Letters to the Editor

Commentary on Berryman HE, Smith OC, Symes SA. Diameter of cranial gunshot wounds as a function of bullet caliber. J Forensic Sci 1995 Sep;40(5):751–4.

Sir:

In "Diameter of Cranial Gunshot Wounds as a Function of Bullet Caliber," Berryman, Smith and Symes wrote that ". . . a wound produced by a .22-caliber bullet can perhaps be distinguished from a .32 caliber and definitely from a .45 caliber." Their data, however, contains findings from neither .32 nor .45 caliber shots. Their Table 1 shows a range up to 0.43 inches for the diameter of holes made by .22 caliber bullets in the skull. Their data also showed that some holes were smaller than the diameter of the bullet causing them: the range for ".38" caliber bullets started at 0.32 inches. Hence, if a 0.357 inch diameter bullet can produce a hole of only 0.32 inch diameter, it appears likely that a .45 caliber bullet could produce a hole of only 0.42 inch diameter—*which is inside the range that Berryman, Smith, and Symes have shown for .22 caliber bullet holes.* Their data, therefore, appears to contradict their claim that it is "definitely" possible to distinguish a .22 from a .45 caliber bullet by the size of the hole each makes in the skull.

Berryman, Smith, Symes noted that out-of-round holes can be caused by bullets yawing in flight and by bullets striking at angles other than perpendicular to the surface struck. Holes made by these bullets might be oblong—but the bullet diameter can still be determined by measuring the shortest (or minimum) diameter of the hole. In their methods, however, Berryman, Smith and Symes wrote that "The maximum dimension of the circular entrance wound was measured." But in their abstract they contradicted this by claiming to have measured "The minimum diameter of .35 cranial wounds. . . ."

In their discussion, Berryman, Smith and Symes wrote that "In our experience, bullet deformation typically occurs within 1 to 2 calibers of target penetration." How did they determine this? Were measurements taken from gelatin test shots? Was this correlated with the expansion patterns of bullets observed at autopsy? Berryman, Smith, and Symes appear to be saying that an expanding .22 caliber bullet expands in less than half an inch of penetration, yet it could take a .45 caliber bullet nearly an inch to begin expanding. In many years of testing a great variety of expanding bullets in gelatin and anesthetized animals we have seen no such relationship between a bullet's diameter and the depth of penetration where its expansion begins, nor have we ever seen this relationship described in the works of others. We have, however, noted a great variation in penetration depth prior to expansion with certain rifle bullets, which appears related to the size of their hollow point hole (1,2).

In their Fig. 4, which shows several holes of about .40 caliber, reported to have been made by a ".22 caliber gun," it appears that all of these bullets would have passed through the temporalis muscle before striking the skull. Could there be a correlation between larger-than-bullet-diameter holes and expanding bullets hitting the skull in areas where they might have passed through sufficient soft tissue (scalp plus muscle) to have started their expansion before reaching the bone?

The second author of this letter has been measuring bullet holes in the skull in his forensic pathology practice for twenty years and has never seen one, caused by a .22 caliber bullet, larger than about .30 caliber. What was the bullet type of the .22 caliber bullets that caused the .40 caliber holes shown in figure four? Were they, perhaps, rifle shots with expanding bullets of the relatively new "hyper-velocity" type? Were the bullets recovered so that their caliber could be verified with certainty? If so, were their tips flattened to near .40 caliber?

It seems to us that determining the mechanism by which some bullets cause a considerably larger-than-bullet-diameter hole in the skull is critical to making use of these holes for ruling out, or in, certain calibers—which is far more often the forensic question than is determining the exact bullet caliber. Are these considerably larger-than-bullet-diameter holes made only by bullets of expanding type that have begun to expand before striking the bone? Or can a soft lead bullet that does not expand in tissue, such as a round nosed .38 Special or a .22 rimfire, sometimes flatten out on the bone before perforating it? If such flattening occurs, does it happen more frequently with higher bullet striking velocities? Does it have anything to do with the thickness of the skull at the site of the hole? or with the flexibility of the skull as estimated from the age of the victim? These questions can only be answered by collecting a large series of cases in which the necessary information is known.

Far more useful than the table, the statistics, and the first three figures in this paper would have been a simple listing of the raw data, which should include:

- the size and shape of the hole
- the precise location of the hole in the cranial vault
- the age, sex, and race of the victim
- the bullet caliber, weight, type, and manufacturer
- the distance of fire and the barrel length of the weapon
- photographs or descriptions of the recovered bullets.

We are presently collecting such data and would be happy to collaborate with others who would be able to furnish a significant number of cases in which the above listed information can be established.

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Further Commentary on Berryman HE, Smith OC, Symes SA. Diameter of cranial gunshot wounds as a function of bullet caliber. J Forensic Sci 1995 Sep;40(5):751–4

Sir:

In their excellent article, Berryman, Smith, and Symes report that ". . . a .44-caliber measures 0.423 in. and a .45-caliber 0.451 in." It might be useful to clarify a point of possible confusion.

Modern .44-caliber handgun cartridges, consisting of the .44 Special and .44 Magnum, both nominally 0.429 inch in diameter, are loaded with bullets of between 0.429 and 0.431 inch diameter, depending on the manufacturer and bullet style. Various obsolete .44-caliber cartridges, including .44 Smith & Wesson Russian (0.431″), .44 Smith & Wesson American (0.434″), .44 Bull Dog (0.440″), and the .44 Webley (0.436″) illustrate the wide range of bullet diameters which have been used in nominal .44-caliber cartridges. In particular, the historic .44–40 (.44 WCF), as used in the Model 1892 Winchester rifle and in the 1873 Colt Single Action Army revolver, carried a bullet of about 0.426″ diameter. This obsolescent cartridge is still manufactured and may be encountered as used in older firearms or in modern replicas. While this currently uncommon cartridge could be estimated at close to 0.423 inch, it seems misleading to state that a .44-caliber measures 0.423 inch, because there is a far higher probability of encountering a .44 Magnum or .44 Special.

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Author's Response

Sir:

After reading the comments by Drs. Fackler and Mason, it is apparent that the intent of our article was not adequately communicated. The article was specifically directed toward anthropologists who have little direct experience with wound ballistics and who might be tempted to directly measure a well-defined, circular gunshot wound to bone for bullet caliber prediction. The purpose of the paper was two-fold: (1) to demonstrate that statistically significant differences can be established between gunshot entrance wounds (that is, .38-caliber wounds differ from both .22-caliber and .25-caliber), and (2) to outline many of the variables that must be known before attempting to identify caliber from a single measurement of the wound. Unfortunately, outside of a laboratory setting and specifically with decomposed or skeletal remains, these variables are often undiscoverable. The point is that although statistically significant differences can be demonstrated, there are many other calibers not included in our analysis and too many undiscoverable variables for accurate prediction of bullet caliber from wound dimensions. With this once again stated, the five areas presented by Drs. Fackler and Mason can now be addressed.

(1) We state "*If no other factors are involved,* a wound produced by a .22-caliber bullet can perhaps be distinguished from a .32-caliber and definitely from a .45-caliber" (italics added for emphasis). It is true that our data ". . . contains findings from neither .32 nor .45 caliber shots." However, the statement in question appears in the section of the paper in which examples and scenarios are used to outline those variables that are difficult or impossible to ascertain outside of a laboratory setting. The statement is used here to stress that the bullet universe is not confined to .22-, .25-, and .38-caliber bullets alone; rather, accurate determination of bullet caliber from measuring a bony defect must include the wide variety of bullet calibers available. Also, our statement that ". . . a wound produced by a .22-caliber bullet can perhaps be distinguished from a .32-caliber and definitely from a .45-caliber" is not in conflict with our data. An examination of our Table 1 shows that .22- and .25-caliber bullets differed statistically ($P < .001$) from .38-caliber in spite of a range overlap of .11″ (upper range of .22-caliber was .43″ and lower range of .38 was .32″). In other words, if all other undiscoverable factors could be eliminated from consideration, it might be possible to distinguish a .22-caliber bullet from a .32 (we have no feel for this from our analysis of .22-, .25-, and .38-caliber wounds) and definitely from a .45 (our study demonstrated that statistically, .22-caliber wounds differ from .38; therefore, we assume that since the .45 is larger than the .38, the difference should persist).

(2) In the abstract we state that the "minimum" diameter was used while in the methods section we use the term "maximum." This is confusing and deserves clarification as follows: the dimension of the entrance wound was measured avoiding portions of the wound obviously fractured beyond the dimensions of the projectile. Such defects were closely examined for bullet wipe and remnants of the circular contour from which a more meaningful measurement could be taken.

(3) The statement, "In our experience, bullet deformation typically occurs within 1 to 2 calibers of target penetration" is an observation made by the second author (O.C.S.), which apparently differs from those of Drs. Fackler and Mason.

(4) Concerning our Fig. 4, there is no doubt temporalis muscle provides an "intermediate target" between bullet and bone—as would articles of clothing such as a hat or any other intermediate object. This study uses bone trauma specimens from autopsies over the past decade where details beyond caliber were often missing from reports. However, Drs. Fackler and Mason's mention of the expanding potential of smaller caliber hyper-velocity bullets may be added as yet another potentially undiscoverable variable needed for correct caliber classification, specifically in cases commonly seen by anthropologists. In like manner, Mr. Turner notes that the diameter of a single caliber may vary depending upon manufacturer and bullet style. Specifically, .44 Special and .44 Magnum bullet diameters range from 0.429 to 0.431 inch.

(5) Drs. Fackler and Mason outline a number of questions involving both intrinsic and extrinsic factors that influence size and shape of wound defects (for example, effects of bullet structure, composition and velocity, and bone characteristics). They further identify raw data that they feel would have been "[f]ar more useful than the table, the statistics, and the first three figures . . ." used in our article. We agree that the raw data presently being collected by Drs. Fackler and Mason (such as, wound size, shape, and location: age, sex, and race of the victim; bullet caliber, weight, type, and manufacturer; distance of fire and barrel length; and

descriptions of recovered bullets) are worthwhile, and we look forward to their publication of these data. But again and finally, this is outside the stated intent of our paper (reference the last paragraph of our introduction) for which the table, the statistics, and the first three figures are needed.

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Commentary on Rosenbluth W, Hicks L. Evaluating Low-Speed Rear-End Impact Severity and Resultant Occupant Stress Parameters, J Forensic Sci, 1994;39 (6, Nov):1393–1424

Sir:

I wish to point out two incorrect references in the above article. On page 1401 the authors state

"Several low-speed impact references [3–7] show real time vehicle frame stress parameters [Gs] correlated to real time manikin occupant stress parameters and their associated phasing, attenuation and amplification factors."

Their references 6 and 7 (1,2) are both concerned with the dynamics of vehicles in minor rear-end automobile collisions. The focus of these articles is primarily on bumper properties and on simple analytical methods for determining collision severity from bumper performance data. None of the experimentation, analysis or discussion in these articles was related to vehicle occupants. The only mention of occupant motion in either of these articles is in some introductory comments in the second article (their Ref 7) that are intended to educate the reader on the concept of velocity change (ΔV) as a measure of impact severity.

As an aside, it was unnecessary for the authors to invent the *aggregate actuation ratio, net dynamic capability,* and *dynamic actuation product* to arrive at the *calculated BEV.* Rather than this empirical approach, the authors could have used three basic principles of physics (conservation of momentum, conservation of energy, and restitution). Their dynamic bumper isolator compression data coupled with the vehicle acceleration data and vehicle mass would have allowed them to compute the energy absorbed by each vehicle in the vehicle to vehicle impacts. The acceleration data could also have been used to compute the coefficient of restitution. Then application of conservation of momentum and conservation of energy (see for example equation 3 in their Ref 6) would have yielded a value for their BEV. (Note that it is customary to apply this process in reverse: to compute the velocity change from the BEV. BEV data are more abundant and the test protocol is more straightforward, and one is primarily interested in velocity change, not BEV).

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Author's Response

Sir:

Mr. Bailey's comments on one sentence of my 30-page report appear trite and nitpicking when compared with the wealth of original data and conclusions, contained in my paper with Mr. Hicks. Notably, Mr. Bailey does not comment on—or challenge—any of the original data, the methodology or the conclusions expressed therein.

Conversely, Mr. Hicks and I have received direct comments from several readers commenting on the validity and favorable contributions of the methodology and data that we published.

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Commentary on "Another Courtroom Assault on the Confidentiality of the Psychotherapist-Patient Relationship," Leong et al. J Forensic Sci 1995;40(5):862–4

Sir:

Gregory Leong, J. Arturo Silva, and Robert Weinstock write about "another" courtroom assault on the confidentiality of the psychotherapist-patient relationship in reporting on the recent ruling by the California Supreme Court in *People v. Webb* allowing the defense to have access to the psychiatric records of a prosecution witness (Sept. 1995).

Actually, the ruling is not unprecedented. In *United States v. Lindstrom,* 698 F.2d 1154 (1983), the 11th Circuit held that the right of cross-examination was unconstitutionally curtailed by denying the defendant access to medical records relating to the psychiatric treatment of the government's chief witness. The defense contended that the witness was conducting a personal vendetta. The witness's records indicated a diagnosis of hysterical personality, paranoia, delusions, hallucinations, and chronic misinterpretation of the words and conducts of others. Because the witness provided key testimony for the government's case and the line of inquiry sought by the defense was relevant to the witness's perceptions and motives in testifying, the court concluded that the constitutional right of confrontation dictated access to the records.

The *Lindstrom* decision is one of many holding that due process and confrontation require the disclosure of information including psychiatric records that are relevant to the preparation and presentation of a defense.

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Authors' Response

Sir:

We appreciate Professor Slovenko's interest in our article (1). However, his example of *U.S. v. Lindstrom* is an extreme one involving a witness with psychotic symptomatology. In the case described in our article, *People v. Webb,* the witness in question suffered from no psychotic symptomatology. As we mentioned in our paper, psychotic persons (such as in Professor Slovenko's citation) probably are easier to discredit as witnesses than non-psychotic persons, particularly those with personality disorders who can appear as very credible witnesses.

More importantly though, Professor Slovenko's concern for "relevance," which allows the judicial system the potential to embark essentially on a fishing expedition into a person's psychiatric records, is of major concern to psychiatrists and other mental-health clinicians. Just knowing that one's psychiatric records are being scrutinized by persons not connected with one's treatment carries the potential for harassment of a witness. While we agree that it is possible that a witness' psychiatric records may have some "relevance" to the legal proceedings, the potential for misuse of these records would seem to almost always outweigh "relevance."

We emphasize, however, that the primary intent of privilege is to prevent entry of "relevant" information into legal proceedings. Such a privilege exists in California for psychotherapist-patient communications where *Webb* was adjudicated. Interestingly, Professor Slovenko cites a federal case, *Lindstrom*. There was no psychotherapist-patient privilege recognized in federal courts at the time *Lindstrom* was decided. Therefore, it is unclear as how to compare these two cases. Nonetheless, maintaining protection of private, privileged psychotherapist-patient communications merits closer review by a combined task force composed of the members of the legal profession and psychiatrists and other mental health clinicians.

Reference

(1) Leong GB, Silva JA, Weinstock R. Another courtroom assault on the confidentiality of the psychotherapist-patient relationship. J Forensic Sci 1995;40:862–4.

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The Meaning of the O.J. Simpson Verdict

Sir:

The never-ending murder trial of O.J. Simpson finally came to an end when he was found not guilty on 3 October 1995. After the jury deliberated more than a year's worth of evidence in just four hours, an African-American icon, accused of brutally stabbing to death his white ex-wife and her white male companion, was set free. 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. In effect, the defendant had autographed the crime scene with his own blood. In addition, a history of domestic violence, wife battering, and the recent termination of the Simpson marriage existed. The team of defense lawyers also exposed a rogue cop with racist beliefs, which introduced the issue of race into the trial.

One of the most important issues to emerge from the Simpson verdict is jury nullification. Although one cannot prove that jury nullification occurred, it deserves consideration. Jury nullification, in the hands of those who feel disenfranchised because of race, gender, class, religion, etc., can be used as a political weapon to undermine the current criminal justice system. Political justice can be achieved through jury nullification, which was often invoked in the past when juries were dominated by white males. Jury nullification can also eliminate the need for people to protest, march, riot, burn down neighborhoods, and even vote. At present, the judicial system is at the mercy of jurors who invoke their right to use jury nullification to express their dissatisfaction with the system or with opposing viewpoints. After the Simpson trial, 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? Will the additional expense guarantee that jury nullification not be used arbitrarily to disregard the facts of a case? Will non-unanimous jury verdicts eliminate jury nullification? The Simpson verdict has forced America to openly discuss the "R" (racism) word, the most feared issue in America today. Racially biased jury decisions will not only negate the work of forensic scientists, but also jeopardize the integrity of the present criminal justice system.

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Commentary on Gomez HF, McKinney P, Phillips S, Roberts DV, Brent J, Watson WA. Postmortem acetaminophen pharmacokinetics: An experimental study of site and time-dependent concentration changes, J Forensic Sci 1995 Nov;40(6):980–2

Sir:

Britain and America are often said to be two countries divided by a common language. One example of the confusion of language is that the drug that you call acetaminophen we call paracetamol. Perhaps this is why Gomez and colleagues (1) did not refer to the wider literature on postmortem paracetamol (sic) redistribution (2–4). The very detailed published human case data indicate that there is little, if any, paracetamol concentration in solid organs, and consequently, little or no potential for postmortem paracetamol redistribution from these solid organs into blood. In those human case fatalities in which substantial site-to-site differences in blood, paracetamol concentrations are seen, the explanation appears to be either postmortem diffusion from unabsorbed gastric drug residue, or from contamination of the airways, agonal, or postmortem by gastric material that is drug rich.

A rat model of postmortem drug redistribution (5) demonstrated convincingly that airways contamination during drug administration by Jvage causes time dependent increases in postmortem cardiac, and inferior vena cava blood concentrations of amitriptylene. A human cadaver model (6) demonstrated the same phenomenon following alcohol, propoxyphene, and paracetamol airways contamination postmortem. Case studies (7) have also suggested that agonal aspiration of vomitus leads to absorption of alcohol through the lungs and artefactually elevated postmortem levels.

Gomez and colleagues (1) observed a time dependent postmortem elevation of central blood concentrations of acetaminophen

in their rabbit model. However, the drug was given by oral Jvage with the consequent unavoidable risk of airways contamination. It seems likely that the explanation for the observations of Gomez and colleagues (1) is airways contamination by drug during the experimental procedure rather than any drug diffusion from solid organs.

Postmortem diffusion of unabsorbed drug in the stomach is almost certainly a confounding factor given that the animals were sacrificed 20 min after Jvage. A rat model (8) has shown a similar postmortem diffusion of amitriptylene from the stomach into surrounding tissues and blood. Similarly, a human cadaver model has demonstrated significant postmortem diffusion of ethanol (9) and amitriptylene, lithium and paracetamol (10) from gastric residue into blood and tissues. Human case data (11) support the contention that postmortem diffusion of gastric residue drug into the adjacent liver and lung may be marked.

In brief, the animal model used by Gomez and colleagues does not allow a distinction between the phenomena of drug diffusion from inadvertent contamination of the airways, from gastric drug residue, and from solid organ drug reservoirs. Of these three phenomena, the most significant is likely to be diffusion from airways contamination by drug, and this issue is not addressed in the discussion. Even so, the paper does demonstrate the complexity of postmortem drug changes, and the bottom line conclusion is undoubtedly correct: peripheral blood drug concentrations reflect antemortem drug levels more accurately than central sites.

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(1) Gomez HF, McKinney P, Phillips S, Roberts DV, Brent J, and Watson WA. Postmortem acetaminophen pharmacokinetics: an experimental study of site and time-dependent concentration changes. J Forensic Sci 1995;40(6):980.

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(11) Pounder DJ, Adams E, Fuke C, Langford A. Site to site variability of postmortem drug concentrations in liver and lung. J Forensic Sci (submitted).

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Authors' Response

Sir:

Professor Pounder has contributed extensively to postmortem drug redistribution literature, and we appreciate his insightful comments and observation that the "bottom line" conclusion is undoubtedly correct: peripheral blood drug concentrations reflect antemortem drug levels more accurately than central sites. We welcome the suggestion that airway contamination via aspiration of gastric contents with secondary diffusion of drug to the great vessels may be added to the list of possible causes of increasing central vessel drug concentration over time. However, we do not believe enough evidence exists at this time to suggest that in our study "the phenomena the most significant is likely to be diffusion from airways contamination by drug." The investigation was simply not designed to establish the etiology of the drug concentration changes (1).

The articles cited as evidence that airways contamination likely took place by oral gavage are not directly applicable to this investigation because of animal species and technique differences. For example, in the rat model of postmortem drug redistribution by Hilberg et al. (2), the rats were sacrificed before gavage, thus eliminating any possibility of clinical evidence of aspiration (such as cough or difficulty in breathing) during the gavage procedure. Our animals were alive during the gavage procedure, and were capable of exhibiting clinical evidence of airways contamination via cough or respiratory distress had the gavage needle entered the trachea. Furthermore, the animals selected for our study were rabbits, which are incapable of vomiting as a result of the anatomic arrangement of the cardiac sphincter (3). The prospective human cadaver model by Pounder et al. (4) demonstrated that postmortem drug diffusion from airways to large vessels may take place, however, the technique used in this study involved the purposeful injection of drug and ethanol directly into cadaver tracheas via needle puncture (4). This technique is hardly comparable to that used in our investigation (1).

We reviewed, with interest, the coproxamol autopsy case series cited by Prof. Pounder (5,6). In an autopsy case series of four, both liver and kidney tissue acetaminophen concentrations were consistently higher than lung or other solid organ drug concentrations in all cases (5). In an autopsy case series of three, either liver or kidney drug acetaminophen concentrations were higher than other solid organ concentrations in all three cases (6). The significance of these concentration differences is unclear because statistical analysis was not used in either study. Nonetheless, both of these solid organs that receive a significant portion of the cardiac output are responsible for the metabolism, excretion of the vast majority of pharmaceuticals, and may thus account for a portion of redistribution even in those drugs (such as acetaminophen) that apparently have a small volume of distribution. We note that in virtually every case cited in both autopsy series, the gastric and duodenal drug acetaminophen concentrations were several fold higher than all solid organs sampled, and agree that this suggests that the proximal GI tract was the single largest source of drug diffusion in those cases (5,6).

Our study confirmed, in a controlled fashion, findings noted in autopsy case series that postmortem blood samples may not accurately reflect the concentration of drug at the time of death. Using a rabbit model, we found that both the elapsed time interval and the sampling site are important variables in postmortem drug levels (1). The intent in conducting this investigation was to characterize in a controlled and randomized fashion the dual variables

of time and sampling site, not to "allow a distinction between the phenomena of drug diffusion from inadvertent contamination of the airways, from gastric drug residue, and from solid organ drug reservoirs." Although meaningful data have been collected in autopsy case series, there are inherent logistical difficulties in collecting immediate antemortem blood (and tissue) samples and controlling for specific time intervals after death. Comparing drugs with differing antemortem pharmacokinetic attributes, we believe that further advances in the important area of postmortem pharmacokinetics may be achieved through the use of rigidly controlled, randomized, and blinded investigations.

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Through the looking glass: "The time has come to talk of many things." Evolution, status, and needs of the AAFS Pathology/Biology Section.

Sir:

The following is the text of a presentation given at a panel discussion held during the Pathology/Biology Session, 48th Annual Meeting of the American Academy of Forensic Sciences in Nashville TN, February, 1996.

As Ken Field has mentioned, a history of American Academy of Forensic Sciences (AAFS) will be available about one year from now. Today, I will summarize some of the Pathology/Biology Section's history and will include an account of questions and concerns I have heard from you and about this Section. My goal is to reiterate questions and issues you have raised that might be addressed in planning the Section's future.

"The First American Medicolegal Congress" of about 150 people met in 1948. Twenty of 29 papers were given by M.D.s. An ad hoc committee that met later that year in New York under the leadership of Dr. Gradwohl recommended the formation of a permanent organization, and the name American Academy of Forensic Sciences was recommended. Eight of the 17 committee members were MDs. In 1950, in Chicago, at an AAFS organization meeting, a Forensic Pathology Section was created, and Dr. Helpern was appointed Chairman. At the 1950 meeting, there were 99 AAFS attendees of which 43 were M.D.s. Twelve years later, in 1962, the Section name was changed to Pathology/Biology to accommodate those people with doctorates in other biological forensic sciences.

Section membership has grown from about 250 in 1968 to about 800 in 1995, but the portion of total AAFS membership accounted for by Path/Bio has gradually decreased from 43% in 1950 to 20% in 1995. Diminution in the Section's executive role in the Academy is exemplified by this Table of previous AAFS Presidents, there having been seven Presidents of Path/Bio origin in the first 20 years of the Academy's existence, and only five in the past 25 years. Six of the first 10 presidents were MDs. Our struggle in this political area was apparent yesterday at the AAFS business meeting in which our Section's worthiness to hold AAFS office was overtly challenged.

Only six Path/Bio members have received Academy-wide awards. Is this lack of recognition of Path/Bio members deserved because we really haven't contributed much, or are we just overshadowed by the largeness of membership in the other sections? If, indeed, we are not contributing, why do we belong to AAFS? What should we be contributing to, or demanding from the other sections, and vice versa? After all, the touted feature of the Academy is its multi- and interdisciplinary offerings. Are we really taking advantage of this? And how much of the other Sections' activities are really relevant to us? For example, I've heard the question, "What does Jurisprudence have to do with science?" Why are lawyers in the Academy? Although jurisprudence is defined by some as the "science of law," many people perceive that the lawyers' sphere couldn't fall further from the arena of science! Indeed, FORENSIC they are, but scientific? Science is based on predictability and reproducibility, neither of which seem to be characteristics of the legal world. Indeed, we heard at yesterday's plenary session the legitimate concern about the judiciary evaluating science and serving as the gatekeeper for scientific evidence. And look at the 14-hour jurisprudence program this year. Hours of it involve forensic "arts." Many more hours are dedicated to discussion of Jesse James. Do these sessions deal with the science of law? Does the opportunity for interdisciplinary involvement with this section at AAFS meetings really benefit us as pathologists, biologists, and scientists? Four other Academy Sections had joint session with the Jurisprudence Section—why didn't we? Maybe we should plan more carefully our interaction with the other sections. According to the AAFS Strategic Plan Report, the Sections seem to be "turning inward" and isolating themselves, and we need to participate in reversing that trend. Of course, my comments about the Jurisprudence Section are made with tongue-in-cheek. We work in their world, they belong here, and we can mutually benefit through interacting. The point remains, however, that we haven't taken full advantage of the opportunity to interact meaningfully. The same goes for our interaction with other Sections.

The number of Path/Bio members is about the same number as the membership of the National Association of Medical Examiners, and I think we all realize that there is a substantial cross-membership and meeting attendance. As stated in the Academy's Strategic

Plan, its role is NOT to supplant single discipline societies such as NAME, but to provide interdisciplinary approach to forensic science. The inference is that the Path/Bio Section should offer something that NAME does not. But does it? As early as 1976, interactions of Path/Bio and NAME were discussed by this Section. It was generally agreed that NAME is concerned primarily with the administrative aspects of medicolegal death investigation and that the Path/Bio is concerned mainly with the scientific and interdisciplinary technological developments in the field. Despite this differentiation, most who attend the meetings of both organizations know that the scientific sessions are very similar in format and content. Not infrequently, an identical or nearly identical paper is presented at both meetings, even by the same author. Perhaps we need to rethink the issues of science and administration that were raised in 1976, and which relate to our various professional organizations.

Section meeting format and traditions have changed. Manuscripts for publication used to be submitted to the Section Chair or Journal Editor at the meeting. There were half-day slide seminars presented by AFIP and others. For a short time, workshops replaced almost the entire proffered paper session. Many members of our group believe that the marathon nature of our paper and poster sessions lacks excitement compared with the past.

In terms or our meetings now, a problem is the paucity of abstract submissions. Each year, both NAME and the Path/Bio Section struggle to fill the time allotted for the program. The acceptance rate for submitted abstracts is very high because of the need to fill allocated time. But what is the underlying problem? Are our members lazy? Do they not have time to prepare papers? Are people just disinterested? Maybe we've exhausted coverage of relevant topics. To those who come here year after year, presentations seem repetitive. For newcomers, the information is helpful and interesting. How do we meet the needs of both? Should we develop a stratified program with basic, advanced, and masters levels presentations and topics? Maybe members of the various sections should NOT be allowed to attend their own section's scientific program so that interdisciplinary relations and knowledge can be facilitated. Somehow, we must find ways to make the Path/Bio program have distinct and broad appeal and meet the needs of the many subgroups within the Academy. In short, we need to do something that NAME doesn't do, and conversely, NAME may need to rethink its direction in conjunction with this Path/Bio Section.

I've heard it stated that we are being surrounded and infiltrated by increasing numbers of fringe lunatics who thrive on the margins of science. How do we, as a Section, respond to this? The role of ethics review and standards is critical to this issue as we heard yesterday at the Plenary Session and at our business meeting, but we have been largely passive in this area. Dr. Stahl has fully addressed this problem in his comments a few minutes ago.

I haven't forgotten about Path/Bio members who are not physicians, pathologists, or NAME members. Although M.D. pathologists still account for most Path/Bio members, the number of entomologists, botanists, molecular biologists, and other biologists is growing, and this year, for the first time, we have as a Section Officer someone who is not a forensic pathologist. Fifty of us are biologists who make Path/Bio more interesting. But are we meeting the biologists' needs. Are we monopolizing time of theirs that might be better spent? Are they detracting from ours? Incorporation of biology is good if needs of all Section members are met. But what are those needs? This Section needs to define them formally.

I should point out that new sections can be established in the Academy, requiring only 30 people, board approval, and a by-laws change. Are more sections needed? Maybe topic-oriented sessions or groups would be helpful such as "The Exhumation of Long-Dead Famous People Section," or "The Second-Guessers, Doubt-Casters, and Devil's Advocate Section," or more seriously, something like the "Postmortem Interval Sciences Section" (probably not a good name because the acronym would be P.I.S.S.!), or the "Bomb Investigation Section" that are comprised of people who spend most, if not all of their time investigating or studying such fairly limited topics. Multidisciplinary sessions that run concomitantly may be a viable option. Maybe we should shorten our program. But when people pay $1000 or more to come to a meeting, they expect to get sample CME credit for their efforts. Reorganizing the program may, therefore, be a better strategy than shortening it.

Speaking of CME, compliance with CME regulations was another area in which this Section and the Academy were weak, and we have had to re-design our approach to maintain accreditation. Please note that your CME booklet provides you with ample space for comments and suggestions, and I urge you to express your thoughts not only about specific presentations but about ways to improve the Section and program. For the first time, each presenter will receive a report on how their presentation was received. All comments will be reviewed by the CME Committee and used for planning. Hopefully, this will improve the quality of presentations, the program, and the Section. I urge the Section leaders to be creative, and not to follow our usual programming scheme simply out of habit. Along those lines, did you notice that the Saturday morning session was dropped this year, and poster presentations increased in number?

Should we be more selective and reject more papers? If we do, what about people who have to present a paper for their expenses to be reimbursed. The solution is in our hands. If more members would submit a well-prepared abstract, the content of our program could diversify. More papers might then allow for larger numbers of presentations on a given topic and enable subsections within the program, perhaps running concomitantly, thus providing a chance for members to be more selective about which sessions they might attend. Such a plan might also ensure the opportunity for most people who need to present a paper to do so. Success of such an approach, however, depends on participation by more of our members.

In terms of our program's appeal, we tend to apply cute titles to our presentations that make the subject obscure and difficult to preview when trying to determine what we might attend. For example, the title of one paper this year conjured up images that the topic might be paraphia, light bulb electrocution, mutual-starvation and cachexia, or an overdose on light beer. It turned out that the topic was something other than those listed. Creative titles are okay—they make the program fun—but titles should be expanded to clarify the message for those who are searching titles in advance of abstract publication, or later for information or research purposes.

Speaking of publication, few of our presentations are ultimately submitted to the Journal for publication. But they should be. The information in the presentations may be useful to persons who can access the journal, but who cannot attend the meetings or get a copy of the abstract booklet, or who desire more information than that contained in the abstract booklet or presentation.

Until last year, the Section didn't really have a policy and procedure manual. Meeting minute-reporting was inconsistent and incomplete. Committee names, rosters, goals, and progress were not adequately maintained. For example, a Path/Bio committee

was given $500 in 1984 to document the Section's history but never spent the money, nor did the committee document the Section's history. Some other projects have died because of a lack of follow-up. We need to do a more conscientious job in the future to provide year-to-year continuity and follow-up on our projects.

On the positive side, our Section has not totally ignored its problems or its future. Two years ago, the Path/Bio Section, under the direction of Mary Case, established a Discipline Assessment Task Force to forecast trends in forensic pathology and biology in preparation for the year 2020. The Task Force Report was distributed to the membership more than a year ago. The report identified strengths, weaknesses, opportunities, and external threats to our disciplines. It pointed to needs regarding ethics. It also provided an outline to determine what issues exist, why an issue is an issue, the consequences of addressing specific issues, practical approaches to address specific issues, barriers to the approaches, and actions to implement the strategic approaches. Educational activities, the "nontraditional expert," and Clinical Forensic Pathology were specifically addressed. The report was excellent, but it addressed mainly the role of the Academy and Path/Bio Section in promoting Path/Bio and educating of the public, not necessarily those measures for improving Path/Bio Section itself. Nevertheless, I suggest that our Section leaders review and follow up on the Report.

One of the best things we've done was to create the Research Committee, pioneered and nurtured by Liz Laposata beginning in 1985. To date, funds have been awarded for at least 25 research projects, many resulting in publication or presentation. As you heard yesterday, however, no requests for funding have been received in more than a year. Lack of medical examiner office ties with academic institutions, funding cuts to medical examiner offices, and statutes or case law restricting the use of tissues and specimens for research are all issues that may be hampering research, and are issues we need to address actively.

Another plus for our Section is the Best Resident Paper Award that was established in 1984 under the leadership of Jack Frost, who has also served our Section and the Academy long and well in Student Academy activities. Six awards for Best Resident paper have been given since that time, there being no submissions in some years. This program, in my opinion, is in need of greater promotion. A problem is that forensic residents usually begin their tenure in July, and abstracts are due in August. Maybe we should keep a few slots open in the program and allow "last minute entries" to allow five additional months for paper preparation, or require that submitted papers be presented in the year following forensic residency completion. Further thought is needed to realize the goals of this project.

In 1990, a Section Organizational Committee was established to define committees better, which was accomplished. However, committees and ongoing projects are now few. Do we need more, and if so, what should they be?

We established, in 1987, the Section's Milton Helpern Award that has now been bestowed annually to 16 of our members, some living and some dead. Do we need other awards for those in our Section who contribute?

Because of numerous complaints about the promotions policies in the Section, last year the Section removed the "three meeting in five year" requirement to facilitate promotion to fellow status. But I ask you, what benefits are actually derived from fellow status? Why bother? I can think of only three reasons: 1) Academy Officers must be fellows; 2) it sounds more impressive to describe oneself as a fellow rather than a member; and 3) fellow status indicates support through a journal publication or meeting presentation. Do we need to rethink our membership categories, requirements, and benefits? Possibly so.

Finally, a Section Newsletter was published in the mid 1980s, but it subsequently fizzled. Do we need one now, or some other form of ongoing communication?

I'd like to suggest a few things to our Section Officers and members:

(1) review the AAFS Strategic Plan and the Section Discipline Assessment Task Force Report and follow up accordingly;
(2) at meetings, as per 1976 discussions, emphasize forensic science;
(3) offer something that NAME does not, and work with NAME to do so;
(4) move toward interdisciplinary topic orientation rather than, or in addition to our traditional, credential-oriented, somewhat random proferred paper approach;
(5) define and plan to meet the needs of pathologists and biologists in our Section;
(6) define and take on new projects;
(7) work on a plan for ethics review and standards development that includes all forensic scientists that encroach on our discipline and subject matter;
(8) submit more abstracts of better quality with better titles;
(9) consider a move toward an experience-stratified program with more alternatives that might include concomitant sessions;
(10) publish our presentations;
(11) keep better records of activities;
(12) promote research committee and best resident paper committee;
(13) rethink membership classes and promotion procedures; and
(14) follow up on forgotten and newly created projects.

I've now reiterated many thoughts I have heard from you. As a Section, let's think about and act upon them.

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