Correspondence

In this and subsequent issues, the Correspondence section will be at the end of the Journal rather than at the beginning where it was placed for many years. This change will enable us to reduce the time between the acceptance and the appearance of correspondence items.—Editor

Erratum/Retraction of Matsubara K, Tanabe K, Yuasa I, Nakamura H, Tanabe Y, Idzu T, Takahashi S, Kimura K. A unique and sensitive ELISA technique for typing ABH antigens in bloodstains using UEA-I lectin—The removal of detergent with a Sephadex G-25 mini-column improves sensitivity. J Forensic Sci 1996;41 (1,Jan):35–39.

Sir:

We have to apologize to you and to journal readers for the incorrect data in our paper cited above. We tried to reproduce our ELISA method using monoclonal antibody, because now we cannot obtain commercially available polyclonal-antisera from human for blood group antigens. However, the published result was not reproducible. We checked detailed procedure in the previous ELISA method. Our technician had added poly-lysine onto the well when extracted antigen was applied. Without it, the coloration for antigens never occurred. Also, this coloration did not occur when monoclonal antisera and poly-lysine were used. So, we conclude the coloration in the published ELISA method is based on a nonspecific reaction enhanced by poly-lysine between unknown substances related to blood group antigens in polyclonal antisera and UEA-I lectin. We also conclude that A or B antigen would not be aggregated with H antigen. We are now working on a more accurate and reproducible method.

We apologize once again for having this serious trouble and deeply regret any inconvenience it may have caused.

Kojiro Kimura, M.D., Ph.D., Professor
Kazuo Matsubara, Ph.D, Associate Professor
Department of Legal Medicine
Shimane Medical University
Izumo 693, Japan

Editor's Note: Any and all future citations of the above-referenced paper should read: Matsubara K, Tanabe K, Yuasa I, Nakamura H, Tanabe Y, Idzu T, Takahashi S, Kimura K. A unique and sensitive ELISA technique for typing ABH antigens in bloodstains using UEA-I lectin—The removal of detergent with a Sephadex G-25 mini-column improves sensitivity [Retracted by Kimura K, Matsubara K. In: J Forensic Sci 1996;41(6,Nov)]. J Forensic Sci 1996;41 Jan(1): 35–39.

Correction of Crouse CA, Feuer WJ, Nippes DC, Hutto SC, Barnes KS, Coffman D, Livingston SH, Ginsberg L, Glidwell DE. Analysis of HLA DQα allele and genotype frequencies in populations from Florida. J Forensic Sci 1994 May;39(3):731–42.

Sir:

A collaborative study involving the analysis of HLA DQA1 allele and genotype frequencies in Caucasian, African American, Hispanic, and Haitian populations was conducted by four Florida forensic DNA laboratories. The laboratories included the Florida Department of Law Enforcement (FDLE) at Pensacola, FDLE at Tallahassee, Regional Crime Laboratory-Indian River, and the Palm Beach County Sheriff's Office. Briefly, this paper, cited above, demonstrated that all four laboratories' Caucasian and African American databases, as well as the PBSO Hispanic and Haitian databases, did not deviate significantly from Hardy-Weinberg Equilibrium or in allele frequencies among populations. As a result, the four Caucasian databases could be combined to form a "Florida Caucasian Database" and the four African American databases could be combined to form the "Florida African American Database." The Hispanic and Haitian databases are kept separate. The Palm Beach County Sheriff's Office population samples were obtained predominantly from three area hospital clinics in which only the race/ethnicity was recorded on the purple top blood tube to maintain anonymity. The PBSO population log book maintained a record of each sample with the following information: (a) PBSO code number; (b) the hospital which donated the sample; (c) the date PBSO picked up the sample; and (d) the race/ethnicity as determined by the donor at the hospital.

Since the time of the Florida DQA1 publication, PBSO has validated nine additional PCR-based markers including LDLR, GYPA, HBGG, D7S8, GC, CSF1PO, TPOX, THO1, and amelogenin. The original PBSO population samples were used to obtain allele and genotype frequencies for these additional genetic markers. During the course of the accumulation of the population data, it appeared that there might be duplicates in the PBSO databases. On the advice of Dr. Ranajit Chakraborty, all suspected duplicates were analyzed for three additional genetic marker loci (FESFPS, vWA, and HFv, or F13A01) for a total of 12 PCR-based markers per sample. In addition, all duplicates were analyzed using a cocktail of four RFLP markers including MS1 (D1S7), MS31 (D7S21), MS43 (D12S11), and G3 (D7S22) by Cellmark Diagnostics. Suspected duplicates matching at the 12 PCR genetic markers also matched at the RFLP genetic markers.

The duplicates were deemed genuine duplicates because: (a) they matched at 16 genetic marker loci based on the more recent analyses described above; (b) suspected duplicate samples were traced to the same hospital clinical laboratory; and (c) the majority were collected the same week from the hospital clinics. In addition, the most convincing evidence that the samples arose from duplicates is the fact an analysis of the original database showed that there were no corresponding number of partial genotypic matches that would be expected had these samples truely been due from matches among different pairs of individuals. The duplicates have been removed from the PBSO databases and to maintain an appropriate reference population sample size, additional samples have been analyzed for the PBSO PCR-based genetic markers and added to the current databases.

TABLE 1—*Original and current DQA1 genotype frequencies from four Florida laboratories. Observed and (percent observed) HLA DQA1 genotypes in four Florida populations.**

	Population					
	Caucasian		African-American		Hispanic	
Genotype	Original*,† (n = 457)	Current*,‡ (n = 454)	Original*,† (n = 367)	Current*,‡ (n = 363)	Original*,† (n = 100)	Current*,‡ (n = 100)
1.1,1.1	5 (1.1)	5 (1.1)	11 (3.0)	11 (3.0)	2 (2.0)	4 (4.0)
1.1,1.2	24 (5.3)	24 (5.3)	33 (9.0)	32 (8.8)	1 (1.0)	3 (3.0)
1.1,1.3	5 (1.1)	5 (1.1)	3 (0.8)	2 (0.6)	1 (1.0)	2 (2.0)
1.1,2	17 (3.7)	18 (4.0)	13 (3.5)	13 (3.6)	1 (1.0)	2 (2.0)
1.1,3	22 (4.8)	22 (4.8)	8 (2.2)	8 (2.2)	7 (7.0)	7 (7.0)
1.1,4	42 (9.2)	42 (9.3)	34 (9.3)	35 (9.6)	5 (5.0)	7 (7.0)
1.2,1.2	21 (4.6)	21 (4.6)	35 (9.5)	33 (9.1)	2 (2.0)	1 (1.0)
1.2,1.3	7 (1.5)	7 (1.5)	5 (1.4)	4 (1.1)	1 (1.0)	1 (1.0)
1.2,2	32 (7.0)	29 (6.4)	17 (4.6)	17 (4.7)	6 (6.0)	8 (8.0)
1.2,3	39 (8.5)	39 (8.6)	14 (3.8)	15 (4.1)	4 (4.0)	3 (3.0)
1.2,4	39 (8.5)	39 (8.6)	77 (21.0)	75 (20.7)	6 (6.0)	5 (5.0)
1.3,1.3	1 (0.2)	1 (0.2)	1 (0.3)	1 (0.3)	0 (0.0)	0 (0.0)
1.3,2	7 (1.5)	7 (1.5)	4 (1.1)	4 (1.1)	2 (2.0)	1 (1.0)
1.3,3	9 (2.0)	9 (2.0)	6 (1.6)	6 (1.7)	1 (1.0)	1 (1.0)
1.3,4	18 (3.9)	18 (4.0)	10 (2.7)	10 (2.8)	12 (12.0)	9 (9.0)
2,2	7 (1.5)	7 (1.5)	5 (1.4)	6 (1.7)	0 (0.0)	1 (1.0)
2,3	20 (4.4)	20 (4.4)	7 (1.9)	7 (1.9)	1 (1.0)	3 (3.0)
2,4	35 (7.7)	35 (7.7)	24 (6.5)	24 (6.6)	13 (13.0)	7 (7.0)
3,3	16 (3.5)	16 (3.5)	3 (0.8)	3 (0.8)	3 (3.0)	5 (5.0)
3,4	49 (10.7)	49 (10.8)	24 (6.5)	24 (6.6)	19 (19.0)	18 (18.0)
4,4	42 (9.2)	41 (9.0)	33 (9.0)	33 (9.1)	13 (13.0)	12 (12.0)

*Palm Beach County Sheriff's Office, Crime Laboratory, West Palm Beach, FL.
Regional Crime Laboratory at Indian River Community College, Ft. Pierce, FL.
Florida Department of Law Enforcement, Pensacola, FL.
Florida Department of Law Enforcement, Tallahassee, FL.
†Reported in the *J Forensic Sci* 1994 May;39(3):731–42.
‡PBSO data with sample duplicates removed.

Table 1 shows the combined Florida DQA1 observed genotype frequencies reported in the May 1994 paper (Original) and the combined Florida DQA1 observed genotype frequencies using the updated PBSO databases (Current). The current PBSO DQA1 database does not deviate significantly from Hardy Weinberg Equilibrium. Further, when combining the current PBSO DQA1 genotype frequencies with the original three participating Florida laboratories' Caucasian databases, all are in HWE and do not deviate in allele frequency distribution. This holds true for the African American databases as well. Therefore, the four Florida Caucasian databases may be combined and the four Florida African American databases may be combined.

Although it is not possible to determine if there are duplicates in a database in which only one genetic marker has been analyzed, it is imperative that once a series of markers have been tested, the database be analyzed for possible duplicates.

Acknowledgments

PBSO would like to extend a special thank you to Dr. Ranajit Charkraborty, Dr. Bruce Weir, Dr. Robin Cotton, Dr. Charlotte Word, David Sipes, and the authors of the Florida DQA1 manuscript.

Cecelia A. Crouse, Ph.D.
Serology/DNA Supervisor
Palm Beach County Sheriff's Office
Crime Laboratory
West Palm Beach FL 33406

Editor's Note: Any and all future citations of the above referenced paper should read: Crouse CA, Feuer WJ, Nippes DC, Hutto SC, Barnes KS, Coffman D, Livingston SH, Ginsberg L, Glidewell DE. Analysis of HLA DQα allele and genotype frequencies in populations from Florida [Corrected by Crouse CA. In: J Forensic Sci 1996;41 Nov(6):]. J Forensic Sci 1994 May;39(3):731–42.

Commentary on McBay AJ. Cocaine sentencing.[1] J Forensic Sci 1996 Jan;41(1):3–4.

Sir:

Te-may-toe, Te-maa-toe. What's in a name? That which we call a rose by any other name would smell as sweet (1). Luckily, Shakespeare never had to worry about interpretation of section 841 of the US Code title 21.

I would like to reply to several inaccuracies in a letter to the editor entitled "Cocaine Sentencing" which appeared in the January 1996 issue of your journal. Like the author of that letter, I will "beat around the bush" and then get to the real point.

Dr. McBay states, "The problem with the above (US Code and Sentencing Guidelines) is that the penalties depend upon how a chemist reports results." There are problems any time a chemist reports incomplete or inaccurate results, but this is not the fault of the statute. A 100-g mixture containing 1% of cocaine base should be reported as such, not simply as "1 g of cocaine base."

[1]This letter reflects the opinion of the author and does not represent the official position of the Drug Enforcement Administration.

Next, it is stated, "The unscientific name of the hard flaky material is 'crack' ." I would certainly not consider crack to be flaky and the name *crack* is not unscientific; it is nonscientific.

Dr. McBay also states that Congress established a 100 times greater penalty for cocaine base convictions than for cocaine convictions. This is incorrect. Dr. McBay is confusing weight ratios with the length of sentence served. Cocaine base convictions result in only two to seven times the sentence that would be received for an equal amount of another form of cocaine. The penalty for selling 500 g of cocaine *hydrochloride* is a mandatory sentence of at least five years. For selling 500 g of cocaine *base,* the sentence is approximately 15 years from the Sentencing Guidelines, and unlike the five-year mandatory term, this sentence may be reduced by the judge.

It is also stated that "A mixture containing a fraction of a microgram of cocaine and 5000 g of legal substances could result in the same sentence as that given for 5000 g of pure cocaine." This is a hypothetical situation that is unlikely to occur. Sentencing Guidelines (2) state, "Mixture or substance does not include materials that must be separated from the controlled substance before the substance can be used." Ordinary assumptions that the substance was possessed with knowledge of its contents or for the purpose of sale would not apply. The chance that the case would be prosecuted or that a judge would accept anything like half a microgram in 5000 g (0.000 000 01%) as a "mixture" is nonexistent. The US Supreme Court in two instances (3) has specifically declined to include such hypothetical cases in the definition of mixture, leaving great doubt that they would support a sentence based on a weight of 5000 g of substance containing only a trace of drug.

Finally, I believe, Dr. McBay comes to his point. He is trying to prove that the law is ambiguous based on the meaning of the word *cocaine.* He states, "It is obvious that there is ambiguity in the statute and in the sentencing guidelines. The rule of lenity should apply. . . ." Like most words, *cocaine* can have more than one meaning, depending on the context in which it is used. Bogus arguments are often created by taking words out of context, but this is supposed to be done by politicians and lawyers, not scientists.

In 21 U.S.C. § 841, cocaine base has been singled out for special treatment, different from any other form of cocaine. No unbiased person would reach any other conclusion after examining the entire section. The claim for ambiguity in the statute depends on lifting the word *cocaine* from one part of the statute, claiming that this word means "cocaine base" and nothing else, and then claiming confusion with another section of the statute that uses "cocaine base." If the word *cocaine* always means cocaine base and nothing else, do the often mentioned "cocaine cartels" mean the cocaine base cartels? Are there separate cocaine hydrochloride cartels? The term *cocaine* is most often used by scientists and nonscientists to be inclusive of more than one form of cocaine or when the form of the cocaine is unknown. "Cocaine was found in the stomach," for example, would not mean that cocaine base was found.

The author of the letter himself uses the word *cocaine* in the inclusive sense several times when he states, "Cocaine is available as two distinct chemicals, cocaine and cocaine hydrochloride . . ." or ". . . the cocaine overdose death of the basketball player . . ." or ". . . the type of cocaine used and method of ingestion were not reported . . ."

Coca paste is mostly cocaine base mixed with calcium sulfate left over from the processing of coca leaves. The paste is occasionally found in kilo bricks sold as ordinary cocaine (cocaine hydrochloride). There is, however, no indication that Congress intended to punish the dealer of a brick of coca paste with the same harsh penalties established for cocaine prepared for smoking ("cocaine base/crack"). The US Sentencing Commission responds to sentencing inequities and uncertainties by changing their Guidelines. To exclude coca paste from the penalties associated with cocaine base, the Commission defined cocaine base (for sentencing purposes) to be "crack," the currently popular form of smoking cocaine. Dr. McBay objects that the Sentencing Guidelines definition of cocaine base is not scientific. It is not possible to give a single, exact definition for smoking cocaine. The definition given in the Guidelines is an attempt to look at motive or use, rather than a scientific definition of cocaine base.

References

(1) My apology to William Shakespeare.
(2) 18 U.S.C.S. Appx § 2D1.1.
(3) Neal v. United States, US Supreme Court No. 95-6710, decided Jan 22, 1996.

James A. Heagy
Senior Forensic Chemist
U.S. Drug Enforcement Administration
390 Main Street, Room 700
San Francisco, CA 94105

Author's Response

Sir:

I am grateful for the opportunity to correct inaccuracies in my letter and to offer my opinion of the "Commentary on Cocaine Screening" by Senior Forensic Chemist James A. Heagy of the US Drug Enforcement Administration (DEA).

Mr. Heagy comments on the problem of how the chemist reports results. His example is: "100-gram mixture containing 1% cocaine base." I believe that most chemists would report, 100-g mixture containing 1% cocaine, unless there is a scientifically acceptable way of distinguishing cocaine base from cocaine in such a mixture.

Cocaine base has been defined (1) as: " 'Cocaine base' for the purposes of this guideline, means 'crack.' 'Crack' is the street name for a form of cocaine base, usually prepared by processing cocaine hydrochloride and solid sodium bicarboanate(sic), and usually appearing in a lumpy, rocklike form."

I obtained the statement that crack is a "hard flaky material" from DEA publication, Microgram (2), "Crack Process, hard flakey material produced."

The formula for cocaine in my letter should have been $C_{17}H_{21}NO_4$.

At about the time that my letter appeared, the US Supreme Court decision on Jan 22, 1996, in Neal v. US (3), confirmed my concerns regarding sentencing. I offer the following quote from the syllabus of the Neal decision:

> When the District Court first sentenced petitioner Neal on two plea bargained convictions involving possession of LSD with intent to distribute, the amount of LSD sold by a drug trafficker was determined, under both the federal statute directing minimum sentences and the United States Sentencing Commission's Guidelines Manual, by the whole weight of the blotter paper or other carrier medium containing the drug. Because the combined weight of the blotter paper and LSD actually sold by Neal was 109.51 grams, the court ruled among other things, that he was subject to 21 U.S.C. §841 (b) (1) (A) (v) which imposes a 10-year mandatory minimum

sentence on anyone trafficking in more than 10 grams of 'a mixture or substance containing, a detectable amount' of LSD. After the Commission revised the Guidelines' calculation method by instructing courts to give each dose of LSD on a carrier medium a constructive of presumptive weight, Neal filed a motion to modify his sentence, contending that the weight of LSD attributable to him under the amended Guidelines, was only 4.58 grams, well short of §841 (b) (1) (A) (v)'s 10 gram requirement and that the Guidelines presumptive weight method controlled the mandatory minimum calculation. The District Court followed Chapman v. United States, 500 U.S.453,468, in holding, *inter alia* (among other things), that the actual weight of the blotter paper, with its absorbed LSD, was determinative of whether Neal crossed the 10-gram threshold and that the 10-year mandatory minimum sentence still applied to him notwithstanding the Guidelines. In affirming, the *en banc* (meeting of all judges) Seventh Circuit agreed with the District Court that a dual system now prevails in calculating LSD weights in cases like this.

Held: Section 841 (b) (1) directs a sentencing court to take into account the actual weight of the blotter paper with its absorbed LSD, even though the Sentencing Guidelines require a different method of calculating the weight of an LSD mixture or substance. . . . In these circumstances, this Court needs not decide what, if any, deference is owed the Commission in order to reject its contrary interpretation. Once the Court has determined a statute's meaning, it adheres to its ruling under *stare decisis* (to stand by decided matters) and assesses an agency's later interpretation of the statute against that settled law. It is the responsibility of Congress, not this court, to change statutes that are thought to be unwise or unfair."

"Reasoning that the 'LSD is diffused among the fibers of the paper . . . and cannot be distinguished from the blotter paper, nor easily separated from it,' " (Chapman at 462), "we held that the actual weight of the blotter paper with its absorbed LSD, is determinative under the statute." (Chapman at 468).

Although Chapman established that the weight of the blotter paper must be taken into account, it did not address how courts should do so." (Neal at B592). Chemists' reports should be admitted as evidence only if the weights of controlled substances on blotter papers or in mixtures and in other substances have been determined and reported.

"As a threshold matter, it is doubtful that the Commission intended the constructive-weight method of the Guidelines to displace the actual-weight method that Chapman requires for statutory minimum sentences." (Neal at B595).

I believe my letter explained my definition of cocaine that seems to agree with that of DEA chemist, Mr. Clarke and other scientists. Many times I have determined minute amounts of cocaine in human tissues. In most cases, there was no way that I could determine whether cocaine or cocaine hydrochloride was ingested. The form of cocaine ingested could not be determined in the overdose case of the basketball player or in millions of urine specimens examined by workplace drug testing programs.

Mr. Heagy's letter mentions that, "The US Supreme Court in two instances has specifically declined to include such hypothetical cases in the definition of mixture. . . ." His reference is to Neal (3) which refers to Chapman (4). Both of these decisions appear to support my belief that the weight of the mixture and not of cocaine is used in sentencing. I do not see how they excluded my hypothetical case. In both cases, LSD was on blotting paper. In Chapman, the LSD and the blotting paper were considered to be a "mixture." The dissenting justices stated, "Neither the ambiguous language of the statute nor its sparse legislative history supports the interpretation reached by the majority today. Indeed, the majority's construction of the statute will necessarily produce sentences that are so anomalous that they will undermine the very uniformity that congress sought to achieve when it authorized the Sentencing Guidelines." (Chapman at 468).

Forensic chemists should report the identification and quantitation of what they find and that is cocaine and should not report cocaine base unless they have a scientifically accepted method for distinguishing cocaine base from cocaine. On the basis of the US Supreme Court decision in sentencing must be based upon the actual weight of mixture or substance. A user possessing 60 g (about 2 oz) of a cocaine mixture, containing 10 g of cocaine reported as being cocaine base, and a wholesale dealer possessing 10 Kg (10,000 g, about 22 lb) of pure cocaine, faced the same sentences of not less than 10 years or more than life, without probation, suspension, or parole (5). The cocaine to cocaine base quantity ratio is 1000 to 1. I doubt this was the intention of Congress.

The statement "100 times greater penalty for cocaine base convictions than for cocaine convictions" is incorrect. Elsewhere, I stated there was 100 to 1 quantity ratio.

A very recent publication of Dr. Cone of the NIDA Addiction Research Center (6) indicates that there is little difference in the physiological effects produced by cocaine. The amounts of cocaine and methods of administration he used were: 42 mg smoking, 22 mg intravenously, and 28 mg intranasally.

Commenting on the cardiovascular and subjective effects of smoked and i.v. delivered cocaine, Foltin and Fischman (7) stated: "The potency of smoked cocaine was about 60% of that of i.v. cocaine, i.e., a 50-mg dose of smoked cocaine had effects similar to a 32-mg dose of i.v. cocaine." They also stated: "Thus, smoked cocaine produces many of the positive effects of i.v. cocaine, i.e., a rush, without some of the negative aspects: (1) painful self-injection; (2) possible contact with the AIDS virus; (3) other health risks associated with i.v. drug use; and (4) the stigma of being an i.v. drug user."

Defendents could receive lighter sentences by powdering the lumpy, rocklike cocaine. Smoking powdered cocaine is as effective as smoking lumpy, rocklike cocaine.

On March 4, 1996, the US Court of Appeals for the 3rd. District in US v. Keith James No. 95-3135 vacated the defendant's sentence and remanded the case to the district court for resentencing. The appeal court's opinion was "the court erred in its application of the Sentencing Guidelines enhancement for crack in absence of proof by a preponderance of the evidence that the form of cocaine base James sold was actually crack."

The words "Cocaine base" and "of a mixture or substance containing a detectable amount" should be deleted from the statute and sentencing guidelines. Cocaine sentencing should be based on the weight of the psychoactive substance, cocaine.

References

(1) US Sentencing Commission Guidelines. 18 USCS Appx § 2D1.1.
(2) 'Crack': What is it and what it does. Microgram. Drug Enforcement Administration, Washington. 1986;19:160.
(3) Neal versus United States 56 CCH S. Ct. Bull. p. B585-B598, also 64 U.S.L.W. 4077–80.

(4) Chapman et al. versus United States 500 US.463–77.
(5) 21 USC § 841 (b) (1) (A).
(6) Cone EJ. Pharmacokinetics and pharmacodynamics of cocaine. J Anal Toxicol 1995;19:459–78.
(7) Foltin RW, Fischman MW. Self-administration of cocaine by humans: Choice between smoked and intravenous cocaine. J Pharmacol Exper Ther 1992;261:841–9.

Arthur J. McBay, Ph.D.
Forensic Toxicologist
V 306 Carolina Meadows
Chapel Hill, NC 27514

Proposed international "standard specification/ test method for slip resistance of walkway surfaces (and footwear) in the field and laboratory as measured by a drag type friction tester"

Sir:

This specification/test method was written to establish specifications for both walkway surfaces and footwear and a test method that would be used in both the laboratory and field when measuring the static coefficient of friction. The section on "scope," "terminology," and "references" are included for the information of readers.

The specification/test method is based on the definitions of "slip resistance," "slip resistant walkway," and "slip resistant footwear." These definitions were approved by a majority of respondents to an international survey of "Proposed Uniform Definitions" by the Slip Resistance Coordinating Committee of the National Association of Safety & Health Professionals (NASHP).

Several task groups are currently reviewing the subject document. Pertinent comments will be included in the document before presentation to a standards organization. Anyone interested in further information should contact D. Meserlian at 201/228–2258 or fax 201/228–0276.

This document is a proposed international standard which is being presented to both ASTM and ANSI in the united states and to the international organization for standardization ISO, (Technical Committee ISO/TC 189-Ceramic Tile) etc. This document is being coordinated by D.C. Meserlian P.E. (201) 228-2258 Chairman, Slip Resistance Coordinating Committee of NASHP.

Standard Specification/Test Method for Slip Resistant of Walkaways and Footwear, in the Field & Laboratory, as Measured by a Drag Type Friction Tester.

1. Scope

1.1 This specification / Test method covers the static coefficient of friction requirements for walkway surfaces in order to be called "slip-resistant walkways" under both dry and wet conditions. The method, described herein, provides several specific instances of walkway surface conditions where this specification is not suitable, and applies to both uncoated and coated walkways. This specification/test method is based on approval by a majority of users who require uniform definitions of "Slip resistance" "Slip Resistant Walkways" and Slip Resistant Footwear."

1.2 The values stated in SI units are to be regarded as the standard. English units are shown in parenthesis.

2. Referenced Documents

2.1 Federal Specification KK-165C Leather, Cattlehide, Vegetable Tanned and Chrome Retanned, Impregnated and Soles (Type 1 Factory (shoemaking) Class 6 Strips).

2.2 OSHA-"Walking and Working Surfaces Compliance Guidelines," Federal Register 4/10/90, 29CFR Part 1910, Appendix A to subpart D.

2.3 BOCA National Building code/1990, Commentary Booklet on Sect. 803.5/ "Floor Surfaces."

2.4 Ceramic Tile Institute Field Report CTI82-1-1 (R85) "Coefficient of Friction between Footwear and Ceramic Tile."

2.5 ASTM D2047, "Standard Test Method for Polish Coated Flooring as Measured by the James Machine."

2.6 ASTM D4101, "Standard Specification for Polypropylene Plastic Injection and Extrusion Materials."

2.7 ASTM C1028, "Standard Test Method for Evaluating the SCOF of Ceramic Tile and Other Like Surfaces by the Horizontal Dynameter Pull Meter Method."

3. Terminology

3.1 Definitions

3.1.1 *Friction*—The resisting force that arises when a surface of one substance slides, or tends to slide, over an adjoining surface of itself or another substance.

3.1.2 *Static Coefficient of Friction (SCOF*—The ratio of the horizontal component of force (parallel to the walkway surface and passing through the tester center of gravity and aligned with the tester's major axis) required to overcome the resistance to begin movement to the normal component of the vertical force (weight) of the object.

3.1.3 *Slip Resistance*—That property of a walkway surface which significantly reduces the probability of a person slipping thereon when wearing any footwear. (Ref. "Slip Resistant Walkway" & "Slip Resistant Footwear.")

3.1.4 *Slip Resistant Walkway (Primary Criteria)*—A walkway surface wherein the SCOF between standardized leather and a clean, dry level surface is 0.50 minimum when using a sensor pressure similar to walking sole pressure [69–173 KPa (10–25 PSI)].

3.1.5 *Slip Resistant Walkway (Outdoors or Indoors with Floors Normally Subjected to Water Contamination)*—A walkway surface wherein the SCOF between Neolite and a level, dry or wet (water) surface is 0.60, 0.50 minimum, respectively, when using a sensor pressure similar to walking sole pressure [(69–173 KPa (10–25 PSI)]. The wet kinetic (dynamic) coefficient of friction (KCOF) shall be 0.30 minimum.

3.1.6 *Slip Resistant Footwear (Primary Criteria)*—Any day, smooth unpatterned shoe sole or heel material with a 0.35 minimum SCOF against the "Primary Standard Polypropylene (leather) Test Panel" (Definition of "Standard Leather") when using a sensor pressure similar to walking sole pressure [(69–173 Kpa (10–25 PSI)]. Running sports footwear materials should have a 0.60 minimum SCOF when tested as previously stated.

3.1.7 *Slip Resistant Footwear (Footwear Normally Subjected to Water Contamination)*—Any smooth sole or heel material having a minimum SCOF of 0.60 dry and 0.35 wet, on the "Standard Florida Tile" specified in 2.7, when using a sensor pressure as stated in 3.1.6. The wet Dynamic COF (KCOF) shall be 0.30 minimum.

Note 1—These definitions were approved by a majority of the respondents to an international survey of "Proposed Uniform Definitions" by the Slip Resistance Coordinating Committee of the National Association of Safety and Health Professionals (NASHP).

3.2 Test Method Terms

3.2.1. *"Standardized Leather"*—Leather which has been tested against the "Primary Standard Polypropylene Test Panel" per paragraph 5.5. The average SCOF obtained has been shown to maintain its original 0.35 value over time.

3.2.2 *"Secondary Standard Polypropylene Test Panel"*—A test panel which is marked with a "Standard SCOF," based on simultaneously testing with the "Primary Standard Polypropylene.

Donald C. Meserlian, P.E.
264 Park Ave.
N. Caldwell, NJ 07006

Commentary On Grellner W, Madea B, Sticht G. Pulmonary histopathology and survival period in morphine-involved deaths, J Forensic Sci 1996:41(3):433–437.

Sir:

Dr. Grellner and colleagues are to be commended on their study referenced above which evaluated the contribution of histological findings in determining survival time after heroin injection and compared histological conclusions to conclusions drawn from toxicological findings. While they cited my papers for the toxicological model they did not explain whether they used the Beagle Rules, the KnowledgeMaker classification tree or the Expert 4 Prototype for classification using their toxicological findings. All three models are discussed in the reference cited (Spiehler, J Forensic Sci 1989;34(5):1104–15). The reported results (concordance in 46 out of 56 cases) are similar to the concordance with anamnestic data found for the Beagle Rules.

If one uses the Beagle Rules, then it is possible to obtain a probability of a rapid death depending on how many of the rules are true and which rules are true. Were time interval estimates in the nonconcordant cases of high probability or low probability compared to the concordant cases? In one third of our cases, the rules were unable to determine the time interval classification. I considered the time interval "undetermined" when the probability was less than 0.75. Is this what is meant in the ten cases reported in this paper as "not exactly evaluated" by toxicological findings? Are any of these ten cases implicated in the three nonconcordant cases which were nonconcordant with anamnestic data or the ten cases which were nonconcordant with histological evaluation?

What probability is required in the German courts to reach a medical certainty or scientific certainty of a conclusion such as that a death due to heroin injection occurred in a specific time interval?

Vina Spiehler, Ph.D., DABFT
422 Tustin Ave.
Newport Beach, CA 92663
spiehleraa@aol.com

Author's Response

Sir:

We appreciate the interest of Dr. Spiehler in our article published previously in this journal (1). Her questions can be answered as follows: For the classification of our toxicological findings, the KnowledgeMarker classification tree was used in the same manner as described in the paper of Spiehler (2). This system comprises a decision tree for prediction of time interval between last morphine dose and death, but it does not provide the calculation of probabilities or certainty factors. The Beagle Rules were not included in our study. Therefore, it cannot be said, whether time interval estimates in the nonconcordant cases were of high or low probability compared to the concordant cases. It must be regarded as incidental, that the rate of concordances in the evaluation of the survival time between histological and toxicological methods was similar to the concordance with anamnestic data found for the Beagle Rules by Spiehler. Pulmonary histopathology and toxicology represent different tools to achieve the same aim—estimation of the survival time after morphine injection.

Those ten cases which were classified as "not exactly evaluated" by toxicological methods include constellations with mixed intoxications, small amounts of morphine and cases without differentiation between unconjugated and total morphine. One of these ten cases was implicated in those ten cases which were nonconcordant with histological evaluation. The three cases with nonconcordance between toxicological findings and anamnestic data represent constellations with exact, but clearly wrong classification by the KnowledgeMarker system (toxicological survival times of 3 to 12 h, real intervals: Less than 1 h or more than 24 h). Two out of these three cases (real time intervals <1 h) could have been correctly classified by toxicology if the results of acetylmorphine analysis in brain and urine were included. We therefore recommend the additional detection of 6-acetylmorphine (6-MAM) in brain and urine to make estimations of survival times as correct as possible (3). High levels of 6-MAM of >100 μg/kg brain tissue point to very short survival times of less than 1 h.

German criminal courts require a probability adjacent to certainty (approximately 99.8%) ("no reasonable doubt"). It is of course difficult or even impossible to quantitate toxicological and, in particular, histological findings in this sense. However, both methods can gain importance as indicators during the course of judicial inquiries.

Wolfgang Grellner, M.D.
Burkhard Madea, M.D.
Guido Sticht, Ph.D.
Institute of Forensic Medicine
University of Cologne
Melatenguertel 60-62
D-50823 Koeln, Germany

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The Business of Forensic Pathology

Sir:

Membership rosters of forensic organizations, forensic journals, and newsletters are often used as conduits for governmental agencies to advertise job openings for forensic pathologists. Recently, the Oregon Department State Police in Salem, Oregon sent out notices nationwide announcing the availability of a Deputy State

Medical Examiner position which offered $69,180 to $92,760 annually. The job description stated that the applicant must be Board-certified in anatomic and forensic pathology, be eligible for medical licensure in Oregon, and have a minimum of two years experience practicing forensic pathology in a large medical examiner office. Preference was to be given to candidates who had performed or supervised 1,000 or more forensic pathology/medical examiner autopsies and who have experience teaching forensic and anatomic pathology at a United States medical school. Applicants were instructed to send their curriculum vitae to the Department of the Oregon State Police.

Let's consider the business realities of this job advertisement. The State of Oregon is basically offering a pittance for medical doctors with at least four or five years post-graduate training and the equivalent of at least four years clinical experience. Although they are requesting that candidates have a minimum of two years practical experience, preference will be given to those who have performed over 1,000 medicolegal autopsies. The higher salary range will surely be offered to the applicant with the most experience even though it is not commensurate with salaries in other parts of the country. According to the Standards of the Inspection and Accreditation Committee of the National Association of Medical Examiners, forensic pathologists should not be performing more than 250 autopsies per year. Therefore, the State of Oregon plans to pay a pathologist with approximately eight years of experience a paltry sum for his expertise. In contrast, in the mid-1980s, a graduate from a prestigious three-year law school could expect to earn a starting salary as high as $80,000 working for a posh Wall Street law firm. About twelve years ago, a forensic pathologist fresh out of residency with about 300 medicolegal autopsies to his credit and Board-eligibility in anatomic, forensic, and clinical pathology could have expected to earn about $60,000–$65,000 annually as a deputy medical examiner in an affluent suburban community. Based on those figures, one does not have to be an economist with Alan Greenspan's credentials to predict that, by 1996, starting salaries for forensic pathologists would be well over $100,000 annually.

Another disturbing aspect of the Oregon job "opportunity" is that the medical examiner's office is portrayed as an arm of the State Police. For years, forensic pathologists have preached at national meetings that the work of medical examiners should be independent of law enforcement agencies and prosecutorial offices. In most jurisdictions, the medical examiner's office falls under the umbrella of the Department of Health. Medical examiners hold an awkward position in the criminal justice system because they are both insiders and outsiders. When they enter the halls of justice to testify as medical/forensic experts, they become temporary insiders. Yet, they are always outsiders because they are not members of the legal profession who control the criminal justice system. It is conceivable that a medical examiner who accepts the Oregon job runs the risk of being cross-examined by a defense attorney about his loyalties to the police and prosecutor. Moreover, any medical examiner who has ever testified as a defense witness in the past is unlikely to be considered for the job. It is common knowledge that members of law enforcement perceive criminal defense attorneys and their forensic experts as antiestablishment and enemies of the people. Unfortunately, these political realities of the medical examiner's business exist, not only in Oregon, but also in many other jurisdictions in the country. The job opportunity in Oregon should remind forensic pathologists that they serve at the pleasure of the legal profession and their autonomy and separation from the adversarial system is only a state of mind.

Mark L. Taff, M.D.
Clinical Associate Professor
Dept. of Pathology
Mount Sinai School of Medicine
New York, NY
Correspondence: 511 Hempstead Ave., Suite 2
West Hempstead, NY 11552

Commentary on Meloy JR. Pseudonecrophilia following spousal homicide. J Forensic Sci 1996 Jul;41(4):706–8

We take exception to Meloy's recent publication on pseudonecrophilia following spousal homicide primarily because his interpretation of events allows a defendant to raise a special condition and unique defense in an attempt to absolve himself completely of criminal responsibility, or to support a claim of diminished capacity or diminished responsibility (1). The author has provided the defendant with at least three different self-serving biases in an effort to reduce his blameworthiness for what we believe is a marital rape-murder (2). According to the defendant, the bizarre sexual act occurred after the murder which would prompt one to believe that he suffered from temporary insanity or diminished responsibility. Secondly, the defendant claims that his wife provoked the fatal confrontation and thus, he is able to invoke a self-defense. Finally, the defendant introduces the issue of limited amnesia due to acute alcohol intoxication, the most commonly invoked excusing condition in criminal cases (3). His temporary loss of memory is unconvincing and does not dissuade us from believing that this case represents a marital rape preceding a multiple stab wound murder. In addition, the defendant was diagnosed on two occasions to be a malingerer indicating that one should be skeptical of his story.

We believe that the evidence clearly shows a classic example of obsessive marital rape because the defendant's sexual interests run toward the strange and the perverse (4). Although the prevalence of marital rape is relatively low, it still occurs more frequently than necrophilia. In Russell's classic study of 930 married women, 14% of them had been the victims of at least one completed or attempted rape by their husbands or ex-husbands (5). In the case herein, the couple's sexual history indicates that rape for the commonlaw husband was the preferred style of sexual arousal and he seemed to derive pleasure from inflicting pain. One must wonder how the husband could inflict pain on a dead body and still satisfy his own sadistic needs. The use of pornographic material to facilitate sexual arousal also supports obsessive marital rape. The autopsy showed that the decedent sustained multiple (6) stab wounds distributed about her body. The presence of "defensive wounds" on her upper and lower extremities suggests that a struggle ensued and the victim attempted to ward off her assailant. The presence of semen in the vagina in the absence of trauma to the genital area is consistent with either rape, in which the female acquiesces to the forceful sexual advances of her male partner, or necrophilia.

The case report also fails to address several crucial forensic issues regarding the defendant. Since he was taken into custody approximately one hour after the victim was found by the police, one would assume that he would have been subjected to a complete clinical forensic medical examination which would have yielded important trace evidence linking him to the decedent. For example, we are curious to know about the defendant's clothing regarding the presence of blood and seminal stains. We would also be interested in knowing if he changed clothes and washed himself off at the murder scene. For a necrophiliac to engage in sexual intercourse

with a bloody corpse, he should have theoretically been drenched in the victim's blood. A trail of blood should have also been apparent at the death scene.

We believe that the defendant has engaged in the use of a rapist's vocabulary of motive which provides him with a variety of excuses and justifications used to disavow his deviance (6). The psychiatric/medical model has dominated the literature on rape which is typically viewed as an individualistic, idiosyncratic symptom of a disordered personality, similar to the profiles of pseudonecrophiliacs. In fact, in this case, the defendant met the diagnostic criteria for antisocial personality disorder, polysubstance dependency, and major depressive disorder, single episode. Research has also shown that fewer than 5% of rapists are psychotic at the time of the rape (7). The defendant described herein was not classified as a primary or severe psychopath. It appears that the defendant readily admitted to the sexual assault on his wife, but, attempted to excuse himself by demonstrating that either intent was absent or responsibility was diminished. It is well-known that rapists use alcohol both as an excuse for their behavior and to blame the victim for the crime. Moreover, the defendant has assumed the sick role to portray himself because sick people are not held responsible for their acts, nor for acts committed while in a state of diminished capacity (acute alcohol intoxication). The alcohol defense helps rapists to negotiate a moral identity for themselves by viewing the rape as idiosyncratic rather than typical behavior. The psychiatric perspective of rape often contributes to the vocabulary of motive that rapists use to excuse and justify their behavior.

In summary, we feel that the diagnostic of pseudonecrophilia associated with fatal domestic violence is highly unlikely in this case and affords a clever criminal defense attorney and his psychiatric expert the opportunity to fashion a defense that obfuscates a true martial rape-murder. Apparently, the jury also rejected the defendant's attempts to justify his acts because, ultimately, he was convicted of the homicide.

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Mark L. Taff, M.D.
Forensic Pathologist
511 Hempstead Ave., Suite 2
West Hempstead, NY 11552
Lauren R. Boglioli, M.D.
Fellow, Section of Cardiology
Lenox Hill Hospital
100 E. 77th Street
New York, NY 10021
Bruce L. Danto, M.D.
Forensic Psychiatrist
208 E. Valencia Mesa Drive,
Suite 208
Fullerton, CA

Author's Response

Drs. Taff, Boglioli, and Danto have given the readers of this journal an excellent example of the error that I cautioned against in my case study: the assumption of sexual *causality* in homicides when evidence indicates sexual activity by the perpetrator.

In fact, the doctors go a step further. In their prosecutorial zeal and ideological drive to fit this case into the Procrustean bed of marital rape-murder, they assert that sex occurred *before* the homicide. None of the facts support this theory of the crime. As I wrote in my case report, the victim was stabbed multiple times and then dragged from the kitchen approximately 7 ft onto the living room carpet; holes in her clothing matched the wound pattern on her body, yet she was nude when found, except for clothing pulled above her breasts; and most importantly, "violent screaming and struggling" (p. 706) began at 2:15 and suddenly stopped at 2:25, 3 min *before* the defendant ordered a cable television pornographic video. These times were earwitnessed by several neighbors and documented by the cable television billing records. Both the prosecution and defense agreed on the sequence and veracity of these facts at trial, and only differed on the defendant's intent.

They why would Drs. Taff, Boglioli, and Danto, cognizant of these facts, attempt to argue that this was a "classic example of obsessive marital rape . . . preceding a multiple stab wound murder"? I think the answer is found in a closer scrutiny of their letter. First, they "take exception" to my study because "it allows a defendant to raise a special condition and unique defense in an attempt to absolve himself completely of criminal responsibility." I find this a curious point of departure, because if one agrees with their position, any expert forensic opinion that could be used by a defense lawyer to advocate for his or her client must be wrong or misguided. Are we advocates for the prosecution? Do we infer opinion from the facts of a case, or do we suppress opinion if it is useful to the defense? Are we forensic scientists or litigation consultants? My position, if it is not already apparent, is that we are first and foremost forensic scientists (1), and we advocate for the veracity of our data and opinions, rather than any particular adversarial perspective.

Guarding against such identifications with either the prosecution or defense positions is an especially difficult task if we hold strong social or political opinions about a particular issue. Clearly, Drs. Taff, Boglioli, and Danto are vehemently opposed to any "excuses or justifications used to disavow his deviance" in this case study. Unfortunately, their prosecutorial alliance impairs their ability to carefully reason their arguments. Let me cite three examples. First, they state that the case "is a classic example of obsessive marital rape because the defendant's sexual interests run toward the strange and the perverse." So if we assume "strange and perverse" (whatever that is) sexuality causes marital rape, we would expect some data. Instead, the doctors cite only statistics on completed or attempted rape within marriage. They cite no data on rape-murder within marriage, no data on sexualized murder within marriage (in fact, most sexual homicides occur outside of an attachment or bond (2,3), and no data on the causative link between "strange and perverse" (watching softcore cable pornography?) and "obsessive" marital rape. This is wild speculation without empirical foundation. Parenthetically, recent research indicates that "we know little about exactly who regularly consumes pornographic materials and what impact such materials might have upon them" (4, p. 232).

Second, the doctors write, "the couple's sexual history indicates that rape for the common law husband was the preferred style of sexual arousal and he seemed to derive pleasure from inflicting pain." I found this a curious conclusion, since I wrote in the case study that I could find "no evidence of an interest in, or history of sexual violence, sexual sadism, necrophilia, or any other paraphilias." (p. 707): An absence of data that made this case of pseudonecrophilia all the more intriguing.

Third, I think the term "consistent with" in forensic science has become a refuge for imprecise and muddled thinking. The doctors write, "the presence of semen in the vagina in the absence of trauma to the genital area is consistent with either rape, in which the female acquiesces . . . or necrophilia." Of course it is. It is also "consistent with" any number of consensual sexual acts in which a man ejaculates into the vagina of a woman. This pseudoscientific standard of forensic expert opinion sounds impressive to the layperson, yet is virtually meaningless and contributes little to the sensitivity and specificity of any scientific hypothesis. I would suggest we attempt to disprove our specific hypotheses more often, instead of formulating them in such a general and inclusive manner that disproval is impossible.

The doctors also asked for more case facts. The defendant did attempt to wash off blood from his body in the bathroom of the apartment, but did not change his clothes when he fled the scene of the murder. As there were, of course, blood and semen matches that linked him to the victim. These latter facts, however, do not explain the motivation for, nor temporal sequencing of, the homicide and vaginal intercourse.

The doctors conclude with what appears to be their motivation for writing this letter in the first place: "the psychiatric perspective of rape often contributes to the vocabulary of motive that rapists use to excuse and justify their behavior." I have no quarrel with this deterministic critique of forensic psychiatry and psychology, but do take issue with their distortion of forensic evidence as I presented it to make such a critique, and their confusion of explanation with excuse. From a legal and moral perspective, there was no excuse for what this defendant did to his spouse. But from a forensic science perspective, our moral repugnance should never dissuade us from seeking the explanatory truth.

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J. Reid Meloy, Ph.D.
964 Fifth Ave., Suite 409
San Diego, CA 92101

Commentary on Taff ML. The Meaning of the O.J. Simpson Verdict. J Forensic Sci 1996 Jul;41(4):552

Sir:

Dr. Mark Taff writes in his letter to the editor "In spite of numerous errors committed by the Los Angeles Police and Coroner's offices, there was a mountain of compelling circumstantial and scientific evidence against Mr. Simpson." Dr. Taff is apparently unaware that the "mountain of evidence" to which he alludes was no more than a minuscule speedbump in the defense case after being subjected to legal tests via cross-examination by Mr. Cochran and Mr. Scheck and by the vigorous challenges of the defense experts, most notably Dr. Henry Lee and Mr. Herbert McDonnell. A case in point is the mysterious appearance of large amounts of blood on the socks from the bedroom floor at Simpson's Rockingham residence after being examined by Los Angeles Police Department criminalists who failed to see any blood on the socks. The other "compelling" evidence suffered from the same deficiencies as did the socks. The net result was that the jury was left with no evidence with the requisite integrity necessary to support a verdict of guilty.

He further states "One of the most important issues to emerge from the Simpson verdict is jury nullification." This is a curious statement in light of the issues facing the jury, to wit, the principal witnesses were proven to have committed perjury, blood evidence which mysteriously appeared on the sock and the back gate of the Bundy scene, the obvious destruction of the blood evidence and the complete lack of integrity of the physical evidence upon which the jury was asked to rely for their verdict. Whether or not the Simpson jury engaged in "jury nullification," the verdict itself was certainly justified by the evidence (or lack thereof) presented to it.

Dr. Taff goes on to state "It was claimed that the L.A. police and medical examiner failed to provide adequate services and a major overhaul in these two agencies was needed. If this is so, how much more money and human energy must be appropriated to convince a 12-member jury of a person's guilt?" Dr. Taff mistakenly assumes that the remedies needed to avoid another debacle of the century are inordinately expensive. Nothing could be farther from the truth than this assumption. These agencies do not need a "major overhaul," but instead they need a new set of sparkplugs and a minor tune-up (i.e., properly trained crime scene investigators and the development of homicide crime scene teams with the competence of their S.W.A.T. teams). In this state (California), the development of investigative teams for sexual assault investigations has long since been accomplished. The development of homicide investigative teams, particularly with regard to the crime scene investigation, needs the same level of development as the sexual assault team approach. The development of similar homicide crime scene teams would be neither difficult nor expensive, especially when compared to the losses sustained from willy nilly investigations such occurred in the Simpson case.

There are many valuable lessons to be learned from the Simpson case by the law enforcement community, but it is clear from the many postmortem dissections of the case in the media and in the publications of the principals in the case that these lessons have neither been identified nor addressed as yet. The red herrings of "racism" and "jury nullification" raised by Dr. Taff serve only to deflect attention from the other, more vital lessons to be learned (such as the need for well trained crime scene analysts and properly coordinated investigative teams) from the catastrophe known as "the trial of the century."

Robert R. Ogle, Jr.
Forensic Scientist
124 Valley Oak Lane
Vallejo, CA 94591