Letters to the Editor

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

Sir:

The nicest controversies are those in which all sides miss the point. I refer to the squabble over Billy Dixon's Long Shot at the Second Battle of Adobe Walls, June 1874.

The current discussion was opened by Thornton and Shirokawa in the July 1989 issue of the *Journal of Forensic Sciences* (Vol. 34, No. 4, pp. 1037–1041). With their computer, they tried to reconstruct the trajectory of a bullet from a .50-90 Sharps buffalo gun, which, all witnesses white and native agree, struck a warrior from his saddle at the outlandish range of 1538 yards.

In the July 1990 issue of the *Journal of Forensic Sciences* (Vol. 35, No. 4, p. 782), Julien Mason wrote in to point out that Thornton and Shirokawa had miscalculated the ballistic coefficient of the .50-90 slug, so that its drop and time of flight would really be much greater than their computer told them, and its final velocity and energy much less. This Thornton gracefully admitted.

Neither of these approaches speak to the actual probability of the deed, Thornton thinking it's a fine miracle, Mason thinking it's a fine lie. They have both lost the point.

Billy Dixon and his buddies had been waiting for the buffalo migration to reach Adobe Walls, Texas, for a couple of weeks. There was little to do during this period but drink, gamble, and practice shooting. In their practice, they must have been interested in discovering how much better the new Sharps .50-90-473 could do than the previous buffalo hunting standard, the Government .50-70-450. Any improvement could only be shown at *extreme ranges*. This makes it very likely that the hunters had already practiced at targets at the base of the bluff on which the warrior was killed. They were in little doubt about the sight setting needed, and the details of drop and time of flight made no difference to them.

Dixon, if he wanted to correct for esoterica like wind, temperature, and so forth, only needed to take a sighting shot against some mark on the face of the bluff. The heavy slug would kick up enough dust to show him how to change the sights or point of aim. He would be much more concerned with the *accuracy* of such rounds at such ranges. Happily, there is historic data on this. You don't have to use a computer to get it.

The Sharps .50-90-473 really was better than the current .50-70 Army round. Its point form was the same, but the sectional density was better and its muzzle velocity was about 100 ft/s better (1350 versus 1240 yd). I have not dug up any accuracy statistics for the .50-90, but data exist for the .45-70-405, which has the same point form and muzzle velocity, and almost the same sectional density. At 1500 yd, you can expect all your shots to fall in a circle 13 ft in diameter. If you take the area of a standing man as 8 ft^2, you will see that there is one chance in 16 of striking the man you shoot at with each shot you take. That is, if the target doesn't catch on that he is being shot at and move around. And perhaps he won't, if the range is so long that it seems unlikely, and wind carries away the smoke of your firing and its sound.

But the real weakness of both the original paper and its critic is in the assumption that Billy Dixon hit exactly the Indian he was aiming at. The Indian accounts of the battle make it clear that there was a whole war council atop the bluff. So Dixon's mark was not just a single man, but five or six, *and their horses*.

Here is an account from interviews with the other side (from *Comanches—Lords of the South Plains*, by Wallace and Hoebel):

> Coyote Droppings, his naked body painted yellow, sat throughout it all upon his white horse on a distant hill. A Cheyenne father who had lost his son in the fighting taunted him

> with a dare to go down and bring out his son's body, if he really had immunity to bullets. He refused to move. As though to mock him further, a long-range shot from one of those deadly rifles knocked a member *of his party* from his horse as he sat with the Prophet watching the fiasco.

Coyote Droppings, also known as Ishatai, was the brains of the operation. He had scientifically determined, through peyote visions, that a certain plan of attack would roll back the tide of white settlement and permit the buffalo to return. Few doubted that he understood the subtle points of war, for he could swallow cartridges and bring them up at will.

Put yourself in the place of Billy Dixon. Atop the bluff, your best mark is a naked man painted yellow on a white horse. Firing at him, you hit one of his staff. Have you the moral strength to admit that your kill was dumb luck? Or will you let rumor take its course? Remember, as a buffalo hunter, your marksmanship is *negotiable*.

We see that Billy Dixon's long shot only looks like a miracle if you ignore the tactical factors. The eyewitness testimony makes clear that he was firing on a group target. Thus, he had a fine chance of hurting *something* useful to the enemy, even if it was not what he had aimed at. And, in fact, this piece of white folks' magic entirely deflated the mystical guidance of Coyote Droppings. For the remainder of the Red River War, he was not consulted.

Here is another account along the same lines, from James Haley's *The Buffalo War*:

> Billy Dixon and some others had been keeping a lookout for hostiles, and when a party of about *twenty warriors* topped a small butte maybe a mile east of the post, some of the other men urged Dixon to demonstrate his famous marksmanship by trying a shot. . . . a brave named To-hah-kah crumpled to the ground.

It is still fine shooting, of course, but the miraculous aspect of it evaporates when you consider the size of the target. The Indians had been primed to believe in white man's magic (and devalue their own) by an incident the day before, when Ishatai's horse, though magically painted to resist bullets, was killed with a slug through the forehead. In that case, the horse had been *behind and below* the crest of the ridge from the hunters' camp. Big, slow bullets have a final trajectory nearly as steep as that of a mortar round, so it is possible to hit things that you cannot see. Nobody knows whose bullet killed the horse, but anyone looking for a miracle shot to befuddle the computer could set it to work on that one.

Billy Dixon's Long Shot is closer to the routine than to the impossible.

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Author's Response

Sir:

Mr. Day himself has missed the point of the original article by Thornton and Shirokawa (*Journal of Forensic Sciences*, Vol. 34, No. 4, July 1989, pp. 1037–1041) and of my own subsequent comments (Vol. 35, No. 4, July 1990, p. 782). Scientists evaluate occurrences differently from those who have a romantic interest in historical events.

I believe the work by Thornton and Shirokawa was not intended to end any controversy surrounding Billy Dixon's Long Shot, but designed to show the physical *possibility* of the act using modern scientific analysis. To dive into the actual *probability* of the feat goes beyond that which is scientific (the physics of bullet flight) and into the murky waters of human abilities and "dumb luck." It is unfortunate that the article was flawed by a small error early in the work that tainted all of the conclusions reached concerning the bullet flight. However, an inference can be made that a fatal wound from a .50-90 Sharps is *possible* at a distance of 1538 yards, and to this conclusion, I must agree.

As a scientist, it is my nature to question. I neither dispute nor do I accept Billy Dixon's claim to fame. I only question the reported facts surrounding the incident. Eyewitness accounts of a warrior being shot from his saddle approximately a mile away make me wonder, as does the reliability of statements made by captured natives anxious to say anything to please their captors. As to Mr. Day's suggestion of a sighting-in shot, I believe the small amount of dirt kicked up by the extremely slow-moving bullet would be next to impossible to see at such "an outlandish range."

I do not think it fair to label the story either a lie or a miracle. Nor do I think it comes close to being the routine, as Mr. Day concludes. Had it been a routine shot with a 1 in 16 chance of success, Billy Dixon's name, and this controversy, would not have survived through 116 years of history.

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Author's Response

Dear Sir:

I learn from Mr. Day's first paragraph that Julien Mason and I are now engaged in a "squabble." I didn't think it was a squabble, and I doubt that Julien Mason did either. All of this was just fun, up to the point of becoming a squabble. I don't *really* think it is a squabble, but if it is, then it isn't fun anymore and I would have no enthusiasm for continuing it.

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Discussion of "Further Evaluation of Probabilities in Human Scalp Hair Comparison"

Dear Sir:

I read with considerable interest the recent article "Further Evaluation of Probabilities in Human Scalp Hair Comparison" in this journal (Vol. 35, No. 6, Nov. 1990, pp. 1323–1329) by Wickenheiser and Hepworth. There are several issues raised by the article which need correction, elaboration, or clarification.

First, the authors indicate that the most notable probability set forth by Gaudette and Keeping [*1*] was that "if one human scalp hair found at the scene of a crime is indistinguishable from at least one of a group of nine dissimilar hairs taken from a given source, the probability that it could have originated from another source is small, about 1 in 4500." This statement by Wickenheiser and Hepworth ignores the correction of this probability figure to *1 in 57* for the *relevant* population of individuals (*Journal of Forensic Sciences*, Vol. 27, No. 2, April 1982, p. 283) by Gaudette in his rebuttal to the critique by Barnett and Ogle [*2*].

Second, Wickenheiser and Hepworth conclude that "macroscopic" selection of 5 to 13 mutually dissimilar hairs was frequently unrepresentative of the range of features present in the known samples. This observation illustrates quite convincingly that "experience" is not an adequate substitute for experimentation, since much of the "knowledge" regarding the foundations for individualization is said to be derived from "experience" and is therefore no more than a group of untried assumptions. The "experience" of the examiner is frequently quoted as the justification for assertions that have been neither tested nor published, therefore making the assertions immune to criticism by other workers in the forensic sciences and immune to cross-examination by an opposing counsel in legal proceedings. Wickenheiser and Hepworth have tested the (heretofore unchallenged) assumption that 5 to 13 mutually dissimilar hairs will adequately represent the range of hair types on a given scalp, and have found the assumption wanting. They state, justifiably, that experimental work aimed at determining the optimum composition of a representative known hair sample is warranted. More to the point, such untried assumptions *must* be tested and verified prior to their use as essential parameters in any experiment in order to provide credibility for any of the experimental findings.

Third, the "match" of only 1 of the 53 duplicate (blind sample) hairs in the study to any of the hair standards from the 97 individuals selected for the study raises the question as to how many "incorrect" associations were avoided inadvertently owing to the extremely low incidence of matches of the duplicate hairs, when the predicted rate of correct matches (probability "assumed" = 1.0) should have produced a "match" for each of the 53 randomly chosen hairs from the experimental population. Since only 1 of the 53 hairs was determined to match (a failure rate of 98%), it can be postulated that a number of false associations were avoided by the same mechanism. This consideration nullifies the authors' statement that "assuming a one-to-one match exists, this study demonstrates that the probability of incorrect association in routine forensic hair comparison is remote."

Fourth, the authors' method for "classification" of the hair "types" present in the study population should have generated valuable data on the occurrence and frequency of the various hair types present on each scalp and in the study population as a whole. However, the lack of any consistency in the classification efforts between the two authors, or by each author over time, rendered these data meaningless. Although this inconsistency was explained as a consequence of variation of feature evaluation over time, the inconsistency is more likely the result of the authors' failure to establish reference standards for each of the features classified prior to scoring the specimens. Where any array of *continuous* variates is to be classified, it is essential that one first establish archetypes which define the limits within which each variate is to be placed. The inability of the authors to categorize each feature consistently emphasizes the rule that invalid results are obtained when "experience" is used as an expedient surrogate for the, alternatively, sound scientific method of establishing concrete classification parameters *prior* to any attempt to identify the category to which any member of the class is to be assigned.

Finally, it is most curious that the authors selected 5 to 13 *mutually dissimilar* hairs from each individual in the study for intercomparison, yet they were able to "match" 12 pairs of these "dissimilar" hairs. The authors state, "Based on the large number of *potential* [emphasis added] one-to-one comparisons (431 985) conducted, it can be concluded that, if a one-to-one match is insisted upon, there is a very low incidence of error." Since the 5 to 13 hairs selected from each individual to represent the "range" of hair characteristics present over each scalp were admitted to be "frequently unrepresentative of the microscopic range [sic] of features in the known samples" (p. 1329), we are left with the question of how many incorrect associations would have been made if the experimental work had been accomplished using exemplar samples which were truly representative of *all* the hair types present in the scalp of each individual in the study population. Even for the extremely small study population used by the authors (n = 97), the "large number" of potential one-to-one comparisons cited (431 985) is infinitesimal in comparison with the number of one-to-one comparisons necessary to compare each hair from each individual's scalp with each hair from each other individual's scalp. Unless the exemplar hair sample selected from each individual can be shown to be truly representative of all the hair types present on that individual's scalp, it is necessary to perform this much larger number of comparisons (an impossible task) in order to establish meaningful data on estimates of the "remoteness" of incorrect associations.

In summary, the experiment conducted by these authors has not provided an answer to the question, "Once a match between a questioned hair and the exemplar specimen from one individual has been effected, what is the probability that the questioned hair would also match the hair exemplar from another individual in the relevant population?" Until experiments based on "comparison" data are replaced by scientific research efforts directed toward *frequency of occurrence* data for the microscopic hair "features" and hair "types" present in the various human populations, no hard data will be available for statistical evaluation. The most sophisticated statistical treatment of data derived from invalid experiments cannot produce meaningful probability estimates.

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Authors' Response

Dear Sir:

We, the authors of a recent article, "Further Evaluation of Probabilities in Human Scalp Hair Comparisons," published in this journal (Vol. 35, No. 6, Nov. 1990, pp. 1323–1329), appreciate the interest and comments by Mr. Ogle. They serve to highlight some of the common misconceptions regarding the area of forensic hair comparisons as practiced in Canada. In our article, we perhaps assumed too high a level of knowledge regarding the intricacies of our methodology and therefore feel that some elaboration and clarification might be helpful in providing a more complete understanding of the relationship of forensic hair comparison casework to our study. Issues two through five raised by Mr. Ogle will be addressed through this overview of our two-step hair comparison technique, followed by a response to Mr. Ogle's first issue.

As associative evidence, scalp hair has many advantages. Nearly all persons possess these hairs in large numbers. They are durable, readily shed, frequently transferred, and carry information about the appearance, race, and habits of an individual (such as his or her cleanliness, hair treatment, hair length, substance abuse habits, and other characteristics). With these positives, also come negatives. Hair is not at present a 100%-positive means of association (with the exclusion of the deoxyribonucleic acid (DNA) present in root sheaths). It may vary considerably on the scalp of an individual (interpersonal variation), and it is highly susceptible to both immediate and gradual changes on the scalp over time (hair treatment, cutting, greying, natural bleaching, and so forth).

The very nature of scalp hair, which makes it useful as forensic evidence, also imparts drawbacks for its use as an associative tool. The average individual may possess 150 000 to 200 000 scalp hairs [*1*]. Each hair is a biological entity and, as such, it is probable that no two hairs are absolutely identical in every single respect. A typical scalp of hair represents a virtual continuum of some 15 to 20 discernible characteristics within a range of genotypes capable of being exhibited by the phenotype of the individual, coupled with his or her hair-related personal habits, such as cutting, dyeing, environmental exposure, and other factors. This is a very large number of characteristics to be adequately represented by the 80 to 100 scalp hairs generally accepted as an adequate known standard sample. Without doubt, in a forensic hair comparison there will be hairs left on an individual's scalp which will not fit the range of characteristics established by the normally assumed representative sample. Some of the remaining hair may be atypical, exhibiting unusual or rare characteristics not found in the known sample, or may merely fall outside the range exhibited in the 15 to 20 characteristics described. Therefore, the assumption that a small subsample of hairs from an entire scalp (less than 0.1%) is to represent all characteristics and ranges of characteristics found on that scalp can only lead one to the conclusion that incorrect eliminations will occur—that is, hairs which originated from an individual will not match the standard selected from that individual and will therefore not result in a positive association.

In the two-step hair comparison technique, not only must a questioned hair fit the range of characteristics set out by the known sample, but as indicated in our study, a 1:1 match is required before a positive conclusion is drawn. This second step requires the examiner to find at least one hair in the known sample which shares all characteristics in common with the questioned hair, shows the same variation from root to tip as the questioned hair, and does not possess any significant differences.

By virtue of these very rigid selection criteria, situations arise in which questioned hairs which fit the range set out by the known sample, and which are very similar to individual hairs within the known sample, are eliminated because no 1:1 match exists. While it is quite probable that such hairs originated from the known source, the application of less strict criteria would undoubtedly increase the rate of incorrect associations.

Bearing in mind the concept of rigid selection criteria, one must look at the implications of selecting very rigid as opposed to less strict criteria for positive conclusions—that is, one must choose between incorrect eliminations and incorrect associations. Certainly, in our justice system falsely incriminating an innocent individual is a much greater wrong than narrowly eliminating a number of potentially valuable pieces of associative evidence. By insisting on very high selection criteria, as illustrated in our study as well as in day-to-day forensic casework, the potential increase in incorrect eliminations is a conscious sacrifice made for a high confidence level for association.

To address the third and fifth issues, the predicted probability rate of correct matches of the questioned hair is therefore less than 1.0. We agree that the two-step method of hair comparison employed in our study reduces false associations, as the study demonstrates. The experimental design of using 5 to 13 macroscopically different hairs to represent a 150 000 to 200 000-hair scalp further magnifies the number of incorrect eliminations to probably many more than would occur in routine forensic situations, in which the entire 80 to 100 known hairs would be available for 1:1 comparisons. In our view, the reduced potential for incorrect association justifies the application of strict selection criteria, and the potential for an increase in incorrect elimination is a natural by-product of the method.

In the second and fifth issues raised, we are criticized for the use of 5 to 13 hairs to represent a known sample. This study was a "modified repeat of the hair probability study by Gaudette and Keeping" (p. 1323) and as such used a manageable number of hairs to cover a range seen macroscopically (with the naked eye) to keep the number of comparisons to a reasonable level. Even with these reduced numbers, it took a considerable length of time to complete the study. While we agree that limiting the size of the standard, be it 6 to 11 hairs [*2*] or 5 to 13 hairs, makes it frequently not representative of the full range of microscopic characteristics present in a known hair sample, we have no reason to believe that a larger number of comparisons would produce any different results with respect to incorrect associations.

Several of Mr. Ogle's points refer to ideals which, in our view, are either unattainable or unrealistic. In the fourth issue he raised, he refers to archetypes of hair. As previously mentioned, each human scalp hair is a unique biological specimen, possessing many characteristics that can and often do vary along the shaft of the hair. Establishing archetypes for each of 15 to 20 features would be arbitrary, impossible to convey to fellow scientists without having them actually view the hair, and limited to one field of view under the microscope. If one examines the archetype theory in detail, it becomes readily apparent that several hundred fields of view may be necessary to examine a hair from root to tip. The end result is that once again the examiner's judgment and discrimination ability would be called upon in describing the particular archetypes associated with each hair. No examiner, except those with access to the archetypes, would be able to enter values into a database or evaluate a questioned hair for comparison with the database. We would agree that, ideally, Mr. Ogle's concept of archetypes is excellent, we also believe that realistically it is an unattainable goal.

Finally, there seems to be some confusion as to which of the six questions regarding incorrect associations discussed by Gaudette's rebuttal [*3*] to Barnet and Ogle's criticism [*4*] is the relevant one. We feel the appropriate question regarding forensic hair comparisons is Gaudette's Question 2 [*3*]: "If one hair chosen at random from A is compared to a representative standard hair sample obtained from B, what is the probability that it will be consistent with having originated from B?" Gaudette and Keeping found this value to be approximately 1/4500 for Caucasian human scalp hair [*2*]. The fact that no incorrect associations were made in our study does not allow us to calculate a probability in the same manner as Gaudette and Keeping. At the very least, our results support those of Gaudette and Keeping in that the absence of incorrect associations in our study implies that, using our rigid selection criteria, the Gaudette and Keeping figure of 1/4500 is conservative. In our view, the selection criteria used in our study may be viewed by some as being too rigid. By setting strict selection criteria, however, we have no doubt that the confidence level associated with positive associations is high.

In conclusion, it seems that Mr. Ogle does not agree with the reasoned application of experience within the field of forensic science, since it appears he believes it offers little avenue for criticism by those who lack it. We would argue that experience gained in hair examination is done within the framework of established scientific principles. Similar experiences grow to represent a body of knowledge that can ultimately be drawn upon through inductive reasoning. In our view, society in general would be done a serious disservice if the courts were not allowed to draw upon that vast body of knowledge called "experience."

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Distribution of the Third Digit in Breath Alcohol Analysis

Sir:

Jurisdictions performing breath alcohol analysis typically report the results truncated to two digits, as recommended by the National Safety Council Committee on Alcohol and Other Drugs [*1*]. Currently employed breath alcohol instrumentation has the analytical capability of generating a significant third digit, even though it possesses uncertainty. In fact, it becomes necessary to employ the third digit when evaluating within-run instrumental precision using control standards since the variability, and thus the uncertainty, will exist in the third digit [*2*]. This is appropriate since recommended practice in analytical procedures is to include only the first uncertain digit [*3*]. The practice of truncating to two digits is prudent, however, in forensic science applications of breath alcohol measurements where significant penalties result from "per se" legislation.

This practice of truncation means that individuals interpreting the results will typically not have knowledge of the third digit. However, the probability of a particular third digit value can be estimated from the distribution of third digit values. The computational effect of truncation (although appropriate to forensic science applications) can thereby be evaluated.

An earlier article addressed this issue but with a limited (n = 500) amount of data [*4*]. The purposes of this letter are to expand this evaluation by including a larger data set and also to evaluate the nature of the distribution generated.

A total of 11 480 breath alcohol measurements performed on the BAC Verifier Datamaster (National Patent Analytical Systems, Inc., Mansfield, Ohio) were selected for analysis. Each value is the breath alcohol concentration of the first of duplicate breath samples performed under the approved field protocol in Washington State for subjects arrested under provisions of the implied consent statute. Only breath alcohol values of 0.01 g/210 L or greater were selected in an attempt to eliminate results from tests that were done for practice or were otherwise not relevant. The tests were performed between 1 June 1990 and 30 Sept. 1990. The statistical analysis was performed with SPSS/PC+ (SPSS, Inc., Chicago, Illinois).

The breath alcohol analyses as determined from the two-digit truncated values resulted in mean = 0.165 g/210 L, standard deviation (SD) = 0.056 g/210 L, and span = 0.01 to 0.42 g/210 L. Figure 1 shows the distributions of the third digit result where n = 50 and n = 11 480. Table 1 shows the percentage range for the ten digits as well as the Kolmogorov-Smirnov nonparametric statistical value for the frequency distribution's conformity to the uniform distribution with different values of n (H_o: distribution = uniform). A truly uniform frequency distribution would be defined by $f(x) = 1/10$ for $0 \leq x \leq 9$, with x being the discrete random variable representing the third digit [*5*]. The probability of any individual digit, therefore, would be 10%. It can be observed that as n increases the Kolmogorov-Smirnov statistic increases and the P value decreases. This demonstrates how the power of a statistical test increases with n and, therefore, that the likelihood of rejecting H_o increases along with n as well [*6–8*]. The Kolmogorov-Smirnov statistic evaluates a distribution's goodness-of-fit to a particular distributional form [*9*].

The statistical analysis needs to be interpreted in light of real-world applications. Statistical significance does not always mean real or clinical significance [*7*]. Figure 1 shows the distributions where n = 50 and where n = 11 480. It is clearly evident that

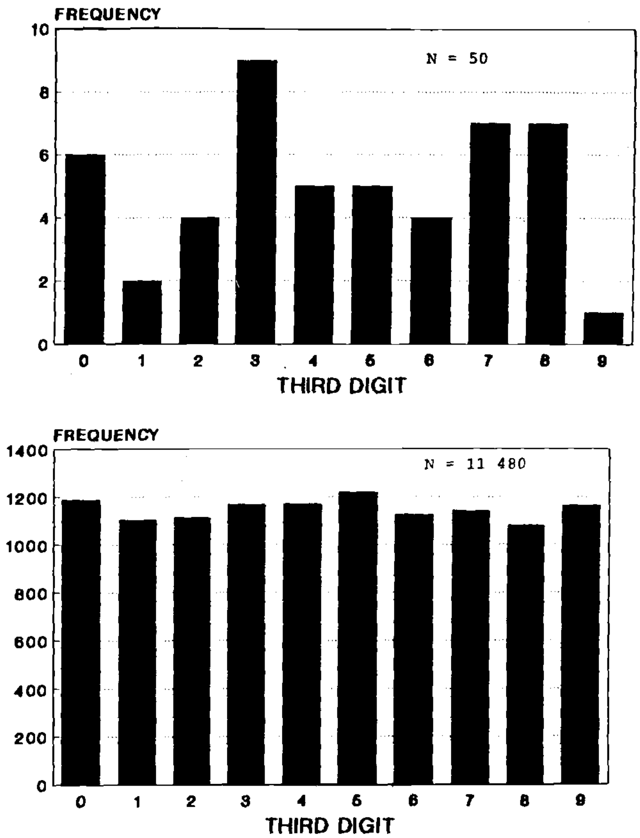

FIG. 1—*Third-digit distributions where* n = *50 and* n = *11 480.*

TABLE 1—*Uniform Distribution Properties of the third digit as related to sample size* (n).

n	Range, %	K-S[a]	*P*
50	2.0 to 18.0	0.85	0.468
100	5.0 to 14.0	1.1	0.178
200	8.5 to 12.0	1.3	0.054
400	7.0 to 13.0	2.3	0.000
600	8.8 to 12.3	2.4	0.000
5 000	9.7 to 10.5	7.2	0.000
11 480	9.4 to 10.6	11.1	0.000

[a]Kolmogorov-Smirov statistic.

the larger distribution conforms better to the uniform distribution model, even though its conformity to the uniform is statistically significant ($P < 0.000$).

The practical implication of this analysis is that the unknown third digit cannot be predicted with accurate probability (at least not for the breath alcohol analysis system used to derive the present data). The third digit follows a uniform distribution, which implies that it is random in nature and thereby assigns an equal probability to the occurrence of each of the ten possible digits. However, the third digit is still analytically significant when considered in an individual case. As a result, one cannot argue that the third digit, when unknown, is more likely to be a zero than a nine. This information needs to be considered and understood by forensic scientists interpreting and explaining breath alcohol results in a legal context.

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Addendum to "Methenamine—An Unusual Component in an Improvised Incendiary Device"

Sir:

I became aware of the following information after it was too late to include it in my paper published in the January 1991 issue of this journal [1]. An underground publication called *ANARCHY 'N' EXPLOSIVES* has recently been showing up on computer bulletin boards. It consists primarily of recipes for the manufacture and use of various explosive, pyrotechnic, and incendiary devices. The author is anonymous but goes by the nom de plume "Doctor Dissector."

So far, I have seen eight volumes, with Volume 8 having a date of 19 June 1989. The term "volume" is used rather loosely as each volume consists of just a few pages. Much of the material appears to have been blatantly stolen from *The Anarchist Cookbook* [2], but on the fourth and fifth pages of Volume 7, dated 16 June 1989, is a section on RDX which includes a simple recipe for its preparation from hexamethylenetetramine ($C_6H_{12}N_4$) (also known as methenamine).

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