Letters to the Editor

A Discussion of "Ultraviolet Radiation and Its Role in Wound Pattern Documentation"

Dear Sir:

The authors of this article (*Journal of Forensic Sciences*, Vol. 37, No. 6, November 1992, pp. 1466–1479.) are to be congratulated for an academic presentation bringing to the reader in one paper important information obtained from the literature of many scientific disciplines.

Personal communication with one of the authors (R.E.B.) verifies that the information represented in many of the figures was not original. It would have been very helpful to the reader if the source of the information depicted was appropriately credited within the figure's caption.

On page 1470 the author's discussion of a challenge presented in my 1983 presentation [*1*] neglects to properly assess the value of his "discovery" and work presented at the 1984 AAFS meeting [*2*], then published in 1987 [*3*]. The "discovery" mentioned is post traumatic hyperpigmentation, long recognized in the field of dermatology. Hempling (in 1974) and Ruddick (in 1981) published on the subject in the English literature [*4,5*]. The use of reflective ultraviolet photography for tissue injury pattern identification was first brought to the attention of the American forensic community during my 1983 presentation [*1*] and was published in 1985 [*6*]. The value of Dr. West's work was not as a "discovery," but as an independent verification of a previously presented technique and application. This important verification helped to fulfill the necessary scientific predicates required for the scientific community to legitimately adopt the use of the technique in tissue injury pattern identification.

The basic discussion of focus shift presented in the article (p. 1474) is only valid for lenses without chromatic aberration correction (simple lenses). Most modern high quality cameras use an achromatic compound lens (color focus correction for two colors, blue and red) to achieve acceptably sharp color photographs. With an achromatic compound lens you must correct for the difference between visual point of focus and UV point of focus by shifting *in the same direction* as required for infra-red.[1,2,3] With an achromatic compound lens, the article's recommended focus shift, opposite to the direction required for infra-red, actually decreases the resolution instead of improving it. The February 1993 issue of *Photo Electronic Imaging* contains an article with in-depth information on and discussion of this topic.

The reader should be alerted that the dedicated band-pass filter mentioned on p. 1475 (provided by Nikon with the quartz Nikkor UV105 F4.5 lens) transmits too much visible light to be appropriate for reflective UV tissue injury documentation. It is more suitable for UV photography of hard surface material, such as conducted in document examination.

[1]Davidhazey, Prof. Andrew, Rochester Institute of Technology, Rochester, N.Y.—personal communication.

[2]Nikon Research, provided by Robert Carruthers, Nikon Forensic Technical Services—personal communication.

[3]Hyzer, William G., Forensic Consultant in Engineering and Applied Science, Janesville, Wisconsin—personal communication.

With these exceptions, the article "Ultraviolet Radiation and Its Role in Wound Pat tern Documentation" is a valuable addition to the literature.

Thomas C. Krauss, D.D.S.
Forensic Consultant
252 F Street
Phillipsburg, KS 67661

References

[1] Krauss, T., "Photographic Techniques Usable in Bite Mark Evidence Collection," *Proceedings of The American Academy of Forensic Sciences*, 35th Annual Meeting, Odontology Section, February 17, 1983, Cincinnati, OH.
[2] West, M. H., Wallace, F. E., and Billings, J. D., "Ultraviolet Photography: Bite Mark on Human Skin," *Proceedings of The American Academy of Forensic Sciences*, 36th Annual Meeting, Odontology Section, February 25, 1984, Anaheim CA.
[3] West, M. H., Billings, J. D., and Frair, J. A., "Ultraviolet Photography: Bite Marks on Human Skin and Suggested Techniques for the Exposure and Development of Ultraviolet Photography," *Journal of Forensic Sciences*, Vol. 32, No. 5, September 1987, pp. 1204–1213.
[4] Ruddick, R. F., "A Technique for Recording Bite Marks in Forensic Studies," *Med Biol Illustr*, Vol. 24, No. 3, 1974, pp. 128–129.
[5] Hempling, S. M., "The Applications of Ultraviolet Photography in Clinical Forensic Medicine," *Medicine Science and the Law*, Vol. 21, No. 3, 1981, pp. 215–322.
[6] Krauss, T. C., Warlen, S. C., "The Forensic Use of Reflective Ultraviolet Photography," *Journal of Forensic Sciences*, Vol. 30, No. 1, January 1985, pp. 262–268.

Authors' Response

Dear Sir:

In response to Dr. Krauss remarks on our paper (*Journal of Forensic Sciences*, Vol. 37, No. 6, November 1992, pp. 1466–1479), we wish that he better understood the concept of reflective ultraviolet photography of patterned injury on human skin. We will address his many issues in order.

The figures are in fact all original. As noted in the text at page 1467, Figures 1 through 4 are taken from the pioneering work of Drs. Bachem and Reed. These graphics interpreted the essential findings from several cluttered and confusing numerical tables in their early publication. We are sorry if Dr. Krauss misinterpreted the off-hand remarks made by one of the authors to him; however, if he were truly familiar with the Bachem and Reed article he would have been able to arrive at the truth. Figure 10, again as clearly referenced in the text, is an interpretation of an illustration in an article by A. R. Williams. The other figures were designed and drawn by the authors and in fact have been used by us in our lectures since 1990.

We fail to find any mention in the article claiming credit for the "discovery" of traumatic hyperpigmentation. The discovery mentioned is the fact that UV photography can record an image of a patterned injury long after all visible traces are gone. Dr. Krauss's 1983 abstract fails to mention this. In addition, the 1985 article by Krauss and Warlen referenced in his letter only points out the five month elapsed time between exposures in the figure captions, not in the body of the text. Finally we should like to point out that although the dates are clear on those photographs, the article was not submitted until May of 1984, well after Dr. West had presented his findings in Anaheim in February 1984. In actual point of fact, the earliest documented case that we can find concerning the use of UV in patterned injuries was in 1931 by Dr. Herman Goodman [1].

The "discovery" that Dr. West would take "credit" for is that a patterned injury visible only through reflective ultraviolet photography was *not* related to traumatic hy-

perpigmentation. Dr. Krauss apparently labors under the misconception that all UV visible wound patterns are due to hyper- or hypo-pigmentation. A biopsy of the wound pattern on Dr. West's arm showed *no* change in the melanin; however, there was a concentration of hemoglobin derivatives as well as cross patched (healing) collagen fibers. It should be clear that there are numerous biochemical and histological factors involved in the transmission, reflection, and absorption of UV radiation in human skin.

We contend that the discussion of focus shift is valid for the lenses that we used at the time. Dr. Krauss contends that one should not use the simple method of refocusing (as has been accepted for years in the infrared field) to correct for UV focus shift. He refers the reader to an article in the February 1993 issue of another magazine. In that article he states that in reflective UV photography the camera must be moved *away* from the subject to compensate for UV focus shift. A table of values for camera movement ranging from 1.2 mm to 150.4 mm is given dependent upon the lens and magnification chosen. In every case this movement is *away* from the subject. In his letter to us he finds fault with our method and suggests that if one chooses to refocus the lens rather than move the camera, one should do so in the same direction as for infrared. However, every lens that we own would require the user to move the camera *toward* the subject to compensate for infrared focus shift—the opposite of the correction he states is required for UV focus shift. Again the astute reader will remember that we recommended thorough calibration of each individual camera system and the lens refocusing was merely a starting point. Figure 5 in the article illustrates that the focus shift lines cross near the UV range dependent upon the type of glass comprising the lens elements. Furthermore, under field conditions we doubt that one could confidently reposition the camera a mere 1 or 2 mm while keeping the subject still, particularly if the subject was alive. Again as outlined in the article a smaller aperture will obviate the entire problem as will the use of the Nikkor lens (if allowed by the budget). Frankly, focus shift has not been a serious problem in our work.

Finally, we have never recommended the use of the Nikon brand dedicated band-pass filter. Since one can "see" through it, it should be obvious that it is unsuitable for the task at hand. We have always recommended the use of the Kodak Wratten 18A filter.

We hope that this clears up any questions or misconceptions arising from this article.

Michael H. West, D.D.S.
Robert E. Barsley, D.D.S., J.D.
School of Dentistry
Louisiana State University
Medical Center
1100 Florida Ave.
New Orleans, LA 70119

Reference

[1] Goodman, H., "The Use of Ultraviolet Light in Criminal Investigations," *Police Journal*, Vol. 4, 1931, pp. 225–231.

A Discussion of "The Detection and Documentation of Trace Wound Pattern by Use of an Alternative Light Source"

Dear Sir:

The authors of this article (*Journal of Forensic Sciences*, Vol. 37, No. 6, November 1992, pp. 1480–1488) are pioneering a new technique with important potential for tissue injury pattern evidence collection from human tissue. The article provides an introduction

to a technique, but does not provide the basis for its acceptance by the legal or forensic scientific community.

"Alternate light" or monochromatic high intensity light has been used in criminalistics to locate evidence which is then identified by other means. In finger prints or foot prints, when a fluorescent foreign material, such as in a hand lotion, might be present or when a fluorescent chemical wash is applied by the examiner, the technique can be used for pattern identification. Medical laboratories have taken advantage of fluorescence in certain analytical procedures, but apparently this is the first time that blue (450 nm) "alternate light" has been used in an attempt to locate and delineate an area of physiological change occurring in tissue, particularly traumatized tissue. The use of this technique to locate and define tissue injury patterns is a new application and appears to be entirely different from any previous "alternate light" application.

The adoption of any new technique and the interpretation of its results must be carefully scrutinized by the forensic scientific community. In this particular case, to gain acceptance in accordance with the scientific method, the biological basis for this new technique must be fully understood and proven, the reliability of the procedure verified, the resulting information reported in a refereed scientific journal, such as the *Journal of Forensic Sciences*, and it should then be possible to independently duplicate the findings. The suggested technique has not fulfilled the requirements of the scientific method.

On occasion, experts have expressed the opinion that the suggested "alternate light" technique is an extension of the accepted reflective ultraviolet photographic technique and that any difference is merely one of semantics [*1*]. Admittedly, both use established photographic principles and both report using the skin's response to trauma. However, that is where the similarity ends.

First, reflective UV photography is documentation of human epithelium's absorption and reflectance of *invisible ultraviolet light energy—UVA* (320 to 400 nm) using *reflective photography*. The suggested "alternate light" technique is documentation of the human epithelium's fluorescent response to *visible blue light energy* (450 nm) using *fluorescent photography*. Second, the absorption and reflectance characteristics of skin to UVA are *well explained and documented* in the medical literature [*2*]. Penetration of the skin by visible blue light (450 nm) is confined to the epithelium [*2,3*] and there appears to be *little or no explanation* in the literature of epithelial fluorescence, particularly regarding the influence of trauma.

In summary, the energy wave lengths used are different, the photographic principals used are different, and there is an explanation with significant supporting documentation for the reflective ultraviolet photographic technique, but no explanation for the "alternate light" photographic technique. Acceptance of reflective ultraviolet photography does not sanction the acceptance of "alternate light" photography for tissue injury documentation.

The justice community must base acceptance of this new evidentiary technique on compliance with the scientific method and not on empirical assumptions, especially when a person's life or freedom is at stake. The technique must also meet the legal predicates exemplified by the "Frye test." Such was the case in Mississippi, when a Kemper County Circuit Court ruled in a "Frye hearing" on evidence obtained using the "alternate light" technique [*1*].

Evidence examined by the court included photographs presented in the article as case number one: Fig. 4A, a knife allegedly used in the homicides and Fig. 4B, an "alternate light" photograph showing an *experimentally* produced pattern on a model's hand (testimony indicated it was observable for 30 minutes or less). Not included in the article were photographs, made available by the prosecutor and entered into evidence by the defense, identified as taken of the defendants hand ten days after the crime using the "alternate light" technique.

The court ruled that the "alternate light" evidence was inadmissible. Six days later

the indictment was dismissed by the District Attorney relieving the defendant of all capital charges. When challenged, the suggested "alternate light" technique has not met the requirements for either the legal or scientific acceptance.

It is the intent of this letter to stimulate the authors, and others interested in the technique, to accept the responsibility for meeting legal and scientific requirements. The necessary credibility must be established for the legal and forensic scientific community to legitimately accept the use of "alternate light" in tissue injury pattern documentation.

I would offer a suggestion to improve the technique. The article suggests using Omnichrome's yellow plexiglass placed in front of the camera lens acting as a narrow band pass filter. Kodak's number nine filter, an optical grade filter, has the same transmission characteristics as the suggested plexiglass. Substitution of an optical grade filter for the plexiglass would enhance the photographic definition.

Thomas C. Krauss, D.D.S.
Forensic Consultant
252 F Street
Phillipsburg, KS 67661

References

[1] State of Mississippi vs Larry Maxwell, Circuit Court of Kemper County, Cause Number 5139, December 4, 1992 (proceedings held in Meriden, MS).

[2] Regan, J. D. and Parrish, J. A., *The Science of Photomedicine*, Plenum Press New York and London, (1982).

[3] West, M. H., Billings, J. D., and Frair, J. A., "Ultraviolet Photography: Bite Marks on Human Skin and Suggested Techniques for the Exposure and Development of Ultraviolet Photography," *Journal of Forensic Sciences*, Vol. 32, No. 5, September 1987, p. 147.

Authors' Response

Dear Sir:

In response to Dr. Krauss's letter, (*Journal of Forensic Sciences*, Vol. 37, No. 6, November 1992, pp. 1480–1488) we were quite surprised that he would find narrowband illumination (NBI) of any interest. In fact in an affidavit dated 21 April 1992 he states that he has neither used nor has he ever written or presented any material on the subject.

He is correct in pointing out that NBI has been used in criminalistics for quite some time. The article in question was written some 24 months ago and can be considered "current" only within that time frame. The methods and techniques suggested were the first publication and presentation of the adaptation of this well-accepted forensic technique to patterned injury on human skin. As of this writing, several other forensic scientists have also conducted research in this specific field and have in fact presented their findings at the Annual Meetings of the AAFS in Boston (1993) and New Orleans (1992).

Dr. Krauss seeks to confuse the issues by placing words in our mouths which we neither uttered or printed. We have never suggested that NBI is an extension of reflective ultraviolet photography. While it is true that certain shortcomings in that field prompted us to look in the direction of NBI, the technique itself is more nearly an extension of fluorescent UV photography. This a field with which Dr. Krauss is quite familiar and we fail to understand why he (and by extension the attorneys for the defense) continue to attempt to perpetrate this untruth.

There are numerous references in print yielding an explanation of biofluorescence of the skin. In an article on NBI currently (4/93) being finalized for submission to JFS for review, we list in excess of 60 citations on this specific subject. To quote from that article,

> "[t]o completely describe the histochemical, and absorption spectrum of Narrow-band illumination . . . of wounds is beyond the scope of this paper. It is sufficient to state that the phenomenon of NBI of wounds on human skin is as well based in science and is understood as well if not better than UV photography."

In fact, Case Three in the November 1992 article alludes to the fact that the patterned injury is not merely an epithelial event. The careful reader would realize that the most likely explanation for embalming fluid destroying the pattern would operate at a level other than epithelial.

As to the question of the Frye hearing in Mississippi, in this very early case, Dr. West was unable to capture the image on film, although the presence of the pattern was verified by other observers at the time. In essence the judge did not dismiss the technique, he merely ruled that if Dr. West could not produce good evidentiary photographs, then he would not be allowed to testify. For a number of reasons (certainly including that one) the case was dropped by the District Attorney. NBI on skin has been admitted into evidence in cases in Mississippi, Louisiana, and even Kansas.

As to the experimentally produced patterns, they were exactly that, as the article clearly stated. There was never any attempt to duplicate the wound or injury itself, only the pattern. Again the article is quite specific on this point.

The authors are confident that they have addressed the fundamentals of and have dutifully shouldered the responsibility for meeting the scientific and legal requirements implicit in the forensic use of NBI. No one enters a courtroom unaware of the heavy burden of responsibility placed on the efforts and conclusions of the expert witness. We would hope that Dr. Krauss would shoulder some of this responsibility himself by conducting research in the areas he is so quick to criticize. Some odontologists seem condemned to live their lives in the past, replaying old cases in an attempt to change outcomes that they are not pleased with.

For example, the suggestion by Dr. Krauss that we substitute a Kodak number 9 filter in place of the yellow plexiglass highlights his failure to understand the basics of NBI. This particular long-pass filter is shifted nearly 100 nm toward the blue end of the spectrum and would be subject to reflected light spillover resulting in washout of the fluorescent image. Our continuing research, funded in the competitive grant arena, is addressing this among other refinements to the technique.

In closing, we wish to reiterate that the goal of the November 1992 publication was to stimulate others in the field to conduct independent research on this and related techniques. We are quite pleased that some accepted the challenge and we hope that the forensic science community will profit from our efforts.

Michael H. West, D.D.S.
Robert E. Barsley, D.D.S., J.D.
Clinical Assistant Professor and Professor, respectively
School of Dentistry
Louisiana State University, Medical Center
1100 Florida Ave.
New Orleans, LA 70119

A Discussion of "The Detection and Documentation of Trace Wound Patterns by Use of an Alternative Light Source"

Dear Sir:

The authors of the article (*Journal of Forensic Sciences*, Vol. 37, No. 6, November 1992, pp. 1480–1488) deserve commendation for their successful publication about this new method of forensic investigation. It helps define the limits of this new technology.

In my opinion, some explanatory notes are in order to help clarify the reader's interpretation of the case in question. The use of an alternate light source, (ALS) has recently been applied to photographic documentation of pattern injuries on human epidermis. To date it has been referred to as alternate light, alternative light, high intensity monochromatic light and narrow band illumination. They are all synonymous. Forensic investigation using monochromatic light evolved from dermatological research conducted with reflected ultraviolet light energy, [*1–3*] and more recently high intensity lasers [*4,5*].

In Michigan vs. Victor K Malone, #8568499 FC, Jan. 6, 1986, photographic evidence of a lasered fingerprint that had been fumed with cyanoacrylate and washed in Rhodamin 6G was deemed admissible during a Frye hearing on this scientific technique.

Fluorescent photographic documentation of pattern injuries, particularly bite marks, requires no cyanoacrylate fuming or fluorescent chemical processing. The fluorescent organic matrix of the human epithelium and underlying dermis are known [*4,6*], however little scientific investigative data exists on the cause-effect relationship of traumatic injury and tissue fluorescence.

This article presents some experimentally reproduced material: A photograph of the hand of a volunteer, (not of the defendant), Fig. 4B, depicting an experimentally reproduced pattern of the rivets from a knife handle, Fig. 4A, (the alleged murder weapon); but not the actual ALS photographs taken of the defendant's hand which were submitted to the court.

It is my personal experience that indeed, ALS illumination frequently can make subdermal hemorrhagic response to injury appear more enhanced than when viewed under full spectrum light. What is important to note is that the evidence is already visible under normal lighting conditions, not invisible to the naked eye. The article is misleading to the reader in that the message herein is that the wounds were not visible to the naked eye when originally viewed, but became visible when scanned with alternate light. Yet there was no photographic substantiation of this in the publication.

The principles of the Scientific Method demand testing a hypothesis with independent corroboration of results in order for a new scientific technique to be acceptable to the judicial arena. What is needed is a proven explanation of the biological mechanics of fluorescence and a standardized technique for fluorescent photography, corroborated and described in the literature. Then and only then will ALS documentation of pattern injuries become an important and useful adjunct to forensic investigation.

Gregory S. Golden, D.D.S.
Coroner/Odontologist
San Bernardino County
77 East 7th St.
Upland, CA. 91786

References

[*1*] Bachem, A. and Reed, C. I., "The Penetration of Light Through Human Skin," *American Journal of Physiology*, Vol. 97, April 1931, pp. 86–91.

[*2*] Hempling, S. M., "The Applications of Ultraviolet Photography in Clinical Forensic Medicine," *Medicine Science and the Law*, Vol. 21, No. 3, 1981, pp. 215–322.

[*3*] Krauss, T. C. and Warlen, S. C., "The Forensic Use of Reflective Ultraviolet Photography," *Journal of Forensic Sciences*, Vol. 30, No. 1, Jan. 1985, pp. 262–268.

[*4*] Anderson, R. R. and Parrish, J. A., *"The Science of Photomedicine,"* Plenum Press New York and London, (1982)

[*5*] Tang, G. C., "Spectroscopic Differences Between Human Cancer and Normal Lung and Breast Tissues," *Lasers in Surgery and Medicine*, 1988, Vol. 9, p. 295.

[*6*] Guilbault, G., "Practical Fluorescence," *Modern Monographs in Analytical Chemistry*, 2nd Edition, University of New Orleans, Marcel Dekker, Inc., New York, Basel, Hong Kong, 1991.

[7] State of Mississippi vs. Larry Maxwell, Circuit Court of Kemper County, Case #5139, December 4, 1992.

Authors' Response

Dear Sir:

We value Dr. Golden's comments on narrow-band illumination (NBI) as he has also conducted research on the topic. It's interesting that some critics only wish to debate, while others direct their energies to researching a problem to expand the boundaries of knowledge. We also feel that it is important to realize that the article referred to was written nearly 24 months ago and covered only our earliest work conducted prior to that time.

In response to his comments about the photographs of the experimentally reproduced patterns, the article made it abundantly clear that they were exactly that and nothing else. The point of the paper was to report on the phenomenon and hopefully encourage others to investigate and corroborate our early results. Case One was in fact one of the earliest attempts to capture the NBI image of the patterned injuries on film. Due to the extremely low light levels we were unsuccessful; however, the fact that the patterns were visible was verified by witnesses to the procedure as well as documentary note-taking and sketching. In Case One, as noted in the text, the patterned injury was in fact not visible or at least not recognizable to the naked eye. It is essential that the reader understand that although NBI will often yield a greater extent of, or clarity to, a pattern already visible, *some patterns are only visible under NBI.* In particular, imprints in the palm of the hand (West Phenomenon) and the imprints on the chests and backs of victims of Shaken Infant Syndrome (SIS), are generally not visible under standard illumination.

Our ongoing research in this field is directed at finding the optimal photographic technique(s) to capture these images. To date, CCD videocameras of the low lux variety seem to offer the best potential for recording the most realistic and useful image as actually visualized by the human eye.

A reference to the previous letter of response to Dr. Krauss concerning NBI should answer any further questions Dr. Golden has as to the biological basis for the observed fluorescence in these cases. We contend that NBI documentation of patterned injuries is already a useful and reliable adjunct to forensic investigation.

Michael H. West, D.D.S.
Robert E. Barsley, D.D.S., J.D.
School of Dentistry,
Louisiana State University,
Medical Center,
1100 Florida Ave.
New Orleans, LA 70119

A Discussion of "Deaths Caused by Lightning"

Dear Sir:

Drs. Lifschultz and Donoghue [*1*] presented a well-written article regarding the investigation of lightning deaths. The enclosed comments are intended to contribute more to the person investigating a potential lightning fatality.

The authors list voltage as being between 10 and 100 million volts. However, the current level is responsible for the level of damage. Uman's data lists lightning current as being between 10,000 and 110,000 amperes [*2*]. While knowledge of a current level may seem to be trivia, the magnitude of currents involved lead us intuitively to better investigation techniques.

An outdoors lightning victim may have on his person electronic equipment, such as a watch, radio, telephone, tape deck, or pager. The current passing through the human body from lightning will generate a large magnetic field, just as it does with any conductor [*3*]. This magnetic field will attempt to induce currents in electronic gear, with damage possibly occurring. This engineer examined a pager and a watch removed from a lightning victim, and both showed no gross signs of lightning damage, such as arcing, beading, or flashover. Internal examination found both to be nonfunctional, due to semiconductors exhibiting 'punch through,' an indicator of lightning damage.

The authors advise that phones and electrical equipment be examined inside a building if lightning is suspected; this is sage advice. In some instances, the only hint of damage may be due to a piece of equipment no longer working. In other instances, the damage may be subtle, such as a weakening of dielectric strength on insulators within equipment. A 'megger' can detect such overstressing. As with the watch and pager, lightning current does not have to pass directly through an object in order to wreak havoc.

The authors comment that metallic objects can be examined for signs of arcing. A further test would be to check for magnetization using a compass or a Hall effect probe. The magnetization is a direct result of thousands of amperes flowing nearby. Obviously, aluminum will not be affected, while steel will be the most telling. If, however, arcing occurred on the metal, magnetization may not be detected, in that the Curie temperature will have been reached, destroying magnetic alignment within the metallic object.

Mark E. Goodson, P.E.
Consulting Engineer
3370 Forest Glen
Denton, Texas 76205

References

[*1*] Lifschultz, B. D. and Donoghue, E. R., "Deaths Caused by Lightning," *Journal of Forensic Sciences*, Vol. 38, No. 2, March 1993. pp. 353–358.
[*2*] Uman, M. R., *Lightning*, Dover Publications, New York, 1984.
[*3*] Sears, F., *Electricity and Magnetism*, Addison-Wesley Publishing, Reading, Massachusetts, 1946.

Authors' Response

Dear Sir:

We thank Mr. Goodson for his comments on our paper, and for bringing this additional information to the attention of readers of the journal.

Barry D. Lifschulz, M.D.
Deputy Chief Medical Examiner
Edmund R. Donoghue, M.D.
Deputy Chief Medical Examiner
Office of the Medical Examiner of Cook County
Cook County Institute of Forensic Medicine
2121 West Harrison St.
Chicago, IL 60612-3705

Commentary on the "Ceiling Principle"

Dear Sir:

The National Research Council report [*1*], which was issued in May of 1992 addressed many issues of concern involving both PCR and RFLP analysis of DNA for forensic

purposes. The report's examination of genotype frequency determinations in various populations is a response to the courtroom testimony and published articles of a number of population geneticists [2,3]. The NRC report has apparently had a great deal of impact resulting in a decision by the Supreme Judicial Court of Massachusetts in July of 1992, that DNA profiling results are inadmissible in the State of Massachusetts on the grounds that the technique is not generally accepted in the relevant scientific community. The Court noted that a ceiling-principle, as recommended by the NRC report, had not been applied in calculating genotype frequencies. The FBI published its response to the NRC recommendations in Crime Lab Digest [4] as well as an explanation of how the FBI calculates ceiling frequencies when requested to do so [5]. Devlin, Risch and Roeder also published a response to the NRC report [6] specifically addressing the problem of the statistical evaluation of DNA Fingerprinting findings. The primary issue concerns the question of subpopulations (population heterogeneity). The authors of the NRC report have made their recommendations on how to estimate genotype frequencies based upon the existence of such genetic diversity. As of this date, the debate continues regarding how significant the diversity truly is in various ethnic groups.

The NRC has suggested using a ceiling principle when dealing with data bases in forensic DNA cases. The ceiling principle calls for drawing random samples of 100 persons from approximately 15 to 20 homogeneous populations and taking as the ceiling frequency the largest frequency found in any of these populations. In this way the calculated genotype frequency will be the largest value possible.

Because their suggestion almost automatically results in a very conservative estimate of genotype frequencies, we tend to agree with the overall concept. However, we do find that the use of the ceiling principle, as described in the report, can sometimes result in questionable conclusions. It should be noted that the comments made above pertain to population data bases regardless of the particular ethnic group under study, regardless of the method used to analyze DNA (such as, PCR, RFLP, AmpFLP) and regardless of whether the alleles are discrete or non discrete (form a continuum).

Although the following example is rather extreme for highly polymorphic VNTR systems, it is possible that in other systems this can arise. If for a particular gene, allele #1 has a frequency of 0.71 in one genetically distinct group and allele #2 has the same frequency in some other genetically homogeneous group, any suspect who is a heterozygote for these two alleles will be assigned a match probability of 1.01 ($2pq=2 \times 0.71 \times 0.71$) with any biological evidence having the same genotype. This makes the gene worthless as a means of identifying any such heterozygous individual regardless of the particular population to which the individual belongs.

An actual example that closely mirrors the above problem involves genotyping with the discrete allele system HLA DQ alpha [7]. Assume that both the biological evidence found at a crime scene and the suspect's blood specimen match and the heterozygous genotype is (1.2,3). The suspect is of European-American descent and the probability of the genotype in the European-American reference population would be taken as equal to 2 (0.197)(.201) = .079 or 7.9%. Under the NRC ceiling principle, we would take the value of (0.469) for allele 1.2 from the Indonesian population and (0.466) for allele 3 from the Japanese population and would arrive at a heterozygote frequency of 2 (.469)(.446) = 0.418 = 0.42 or 42%. It follows that the use of the ceiling principle may require the analysis of so many discrete allelic systems that there may often be insufficient sample to individualize the evidence beyond a reasonable scientific doubt.

In conclusion, the ceiling principle although arbitrary in several ways, is highly conservative and as a result, any bias in calculating genotype frequencies would tend to favor the defendant in a criminal matter. The use of the ceiling principle, however, appears to be questionable for the calculation of genotype frequencies when considering certain discrete allele systems.

References

[1] Committee on DNA Technology in Forensic Science, National Research Council, *DNA Technology in Forensic Science*, National Academy Press, Washington D.C., 1992.
[2] Lewontin, R. C. and Hartl, D. L., "Population Genetics in Forensic DNA Typing," *Science*, Vol. 254, 1991, pp. 1745–1750.
[3] Lander, E. S., "Research on DNA Typing Catching Up With Courtroom Application" (invited Editorial.) *American Journal of Human Genetics*, Vol. 48, 1991, pp. 819–823.
[4] "The FBI's Responses to Recommendations by the Committee on DNA Technology in Forensic Science of the National Research Council, National Academy of Sciences," *Crime Laboratory Digest*, Vol. 19, No. 3, 1992, pp. 49–72.
[5] Budowle B. and Monson, K. L., "The Approach Used by the FBI for Calculating Ceiling Frequencies," *Crime Laboratory Digest*, Vol. 19, No. 3, 1992, pp. 84–94.
[6] Devlin, B., Risch, N., and Roeder K., "Statistical Evaluation of DNA Fingerprinting: A Critique of the NRC's Report," *Science*, Vol. 259, 1993, pp. 748–749,837.
[7] Helmuth, R., Fildes, N., Blake, E., Luce, M. C., Chimera, J., Madej, R., Gorodezky, C., Stoneking, M., Schmill, N., Klitz, W., Higuchi, R., and Erlich, H. A., "HLA-DQα Allele and Genotype Frequencies in Various Human Populations, Determined by Using Enzymatic Amplification and Oligonucleotide Probes," *American Journal of Human Genetics*, Vol. 47, 1990, pp. 515–523.

Lawrence Kobilinsky Ph.D.
Prof. Biology and Immunol.
John Jay College of Criminal Justice
City University of New York
445 West 59th Street
New York, N.Y. 10019

Louis Levine Ph.D.
Prof. Genetics
City College of New York
City University of New York
138th Street and Convent Avenue
New York, New York 10031

A Concern Associated with Single Breath Alcohol Analysis for Forensic Purposes

Dear Sir:

There are, undoubtedly, several reasons why jurisdictions choose to employ single quantitative breath alcohol concentration (BrAC) analysis for forensic purposes. These reasons may include reduced cost, protocol simplicity, local court acceptance, tradition, etc. Single analysis, however, introduces uncertainties along with some risk—particularly in jurisdictions having "per se" driving while intoxicated (DWI) statutory language.

One important risk, at least from the defendant's perspective, is that a single analysis may reveal the individual to be in excess of the "per se" level, while a subsequent measurement, or the mean of the duplicates, may have resulted in a value below the critical level. This could, perhaps, create some doubt as to whether the individual was in violation of the "per se" offense. An important question for jurisdictions performing single analysis and thereby prosecuting on that value is: what is the probability of this occurrence and is it related to the concentration at which the "per se" level exists?

An observational/retrospective study was conducted where a total of n = 39,496 duplicate BrAC results from the state of Washington for 1992 employing the BAC Verifier DataMaster (National Patent Analytical Systems, Inc., Mansfield, Ohio) were evaluated. Within these duplicates, both the first and second result were $\geq$0.01 g/210L. The data were selected from a total of 205 instruments. Cases were selected where the first measurement was greater than or equal to the hypothetical "per se" value while the second measurement, or the second measurement along with the mean of the duplicates, was less than the critical value. The proportion of cases meeting these criteria were deter-

mined at each of four hypothetical "per se" values: 0.04, 0.08, 0.10 and 0.20 g/210L. The 0.20 g/210L is included only to illustrate the relationship between proportion at risk and concentration, not that any jurisdictions would consider "per se" statutes at that level. The mean of duplicates was considered since it provides a better estimate of the individual's true BrAC, having less uncertainty than individual measurements [*1,2*]. Since the total number of cases varied at each of the four concentrations considered, the number of cases where the first measurement equalled the critical "per se" value was determined and provided the basis for comparing relative proportions at each concentration. All breath alcohol measurements were truncated to two decimal places while the means were rounded to three, resulting in either a zero or five in the third place.

Table 1 shows the results of analysis. The percentage of cases compared to the entire data set as well as the relative percentage for each "per se" concentration are shown. Cases where the mean is considered represents the situation of greatest concern since the best estimate of the individual's true BrAC (the mean of replicate analyses) is below the critical level. Circumstances where the second analysis is below the "per se" value while the mean remains above is of less concern and explained by random measurement variability. In these cases, the first single analysis reasonably approximates the sample mean as being above the "per se" level. The present data, representing the state of Washington where a 0.10 g/210L "per se" statute exists, indicates that 1.14% of the individuals would be at risk of being convicted of DWI on the basis of only one analysis while their mean BrAC (or best estimate) is actually below the "per se" level. Further, considering the relative percentages, one observes that the relative risk increases with concentration.

The simplifying assumption in this analysis is that the breath alcohol results alone are the basis for prosecution, while this is typically not the case in the real world. One must interpret the present results within the context giving rise to the data. It may not be valid to extrapolate to other jurisdictions where the BrAC distribution may be different for a variety of reasons. The results may also differ where jurisdictions have 0.04 or 0.08 g/210L "per se" statutes, thus perhaps shifting the total distribution downward. Different n at each of the critical levels considered would alter the relative proportions. In addition, one is cautioned about extrapolating the results to a workplace breath alcohol testing environment where the same results may not occur due to differing measurement context, protocol, cooperation of subjects, BrAC values incurred, etc. The truncation of results to two digits is another consideration since it will tend to bias the mean downward, thus underestimating the true population mean by some amount. The risks reported in Table 1 for the mean may actually be less if the third digit were included in the analysis. Finally, the present results do not consider that the BrAC is typically less than corresponding venous blood alcohol concentration since the critical element is the BrAC where the statutory language is thus defined.

TABLE 1—*Summary of cases placed at risk considering four "per se" concentrations relative to entire data set and each concentration.*

Critical BrAC (g/210L)	Group total	BrAC1 ≥ CB > BrAC2			BrAC1 ≥ CB > Mean		
		n	Percent of grand total	Relative percent	*n*	Percent of grand total	Relative percent
0.04	228	36	0.09	15.8	34	0.09	14.9
0.08	957	209	0.53	21.8	186	0.47	19.4
0.10	1834	554	1.40	30.2	452	1.14	24.6
0.20	1893	1156	2.93	61.1	856	2.17	45.2

NOTE: Grand Total Data Evaluated: n = 39,496.
CB = Critical BrAC.

One important observation is that the proportion of cases placed at risk due to a single analysis protocol appears to increase with concentration. The likely explanation for this is that measurement variability is proportional to concentration [*3,4*]. Lower concentrations, along with reduced variability, have relatively fewer cases where the second breath alcohol result will fall below the critical level. This would suggest that duplicate test protocols are probably quite adequate in the context of 0.08 or 0.04 g/210L statutory language.

The results of the present study should be informative for jurisdictions performing single analysis or for those considering amendments to or development of analytical protocols. One approach to reducing the risk illustrated here is to perform a second analysis in those cases where the first result is near the "per se" level (for example, 0.10 to 0.12 g/210L). Ideally, each jurisdiction should assess their own unique breath alcohol distributional characteristics within the context of their instrumentation, protocols, operators, statutory provision, etc. Without question, even within an n replicate analyses protocol, it can be argued that an $n + 1$ measurement may fall below the "per se" level. The mean, however, should be less influenced in such a case. Although the risk appears to be small (<2%) for a 0.10 g/210L statute, there are still persuasive reasons for performing duplicate breath alcohol analyses from a quality control and forensic perspective—one of which is the virtual (but not total) elimination of the risk discussed here. Further, duplicate analyses is a recommendation of the National Safety Council Committee on Alcohol and Other Drugs [*5*].

Rod G. Gullberg
Washington State Patrol
Breath Alcohol Test Section
811 East Roanoke
Seattle, Washington 98102

References

[*1*] Devore, J. L., *Probability and Statistics for Engineering and the Sciences*, Monterey, Calif.: Brooks/Cole Publishing Co., 1982, p. 12.

[*2*] Bennett, C. A. and Franklin, N. L., *Statistical Analysis in Chemistry and the Chemical Industry*, New York: John Wiley and Sons, Inc., 1954, pp. 20–24.

[*3*] Jones, A. W., "Limits of Detection and Quantitation of Ethanol in Specimens of Whole Blood From Drinking Drivers Analyzed by Headspace Gas Chromatography," Letter to the Editor, *Journal of Forensic Sciences*, Vol. 36, No. 4, 1991, pp. 1277–1279.

[*4*] Gullberg, R. G., "Determining Total Method Level of Detection and Level of Quantitation for Breath Alcohol Analysis Programs," Letter to the Editor, *Journal of Forensic Sciences*, Vol. 37, No. 5, Sept. 1992, pp. 1208–1210.

[*5*] Dubowski, K. M., "Duplicate Breath-Alcohol Testing," *American Journal of Forensic Medicine and Pathology*, Vol. 9, No. 3, 1988, p. 272.