Correspondence

Increasing the Identification Potential from Human Foot Remains

Sir:

In 1978, Robbins (1) stated that, "the shape or form of an individual's foot is uniquely his or her own." Indeed, "heredity and life experiences are operative in determining the size and shape of our feet (1)." Although research into foot morphology remains scant, a few preliminary studies have established that barefoot impression evidence is unique to individuals (2–4).

Due to their protected nature in footwear, feet often escape the trauma sustained by other anatomical elements. Moreover, the short and stout architecture of the metatarsals provides a degree of protection (5). Given the unique aspects of the human foot and the factors leading to their preservation, feet contain the potential for positive identification not found in some other anatomical regions. Therefore, the foot can be of value in forensic cases involving scattered, fragmented, and incomplete remains.

Although there exists a body of anthropometric research assessing sex, stature, and racial affinity from foot bones (5–9), the forensic literature contains very few cases matching foot remains with footwear in a non-metric effort to increase the identification potential (10–12). There are three possible reasons that may account for this. First, forensic scientists may not be familiar with these cases and therefore missing opportunities to "match" foot remains with footwear. Second, forensic scientists may be familiar with this approach to identification yet do not find it useful. Finally, there may be too few opportunities to use this adjunct method of identification.

Arguably, the most celebrated and critical use of matching foot remains to footwear is the Ruxton case of 1935. An increased awareness of this "identification" method may enhance its utilization.

On September 29, 1935, human remains were discovered along the roadside in Moffat, a town in Dumfriesshire, Scotland (11,12). Underneath the Gardenholme-Linn Bridge of Moffat-Edinburgh Road, several cotton bundles were found containing various body parts. In one bundle, two legs were tied together with the feet protruding from the cotton sheeting (11). Within another bundle, two additional legs were discovered. During the next several weeks, numerous remains were retrieved along the roadside including a left foot wrapped in newspaper. A total of 70 body parts were collected. The lower extremity remains included four legs: a right and left leg complete with feet, two legs without feet and an isolateral left foot. One right foot, however, was not recovered in the investigation.

Though severely mutilated, the bodily remains were thoroughly evaluated for potential identity features. The left foot with leg and the isolated left foot (numbered one and two) were compared, respectively, with the known pedal features of two missing individuals, Mary Rogerson and Isabella Ruxton. Foot number one presented a mild hallux valgus deformity. This feature was consistent with Ms. Rogerson's previous foot conditions. However, foot number two showed evidence of considerable mutilation, presumably to mislead any investigation and was extensively examined for personal features.

Consultation with Mrs. Ruxton's chiropodist (i.e., podiatrist) revealed that she had an "inflamed bunion" of her left foot at the time of her last appointment (11). In an attempt to conceal the bunion deformity, skin and underlying tissues were removed from the first metatarsophalangeal joint of foot number two thereby exposing bone and an opened joint. Moreover, toenail condition could not be assessed because the distal portions of the toes were removed. Postmortem radiographs of this foot revealed an exostosis at the head of the first metatarsal. Furthermore, a double (bipartite) medial hallucal sesamoid was noted radiographically.

The preliminary examination of the remains included placing the feet into the footwear of Ms. Rogerson and Mrs. Ruxton. In preparation for this analysis, the feet were dried, powdered, and covered with a silk stocking. The results demonstrated that foot number two, a left foot, could not fit Ms. Rogerson's left shoe. However, this foot did indeed fit into Mrs. Ruxton's left-sided footwear.

Subsequently, master casts of the feet were constructed using measurements of the feet and footwear. Allowances were made for the mutilations present in foot number two. The casts were made with a piece-mold in plaster, and from the piece-molds, copies were produced from a flexible material consisting of zinc oxide, glycerin, and gelatin. These chemicals enabled the casts to flexibly fit into the contours of the footwear, thereby simulating a realistic fleshed foot.

Foot number one's cast conformed "perfectly" to Ms. Rogerson's footwear (11). The mild degree of hallux valgus present in this foot fit well into the inner medial border of the shoe. Foot number two's cast matched Mrs. Ruxton's footwear in a similar fashion. The moderate degree of hallux valgus present in this foot permitted the big toe to adapt well into the concavity produced by the bunion. Therefore, Mrs. Ruxton's antemortem foot conditions were indeed consistent with features found on foot number two.

Matching foot remains to footwear, however, has several underlying limitations. First, footwear evidence must be made available for this approach to identification. Since there are a finite number of shoe sizes, fit comparisons could be purely circumstantial. In addition, considerable inter- and intra-observer unreliability are associated with this form of examination. For example, one investigator's qualitative evaluation of cast fit might be different from another scientist's analysis. Because the casts were constructed from flexible materials, investigators could force the casts into the footwear. Therefore, this approach to identity would increase the potential for incorrectly evaluating the owner of the footwear.

Glaister and Brash have stated that, "A clear demonstration that a foot could not possibly fit a particular shoe would have the same effect in precluding identification of a body as irreconcilable evidence regarding sex, age, or stature (11)." Although this qualitative approach increased the identification potential in the Ruxton case,

for it to become a forensically reliable technique, comparisons between foot remains and footwear need to be further studied.

In medicolegal contexts, a non-metric evaluation of pedal osseous features can augment identity reconstruction. Of note in the Ruxton case were the bipartite medial hallucal sesamoids present in foot number two. Bipartite hallucal sesamoids have multiple etiologies including, trauma, osteochondritis, and osteoarthritis (13–15). Since these bones commonly present with hallux valgus deformities, the findings were consistent with Mrs. Ruxton's antemortem bunion presentation, thereby increasing the likelihood that foot number two was Mrs. Ruxton's left foot.

Furthermore, the tissues removed from the first metatarsophalangeal joint of foot number two most likely consisted of subchondral bone cysts. Trauma and repetitive stress may form subchondral bone cysts at the head of the first metatarsal. These cysts often appear in conjunction with hallux valgus and osteoarthritis. Therefore, these histological findings would have been consistent with Mrs. Ruxton's antemortem bunion presentation.

In summary, the Ruxton case provides a historical example of matching foot remains to footwear as an adjunct method of identification. Given the potential forensic value of this technique, further empirical research in the following two areas seems warranted. First, since biomechanics and heredity play a critical role in foot morphology, the individuality, reproducibility, and reliability between foot remains and footwear needs to be quantitatively examined. Second, pedal osseous features must be further scrutinized for important forensic implications when compared with footwear evidence.

The author gratefully acknowledges Drs. Barbara Wolf, Arthur Washburn, and Sgt. Robert B. Kennedy for their editorial suggestions.

References

1. Robbins LM. The individuality of human footprints. J Forensic Sci 1978; 23(4):775–8.
2. Laskowski GE, Kyle VL. Barefoot impressions—a preliminary study of identification characteristics and population frequency of their morphological features. J Forensic Sci 1988;33(2):378–88.
3. Kennedy RB. Uniqueness of bare feet and its use as a possible means of identification. Forensic Sci Int 1996;82:81–7.
4. Tripathi RS, Jogulamma MR. Individualization from footwear—a case report. Med Sci Law 1982;22(2):115–8.
5. Robling AG, Ubelaker DH. Sex determination from metatarsals. J Forensic Sci 1997;42(6):1062–9.
6. Smith SL. Attribution of foot bones to sex and population groups. J Forensic Sci 1997;42(2):186–95.
7. Steele DG. The estimation of sex on the basis of the talus and the calcaneus. Am J Phys Anthrop 1976;45:581–8.
8. Byers S, Akoshima K, Curran B. Determination of adult stature from metatarsal length. Am J Phys Anthrop 1989;79:275–9.
9. Introna Jr F, Di Vella G, Campobasso CP, Dragone M. Sex determination by discriminant analysis of calcanei measurements. J Forensic Sci 1997;42(4):725–8.
10. Abbott JR. Establishing footwear ownership. In: Footwear evidence: The examination, identification, and comparison of footwear impressions. Springfield, IL: Charles C Thomas, 1964;72–88.
11. Glaister J, Brash JC. The feet and the shoes. In: Medico-legal aspects of the Ruxton Case, Baltimore: William Wood and Company, 1937; 138–43.
12. Rentaul E, Smith H. Identification. In: Glaister's medical jurisprudence and toxicology, 13th ed. Edinburgh: Churchill Livingstone, 1973; 59–112.
13. Aseyo D, Nathan H. Hallux sesamoid bones. Anatomical observations with special reference to osteoarthritis and hallux valgus. Int Orthop 1984;8(1):67–73.
14. Potter HG, Pavlov H, Abrahams TG. The hallux sesamoids revisited. Skeletal Radiol 1992;21(7):437–41.
15. Goldberg I, Nathan H. Anatomy and pathology of the sesamoid bones of the hand and foot. Int Orthop 1987;11(2):141–7.

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Corrections of: Seward GH. Practical implications of charge transport model for electrostatic detection apparatus (ESDA). J Forensic Sci 1999 Jul;44(4):832–6

I am writing to you about a flaw in the protocol described in my recent article referenced above. One of the specified steps is not possible with the basic ESDA unit. Specifically, the corona wire cannot remain active after the vacuum is terminated. This error was identified by Miss Tracey Johnson of Marshall University while working as an intern for the West Virginia State Police. I wish to thank Tracey Johnson for her interest in my work and as well as her constructive criticism.

I do have specific recommendations regarding the limitations of my specified protocol, but first, I must apologize for my slow response to the important issues raised by T. Johnson. I no longer work at IISI Corporation. Consequently, delivery of her letter to me was delayed by many weeks. After further correspondence with T. Johnson, I now have the following message for the readers of Journal of Forensic Sciences.

In my recommendations for optimization of the ESDA protocol, I erroneously assumed that the control of the vacuum and corona-bar were independent. My work on the ESDA phenomena was done on a machine other than ESDA. My protocol specified continuation of the corona charging after termination of the vacuum. Such a procedure is not possible on the ESDA unit as provided by Foster & Freeman Ltd.

The fundamental science of my protocol is still sound, but the practical limitations of the ESDA unit indicate some sort of compromise in the present with the possibility of full implementation in the future. In the present, I recommend the following procedures.

If the relative humidity (RH) is greater than 40%, then a charge-and-pump for the entire 2 min should be very effective. Based upon past experience, I don't think the loss of moisture will be significant with an ambient RH of 40% or greater.

If the RH is 20% or less, then some serious problems exist. The specimen will lose significant amounts of water during a 2 min charge at 20% RH. Such a loss of moisture can render the image beyond recovery within such a reasonable length of time as 60 min. Shorter durations for the charge-and-pump can also be ineffective due to a lack of sufficient deposition of charge. I encountered both of these problems during my research. The only reliable solution for me was to humidify the entire room. Another option is to let the charge decay slowly overnight.

Perhaps future versions of the ESDA system could offer independent controls for the pump and corona bar. It should even be possible to retrofit the existing unit with two switches where there is now only one.

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Editor's note: Any and all future citations of the above referenced paper should read Seward GH. Practical implications of charge transport model for electrostatic detection apparatus (ESDA). [published erratum appears in J Forensic Sci 2000;45(2)] J Forensic Sci 1999;44:832–6.

Commentary on Wu AHB, Hill DW, Crouch D, Hodnett CN, McCurdy HH. Minimal standards for the performance and interpretation of toxicology tests in legal proceedings. J Forensic Sci 1999;44(3):516–522:

Sir:

The article of Wu et al. is a thought-provoking discussion of a number of relevant points concerning interpretation of toxicological testing results. The authors make the statement that there is no published conversion factor relating concentrations of 11-nor-delta-9-tetrahydrocannabinol-9-carboxylic acid (THCA) in serum to those in whole blood. While it is inconsequential to the authors' conclusions, that is not quite accurate. The data of Hanson et al. (1) quite clearly show that, in a series of nearly 50 subjects, the blood/serum concentration ratios for both delta-9-tetrahydrocannabinol (THC) and THCA are the same and that they average 0.57 (range, 0.50–0.67). I apologize for not stating this more explicitly in the 1983 article.

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Reference

1. Hanson VW, Buonarati MH, Baselt RC, Wade NA, Yep C, Biasotti AA, Reeve VC, Wong AS, Orbanowsky MW. Comparison of ^{3}H- and ^{125}I-radioimmunoassay and gas chromatography/mass spectrometry for the determination of Δ^{9}-tetrahydrocannabinol and cannabinoids in blood and serum. J Anal Tox 1983;7(2):96–102.

Commentary on Keto RO. Analysis and comparison of bullet leads by inductively-coupled plasma mass spectrometry. J Forensic Sci 1999;44(5):1020–6.

Sir:

It appears to me that there is more information in Keto's (1) bullet lead impurity data than the author supposes.

Keto (1, pp. 1024–25) computed the equivalent of the scalar products of 1,770 pairs of bullet impurity concentration profiles, considered as 8-dimensional vectors. He is thus able to show that sample bullets of the same brand tend to resemble one another more often than they resemble bullets of another brand, sometimes even to the exclusion of other brands.

However, citing data insufficiency, he feels unable to assign a complete set of probabilities of brand membership to each of the concentration profiles he has available (1, Table 4). He claims only that his data "suggests that when two element signatures match, it is unlikely that the bullets originated from different sources," and that "[g]iven a sufficient database, [the scalar product] could be a useful tool in establishing the 'rarity' or 'commonality' of a specific elemental signature, and the probability of a random match [between bullets] could be estimated."

I decided to see whether a Bayesian (2,3) treatment of Keto's data might yield useful brand membership probabilities, and this appears to be the case.

By means of a multivariate Bayesian analysis of the data in Keto's Table 4, I computed brand membership probabilities. Because I lacked a separate test set, I used Keto's sample bullets both collectively, as the parametric data set, and individually, as the test set. The mutual independence of the concentration data for different elements permitted me to do this. Keto (1, p 1023) states that "[s]catter plots of each element against each of the other elements showed no visual correlations, either linear or non-linear."

Because of software limitations, I limited my analysis to ten of Keto's 12 bullet brands, ranging alphabetically from Defence through Toledo. I did, however, use all eight of Keto's element concentrations for each bullet.

I compiled, for each of 50 bullets, a probability distribution over ten bullet brands, as a function of that bullet's concentration profile. For the sake of brevity, and because the probability for the "correct brand", even when low, generally dominates the other nine values, Table 1 displays only "correct brand" assignment probabilities. Note that the table's probability scale runs from 0.50 to 1.00. (The complete parametric data set and the complete set of brand probability distributions are available on request. In only one case out of the 50 was there some ambiguity about the correct brand.)

With Table 1 in hand, one can now consider the question of decision threshold. A juryman may want a defendant's ammunition connected to the crime with a probability greater than 0.999 (odds of ~1,000 to 1), in order to vote "guilty." A prosecutor may want a probability greater than 0.85 in order to bring a case to trial. A police officer may feel that 0.75 is enough to justify arrest, and that 0.60 or more indicates "prime suspect." Assuming all this, Table 1 suggests that a Bayesian comparison of a crime scene bullet with the perpetrator's ammunition would exceed the "prime suspect" threshold about 96% of the time, that it would exceed the arrest threshold about 90% of the time, and that it would exceed the prosecution threshold about 78% of the time. As for the juryman, the bullet-brand evidence may not be quite enough, by itself, to support a "guilty" vote. The highest brand probability value I obtained was 0.998.

In closing, I point out that the issue of bullet source identification is not necessarily related to brand differences. Conceivably, several suspects may each possess a box of ammunition of the same brand (which is stamped on the case heads), each box being the result of a different production "run", with a more or less distinct set of bullet lead impurity profiles. Or so we must hope.

TABLE 1—*Distribution of 50 "test" bullets by the probability which was computed for the correct brand.*

	Correct Brand Probabilities									
Probability Range	0.50–0.55	0.55–0.60	0.60–0.65	0.65–0.70	0.70–0.75	0.75–0.80	0.80–0.85	0.85–0.90	0.90–0.95	0.95–1.0
Number of Bullets	1	1	0	2	1	3	3	4	12	23

References

1. Keto RO. Analysis and comparison of bullet leads by inductively-coupled plasma mass spectrometry. J Forensic Sci 1999;44(5):1020–6.
2. Box GEP, Tiao GC. Bayesian inference in statistical analysis. Wiley, 1992.
3. Schmitt SA. Measuring uncertainty: An elementary introduction to Bayesian statistics. Addison-Wesley, 1969.

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Author's Response

Sir:

Mr. Promish appears to have missed one of the major points put forth in my article. Many of the bullets within a box do not resemble others from the same box any more than they resemble bullets from other manufacturers. This was illustrated graphically in Fig. 2 on page 1024. Calculating an "average" elemental composition for a box, and then showing that individual bullets from that box more closely resemble that average than they do averages of other boxes is meaningless. What matters, in a real case scenario, is whether known and questioned bullets match.

The purpose of bullet lead analysis, in most situations, is not to brand identify unknown bullets, as suggested in the final paragraph of Mr. Promish's letter. This is because the trace elemental composition of bullets from a single manufacturer can vary almost continuously over time, depending on the source for the lead. The analyst's opinion is usually limited to stating that the suspect bullet is consistent with (or could have come from) the same source as the known bullet. This is the same standard that applies to other forms of trace evidence as well, such as paint chips, glass fragments, and fibers, in the absence of a physical match. The "scalar products," or correlation values, were calculated in an effort to quantitate the quality of the match between two bullets, and arrive at a means of unambiguously distinguishing between known matching and nonmatching bullet leads.

While statistical approaches to data interpretation may be useful, they are, as stated in the final paragraph of my paper, "not a substitute for direct comparison of the raw elemental data when formulating an opinion as to the similarity of two bullets". A 95% probability that two bullets match means little when a look at the raw data shows that they could not be from the same melt.

I appreciate Mr. Promish's enthusiasm, and applaud his volunteering his probabilistic approach. More of this type of thinking needs to be applied to the forensic sciences. However, it must be applied with caution. I question the advisability of using the probability of a bullet match as a decision threshold for arrest, prosecution, or finding of guilt. Such decisions can only be based on a much broader scope of evidence, which could include bullet comparison. The bullet analyst cannot be expected to give a qualified opinion as to the certainty of a match; his findings must be either positive or negative to be of use.

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Verbal Conventions for Handwriting Opinions

Sir:

A paper of mine in Science and Justice (1) on reporting conventions was recently the subject of a spirited debate on Docexam_L, the forensic document examiners e-mail forum (membership enquiries to andersonc@docexam.com.au). The rough and tumble of an e-mail discussion is all very well but it seems to me that if the core issues are to be resolved then the appropriate forum lies within the pages of a peer-reviewed journal. Nothing of what I say here is original and the subject has been covered in greater detail elsewhere but it seems to me to be appropriate that I should state my case in the journal that carried the letter that announced the reporting convention with which I take issue.

The letter from McAlexander, Beck and Dick (2) is to be applauded for its motivation. It promotes the idea that there is a need for standardization of terminology among experts when they express opinions. It also argues convincingly for the need to consider handwriting evidence probabilistically. Rightly, the authors pointed to the weaknesses of phrases which appear, regrettably, to be in widespread use in the forensic science world: I refer, in particular to the use of "could have" and "consistent with." I agree with McAlexander et al. that these phrases should have no place in any convention for expressing the weight of an item of scientific evidence.

The letter described a reporting convention which became the subject of ASTM standard E 1658–96 (3). My copy is headed "Standard Terminology for Expressing Conclusions of Forensic Document" (sic).

Laudable though the attempt at standardization undoubtedly is, I must point out that there is a serious problem with the manner in which the convention uses the notion of probability. In my opinion, the reporting convention is incompatible with a logical approach to evidence interpretation. Whether or not one agrees with me that this is a problem depends on whether or not one wishes to view handwriting comparison as having a sicentific rationale. If is scientific, it has to be logical; it follows that if probability is to be invoked, then the laws of probability cannot be violated.

Probabilistic thinking in relation to forensic science has, until comparatively recently, been seen to be something that evolved in the 1970's, when the paper by Finkelstein and Fairley (4) was an important milestone—though that, in turn, had evolved to some extent from lines of reasoning followed by Mosteller and Wallace (5) in considering the authorship of *The Federalist* papers. However, recent research at the University of Lausanne (6) has pinpointed the work of Poincaré, Darboux, and Appell as, apparently, the first example of what we now call the Bayesian view of forensic evidence. It is particularly germane that the reasoning of Poincaré and his colleagues was concerned with a critical review of Bertillon's evidence in a notorious handwriting case: the trial of Dreyfus for treason. In modern parlance, we would say that Bertillon committed what Thompson and Schumann (7) called the "prosecutor's fallacy". Poincaré and his colleagues pointed out the error.

It is not necessary for me to explain the Bayesian view here because of the extensive body of literature that now exists in the forensic sphere. Useful introductions to the ideas are provided by Robertson and Vignaux (8) and Aitken and Stoney (9). The key principles that emerge from this view include, first, the notion that the forensic scientist should always consider (at least) two propositions that, in the adversary system of justice, will represent the defence and prosecution positions. Next, the fundamental principle is that the scientist must address questions of the kind "what is the probability of the *evidence* given the proposition?". Questions of the kind "what is the probability of the proposition given the evidence?" are the province of the jurors, who will not only take into

account the scientific evidence in their deliberations, but also all of the other evidence that is put before them.

In the case where there are no more than two competing propositions then the weight of evidence in favour of one or other of them is a function of the *likelihood ratio*. This is the ratio of the answers to two questions:

> What is the probability of the evidence if the prosecution proposition is true?
> What is the probability of the evidence if the defense proposition is true?

A likelihood ratio greater than one means that the prosecution proposition is supported whereas the defence proposition is supported when the likelihood ratio is less than one. This inspires the notion of a reporting convention based on the use of the word "supports" together with appropriate qualifiers that has been explained elsewhere—for example, Evett and Weir (10).

The consequence of this logical view of inference in the legal framework is that is is not appropriate for the scientist to frame his/her opinion in the form of a probability for the truth of a proposition. Interestingly, a similar view was reached through different methods and in a broader context by Popper (11) who said (p 394) "I regard the doctrine that the degree of corroboration or acceptability cannot be a probability as one of the most interesting findings of the philisophy of knowledge".

Yet this logically impermissible kind of probability statement underpins the ASTM standard, which embodies such expression as:

> "There is *strong probability* that the John Doe of the known material wrote the questioned material, or it is my opinion...that the John Doe of the known material *very probably* wrote the questioned material."

The convention also sanctions the use of the word "unlikely" (as an equivalent to "improbable") within the context:

> "It is unlikely that the John Doe of the known material wrote the questioned material."

This suffers the same logical fault as the former. These are probability statements about the truth of propositions.

It is my view, therefore, that if the ASQDE has the policy of promoting the view that forensic handwriting comparison is a science then it must change its reporting convention, because it cannot be logically justified.

References

1. Evett IW. Towards a uniform framework for reporting opinions in forensic science casework. Science and Justice 1998;38:198–202.
2. McAlexander TV, Beck J, Dick RM. The standardization of handwriting opinion terminology. J Forensic Sci 1991;36:311–9.
3. Anon. Standard terminology for expressing conclusions of forensic document. American Society for Testing and Materials, Designation E 1658–96. Reprinted from Annual Book of ASTM Standards Vol. 14.02.
4. Finkelstein MO, Fairley WB. A Bayesian approach to identification evidence. Harvard Law Review 1970;83:489–517.
5. Mosteller F, Wallace DL. Applied Bayesian and classical inference: the case of The Federalist papers. New York: Springer-Verlag, 1964.
6. Taroni F, Champod C, Margot PA. Forerunners of Bayesianism in early forensic science. Jurimetrics 1988;38:183–200.
7. Thompson WC, Schumann EL. Interpretation of statistical evidence in criminal trials—the prosecutor's fallacy and the defense attorney's fallacy. Law and Human Behavior 1987;11:167–87.
8. Robertson B, Vignaux GA. Interpreting evidence: evaluating forensic science in the courtroom. Chichester: Wiley, 1995.
9. Aitken CGG, Stoney DA. The use of statistics in forensic science. Chichester: Ellis Horwood, 1991.
10. Evett IW, Weir BS. Interpreting DNA evidence: statistical genetics for forensic scientists. Sunderland, MA: Sinauer, 1998.
11. Popper KR. The logic of scientific discovery. London: Hutchinson, 1959.

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