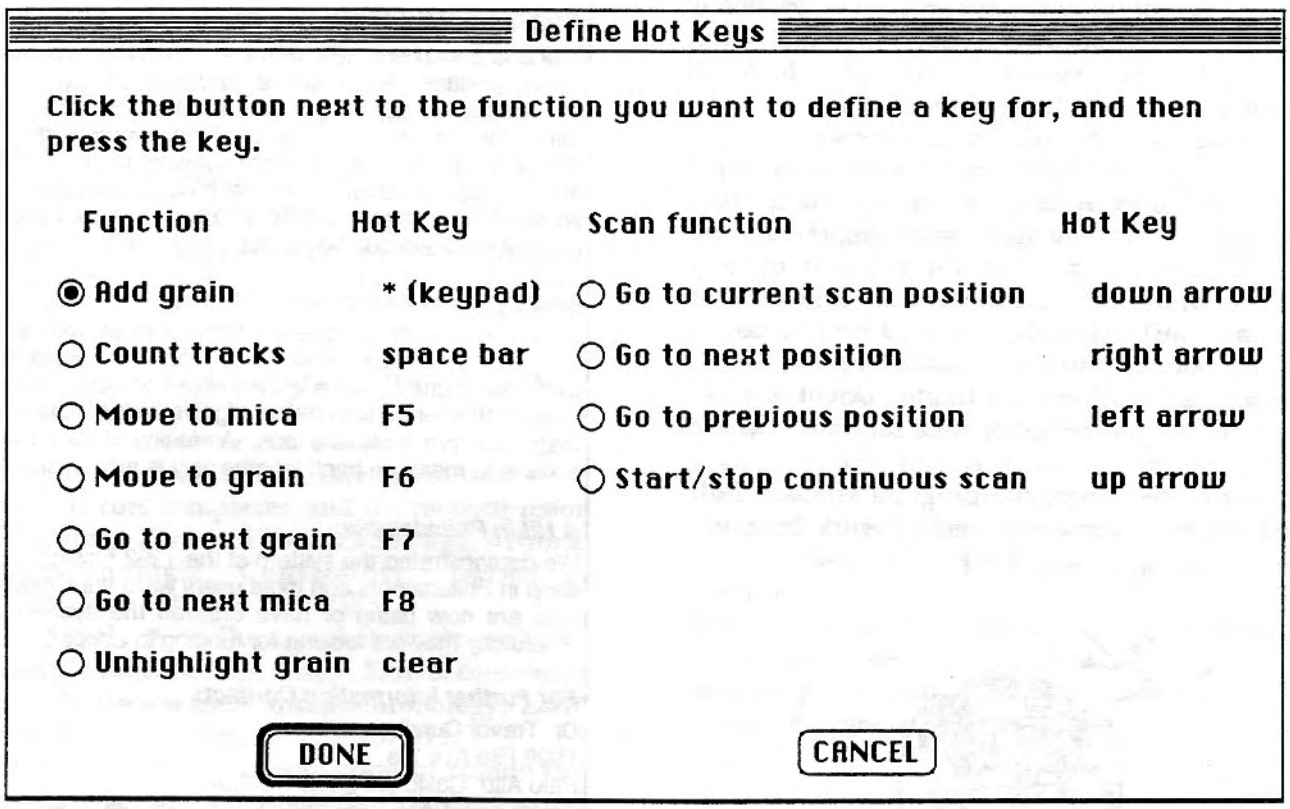


Figure 2: The "Hot Keys" definition dialog

it, instead of coming to a halt. The Kinetek stage at the University of Texas seems to occasionally have some troubles in this respect when certain combinations of commands are sent to it, so the software has been specifically modified to deal with this eventuality.

On-line help: There are opportunities to ask for help liberally interspersed throughout the program, so if the user is ever unsure of what to do or the meaning of some request the computer is making, helpful hints are usually a button away.

AVAILABILITY

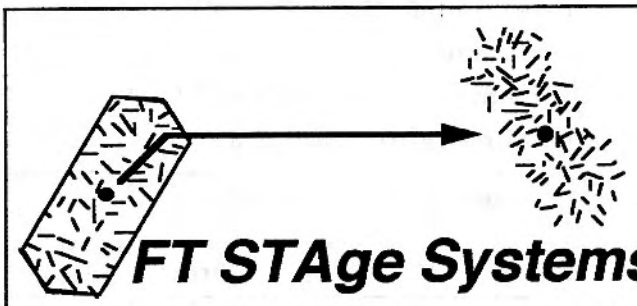
Anyone interested in OmniTrack can copy it freely from anyone else who has it, or they can request a diskette from me. In this latter case, I will request five dollars to cover the cost of the diskette, mailer and postage. The diskette includes the program (including source code), extensive documentation, and examples.

Anyone who has the program source code may, of course, make any additions or modifications they see fit. If this does happen, and the new function seems like it might be useful to fission trackers in general (such as, perhaps, adapting the program to a small Mac screen, adding new graphics, or enabling it to use a different stage or digitizer), it would be a nice thing to send me a copy of the changes, so that I can incorporate them into the version we have here, for our own benefit and that of anybody else who requests the program. It would be a very good result, actually, if this program started to grow as a result of a cooperative effort among its users in the fission-track community, and I am willing to be the "central" point at which all modifications are integrated into the system.

Again, the software, source code and manual are free. However, the following restrictions do apply to copying the software and the manual: the manual must accompany any redistribution of OmniTrack, and OmniTrack may not be sold for a profit by any organization under any circumstances. Likewise, no part of the OmniTrack code, modified or not, can be sold, or incorporated into any program that is sold.

I am also available to a limited extent to make modifications to the program, but for this I would charge.

Past these things, the program is free. Still, any kind words, acknowledgments, favors, beer, etc. would be greatly appreciated. ❑



Automated microscope stage systems greatly increase operator productivity by automating tedious aspects of microscope work.

Outstanding Hardware:
Our integrated stage system features a highly-reliable, high-precision Kinetek™ computer-automated microscope scanning stage. Several hundred Kinetek stages are currently in operation, primarily in the microelectronics industry. Compatible with almost any brand of microscope. System also includes a highly-rated Calcomp™ 12x12" digitizing tablet.

Outstanding Software:
Software is a complete, highly sophisticated Macintosh program developed with careful attention to all aspects of microscope work. Fully integrates dating, length measurement, slide scanning, and file management functions. Moves from grain to mica print in 3 seconds. Very user friendly. Fully functional on even the least expensive Macintosh computer. Portions of computer code can be made available on request if you would like to modify the program for your special purposes.

Innovative, Highly-Intuitive Stage Control System:
Stage is driven primarily with the digitizing tablet cursor rather than a joystick. For example, to center an object, just superimpose the cursor on it, push a button, and the stage automatically centers the object. This avoids tedious manual centering via the joystick. Most software commands are driven from the cursor buttons, which are easily distinguished by feel, so there is no need to look away from the eyepieces to the computer screen and keyboard.

New Systems Available or Planned:
A new version of the system that operates with a European-made stage is now available to serve the European fission track community. We are also eager to adapt the system on request to operate with other stages such as a Japanese-made stage to serve Japanese labs. A version of the system without a stage to measure track lengths only is also available.

A Hit in Philadelphia:
We demonstrated the system at the 1992 Fission Track Workshop in Philadelphia and most users were very impressed. Six labs are now using or have ordered the system and many others say they are looking for funding to adopt it.

For Further Information Contact:
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1099 Fife Avenue
Palo Alto, California 94301 U.S.A.
Telephone **1-415-725-1328**; Fax **1-415-725-2199**

Recent Fission-Track Papers

[Thanks to *Dennis Arne, Tohru Danhara, Trevor Dumitru, Istvan Dunkl, Paul Fitzgerald, Tony Hurford, D. Seward,* and *Takahiro Tagami* for this issue's listing of papers. Editor]

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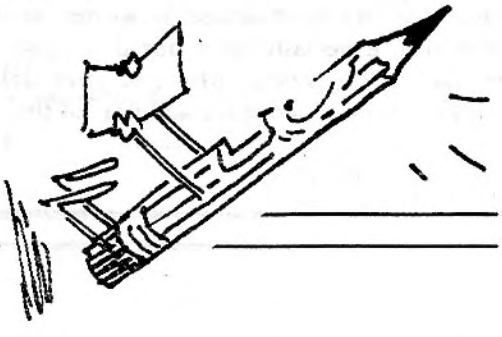

Short Tracks

* Heard of *International Nuclear Track Society* (INTS)? Yes, it exists. I didn't know about it until I heard from Prof. Durrani of Birmingham University, who is the Executive Vice-President of the Society. Dr. P. Vater of University of Marburg is the Secretary of the Society. INTS also publishes a periodical newsletter, individual subscription being $10 per year.

* *Ewald Hejl*, Ph.D. from Max-Planck, has moved to Institut fur Geologie und Palaontologie de Universitat Salzburg, Hellbrunnerstr. 34/III, A-5020 Salzburg, Austria.

**Rod Brown* after completing his Ph.D at La Trobe has joined Tony Hurford's group in London. Note his new address.

**Amanjit Sandhu* after doing a post-doctoral term at the University of Toronto went back to Guru Nanak University in India, where he had obtained his Ph.D.

**Tracy Paul* of Arizona State University (ASU) completed her Ph.D. Her thesis was on "Transmission Electron Microscopy Investigation of Latent Fission Tracks in Fluorapatite: physical characteristics and annealing behavior". Preliminary results of the study appear in *American Mineralogist* (Volume 77), and some additional papers are in progress. Drs. George Wolf, John Holloway, Sue Kiffer, Simon Peacock, and Dave Smith of ASU, and Dr. Steve Bergman of ARCO served as doctoral dissertation advisors to the project.

**Richard Weiland* submitted his M.S. thesis titled "Plio-Pleistocene unroofing of the Irian fold-and-thrust belt south of the Gunung Bijih (Ertsbeg) Mining District, Irian Jaya, Indonesia: apatite fission-track thermochronology" under the supervision of Dr. Mark Cloos at the University of Texas at Austin. Richard will be doing a Ph.D at the same university on the thermochronology of metamorphism related to ophiolite emplacement and the Tertiary arc-continent collision in northern Irian Jaya, Indinesia. The field work is scheduled to start in June 93.

* *Leslie A. White* finished her Ph.D. under the supervision of Mark Cloos at UT-Austin in May 1992. The title of her dissertation is: "Thermal and unroofing history of the western Transverse Ranges, California: Results from Apatite Fission-Track Thermochronology. Leslie now works for Exxon Production Research Company in Houston, Texas.

**Paul Fitzgerald* and *Tim Redfield*: Back from Antarctica:

"Some friends have asked how our season went in Antarctica, especially after our dire predictions in the last issue of *On Track*. Much to your delight, Rasoul was not on the lookout for articles, so this will be very short. Our season was very successful: no delay in New Zealand, only a nine day getting into the field for the fist time, and we got pulled out only one day after schedule. Following that, we did wait 14 days in McMurdo (US base) waiting to get into the field again, and did spend New Years eve at an angle, in a wee tent, in a windy spot high on a very cold mountain. The next day we had a hard landing so that ended that part of the season. I departed for New Zealand, with only a couple of days delay and Tim Redfield waited only three weeks to get a weeks cruise on the ice-breaker."

P.G.F

* *Dennis Arne* is the next editor of On Track (November 93 and May 94). Dennis did his Ph.D. at Melbourne with gurus Paul Green and Ian Duddy in 199o, and after doing a short post-doc in Antarctica and working for Geotrack for two and half years, he moved to the University of Dalhousie in December 1992 on a one-year Killam Scholarship.

* *Richard Weiland* of the University of Texas at Austin will probably take over the editorship of *On Track* for the November 94 and May 95 issues, after Dennis' term is over. A definite decision on this matter will be made by Richard and Dennis in the near future and will be announced in an upcoming issue of *On Track*.

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:

***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

Directory of the International Fission-Track Community

Prepared by *Rasoul B. Sorkhabi*
Editor of *On Track* (1992-93), Arizona State University

Editor's Note: This directory has been prepared mainly from the answers I received to the yellow forms distributed with the November 1992 issue of *On Track* and partly based on the mailing list of *On Track*. It is published solely for the information of fission-track researchers; this is neither a comprehensive directory including all fission-track researchers nor an official document endorsing the scientific stand of individuals by the fission-track community. I would like to take this opportunity to thank all those who responded to the yellow forms. It is suggested that the directory be upgraded from time to time by future editors of *On Track* since the actual number of fission-trackers must be far larger than what is mentioned here (my estimate is 450±50), and also to incorporate new changes and to correct typographical errors, if any.

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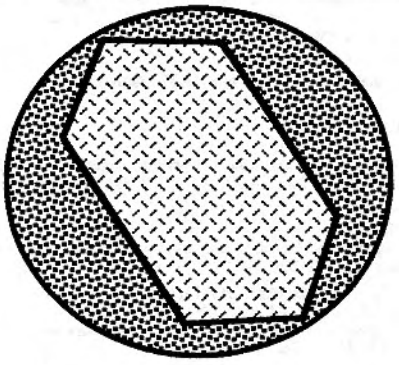

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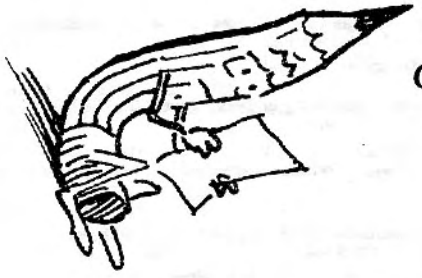

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