Letters to the Editor

Discussion of "Fractal Surfaces as Models of Physical Matches"

Dear Sir:

As I understand it, there are two premises to Dr. Thornton's technical note, "Fractal Surfaces as Models of Physical Matches," JSFCAS 31(4)1435–1438, 1986. Premise 1 is that methods of analysis using fractal math can uniquely identify complex surfaces, for example, fragmentation patterns. Premise 2 is that the complexity of the surface is somehow proportional to the generation time of a computer graphic pattern.

However, belief in the above premises requires a misinterpretation both of Mandelbrot's work and the degree of significance attributable to a computer program.

Fractal dimension values (Hausdorff-Besicovitch dimensions) are obtained from the following formula: $D = \log N/\log(1/r)$ where N = number of subintervals, and r is the size of the subinterval. Some examples will make this clear.

If a line is divided into 5 parts, then $N = 5$, $r = 1/5$, and $D = 1$. If a square is marked off 5 equal parts to the side, then $N = 25$, $r = 1/5$, and $D = 2$. This is like saying the area of a square is the side squared. If a cube were similarly divided, then $D = 3$. Note that the fractal value equals the number of physical dimensions. For special curves, now called fractal curves, the D values are not whole numbers. A famous curve, the Koch snowflake, has a D of about 1.2618.

In viewing fractal curves generated by computers, two things become apparent. One can predict neither the apparent complexity nor the shape of the curve based on the value of D before seeing the curve. Thus, while Mandelbrot has determined D values for physical phenomenon and these D values appear to have fundamental significance, you can't go backwards. You can't determine a phenomenon or surface from a D value without the generating algorithm.

What this means is that fragmentation patterns may or may not have the same fractal dimension value and that patterns with the same values need not be identical. Thus, even if you could calculate D for a piece of a specific fragmentation pattern and match it to an adjacent edge, the concept is irrelevant.

The premise that computer generation time for a pattern can characterize that pattern is confusing a map with the territory. Program time reflects one thing only, the efficiency of the program. For example, anyone who has worked with search and sort routines has seen that different programs yield dramatically different retrieval times on the same data file. Clearly the data file has not changed its complexity.

The Creative Computing program increases its time exponentially with level based on the algorithm in use, not the complexity of the shape. The Poisson approximations and fast Fourier methods alluded to in "The Fractal Geometry of Nature," by Mandelbrot, may permit a linear rather than exponential increase in time for the same series of figures. This would not imply that fragmentation patterns no longer are unique.

In short, while I agree with Dr. Thornton's conclusion that no two fragmentation patterns are alike, the reasoning in the article is fallacious.

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Author's Response

Sir:

Premise 1 is a bum rap, a straw man seemingly set up for the purpose of being destroyed. I stand behind Premise 2.

Nowhere in my original paper does it say that the fractal dimension *D* of a surface is unique. The fractal dimension will, however, characterize a fracture surface in the same way that the Henry formula may characterize a set of fingerprints, or a library call number may characterize the contents of a book. A point that appears to have eluded Dr. Gault, however, is that a unique surface *will* have a fractal dimension, and it might be of interest to us in any theoretical consideration of fracture matches to know what that dimension is.

I agree with Dr. Gault that you cannot predict the apparent complexity of a surface based on the fractal dimension before actually seeing what the generating algorithm will deliver. *But isn't that precisely what I did in the original paper?* I generated the surfaces without reference to any stated fractal dimension. After the surfaces were generated, they were inspected to determine their resemblance to "real life" fractures. For those rare readers who actually give a whit about this exchange between Dr. Gault and myself, I would invite them to reread the original paper and determine for themselves what I claimed and what Dr. Gault claims that I claimed.

Dr. Gault points out, with reference to Premise 2, that the van Panne computer program increases its time expotentially with the level. Of course it does. That is so patently obvious that it never occurred to me to state it in those terms in the original article. But then Dr. Gault goes on to say that "Poisson approximations and fast Fourier methods . . . may permit a linear rather than exponential increase in time for the same series of figures." I am constrained to note that Dr. Gault is careful to use the word "may." This is yet a surmise.

One final point. Dr. Gault's final paragraph states that I have "concluded" that no two fragmentation patterns are alike, intimating that this conclusion is somehow derivative from the fractal surface work. This is a misstatement of my work. The uniqueness of fracture surfaces was taken as axiomatic from the outset, as was stated emphatically in the opening sentence of my article.

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Discussion of "Winchester Silvertip Ammunition—A Study in Ordnance Gelatin"

Dear Sir:

In their paper on "Winchester Silvertip Ammunition—A Study in Ordnance Gelatin," Vol. 31, No. 3, July 1986, Ragsdale and Josselson conclude that measurements of the radial fissures ". . . do not permit accurate prediction of true temporary cavity size. . . ." The authors cite the study of Krauss and Miller [*1*], but do not comment on the fact that this extensive work (220 shots) *showed excellent correlation of radial fissure length with maximum temporary cavity diameter.*

Our published research [*2-4*], and much additional work, including shots with a variety of projectiles (rifle, handgun, fragment simulators, and spheres) over a velocity range from 500 to 6750 ft/s *has shown a reliable, consistent, and useful correlation of gelatin fissure length with temporary cavity size.* Gelatin cavities were measured with high-speed cine equipment, and cavitation in animals was measured using flash X-rays for initial comparison studies to calibrate our gelatin and continue for an ongoing quality control verification.

Early in our studies we found, quite by accident, that heating the gelatin mixture to temperatures above 65°C during its preparation had detrimental effects on its consistency. We consulted the manufacturer (Kind & Knox, Sioux City, Iowa) and according to Dr. Felix Viro, Director of R & D, excess heating of the gelatin mixture at any time during its preparation is likely to destroy the properties that make gelatin so useful as a tissue simulant. Written directions for preparation of the gelatin furnished to us by Kind & Knox recommend "always start with cold water," but unfortunately no caution against excess heating is mentioned. Dr. Viro states that 40°C is more than adequate to dissolve all the gelatin after the period of hydration in cold water—and he is in accord with our recommendations of a temperature limit of 65°C.

Recently, in comparing data with another investigator, we found inconsistencies and traced them to his use of boiling rather than cold water to reconstitute his gelatin. In the literature only Berlin et al. [5] have found gelatin to be ". . . not a suitable simulant. . . ." Their gelatin was ". . . dissolved in water at 85-90°C. . . ," a procedure found to produce unsuitable gelatin in our laboratory.

Personal communication to Dr. Ragsdale reveals that he started with boiling water to reconstitute the gelatin used for these studies. We suggest that the results reported by Ragsdale and Josselson verify our findings that excess heat in preparation can destroy the consistency that makes gelatin such a useful tissue simulant. When ordnance gelatin is prepared in accordance with the recommendations of the manufacturer, radial crack measurement is a reliable indicator of temporary cavity size and location. In our laboratory high speed cine is now used only for periodic quality control checks, having been largely replaced by the far less costly radial crack measurement method.

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Author's Response

Dear Sir:

We are aware that elevated temperature can be detrimental to the gelatin chain network [1]. But some readers may not be aware of what the fuss is about. Calculations employing measurements of fissures that radiate through gelatin from permanent tracks due to penetrating bullets have been proposed as approximations of the size of the corresponding maximum instantaneous temporary cavity (M.I.T.C.). Temporary cavities and radial fissures are also wound mechanisms in living tissue. Fissure measurements in gelatin are widely used by lay and official (police and federal) groups to avoid the expense and technical inconvenience of high-speed cinematography. Anyone who does not use high-speed cinematography or burst X-ray to actually "see" the M.I.T.C.s created by their experimental shots, but rather calculates them, has a major investment in the reliability of the method. The recommended formulas are based on the assumption that the expanded fissure pattern will roughly equate to the dilated circumference of the temporary cavity (Fig. 1). In other words, if the radial splits are the result of stretching the substance beyond its elastic limit, they should be proportional to the dilatation.

References *2*, *3*, and *4* (above) state without proof that "the radially oriented cracks . . . correlate with temporary cavity diameter." It has this axiomatic form without supportive data wherever I have found it in print, except for Krauss and Miller [*1*, above]. No supportive data is offered by Berlin et al. [*5*, above] who Col. Fackler cites as the experimental underpinning of his method [*3*, above], though his method differs. He equates the *two longest* splits with diameter while Berlin et al. double the sum of *all* splits for circumference.

If the "Fissure Axiom" is correct, then fissure measurements should be proportional to and give a fair approximation of the M.I.T.C. regardless of variations in the medium as a result of percentage gelatin (20 versus 10%) or mix temperature. To check the accuracy of fissure based calculations, a separate series of 14 test shots employing 12 different varieties of handgun ammunition were analyzed for which we have fissure measurements at known depths in the blocks as well as high speed camera views of the true M.I.T.C.s [*2*]. The actual M.I.T.C. diameter at the depth subsequently crosscut was determined from the movie film for each block (Fig. 2). One of the calculations tested was Col. Fackler's Wound Profile Method (WPM); this predicts the maximum temporary cavity diameter will be twice the sum of the two longest fissures in a given plane of cross section across the permanent track [*3*, above]. For our 14 test shots, the WPM underestimated the true M.I.T.C. diameter by 30% with a range of 58% under to 67% over the true value. To determine if the WPM underestimated the M.I.T.C. in a way uniform enough to allow statistical predictions, a coefficient of linear correlation (the Pearson Product-Moment Coefficient sometimes called "Pearson's *r*") was applied. A good positive correlation would be signified by upwards of 0.700 with 1.000 being a perfect fit. For the 14 shots, *r* was 0.047, little better than could be expected

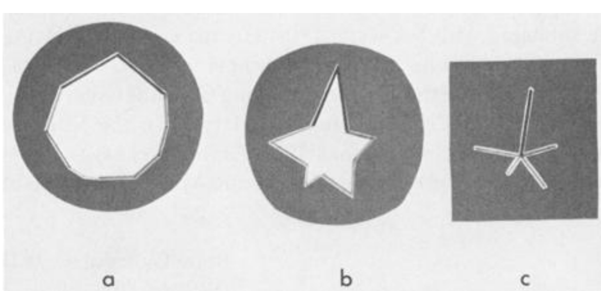

FIG. 1—*From left to right are sequential schematic diagrams of a gelatin block cut perpendicularly across the missile path:* (a) *during maximal expansion of the temporary cavity* (left), (b) *during partial* (center), *and* (c) *complete* (right) *collapse into the permanent track from which radial fissures extend. This figure illustrates the unproven hypothesis that radial fissures accurately represent the collapsed walls of the temporary cavity as the distorted block returns to its original rectangular shape.*

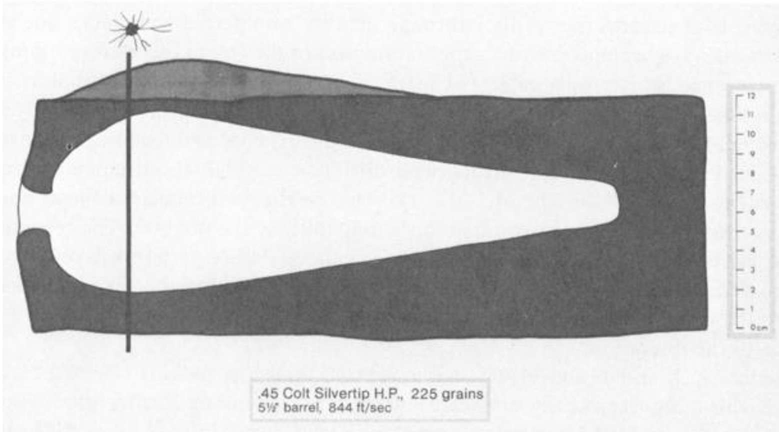

FIG. 2—*Scale drawing of the M.I.T.C. for a .45 Colt Silvertip hollow point bullet as traced from the high-speed motion picture frame. Above is the subsequent radial fissure pattern found at the depth from the block face indicated.*

from the operation of the laws of chance. Obviously, it would be of interest to repeat this work on gelatin blocks prepared at temperatures below 65°C (149°F).

Krauss and Miller concluded that twice the sum of all radial splits provides a "close approximation" to the circumference of the M.I.T.C. Their data base for this consists of eleven shots using a .30-caliber, 44-grain fragment simulator (not bullets), and "a few other instances" (not "220 shots"). Individual fissure predictions of M.I.T.C. range from 28 under to 77% over the true values. The Pearson *r after* throwing out the worst data pair is 0.323, still too weak to be predictive.

This variance argues against relying on any varient of the Fissure Axiom for comparing the cavitational effects of single test shot examples of specific ammunition styles or brands.

Figure 4 of Krauss and Miller is a straight line representing a plot of the longest radial split in 20% gelatin versus temporary cavity diameter. Our attempt to duplicate this correlation found a seemingly random vertical scattering of points for longest radii found in blocks where M.I.T.C. diameters were from 4.0 to 12.8 cm in diameter.

Viewing the surface of radial fissures in oblique light indicates they have an irregular step-like surface resembling the fracture planes of a brittle substance. I am beginning to think they form not so much as a result of stretch, but by shatter analogous to the breaking of ice; those who call them "cracks" may be using a more precise term [*5*, above].

Our blocks are prepared with hot water [*3*]: first, for ease of dissolving the granulated gelatin, and second, so our results will be comparable to others. "Others" who start with water over 80°C (176°F) include the Aberdeen Proving Grounds (Aberdeen, Maryland), The U.S. Secret Service Training Center (Beltsville, Maryland), the FBI Training Academy (Quantico, Virginia), the Frankfort Arsenal (Philadelphia, Pennsylvania—now closed), the old Edgewood Arsenal (Maryland) and yes, Krauss and Miller (1960) who used blocks made at Edgewood.

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Discussion of "Improved Enhancement of Ninhydrin Developed Fingerprints by Cadmium Complexation Using Low Temperature Photoluminescence Techniques"

Sir:

The authors of the article "Improved Enhancement of Ninhydrin Developed Fingerprints by Cadmium Complexation Using Low Temperature Photoluminescence Techniques", which recently appeared in this *journal* (Vol. 31, No. 2, April 1986, pp. 432–445), advocate the use of a cadmium salt for postninhydrin treatment to detect latent fingerprints by fluorescence. This advocacy is based on fluorescence obtained at 77K, namely, with articles (of evidence) immersed in liquid nitrogen. It is further based on superior results reported with certain (yellow) kinds of paper. It is based also on superior results obtained when comparing with zinc salt application under excess moisture or heat, or when the ninhydrin treatment was improperly performed. Finally, it is based on greater ease of filtering when a xenon arc lamp is used instead of a laser to excite the fingerprint fluorescence.

Several remarks are indicated.

1. Practical considerations preclude liquid nitrogen use for general evidence examination for two reasons: (a) universities and some major crime laboratories may be equipped for cryogenic work, but not identification sections of police departments (even those that possess lasers). (b) Cryogenic examination is not compatible with large volumes of evidence, large-size paper items, or, worse even, cardboard boxes and such.

2. Several of the authors of the article under discussion have shown previously that at 77K even relatively weak (3 to 4-W) Ar-laser excitation detects fingerprints with photographic exposure roughly a factor 5 shorter than xenon lamp exposure [1]. They further report in that article as well as the article under discussion that no room temperature fluorescence was found with their Zn or Cd complexation procedure. Laser detection of ninhydrin/$ZnCl_2$ treated prints has amply demonstrated its value in case work by now at room temperature, that is, without the benefit of the well-known increase of fluorescence efficiency with lowering of temperature. The objective of the exercise is to develop elusive latent prints, not to produce inferior devices, even if they are cheap. The issue of laser cost is, in any event, not a very substantive one. A growing number of law enforcement agencies, even small ones, are acquiring lasers (in the United States, the present number is roughly 100). The price of 2 patrol cars is comparable to that of a laser facility. Otherwise unobtained fingerprint evidence in a single major case would save enough in investigative and court costs to easily pay for the laser system.

3. The possibility of incompetent application of ninhydrin or zinc chloride hardly constitutes a reason to advocate another, inherently less sensitive, procedure. Cd causes greater spin-orbit coupling than Zn (because of greater atomic number) and this, in turn, increases intersystem crossing, which quenches fluorescence. Cadmium nitrate postninhydrin treatment does actually produce fluorescence at room temperature, albeit rather more weakly

than the Zn posttreatment (see Ref *2*). This again indicates the superiority of lasers over xenon lamps, or it suggests that the cadmium complexation, too, must be performed with care.

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Author's Response

Sir:

The letter to the editor by Professor Menzel is totally unjustified on scientific grounds. Our paper presents irrefutable evidence to show that cadmium complexation of Ruhemann's purple (formed principally by reaction of ninhydrin with the amino acids present in the latent fingerprint) is superior to zinc complexation in many cases. This result significantly improves the fingerprint expert's ability to obtain evidential data not previously available.

Professor Menzel is a strong advocate for the use of lasers. He rightly points out that lasers can produce results at room temperature with the zinc/Ruhemann's purple complexation method and that this is preferable to working at low temperatures. However, the cadmium/Ruhemann's purple complexation method provides ridge detail not possible by the zinc complexation method. Operational laboratories presently equipped with a suitable laser can obtain these improved results by working at liquid nitrogen temperatures; those laboratories, however, not yet equipped with laser facilities (as applies in many less affluent countries) can obtain equally good results using a xenon arc lamp.

The criticism regarding the need for the use of the cryogenic reagent, liquid nitrogen, is not justified. Liquid nitrogen is readily available and used widely in research and many forensic science laboratories. It can be transported without danger in Dewar containers (not expensive) and stored therein for several days. It is interesting to note that Professor Menzel advocates the use of liquid nitrogen in document examination work [*1*], so why be critical of its use for fingerprint work.

Let me remind readers that there is nothing unique about laser light that makes it superior to light from suitably filtered arc lamps when used in the presently available fluorescence based fingerprint methods. The xenon arc lamp used in our reported study is not sufficiently powerful to cause significant fluorescence in the zinc complexation method at room temperature, but this is directly associated with its power output; there are other arc lamps, however, that can induce this fluorescence at room temperature and are in fact, more powerful in the 480- to 500-nm spectral range than a 20-W argon ion laser.

The Australian National University Forensic Science Research Unit has substantial case work experience having been involved in over 200 crime investigations (the State and Federal police forces handle the regular crime cases) where our advanced techniques for latent fingerprint detection have been required (70 to 80% success rate even with these difficult cases). We always make comparisons between the xenon arc lamp results and those obtained by lasers. Note that a single laser no longer provides all the irradiation wavelengths required for the latest fluorescent fingerprint techniques. (Three different lasers are used in the correspondent's recent paper [*2*]. In contrast, a single xenon arc lamp, suitably filtered, can cover the whole spectral range. We have found no case where improved ridge detail from a latent print occurred using laser illumination compared with that from the xenon arc lamp.

Professor Menzel equates the cost of setting up a laser facility with that of operating two patrol cars. My answer to this is that a fully operational laser facility is 30 to 50 times more expensive than a xenon arc lamp complete with filters (about U.S. $10 000). Police administrators, conscious of the costs involved in fighting crime—our common objective—may well opt for the latter since it would provide a powerful fingerprint capability without the need to withdraw two patrol cars from service!

In conclusion, let me say that we share Professor Menzel's view that "the objective of the exercise is to develop elusive latent prints, not to produce inferior devices, even if they are cheap." We are fortunate in being able to describe a method that develops elusive latent prints better than methods available heretofore. This improvement can be achieved using a laser, but more significantly we show that the xenon arc light source, which is significantly cheaper than a laser, produces comparable results for the forensic science officer. Our science is sound and readers can judge for themselves the benefits offered by the cadmium complexation method compared with the zinc complexation technique. We are not denigrating the zinc complexation method which will work in the majority of special cases; we simply reiterate that there are advantages in the cadmium method and the examining officer should be aware of them.

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[*1*] Sinor, T. W., Wilde, J. P., Everse, K. E., and Menzel, E. R., "Lasers and Optical Spectroscopy in Questioned Document Examination," *Journal of Forensic Sciences,* Vol. 32, No. 3, July 1986, pp. 825-839.

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Apology for Name Error in Reference

Dear Sir:

The mind is an amazing thing. How often the obvious is the most difficult to discover. My sincere apologies to Dr. George G. Stott for misspelling his name in my article on p. 982 of the July 1986 issue of the *Journal of Forensic Sciences.*

The correct spelling is STOTT, not Scott. The Refs *26* and *32* should be Stott, G. G., et al. . . . I will have my prescription checked.

Dr. Francis E. Lipsinic
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"The Trial of Lee Harvey Oswald" Television Program

Dear Sir:

On 21-22 Nov. 1986, SHO Cable TV presented a five-and-one-half hour program dealing with "The Trial of Lee Harvey Oswald." This program, which was filmed and produced by London Weekend Television in London in July 1986, was shown in Great Britain, Australia, New Zealand, and Denmark, as well as the United States. *Time* Magazine, in the concluding statement of a full-page review, called it "the best TV courtroom drama ever."

This program, which will be presented again in the future by SHO Cable, is an excellent teaching vehicle for forensic scientists and trial attorneys. No matter what anyone's personal thoughts and professional opinions may be vis-a-vis the JFK assassination, the Warren Commission Report, the role of Lee Harvey Oswald, the existence of a conspiracy, and so forth, there can be no doubt about the educational value of this program. For example, it could be used in any medical-legal or forensic scientific educational course to demonstrate the purposes and pitfalls of official governmental medical-legal investigation, the practices and procedures of criminal trial lawyers, and several other salient points.

The two expert forensic pathology witnesses in the trial are Dr. Charles S. Petty and the undersigned, both of whom are Past Presidents of the American Academy of Forensic Sciences.

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Further Discussion of "Patterned Injury and Its Evidentiary Value"

Dear Sir:

I have had an opportunity to read Dr. Rao's reply (*Journal of Forensic Sciences*, Vol. 32, No. 2, March, 1987) to my critique of her paper and I have no wish to prolong a debate. However, there are still some points that are unclear and I am a bit concerned that there may be persons who do not have the advantage of working in association with a competent forensic science laboratory who might be led a bit astray by the paper.

I questioned Dr. Rao's method of making tracings on acetate "paper" as a means of preserving the evidence. She has kindly supplied me with a sample which is probably more accurately described as a soft plastic film. She now tells us that this technique was used in addition to the "usual" methods of photography and so forth. It is still not clear whether proper photomicrographs were taken. That information would be helpful.

I questioned Dr. Rao's use of the word "exact" in describing the tracing of the injury. She replies that it is as "exact as it can be." The Oxford Dictionary defines "exact," in part, as: "precise, accurate, strictly correct." My point was, and is, that a hand drawn tracing, while it may be quite useful, is not precise or strictly correct, particularly when accomplished in the way described. A difference of opinion.

I also questioned the use of a Play-doh cast (a mirror image of the sole of the shoe) and that was the main point of my letter. She hasn't responded to that point so we are left wondering why it was done. I questioned the use of Play-doh as an improper material for forensic science use because of its lack of fidelity of size. She justifies it for use by one who is in a small community, with limited resources. Having regard to the fact that one can obtain excellent, high fidelity casting materials for under $40.00, I maintain my opinion. If one cannot afford that much money, one should be looking for assistance from another agency. We must not compromise the integrity of forensic science for the sake of a few dollars.

I queried the degree of certainty she was able to arrive at as a result of this procedure. It sounded like she was saying that it was her opinion that the specific shoe made the mark. She now explains that she testified that the injury pattern "was consistent with" having been made by the shoe and that answers that question. I will refrain from opening a new debate over the propriety of using the expression "consistent with" when testifying before a jury.

I am indebted to Dr. Rao for taking the time to respond to my comments. I am sure we all agree that clarity and precision are important in forensic science reports.

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