

## Letters to the Editor

### Discussion of "The Bite Mark Standard Reference Scale—ABFO No. 2"

Dear Sir:

I am responding to a paper by Hyzer and Krauss, "The Bite Mark Standard Reference Scale—ABFO No. 2," which appears in this issue of the *Journal* (p. 498). I realize that it is a bit unorthodox for a comment on a current article to appear in the same issue of the journal in which it appears, so I thought an initial word of explanation was necessary. I was asked to review the authors' paper for publication, and apparently they disagreed with my comments to such an extent that the article will be published without regard to my suggested revisions. I would like to thank you, therefore, for the opportunity to respond in this manner; I feel that in this sort of instance, such a response is well within the scope of fulfillment of your editorial responsibilities. My comments are, for the most part, a rephrasing of the comments I sent in review of the article, and some of them will read like a review rather than a standard letter to the editor. This is not a "standard" letter to the editor, however, so I hope the members of the Academy will bear with me.

First, I feel that the first paragraph of this paper is misleading because of its definition of "metric analysis." All bite mark analysis of which I am aware employs both metric analysis (measurements) and what the authors call "associative comparisons" (pattern recognition). The fact that some analyses are "refined" (here the authors refer to computerized axial tomography (CAT) comparisons, which depend on complex technology, but are not necessarily "refined," anymore than any other comparison technique) does not mean that they automatically become "quantitative in nature." Quantitative means involving metric measurement (measurement of distances, or some other quantifiable characteristics). Surely the authors recognize that there are many unquantified decisions that go into all bite mark recognition, since human skin moves when bites are made, and thus bite marks can never be assumed to be mirror images of "solid" dentition with absolute confidence.

While a faithful photogrammetric reference scale is certainly desirable, it may not be necessary to use a "standard" scale, the one they suggest or any other, in photographing bite mark evidence. Using a workable and adequate photo scale is, however, a laudable goal. Unfortunately, it is not realized often—photographs of bite mark evidence are rarely taken by ABFO members, but rather by law enforcement and medical examiner's office personnel. The odontologist rarely has the opportunity to inspect and photograph the actual bite mark evidence her/himself.

Using a mirror to insure parallelism between the scale and film plane is an interesting and workable idea. If this is done properly I doubt that very much interpolation and rectification would need to be done. After all, the bite mark will *never* be perfectly parallel to the film plane, unless there is some portion of the human anatomy that is perfectly flat during both biting and photography. In this regard, I am not sure that I completely understand the authors' use of a clip and arm jig to hold the ABFO No. 2 scale on or above the bite mark. On what plane would it be held—the plane that the photographer judged was "nearest" to the plane of the bite mark? I have never come across a bite mark that was flat, so one would have to approximate.

I do not understand how shadows are going to be prevented by aligning a spotlight or flashlight along the same axis as the flash illumination. This would cause the same shadows that the flash would. To prevent shadows, the "extra" illumination would have to be along other axes, to shine light "under" the scale. The more light sources, the better—one would

have to arrange them while looking through the camera to illuminate all areas upon which the flash causes shadows.

Are the accuracy figures the authors give derived from measurement of the printed ABFO No. 2 scale? Plus or minus 1% is a poor accuracy for most stable-base film printing, although my feeling is that in the final analysis this does not really matter because of the practical contingencies of *metric* bite mark comparisons. As an experienced photogrammetrist, I am usually only able to measure with a precision of about 1% from "average" forensic science photographs—and sometimes far less accurately depending on photo quality, the size of the image being measured compared to the actual object, and the like. Most measurers will not do that well, even with sophisticated measuring equipment.

In terms of how to rectify photographically the resultant image during printing, I am not sure whether (if the photograph is taken very close to parallel) any forensic scientist or photographic technician is going to be able to measure differences between the circles on the ABFO No. 2 scale being "truly round" *versus* slightly nonround. This is especially true in the darkroom where low enlarger light levels make measurements of any sort extremely difficult. I seriously doubt whether anyone in the darkroom can insure correspondence between the size of circles or scales within 1% except by the purest chance.

The use of a perspective scale, aided by scribing grid lines guided by the right angled legs of the ABFO No. 2 scale, is a very good idea. Such a perspective scale need not be accompanied by any photographic rectification in the darkroom, but can be used directly with prints made from an enlarger which is arranged so that the plane of the negative is parallel to that of the printing paper.

Rectifying prints by tilting the paper frame is possibly a good idea in theory, but in practice it is difficult unless one has an aerial photographic printing setup by which increment screws control the tilt of the paper frame. I have experimented with such rectification myself, and have seen other photographers attempt it, and it is truly amazing (if somewhat less than professional) to see how many oddly shaped objects that one can lay hands on in the dark can be placed under the enlarging easel without ever bringing it to *quite* the proper tilt.

There is an even more problematical aspect of two-dimensional rectification—it will correct only for linear tilt in two dimensions. This is how aerial photographers use easel-tilt rectification; they must correct for tilt caused by the inertia of a turning airplane, and acceleration, which are two-dimensional. But tilting the paper plane during printing will not correct for "distortions" caused by irregularities of the surface being photographed. Only through the use of stereometric photographs and photogrammetric plotting can such "errors" be compensated for.

Aerial photographers have the same kinds of "distortion" problems to deal with as do odontologists. Their subject is not flat, but has hills and valleys, which are not arranged in a planar way, but which of course vary with the topography. They cannot correct for them by tilting the printing plane, and they recognize that their attempts to do so are simply approximations. To suggest that easel-tilt rectification, aided by the ABFO No. 2 scale or not, corrects all of (or even most of) the "distortions" in a photograph would be highly misleading.

So the point of these authors' paper, that we need to recognize that photographs are geometric accommodations of real world, three-dimensional scenes to a two-dimensional medium (those are my words, not theirs, but I think they distill the paper's major point), is well taken. They are "distorted," if one wants to use that word, but in a regular and quantifiable way. Certainly all trained photographers recognize this, and Dr. Krauss's paper a couple of years back in the *Journal* brings this out. Using a special scale and rectifying the resulting prints, however, does not correct in any way for the "topographic" variations in the subject—that is, those geometric properties that result from the photograph being taken of a real world "scene" on a curved area of the body, and therefore having areas that are varying distances from the principal point of the lens. The ABFO No. 2 scale does not help correct for those very real "topographic" problems. Aerial photographers minimize those problems

by taking their photographs from great distances from the subject, so that the variations in relief of the subject are a small proportion of the distance between subject and camera. This could be accomplished in bite mark photography by using telephoto lenses, and taking photos from as great a distance as possible from the subject.

But all of this photographic, geometric discussion is really a "moot point" in almost all forensic science photography, particularly for most images of bite marks where the photographer *is not even, in many cases, aware that there is a bite mark being photographed*. I have worked on a number of cases involving bite mark evidence photographs, perhaps 400 over the last 10 years, and both in my testimony and that from the "other side," actual measurements have rarely been very conclusive. All odontologists depend to a greater or lesser extent on pattern recognition—the relative placement of individual characteristics of a bite mark pattern with other characteristics thereof. I do not think it can be any other way, given the fact that the method of biting and the response of tissue to that biting is almost infinitely variable in a spatial sense.

So the content of the ABFO No. 2 paper which appears elsewhere in this issue boils down to the fact that it would be ideal to use an accurate, standardized, well printed, two-dimensional (L-shaped), and versatile scale in taking pictures of bite mark evidence. I think, perhaps, that this article may imply something more to many readers, whether or not the authors meant it to. It may convey the impression that if one uses the ABFO No. 2 scale, their photographs will be more useful than if they do not. I am not sure that this is necessarily the case. I *do* think that the scale the authors have devised is just great—I would like to have a few of them myself, in case I ever got to photograph primary bite mark evidence. In my involvement with some 400 bite mark photograph cases, I have never had that opportunity.

The implication the authors leave a reader with, however, is that using their scale—or at least some highly accurate scale—is the only way to go. This is based on the assumption that measurements of the sizes of characteristics of bite marks is crucial to identification. Will their scale help correct for crucial measurements?

As a test, I measured the squares in Fig. 5 included with the authors' article. I found that in the most deviant of those from square, there was an approximately 0.3-mm difference between  $x$  and  $y$  dimensions, or about 3%. Was it rectified? Could one use the differences in the dimensions of the squares to reach some sort of meaningful rectification of distances on the bite mark evidence shown in the picture? I measure an "average" cross-bite mark distance for the three or four marks shown in the photograph as being on the order of 26 to 29 mm. Of this distance, 3% is on the order of 0.8 mm. Is 0.8 mm going to be taken as some sort of "diagnostic" difference in deciding whether this is one person's bite or someone else's? *Of course not*, the odontologists are going to look at the individual characteristics of the marks, perhaps those turned in edges of the lateral incisors or some of the spaces between the teeth, to attempt to match them to, or exclude them from, suspect dentition. Even on a child's abdomen, the skin moves an appreciable amount when bites are made. I am sure they can manage to match this evidence to the suspect's dentition, but it is not going to be done on the basis of "metric" data. It will be done using pattern recognition.

In short, I felt that this paper should have been somewhat redirected to be appropriate for the *Journal*. Of course one wants to have the best possible scale in the photographs they use for evidence, and the ABFO No. 2 scale that the authors present is just fine, in my estimation anyway. Most forensic scientists would discuss the value of the scale that they find in their photographs (which is out of their control in the vast majority of cases), and use the measurements they make from it to determine whether the bite mark was, say, from an animal or a person, or perhaps to confirm that the biting was done by an adult rather than another child (a very important consideration in suspected child abuse cases). Then they would go on to use individual characteristics to show that their bite mark was created by the suspect, or that it was not. There have recently been a number of papers on scales and their use and significance in the *Journal*. I would rather see some kind of discussion of individual characteristics

and pattern recognition, how characteristics are matched with other evidence, and how to quantify that portion of the burden of evidence. For instance, I feel that a currently very important issue in bite mark identification is the use and abuse of overlays—hand drawn or otherwise produced—of occlusal surfaces placed on top of photographs of a bite mark as evidence of the “consistency” of the two.

Dozens of learned expositions on how photographs are “distorted,” how one must have good scales, and how one must interpret them could be written. In and of itself, this article is not wrong or bad in any way. It implies that one scale is better than another, however, and my fear is that it will be used in court to somehow *more* than imply this. As AAFS members and forensic science professionals, we have an even greater responsibility to insure that such implications do not impede justice than we do to make sure we use the best possible photographic scales.

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#### **Discussion of “A Guide to the Rapid Screening, Identification, and Comparison of Synthetic Fibers in Dust Samples”**

Dear Sir:

It has been called to my attention that I made a significant omission in my recent article [1]. In writing my paper I failed to point out that concepts for the identification of fibers in a single liquid were first proposed by Fong [2]. In my paper I cited Mr. Fong’s article as one of the two most practical approaches to the identification of synthetic fibers. However, I did not point out the similarity of our approaches. Dispersion staining allows an experienced microscopist using the single liquid recommended by Fong to obtain definitive identifications quickly of synthetic fibers without the need for birefringence measurements.

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#### *References*

- [1] Petraco, N., “A Guide to the Rapid Screening, Identification, and Comparison of Synthetic Fibers in Dust Samples,” *Journal of Forensic Sciences*, Vol. 32, No. 3, May 1987, pp. 768-777.
- [2] Fong, W., “Rapid Microscopic Identification of Synthetic Fibers in a Single Liquid Mount,” *Journal of Forensic Sciences*, Vol. 27, No. 2, April 1982, pp. 257-263.

#### **Discussion of “A Unique Missile in a Homicide Victim: The Brenneke Shotgun Slug”**

Dear Sir:

I would like to give credit to the firearms examiner at the Metro-Dade Police Department Crime Laboratory who made the definitive identification of the Brenneke Shotgun slug on the material taken from the deceased at autopsy (*Journal of Forensic Sciences*, Vol. 32, No. 5, Sept. 1987, pp. 1435-1439).

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