

Letters to the Editor

Further Discussion on Discussion of "Bite Mark Impressions: A Review of Techniques and Materials"

Dear Sir:

With reference to Dr. Gerald Vale's comments, in the July 1989 issue of the *Journal of Forensic Sciences*, on "Bite Mark Impressions: A Review of Techniques and Materials" (by Benson et al., September 1988). I would like to make the following brief comments.

Dr. Vale is correct in his statement that there are several alternate techniques available for the backing of bite mark impressions. I take issue, however, with his contention that the addition of an orthopedic tape (Hexcelite) is "cumbersome, complex and time-consuming." On the contrary, this technique requires only scissors and hot water and about 8 to 10 min to complete. The plaster technique that he describes requires a mixing bowl, spatula, gauze, scissors, and water and can also be completed in 8 to 10 min. The orthopedic tape technique [1] requires only the possession of the tape and can readily be used with minimal training by odontologists, pathologists, technicians, or law enforcement personnel. It has been used in numerous cases throughout the nation.

Two other advantages should be noted: orthopedic tape is lighter than plaster and will not fracture if dropped.

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Reference

- [1] Sperber, N. D., "Bite Mark Evidence in Crimes Against Persons," *FBI Law Enforcement Bulletin*, July 1981, pp. 16-19.

Author's Response

Dear Sir:

In reply to Dr. Sperber's letter, I refer to page 1239 of the article under discussion [1]. The authors describe a bite mark impression technique in which impression material sets completely, is covered with orthopedic tape and then with another layer of impression material. "Second and third pieces of orthopedic tape with intermediate layers of impression material are added for increased rigidity."

I called the above procedure "unnecessarily complex and cumbersome" because it would result in no less than seven layers of material—three of orthopedic tape and four of impression material. Moreover, the procedure described above is not likely to be performed in 8 to 10 min, because the manufacturer of the impression material advises that the normal setting time is in excess of 5 min for each mix.¹ As noted in the article, the setting time is even longer in the case of refrigerated bodies.

To help resolve this matter, I contacted my esteemed colleague Dr. Sperber and also reviewed the article he referenced. In that article, an impression is taken and covered

¹G. C. International Corp., Scottsdale, AZ, personal communication.

by only one layer of orthopedic tape plus one additional layer of impression material. Dr. Sperber advises that a second or third layer of tape would be used only if additional rigidity were desired—to permit shipment of the impression, for example. The additional layers of tape can be added with a single mix of impression material. Dr. Sperber indicated that two layers of orthopedic tape would usually be sufficient.

I believe that this exchange of correspondence will help clarify the technique described by the authors. Also, it may save some fledgling forensic dentist from spending an unnecessarily long evening in the coroner's office.

Finally, I would remind readers that they may wish also to test other widely used impression-backing techniques, such as the gauze and gypsum method described in the July issue (*Journal of Forensic Sciences*, Vol. 34, July 1989).

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Reference

- [1] Benson, B. W., Cottone, J. A., Bomberg, T. J., and Sperber, N. D., "Bite Mark Impressions: A Review of Techniques and Materials," *Journal of Forensic Sciences*, Vol. 33, No. 5, Sept. 1988, pp. 1238–1243.

Discussion of "Hair Analysis for Drugs of Abuse"

Dear Sir:

A recent article by Baumgartner, Hill, and Bland (Baumgartner, W. A., Hill, V. A., and Bland, W. H., "Hair Analysis for Drugs of Abuse," *Journal of Forensic Sciences*, Vol. 34, No. 6, Nov. 1989, pp. 1433–1453) discusses an interesting and potentially important method of testing for drugs of abuse. In this paper the authors correctly identify the problem of drug extraction (and the related one of dealing with hair damaged by treatment) as central to the success of the technique. Unfortunately, we are informed that their methods for dealing with both these critical problems are considered "proprietary," presumably for financial reasons.

The result of this reticence is that the reader is unable to form a proper judgment of the validity and potential usefulness of the technique, nor is he able to replicate their results. From a scientific (as opposed to an advertising) point of view, this renders their paper of extremely limited value.

There may be legal difficulties as well. It will be awkward to argue that the method has achieved a general acceptance within the relevant scientific community when that community has been denied an opportunity to examine it in detail.

I believe the *Journal of Forensic Sciences* should reconsider its editorial policy and refuse publication to papers which omit information essential to evaluation of the results.

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Author's Response

Dear Sir:

Dr. Frederick Sauls, in his letter to the Editor, questions the appropriateness of publishing the review paper on "Hair Analysis for Drugs of Abuse," by Baumgartner, Hill,

and Bland, in the *Journal of Forensic Sciences*. Dr. Sauls apparently failed to recognize that our paper was part of a conference proceedings and that its submission to the *Journal of Forensic Sciences* had been solicited by the conference organizers.

Furthermore, Dr. Sauls expresses his concern that the findings described in our paper cannot be replicated because of the nondisclosure of our proprietary (patent pending) radioimmunoassay procedures. Although Dr. Sauls correctly recognizes the importance of using nearly 100% efficient extraction procedures for hair analysis, especially when treatment-damaged hair is involved, he confuses this issue with the subsequently applied method for measuring the released analyte.

As we pointed out in our paper, effective measurement of the analyte in hair can only be guaranteed if the hair is reduced to a serum-like condition by one of several recognized methods for dissolving hair. Once this has been achieved, the released analyte can be readily measured by GC/MS procedures, that is, by procedures which are generally recognized as the most applicable for forensic science purposes. In several of the papers cited in our review, as well as in our own laboratory, this approach had been used in forensic sciences cases. What is not disclosed in our paper, because of the pending patent, is how the hair solution is made compatible with radioimmunoassay procedures, a more cost-effective method applicable for mass screening purposes.

Recently, an excellent review on hair analysis was published by others (Harkey, M. R. and Henderson, G. L., "Hair Analysis for Drugs of Abuse," *Advances in Analytical Toxicology*, Vol. 2, R. C. Baselt, Ed., Yearbook Medical Publishers, Chicago, 1989, pp. 298-329). Many of our findings are cited there also without accompanying description of our proprietary radioimmunoassay technology. Obviously, neither that review nor our own review paper was intended to serve commercial purposes; rather, they are attempts to provide an overview of a newly emerging methodology.

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On the Incidence of Gambling Problems in a Substance Abuse Population

Dear Sir:

An article by Lorenz in the March 1989 issue of the *Journal [1]*, dealing with compulsive gambling and mentioning that the incidence of compulsive gambling within the adult population of the United States is from 0.77% to over 3%, leads me to note observations from our program on the incidence of gambling problems in these psychoactive substance abuse patients.

Our substance abuse program admits some 700 male veterans per year, with a full range of psychoactive substance abuse diagnoses; currently, these are mostly alcohol dependence and cocaine dependence diagnoses. A few years ago when increased attention to pathological gambling began in the literature and in treatment programs, our unit heightened its awareness of this problem, including posting a notice to patients to bring gambling problems to our attention if they wished help for them. We previously had asked about and continue routinely to ask explicitly about problems with money management and financial problems. In spite of this attention to gambling and gambling-related problems, it is very rare for us to get patients who acknowledge gambling as a problem: not more than one to three cases per year have been identified over the last several years, which gives an apparent incidence in this population of between 0.1 and 0.4%. (Our patients do report a high incidence of problems with money management and other financial difficulties, but these problems almost invariably relate to failure to

maintain jobs or to excessive spending on alcohol, cocaine, and other psychoactive substances, and not to gambling losses.)

Some possible reasons for this low incidence of pathological gambling were considered. It might be thought that our patients are reluctant to admit to problems with gambling, which some patients might be, but we regularly obtain a much higher incidence of other problems that would appear to be much more embarrassing to admit, including complaints and histories of impotence, spousal abuse, theft from family members, and a variety of legal problems. Insofar as money available for gambling might be a factor, although many of our patients approach indigence at admission, actually the majority of these patients have regular incomes much of the time. A more important factor may be the relative lack of public gambling facilities, such as racetracks and gambling casinos, in this area of the country.

It remains unclear why the apparent incidence of pathological gambling is lower in our population than in published studies, but this finding appears to warrant reporting.

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Reference

- [1] Lorenz, V. C. "Compulsive Gambling and the Expert Witness," *Journal of Forensic Sciences*, Vol. 34, No. 2, March 1989, pp. 423-432.

Author's Response

Sir:

I appreciate the issues Dr. Lacoursiere is raising and suspect that others may share his experiences. Hopefully, this will lead to greater awareness of the complexity of the compulsive gambling population.

First, a bit about compulsive gamblers. As a group they are bright, competitive people, motivated to achieve, who are convinced that money will solve their problems and that being a winner will gain them acceptance from others. They also strongly believe "I must be fully competent" (which does not permit such incompetence or imperfection as mental illness) and "I must be in full control" (which does not permit the use of alcohol or drugs).

Some compulsive gamblers do abuse alcohol. More often the patient is an alcoholic first, maintains sobriety, but turns to gambling as an alternative coping mechanism. Studies on alcoholics in inpatient, private psychiatric hospitals indicate that an approximate 20% are thus dually addicted. The incidence is much lower with drug dependency (mostly cocaine or marijuana), with closer to 5% thus addicted.

Compulsive gamblers who are not chemically dependent do, however, frequently abuse (although do not depend on) alcohol, but only during the final stage of the out-of-control phase of compulsive gambling. The alcohol at that point becomes another means of escaping the guilt, depression, and anxiety they experience.

Compulsive gamblers fear rejection. They crave acceptance. Since they have the beliefs mentioned above, it becomes virtually impossible for them to (1) admit the problem (and their inadequacies), (2) be the subject of more rejection, (3) face the possibility of being

told they must stop gambling, and (4) be left virtually defenseless in a hostile environment lacking effective coping skills.

Further, several studies have been done which indicate that compulsive gamblers are perceptive people, they realize at once when a therapist does not understand compulsive gambling, and they tend to do poorly in the therapy, if they even attend beyond two or three sessions. In hospital or group settings, they tend to touch only on "safe" issues, not on the intimate, very painful, or frightening events in their lives.

In short, alcoholism, drug abuse, impotence, spousal abuse, legal issues, and thefts from families (and employers!) are acceptable inadequacies for compulsive gamblers to admit to, especially in an all-male environment. However, to get at the real traumas and life experiences they cannot handle, the therapist not only needs to be knowledgeable about compulsive gambling but must also work with the patient in a one-on-one setting. That, unfortunately, is not possible in most Veterans Administration medical centers or psychiatric hospitals. This shortcoming is compounded when a treatment program is time-limited. Once again, gamblers feel no one really cares about them as individuals. The rejection is reinforced.

The gambler's resolve to stop on his own is short-lived. Full relapse is only one incident away.

I must dispute, posthaste, Dr. Lacoursiere's suggestion that "the relative lack of public gambling facilities, such as racetracks and gambling casinos, in this area of the country" may account for the low incidence of gamblers in the alcohol/drug abuse patient population.

Gambling, both legal and illegal, is pervasive. The next bet is only one person or one phone call away.

One does not need to drive to a racetrack or fly to Las Vegas to gamble at the casinos. All one needs to do today is to go to the nearest grocery store and buy a state lottery ticket or pick up a phone and call a bet in to a bookie. Or play the office football pool. Or go to the after-hours clubs for a night of cards or dice. Or go to bingo parlors for a 12-hour stint of bingo and all the instant games that bingo parlors offer. Bingo, incidentally, has become a multimillion-dollar parimutuel game, patterned after state lotteries.

Speaking of which, not only does Kansas have a state lottery, but also let us remember those illegal numbers, offered in the black urban areas or by bookies, who give much better odds than lotteries, on credit, and without a percentage going to the IRS. And Lotto America, the coming nationwide lottery, is also only one phone call away. Yes, even state lottery tickets can be purchased by phone, on credit, if one knows the vendor.

Perhaps Kansans are different, but in the rest of the country, fraternal organizations have many forms of illegal gambling, such as slot machines, poker machines, card games, tip jars, pull tabs, scratch-offs, and punchboards. Here in Maryland, slot machines were legalized because the Veterans of Foreign Wars and American Legions testified they could not survive financially without them.

On our national gambling hotline (1-800-332-0402) we get many calls from lower income people who are addicted to these smaller games of chance, which nevertheless also lead to lives of quiet desperation, illegal activities, and suicides.

We get calls from veterans throughout the United States. If they have private insurance to pay for therapy, we will refer them to the nearest gambling treatment center, so that the family can also receive the help it needs. If not, we invariably refer them to the few Veterans Administration medical centers which have dedicated gambling treatment programs, such as the VAs in Brecksville (Ohio), Reno (Nevada), Loma Linda (California), Lyons (New Jersey), Bay Pines (Florida), Brooklyn (New York), and Miami (Florida).

In summary, community acceptance of compulsive gambling as a treatable mental illness is at about the level where alcoholism was 20 years ago. Until the time that

community acceptance is greater, mental health professionals are more knowledgeable in the many complexities of the illness, and these professionals receive training in treating this disorder, there will continue to be a high level of denial among this patient population.

In the meantime, the increase in the number of compulsive gamblers continues.

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Discussion of "A Trajectory Analysis of Billy Dixon's Long Shot"

Sir:

I read with great interest the article in the July 1989 issue of the *Journal of Forensic Sciences* by Thornton and Shirokawa concerning the modern analysis of the historical "long shot" purportedly fired by Billy Dixon in June of 1874 [1]. Although the .50-90 Sharps was the "magnum" of its day, the authors' conclusion that the 465 grain bullet would retain more than 50% of its muzzle velocity after traveling almost a mile is incorrect. Since the gravitational effect on a trajectory is a direct function of velocity, the calculations concerning bullet drop must also be incorrect.

In computing the ballistic coefficient for the "Big 50," Thornton and Shirokawa used the Bugless and Coxe charts given by Hatcher to derive the coefficient of form, i , as 0.5 [2]. Reviewing these charts for the .50 caliber, an approximate ogive value of 2 is obtained for the blunt profile bullet used in the Sharps cartridge, which translates into a value for i of 0.95, with the effect of a .2 caliber flat nose adding to the wind resistance. An i value of 0.5 is more consistent with the spitzer shape of the .50 caliber machine gun bullet than with that of the Sharps. When the coefficient of form is inserted into the equation $C = w/id^2$, the ballistic coefficient of the bullet is calculated to be approximately 0.270, as opposed to the authors' figure of 0.523.

Without taking environmental factors into consideration, the remaining velocity after the 1538-yd (1406-m) trip would have been approximately 500 ft/s (152 m/s), the time of flight greater than 6 s, and the remaining energy closer to 260 ft lb (353 J). The decrease in velocity of 36% from that determined by Thornton and Shirokawa would result in a substantially larger total drop figure. These facts tend to indicate that the probability of Billy Dixon's "long shot" borders on the impossible.

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References

- [1] Thornton, J. I. and Shirokawa, J. M., "A Trajectory Analysis of Billy Dixon's Long Shot," *Journal of Forensic Sciences*. Vol. 34, No. 4, July 1989, pp. 1037-1041.
- [2] Hatcher, J. S., *Hatcher's Notebook*, 3rd ed., Stackpole, Harrisburg, PA, 1966, pp. 570-572.

Author's Response

Sir:

I think Julien Mason is right about the ballistics coefficient being too high. I have never been very comfortable with the ballistics coefficient of the .50 Sharps as determined from

either the Bugless and Coxe charts or the Hatcher formula, and I am willing to accept Julien's figure of 0.270.

This would, of course, cause the trajectory to be even more incredible. Still, I don't question that the shot did occur; even the Indians attested to the fact that the shot was fatal to one of the braves. I certainly agree with Julien that the shot borders on the impossible, but then that's what make it interesting.

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Discussion of "Fatal Thrombosis of Internal Carotid Artery Following Minor Blunt Trauma to the Neck"

Dear Sir:

I have read the work by Tae Lyong An entitled "Fatal Thrombosis of Internal Carotid Artery Following Minor Blunt Trauma to the Neck" in the *Journal of Forensic Sciences*, Vol. 34, No. 3, May 1989.

As to the mechanism of carotid artery thrombosis from trauma close to the neck, the author seems not to take into consideration the possibility that the initial traumatic lesion might consist in broken newly formed capillaries in a carotid intima affected by arteriosclerotic alteration, which would consequently produce intramural hematoma, breaking of the intima, and formation of thrombi, either occlusal or not.

A case of this kind was described by me in 1971 ("Sull'importanza della capillarizzazione intinale aterosclerotica nella genesi della tromboembolia corotidea traumatica," *Revista di Anatomia Patologica e di Oncologia*, Vol. 37, Nov. 1971, pp. 1-11.

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Author's Response

Dear Sir:

Intramural hematoma of the internal carotid artery due to blunt trauma to the neck was listed as one of the common injuries in my article. Such an intramural hematoma would be more likely to develop in an arterial wall already weakened and altered by atherosclerotic changes. This hematoma might then rupture into the arterial lumen, causing thrombosis.

Therefore, what Prof. Aragona described is certainly a possibility.

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Discussion of "The Differentiation of Lysergic Acid Diethylamide (LSD) from N-Methyl-N-Propyl and N-Butyl Amides of Lysergic Acid"

Dear Sir:

I found the article by Clark, "The Differentiation of Lysergic Acid Diethylamide (LSD) from N-Methyl-N-Propyl and N-Butyl Amides of Lysergic Acid," in the May 1989 issue

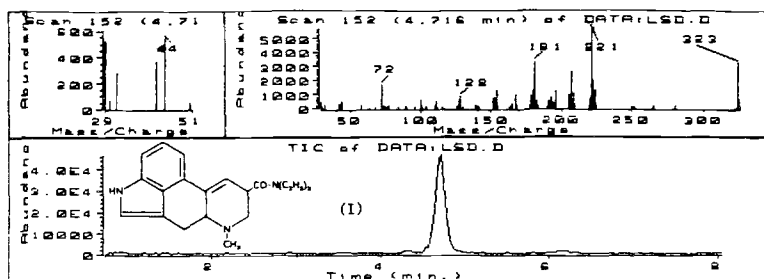


FIG. 1—Chromatogram of the total ion spectra and partial spectra (25 to 51 *m/e*) of LSD.

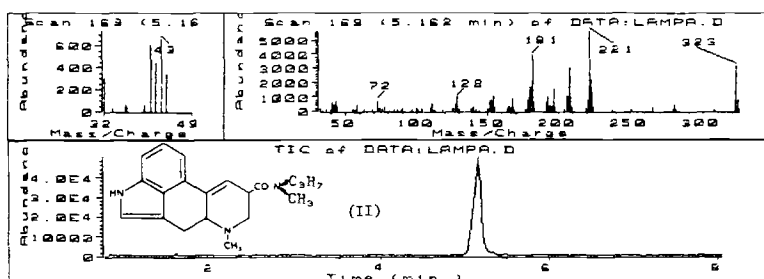


FIG. 2—Chromatogram of the total ion spectra and partial spectra (25 to 49 *m/e*) of LAMPA.

of this journal very informative. However, in this laboratory, an MS program is used in which ions are scanned from 25 to 400 amu, rather than from 40 to 400 amu, as Mr. Clark uses.

This program allows us to monitor the presence or absence of the ethyl ion (29 *m/e*), which is present only in LSD and iso-LSD. In addition, the *N*-methyl-*N*-propyl amides have a propyl ion (43 *m/e*) not present in LSD or iso-LSD. (See Figs. 1 and 2.)

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Author's Response

Dear Sir:

Mr. Boshears states that, in the laboratory in which he is employed, an MS program is used in which ions are scanned from 25 to 400 amu, rather than from 40 to 400 amu, as was mentioned in the referenced paper. This enables him to monitor the presence or absence of the ethyl ion (29 *m/e*), in addition to that of the propyl ion (43 *m/e*), which can be observed using either scan program.

Mr. Boshears states that the ethyl ion (29 *m/e*) is present only in LSD and iso-LSD. I do not know if this is a true statement since I did not scan between 25 and 40 amu when obtaining the mass spectrum of any of the LSD butyl analogs. Perhaps Mr. Boshears has additional information to substantiate his statement. It may be that he meant to say that the 25 to 400-amu scan program shows that EI/MS of the *N*-methyl-*N*-propyl amides does not show an appreciable 29-*m/e* fragment. The referenced paper shows that the EI

mass spectra of all of the LSD butyl analogs are not likely to be confused with the spectrum of LSD, whether or not one scans below 40 amu.

The mass spectra of the *N*-methyl-*N*-propyl analogs are another matter, however. They closely resemble that of LSD, except that the prominent 43 *m/e* is present in the *N*-methyl-*N*-propyl analogs of LSD but lacking in LSD. Thus, the identification of an LSD mass spectrum is based, in part, on negative information—that is, on the lack of a prominent 43 *m/e* fragment. The addition of positive information (the presence of a 29 *m/e* fragment) would enhance one's confidence in the identification.

As mentioned in the text of the referenced paper, background increases as one scans lower mass units. Sources of the background include column bleed and atmospheric gases. The latter can be appreciable if vacuum leaks exist, causing a possible decrease in photomultiplier life as a result of scans which include these intense fragments.

However, if the column background is low and no appreciable vacuum leaks exist, scanning to a low enough *m/e* value to include 29 *m/e* would be a worthwhile change to the methodology presented in the referenced paper.

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