

## Letters to the Editor

### **Discussion of "Seat Belts and Human Rights: An Appraisal"**

Dear Sir:

I have just read the article titled "Seat Belts and Human Rights: An Appraisal" (Vol. 32, No. 1, Jan. 1987, pp. 158-166), and I am compelled to write you concerning it.

The author exhibits a serious lack of knowledge of the subject of occupant crash protection. For example, the three-point shoulder restraint system he illustrates does not exist in the current vehicle fleet. His statement that "The current automotive restraining system, best exemplified by the single lap belt . . ." is incorrect in that the vast majority of cars on the road today are equipped with lap-shoulder belt systems, not just lap belts. Other examples could be cited.

The author's discussion under the heading of "Mechanisms of Seat Belt Injury" portrays an alarmingly inaccurate picture of how safety belts work. He talks about defective buckles, which he does not reference. He also states that because a belted occupant cannot free himself "because he rapidly loses consciousness, death will follow by drowning or conflagration." This myth has been dispelled more than a decade ago. Belted occupants are kept conscious because they do not strike the interior of the vehicle, that is, suffer the "second collision." The fact that this misinformation featured in this paper indicates that Dr. Greenberg does not have even an elementary familiarity with the valid technical literature on this subject.

Seat belt use, whether voluntary or required, is a public health measure. In that context, it is imperative that evaluation of belt effectiveness follow the rules of sound statistical and epidemiologic design. Instead, Dr. Greenberg chose, in his article, to evaluate seat belt effectiveness by using clinical case studies which have no control population, do not cover the range of crash injuries, and give no information on the nature and severity of the crashes involved. Furthermore, he uses references that are between 10 to 20 years old and tries to apply those findings to fit current seat belt technology and effectiveness. This is inappropriate and results in erroneous conclusions.

Dr. Greenberg makes several statements (on p. 159) that are unsupported about the role of state governments, statisticians, safety experts, and physicians. In fact, I challenge Dr. Greenberg to substantiate his statement that "state governments encouraged statisticians and experts in the field of public safety to report only positive findings regarding the use of these devices." If Dr. Greenberg believes what he says about the true picture of belt use effectiveness being distorted then he seems to be guilty, with this article, of the same grievance.

It is unfortunate that the *Journal of Forensic Sciences* accepted this article for publication without review by individuals versed in occupant kinematics, injury mechanisms, and epidemiologic evaluation.

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### Author's Reply

Sir:

The content of this article on the evaluation of automobile restraint systems is not intended to downgrade the importance of the life-saving potential of these devices. It is well established that deaths and injuries in vehicle crashes may have been averted by their use.

It is equally correct to note that documentation is also available relating to harmful effects that have been associated with the application of seat belts in accidents.

What is emphasized in this paper is that two sides of this issue do exist and that the motor-ing public should be acquainted with both. It is education and not legislation that will render the real verdict on the seat belt. The excellent discussion of this matter by Saunders and Pine (*Health Education Quarterly*, Vol. 13, 1986, p. 243) presents this matter in perspective.

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### Discussion of "Seat Belts and Human Rights: An Appraisal"

Dear Sir:

I find this paper completely misleading and inaccurate. I would like to provide counter arguments as to why we should use our automotive restraint systems, and why seat belt legisla-tion is needed.

1. The old injury data presented in the paper regarding lap belts are misleading. Many of the injuries described do not occur with the lap and shoulder belt. The author fails to men-tion that if belts were not worn, the injured occupants would probably have been killed. In that sense, the paper is deceptive and untruthful.

2. Figure 1 is wrong—the diagonal system is referred to as the three-point system. The double shoulder belt system is not used in passenger vehicles.

3. For pregnant car occupants, it has been shown that the lap-shoulder belt is more effec-tive than the lap belt alone.

4. The idea of being trapped by belts due to a fire or being submerged in water is mislead-ing. If the occupant was unrestrained, he/she will sustain more severe injuries, including loss of consciousness and orientation and will not be able to egress the vehicle. If belted, the chances of maintaining consciousness and of escaping are far higher.

5. All injuries described under mechanism of seat belt injury apply to the lap belt. They do not occur or are much less severe with the lap/shoulder belt. The description of vertebral injuries is also only true for lap belts.

6. Legislation is needed because automotive injuries are a societal problem. The cost runs into billions of dollars which must be paid for by everyone through higher taxes, medical bills, medical insurance, automotive insurance, and higher cost of all products and services. No man is an island. Seat belts (lap and shoulder belts) reduce injuries and save lives. The general population is not aware of the magnitude of the problem and needs the belt law to help us all in reducing the needless waste of resources.

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**Author's Response**

Dear Sir:

The data relating to seat belt injuries cited in this paper are a matter of record. They did occur. No one can refute the idea that automobile restraint systems, of whatever design, are helpful in saving lives and preventing serious injuries in the event of collision. It would not, however, be scientifically accurate to state categorically that if one is wearing a seat belt he would not be killed in an automobile accident. Such proof is yet absent. It is of interest to note that most commercial airlines do not require their passengers to wear seat belts during flight.

Most importantly, the data presented in this study are not intended to downgrade the significance of restraint systems, but are meant to show that there is yet another side to this story; that death and injury can be associated with their usage. A public, totally educated with regard to the automobile restraint system, will be well enabled to make a rational choice without need for additional governmental intervention with the rights of a free citizenry.

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**Discussion of "Pseudoabuse—The Misdiagnosis of Child Abuse"**

Dear Sir:

J. Martin Kaplan's recent article on the (purported) misdiagnosis of child abuse [1] demonstrates the necessity to investigate carefully all circumstances surrounding cases presenting as possible child abuse. While no one disputes that additional and more careful investigators may alter the original diagnostic hypotheses, the impression given by this article is that reporting child abuse concerns is tantamount to a final diagnosis or a formal accusation by the physician. Neither is required nor appropriate.

Physicians are required by law to report suspicions of child abuse (physical abuse, sexual abuse, neglect). Proof is not necessary nor often available. It may not be possible to specify a perpetrator even when one can be certain the child was abused. Reporting initiates a process of further investigation by social workers, who likely will visit the home and consult with a variety of sources often unknown by physicians (for example, previous child abuse records, police reports, and so forth). In Iowa, approximately 30% of all reports are substantiated; about 50% of reported families receive services, however. Thus, reporting may be seen as providing help more often than constituting a "founded" accusation. Physicians are neither the judges nor the juries in these cases, as evidenced by substantiation rates for physician reports of 40 to 60% in most states. The dilemma for all mandatory reporters is balancing "sensitivity" and "specificity." States have responded with laws weighted toward "sensitivity," and rightly so, given the terrible consequences that can result by failure to report.

Several of the cases cited by Kaplan apparently constitute neglect and therefore, in our opinion, were rightly reported. For example, excusing parents who are unable to "deal with the welfare apparatus," and thereby allow a child with cerebral palsy to starve slowly (failure to thrive), provides neither a correction for the child's problem nor does it initiate the very system (child protective service workers) which may help the most. Similarly, the mentally retarded adult who was mentioned may have had good intentions by applying a tourniquet to a child's lacerated finger for 18 h. but the consequences for the child could have been disastrous. Indeed, many mentally retarded parents have their child removed from them because of a failure of judgment [2]. Likewise, some retarded parents do wonderfully while child

rearing. Several other cases were no doubt suspicious enough to be justifiably reported even if the final determination was not abuse.

We recommend that physicians continue thoughtful deliberation when faced with suspicious circumstances, but report when in doubt. Along with Kaplan, we have also encountered callous social workers who have mishandled families. (However, fear of occasional thoughtless physicians would not be a valid reason to cease making referrals when indicated.) Anecdotal reports are insufficient. We ask that Dr. Kaplan provide or cite comprehensive, long-term data to support the contention that families are significantly harmed by the child abuse investigation system (for example, are they significantly more dysfunctional five years later than they would be otherwise?). Finally, we caution that children with organic problems may also be abused and are at a higher risk for such abuse.

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### References

- [1] Kaplan, J. M., "Pseudoabuse—The Misdiagnosis of Child Abuse," *Journal of Forensic Sciences*, Vol. 31, No. 4, Oct. 1986, pp. 1420-1426.
- [2] Seagull, E. A. and Scheurer, S. L., Neglected and Abused Children of Mentally Retarded Parents," *Child Abuse and Neglect*, Vol. 10, 1986, pp. 493-500.

### Author's Response

Dear Sir:

I appreciate the letter from Drs. Alexander and Sivan, and share their concerns. I suspect our approach is not that far apart but I would like to add a few points.

When a person reports child abuse, it is indeed not mandatory to specify a perpetrator but "pointing the finger" is not infrequently encouraged. In at least one state, there is a line in the reporting form in which the abusive party is to be identified. If the physician states that the perpetrator is unknown, the parents' name may be filled in because it is felt to be the latter's responsibility to protect the infant. Since these documents are preserved regardless of substantiation, such unfounded accusations are not without harm. If there is a second report, the unsubstantiated initial one is often rescrutinized.

If the physicians are neither judge nor jury, their statements go a long way in deciding about the thoroughness of an investigation and further involvement of a prosecuting agency. If the physician is sufficiently convinced that abuse has occurred and is willing to appear in court, there is a great deal more likelihood that a trial will occur.

I agree with Drs. Alexander and Sivan that a number of the children I described should (and were) reported as abused or neglected. However, the physician's duty extends beyond "calling in the report." An overly aggressive social worker or attorney can pressure and have come close to imprisoning mentally and emotionally incompetent parents [1]. We must accept a role in distinguishing between an incompetent mother or father and a purposefully abusive parent. This cannot be left to overworked, understaffed, and often inexperienced members of departments of social service, attorney general offices, and so forth. We too must participate.

Finally, the request for long-term data, when one considers the number of patients, parental mobility, and difficulty in identifying appropriate controls would appear unrealistic. On the other hand, if one is looking for evidence of dysfunctional families, a series of articles relating to the overzealous prosecution of fathers accused of child abuse and the disastrous results that have been incurred by the child as well as the parent have recently appeared [2,3]. It is not unusual to see a child separated from a parent for extended periods of time because of such misleading accusations. A response to such false reporting, inadequate investigating, and unfair justice meted out to fathers and mothers has resulted in the formation of a new organization, VOCAL, Victims of Child Abuse Laws, that has begun lobbying against such inequities [4].

The physician's first duty is to protect the child, but the child lives in a family. To tear unjustly the youngster from such bonds, particularly if it is to place the child in such an inadequate situation as foster care, is to be assiduously avoided [5,6].

Thank you again for this opportunity to reply to Drs. Alexander and Sivan's letter.

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### References

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### Discussion of "A Critical Analysis of Quantitative Fingerprint Individuality Models"

Dear Sir:

In their article "A Critical Analysis of Quantitative Fingerprint Individuality Models," Stoney and Thornton [1] discuss a model that I described [2] for the "calculation of the chances of false association, assuming a partial fingerprint with a given incidence." Their analysis evidences a lack of understanding of this model. I am writing this letter to point out the problems with their analysis.

Basically Stoney and Thornton make an attack on a model that I proposed for evaluating fingerprint comparisons without realizing that the model was not really what they were upset about. It is something like killing the messenger because you don't like the message. Judging from their discussion, their main concern is with the use of the world population rather than the population of a much smaller suspect group. If they had used their group size as the population, the model would have essentially given the same results as the approach that they proposed instead.

Stoney and Thornton set up the situation where a room contains  $N$  individuals which represent a population of suspects with which the trace print will be compared, and pose the following question:

If fingerprints of an individual in this suspect group match the evidence print, what is the significance of this finding?

The only difference in what they are setting up is the size of the suspect group. I used the world population, and they used a small suspect group in a room. Stoney and Thornton then tell us how they would proceed to answer the question:

To answer the appropriate question which we posed above, one must compare the chance of the evidence occurring under two hypotheses:

$H_1$ : that the individual in fact made the print.

$H_2$ : that another (random) individual made the print.

Under  $H_1$  it is certain that the print would match the individual. Under  $H_2$  the probability is the frequency of incidence multiplied by the number of attempts we have made to compare the print. A likelihood ratio of these two probabilities gives the relative support of the evidence for the two competing hypotheses.

An appropriate Bayesian formulation of the problem is given by the following formula [3]:

$$P(Er/Ev) = \frac{n}{n + (p'/p^*)}$$

or, as derived from the above:

$$P(C/Ev) = 1 - P(Er/Ev) = \frac{1}{1 + (np^*/p')}$$

where  $P(Er/Ev)$  is the posterior probability (that is, after finding that the prints match) of an error in the conclusion, and  $P(C/Ev)$  is the posterior probability of being correct given the evidence of the matching print. The number of people in the suspect group is  $n + 1$  (which is equivalent to  $N$ ). The posterior probability of the evidence occurring given that the person is the one we are looking for is  $p$  (for  $H_1$ ), and the posterior probability of the evidence occurring given that the person is not the one we are looking for is  $p^*$  (for  $H_2$ ). The ratio  $np^*/p'$  is what Stoney and Thornton say they would use, and we agree on the values to be assigned to  $p'$  and  $p^*$ .

This should make it clear that we are looking at a similar analysis of the problem except for the size of  $n$  (or  $N$ ). The value of  $N$  enters into the model from the prior probability that the person whose fingerprints we are going to compare with the trace print is indeed the person we are looking for. If we assume that each person in the group has an equal probability of being the correct one before comparing the fingerprints, and that the probability is one that the person is in the group, then the prior probability is  $1/N$  that a selected person is the correct one.

Normally we do not know with probability one that the correct person is in a limited suspect group, and thus the use of the size of that group for  $N$  would generally not be correct. In a case where a crime occurred on a remote island, and it is known that nobody has arrived or left the island between the time of occurrence of the crime and the time of obtaining fingerprints from suspects, then it would be appropriate to use the population of the island for the value of  $N$ .

As Stoney and Thornton point out, generally a list of suspects is generated using evidence other than the fingerprint. That evidence must be combined with the fingerprint evidence to form an overall conclusion about the guilt of the individual.

We now need to look at the evaluation of a fingerprint match from two points of view: that of the court which must decide upon the guilt of the person, and that of the expert who provides an opinion on the significance of the fingerprint match. The court must consider all

evidence, and the fingerprint evidence adds to the other evidence to formulate the weight to be given to the final conclusion as to whether or not the person is guilty. The fingerprint expert is presenting an opinion as to the weight to be given only to the fingerprint evidence.

The probability for the court before considering the fingerprint evidence would be based upon its assessment of the value of the other evidence against the accused. The prior probability that the court assigns may indeed be of the same order of magnitude as  $1/N$ , where  $N$  is the size of a limited suspect group. This is essentially what Stoney and Thornton are doing.

A fingerprint expert should not base an opinion about the fingerprint match on an evaluation of the other evidence. It is reasonable, therefore, for the fingerprint expert (before actually comparing the prints) to consider that anyone in the world could have made the trace print, and that the prior probability of any specific individual having left the print is  $1/P$ , where  $P$  is the population of the world. This was the point being made in my model by making the initial assumption that the fingerprint was the only evidence against the person. In that case the prior probability is also  $1/P$ , even for the court.

Using the world population as the value for  $N$  for the evaluation of a fingerprint match is a logical choice based upon practical considerations. First, it appears to parallel the way most fingerprint experts evaluate a fingerprint comparison. Second, the use of fingerprints to "positively" identify individuals as a routine matter, and the possible association of that in the jurors' minds with any positive match of trace fingerprints in a trial, make it important to avoid testimony that could be misleading to the jury or court.

In conclusion, Stoney and Thornton's analysis of the model that I proposed is erroneous, and their solution to the appropriate question is partially correct only if their viewpoint is that of the court and not that of the fingerprint expert.

The model under discussion here has applications to a range of evidence types where individualization is of concern. It is clearly necessary to examine more closely the interpretation, and the presentation, of opinion with respect to physical evidence types which lend themselves to conclusions that are far less certain than is the case for fingerprints. Comparisons involving hair, fibers, paint, and blood are some examples. Some of these situations, and considerations relative to the evaluation and presentation of opinions thereon, will be discussed in future papers.

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### References

- [1] Stoney, D. A. and Thornton, J. I., "A Critical Analysis of Quantitative Fingerprint Individuality Models," *Journal of Forensic Sciences*, Vol. 31, No. 4, Oct. 1986, pp. 1187-1216.
- [2] Kingston, C. R., "Probabilistic Analysis of Partial Fingerprint Patterns," D.Crim. dissertation, University of California, Berkeley, 1964.
- [3] Kingston, C. R., "Applications of Probability Theory in Criminalistics—II," *Journal of the American Statistical Association*, Vol. 60, Dec. 1965, pp. 1028-1034.

### Authors' Response

Dear Sir:

Dr. Kingston's concern is that he believes we are using other (nonfingerprint) evidence to define a limited suspect population and then using this population to define a prior probability. Dr. Kingston argues that in this process one would exceed the proper role of the forensic

scientist and that one would essentially be evaluating the evidence from a judge's perspective. We have two main points.

1. We do not define any population " $N$ " as Dr. Kingston has assumed, nor do we use such a population in the way that he represents.

The "population of suspects" referred to on pp. 1208–1209 of our paper [1] is in no way assumed to be a closed set of some  $N$  individuals that include the actual offender. Accordingly, this population is not relevant to setting up a prior probability, and, indeed, no such suggestion is made.

Our meaning is quite clear in the text: we are concerned with the *number of comparisons* made in an attempt to find a matching print. This issue is discussed fully and is a major theme in the paper [1], pp. 1214–1215:

*Number of Positionings and Comparisons.*

The value of any fingerprint for identification is inversely proportional to the chance of false association. This chance depends on the number of comparisons which are attempted. Each attempt carries a potential for chance correspondence and the greater the number of attempts, the greater the overall chance of false association. "Attempt" means both the number of possible positionings on one individual and the number of different individuals with which the print is compared. A fingerprint model should address this issue and provide a means to determine the number of attempted comparisons.

The authors neither advocate nor suggest the definition of any prior probability whatsoever.

2. We agree with Dr. Kingston that defining priors and attempting to present probabilities of identification exceeds the forensic scientist's role of evaluating a piece of evidence. However, Dr. Kingston's approach is to introduce his own prior based on the world population. In our view this is nonproductive and subjects Kingston's method to his own criticism.

Dr. Kingston mentions neither prior probabilities nor likelihood ratios in his dissertation on fingerprint modeling [2]. In his more general work [3], he contrasts the probability of error in identification with a Bayesian technique that assumes priors.

In our view the practice of assuming a prior of one chance out of the world population is not useful. If we have estimates of random occurrence and of the occurrence under the hypothesis of true association, then we have all the information we could hope to have. The rest of the analysis, incorporating a prior and calculating a probability, involves further assumptions and introduces no more information about the evidence itself. As Dr. Kingston uses it, this amounts to a policy decision that the court ought to assume his prior. In our view this does exceed the expert's role.

We are delighted that Dr. Kingston has taken a renewed interest in these matters. We are certain that he has much to offer and look forward to reading his contributions.

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### References

- [1] Stoney, D. A. and Thornton, J. I. "A Critical Analysis of Quantitative Fingerprint Individuality Models," *Journal of Forensic Sciences*, Vol. 31, No. 4, Oct. 1986, pp. 1187-1216.
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- [3] Kingston, C. R., "Applications of Probability Theory in Criminalistics—II," *Journal of the American Statistical Association*, Vol. 60, Dec. 1965, pp. 1028-1034.

### Discussion of "Free Radical Production from Controlled Low-Energy Fires: Toxicity Considerations"

Sir:

Lowry et al. showed in a recent article in this journal (Vol. 30, No. 1, Jan. 1985, pp. 73-85) that free radicals are detected using the spin trapping technique in controlled low-energy fires when mixtures of materials are burned. They also suggest that these radicals are present in high enough concentrations to be responsible for incapacitation and the deaths resulting from incapacitation in structural fires. We have been investigating the yields of radicals from tobacco smoke [1-3] and from several specific types of building materials [4], and in this letter we wish to present a synopsis of our results and their toxicological implications and relate them to the report of Lowry et al.

We find that radicals are present in the smoke of several common household and construction materials (polyethylene, rubber, cellulose, yellow pine; birch plywood, 1R1 tobacco, and dried exterior paint), but not in the smoke from other materials (nylon, polyvinyl chloride, or polytetrafluoroethylene). We have probed the mechanism for the production of radicals in the smoke from combustion materials [4]. To do this, we performed experiments in which smoke is bubbled into a solvent and a delay time allowed to elapse before the free radical spin trap is added. These experiments demonstrate that the radicals from cellulose smoke can be detected even if the spin trap is added 20 min after the smoke dissolves; this is in striking contrast to the situation for tobacco smoke, where the radicals can only be detected if the spin trap is added within 10 s.

This dramatic difference suggests that radicals trapped from tobacco and cellulose smoke arise by different processes. We have shown that very short-lived radicals occur in cigarette smoke [1,2]. However, these reactive radicals are *continuously* produced in gas-phase cigarette smoke by NO<sub>x</sub> chemistry analogous to that which occurs in smog, giving cigarette smoke radicals an *apparently* long lifetime in the gas phase [3]. However, once these radicals dissolve in solution, they have very short lifetimes. In contrast, cellulose smoke appears to generate *metastable intermediates*, and these intermediates have sufficiently long lifetimes to allow them to dissolve in an organic solvent and then decompose to form radicals that can be spin trapped [4]. Because of the lifetimes of these intermediates both in the gas phase and in solution, it appears reasonable to propose that they are able to travel great distances in a fire environment, to be inhaled, and to reach the distal regions of the lung, where they would decompose and initiate free radical pathology.

The yields of radicals we detect in smoke from the combustion of various materials tested do not correlate with their LC<sub>50</sub> values [5]. It also is worth noting that persons exposed to cigarette smoke for hours, as in smoke filled rooms, are not incapacitated, despite the fact that oxy-radicals exist in high concentrations in tobacco smoke [1-3]. Clearly the radicals in cellulose smoke and tobacco smoke may be quite different. Furthermore, the radicals from the combustion of wood could have more *immediate* physiological effects than do those from tobacco because of their different mechanisms of generation and consequent apparent lifetimes. Nevertheless, it still appears to be inappropriate to state as Lowry et al. do that "... free radicals . . . [are] trapped [in low-energy fires] in concentrations that [provide] an ex-

planation for 'incapacitations without a cause'." Radicals in fires may well play a critical role in the toxic effects of smoke inhalation. However, radicals in smokes cannot be used to explain the incapacitating effects of smokes or other physiological effects until the structures, reactivities and lifetimes of the radicals are known in greater detail.

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### References

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### Author's Response

Sir:

I have followed the research of William A. Pryor, Ph.D. and his coinvestigators concerning free radicals produced by cigarette smoke. He has always been careful and detailed in his experimental conditions and interpretation. As a result he has contributed valuable information to the chemistry of free radicals. It is because of this that I find the comments in his letter most interesting and surprising. Especially surprising is the comment that "radicals in smokes cannot be used to explain the incapacitating effects of smokes or other physiological effects until the structures, reactivities, and lifetimes of the radicals are known in greater detail." The environmental conditions (kinetic and thermodynamic) of a large-scale combustion are totally different than that of a cigarette.

Perturbation effects, energy release, and mass loss, among other factors, cause the behavior of the two combustion situations to differ. The results of the two different kinetic and thermodynamic conditions will result in different products, potentially. Remember, apples grow well in Washington and oranges are produced in the Arizona environment.

We have the results of further studies<sup>1,2</sup> which are to be submitted for publication soon.

<sup>1</sup>W. T. Lowry, W. E. Taylor, and C. S. Petty, "Gas Production and Temperature Profile of a Structural Fire," manuscript in progress.

<sup>2</sup>W. T. Lowry, C. S. Petty, J. E. Trout, L. Juarez, and J. T. Nalbene, "Pulmonary Changes Resulting from Smoke Inhalation—Insite into the Cause of Death," manuscript in progress.

With these studies we have demonstrated significant alterations to the pulmonary surfactant resulting in increased surface tension of the lung thus decreasing gas exchange. As a result of lack of oxygen, incapacitation can occur very quickly allowing the victim to remain in the toxic environment of the smoke. Enzyme studies of glutathione peroxidase (a free radical enzyme) indicate the mechanism is via inhalation of free radicals.

Our studies have not been the carefully controlled laboratory studies of Pryor et al., nor have they intended to be. I agree with Dr. Pryor that identification of the free radical(s) would allow further insight to the toxicology of smoke. Yet we must allow this possibility to be considered in the medical investigation of fire deaths in situations where we cannot explain the cause of death. With medical examiners investigating all possible causes of death we can develop further scientific data to reach eventually a final understanding of fire deaths. The publication in question by Pryor et al. was written to stimulate thought and further data collection. Pryor et al. have shown that the paper initiated thought. I hope soon we can have further data collection.

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### Duplicating Film with an Embossed Dot

Dear Sir:

A problem whereby the identifying bubble on dental X-ray film can no longer be relied upon to determine the left or right sides of the patient is occurring because a major film manufacturer is marketing duplicating film in periapical film size with an embossed dot.<sup>1</sup> While the manufacturer's recommended procedure is to place the emulsion side of the duplicating film in contact with the film to be copied, with both dots going the same direction, a duplicate can still be made with the dots reversed.

Normally, the reversal of duplicating film in sheet form, a common darkroom error, does not cause a problem because left and right must be handmarked. However, relying on the raised dots of periapical duplicating film, a dentist can misinterpret the left and right sides of the patient.

Aside from the difficulty of reviewing personal injury cases with this error, other problems include body identification and potential malpractice liability.

Until this embossed dot problem is resolved, the author believes that use of conventional sheet duplicating film should be encouraged. Thank you.

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<sup>1</sup>Kodak X-Omat Size 2 Duplicating Film  $1\frac{1}{4} \times 1\frac{5}{8}$  in.

### Literary Retrials of Notable Forensic Science Cases—Convuluted History for Sale

Sir:

A recent Gallup poll conducted for the *Los Angeles Times* reported a dramatic decline in the credibility of the media since the time of the last survey taken in June of 1985. The poll included all types of journalists including newspaper, magazine, and television reporters.

The television anchormen for the three networks suffered the worst credibility problem. Dan Rather's rating dropping an astonishing 20% and the others from 10 to 15%. And what was the reason for this erosion of public confidence? It is the public's perception that the political orientation of the broadcasters and their lack of professional standards creates an inherent bias in the way they report the news. With respect to the reporting of the Iran arms incident, seven out of ten of those surveyed believed that "the media helped bring about the crisis in the first place."

In the nonfiction book publishing field, there is in my opinion a similar crisis in credibility relating to publications that can best be described as "revisionary retrials of famous cases." While the authors of most books on famous criminal or civil trials of the past make every effort to recount the events of the investigation and trial in an accurate unbiased manner, there is another type of author who adopts a preconceived point of view at the very outset and then attempts to build a plausible case to support his conclusion. To justify the reopening of an ancient case, he may claim to have fallen upon "sensational" newly discovered evidence which if known at the original trial would have presumably caused the jury to reach a different opinion. Widespread press coverage of some event or person involved in the original trial may persuade a hungry writer to capitalize on the publicity.

Ludovic Kennedy's *The Airman and the Carpenter* [1], dealing with the Lindbergh-Hauptmann kidnapping case tried in 1935 fits the above description perfectly. According to the introduction to his book, Kennedy first became interested in the Hauptmann-Lindbergh case in 1981 when he heard Anna Hauptmann on TV protesting her husband's innocence. Says Kennedy, "The more I listened to Anna Hauptmann, the more convinced I became that she was telling the truth." Then after talking to Mrs. Hauptmann's lawyer, Robert Bryant, who had instituted a suit against the State of New Jersey on her behalf, and after reading two books about the case, *Kidnap* [2] and *Scapegoat* [3], Kennedy makes an astonishing admission when he states: "As a first step (I) contracted with the BBC to make an hour-long documentary film" (titled "Who Killed the Lindbergh Baby?"). So much for objectivity! The book *The Airman and the Carpenter*, published some time later, was the outgrowth of that documentary!

In most literary accounts of famous cases, such as *Perjury*, Allen Weinstein's account of the Alger Hiss trial, the author assumes the role of a juror attempting to evaluate objectively all of the evidence to determine whether he would have agreed with the original jury's findings. Not so with *The Airman and the Carpenter*. Because of Kennedy's blind advocacy for the cause of Hauptmann, it was necessary for him to assume the role of judge, jury, investigator, and of counsel. As a consequence, Kennedy's literary trial of the Hauptmann-Lindbergh case is filled with personal opinions, heresay, conjecture, rumors, omissions of important facts, and disparaging remarks about witnesses of the highest professional caliber. With nobody to object, all of these are accepted by "the Court" with equanimity, as long as they are favorable to the defense. What is worse, the evidence presented by the scientific witnesses at the original trial is distorted or omitted almost entirely from Kennedy's account.

For example, in the handwriting phase of the case, with which all forensic document examiners are most familiar, eight of the foremost experts in the United States were employed by the State of New Jersey to examine independently writings on the 14 ransom notes and compare them with specimens known to have been executed by Hauptmann. They were: Albert S. Osborn, Albert D. Osborn, and Elbridge Stein from New York; John Tyrrell from Milwaukee; Herbert J. Walter from Chicago; Harry Cassidy from Richmond, Virginia; Clark Sellers from Los Angeles; and Wilmer Souder, Bureau of Standards, expert from Washington, DC. All 8 experts reached the same conclusion: "Beyond a reasonable doubt Hauptmann was the writer." At the trial each of the experts demonstrated his opinion with enlarged comparison exhibits which were enormously effective in allowing the jury to see for themselves the evidence connecting Hauptmann to the writing of the ransom notes. Indeed, following the handwriting testimony, Hauptmann himself is said to have remarked, "Dot handwriting is the worstest ding against me."

And how does Kennedy treat these renowned men of science? Using a familiar journalistic device, he first attempts to destroy their credibility. "Looking like senior members of an old folks bowling club," chortles Kennedy. In fact, with the exception of Albert S. Osborn who was in his 70s, the document examiners were in the prime of life, all being in their 40s, 50s, or 60s. "All eight were as much victims of the current epidemic of hallucinations as everyone else," pontificates the author. Then, using another familiar journalistic technique—*distracting the reader from the main issue*, Kennedy expounds at great length about the misspellings in the ransom notes that he *speculates* were dictated to Hauptmann by the police at the time his handwriting specimens were being taken. Even if this were so, and there is absolutely no evidence to support that charge, it does not detract one whit from the identification of the handwriting on the 14 ransom notes as being executed by Bruno Hauptmann.

One does not like to do a misjustice to the author of *The Airman and the Carpenter*, but he surely must have been aware of the significance and tremendous weight of the handwriting evidence against Bruno Richard Hauptmann, and, if fairness was his objective, treated it accordingly in the book. Instead he appears to be anxious to shove under the rug the detailed reasons for the experts' conclusions, using the following incredible explanation: "As their combined testimonies run to some five hundred pages of the trial transcript and are much concerned with technicalities—the shape of a 't' or the curl of a 'y'—and as their conclusions were later challenged by the defense's lone expert using the same material, it is not proposed to go into these in any detail." *Five hundred pages* of trial testimony on one of the most important pieces of scientific evidence connecting Hauptmann with the kidnapping and murder of the Lindbergh baby, and Kennedy does not propose to go into any detail concerning the experts' findings? Ridiculous!

Kennedy states that in his investigation of the Lindbergh-Hauptmann trial he visited the case archives maintained since the time of the trial in Trenton, New Jersey. He does not state whether or not he viewed the enlarged handwriting comparison exhibits prepared by Clark

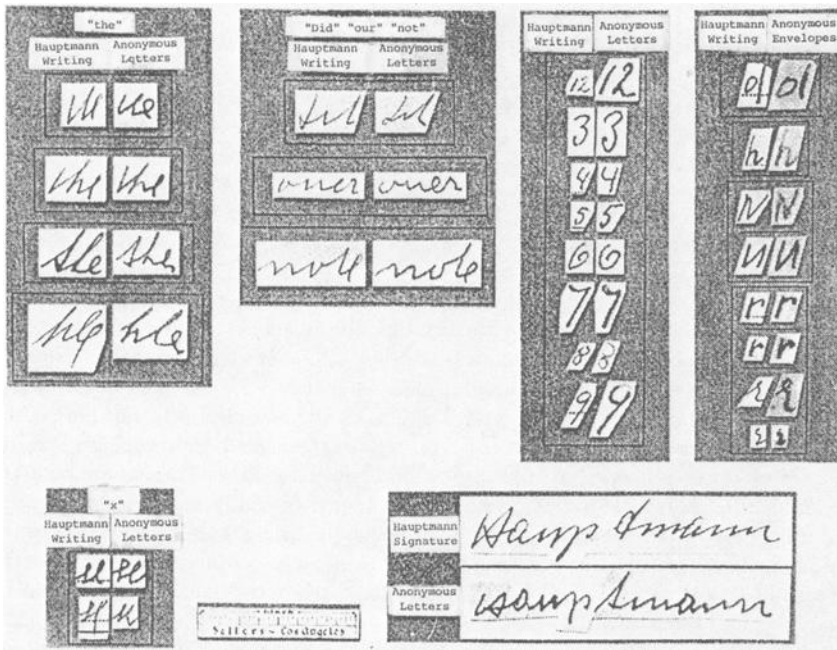


FIG. 1—Set of comparison exhibits used by Clark Sellers at the Flemington trial.

Sellers, John Tyrrell, Herbert J. Walter and the others. (The only handwriting trial exhibits shown in the book are two poorly reproduced photographs beside which are standing Anthony Hauck and Judge Large. The shot was apparently taken by Associated Press.) Had he included in the book the set of comparison exhibits used by Clark Sellers at the Flemington trial (Fig. 1), the readers would have laughed their way through the remainder of Mr. Kennedy's attempted defense of Bruno Richard Hauptmann—the evidence is that compelling.

But what is the answer to books such as *The Airman and the Carpenter* that denigrate our profession and create the impression that miscarriages of justice are a common occurrence in the American justice system? Is this the sort of problem that the forensic science profession should worry about at all?

Yes, I think we should be concerned, but I also think there are positive steps that the Academy can take to educate the reviewing press and public when a book presents the document or other scientific evidence in an unfair or biased manner. One way would be for each section to appoint a book review editor charged with reviewing those chapters of new books dealing with his particular discipline. If he finds the information to be fairly presented, nothing further would be required. However, should the information prove to be deceptive, inaccurate, or incomplete, the reviewer is charged with contacting the major newspapers and magazines that review books with his critique of the offending portion. This would seem to be one of the ways a forensic science group can fight back against the misinformation disseminated in books like *The Airman and the Carpenter*. Perhaps other suggestions will be forthcoming from members of the American Academy of Forensic Sciences.

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### References

- [1] Kennedy, L., *The Airman and the Carpenter*. Viking, New York, 1985.
- [2] Waller, G., *Kidnap*. The Dial Press, New York, 1961.
- [3] Scaduto, A., *Scapegoat*. G. P. Putnam's Sons, New York, 1976.

### Discussion of "Effect of Hypothermia on Breath-Alcohol Analysis"

Dear Sir:

The recommendation by Fox and Hayward (Vol. 32, No. 2, March 1987, pp. 320–325) to measure mouth temperature before breath sampling for alcohol is an interesting one, but I have serious reservations about the practical need for this in every subject to be tested.

Canadian law (*Criminal Code of Canada*) requires that at least two samples of breath, taken a minimum of 15 min apart, be analyzed, and Canadian police forces usually wait 17 min or more to satisfy the Courts that the legal requirement has been met. This, together with the pretest observation period of 15 min or more, means at least 30 min at room temperature from the time of offense to the second breath sample. In reality, this time is likely to be significantly longer, particularly in automobile accidents cited by the authors. The first consideration here is medical attention for the driver, interrogation of the driver, and then breath testing of the driver, if practicable. These events are going to take place in a warmed environment, not on the open highway in mid-winter.

Secondly, Canadian police agencies have adopted the recommendation [1] to analyze a

third sample of breath at least 15 min apart from the second sample, if the first two results differ by more than  $20 \text{ mg} \cdot \text{dL}^{-1}$  (BAC). This policy further extends the period of time the driver spends in a warmed environment and would reveal discrepancies in the analytical results if the driver's BAC is changing significantly during the breath testing procedure.

The *Criminal Code* specifies that the driver must have a BAC that *exceeds*  $80 \text{ mg}$  of alcohol in  $100 \text{ mL}$  of blood ( $80 \text{ mg} \cdot \text{dL}^{-1}$ ). Normally, charges are not laid until the BAC is at least  $100 \text{ mg} \cdot \text{dL}^{-1}$ . It is only at this threshold BAC that the change of as much as 22% proposed by Fox and Hayward becomes relevant. Of course, a condition of hypothermia is of benefit to an accused at this BAC.

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### Reference

- [1] "Recommended Standards & Procedures of the Canadian Society of Forensic Science Alcohol Test Committee." *Canadian Society of Forensic Science Journal*, Vol. 19, No. 3, Sept. 1986, pp. 164-222.

### Authors' Reply

Dear Sir:

We appreciate Mr. Hodgson's comments on our paper concerning the effect of abnormal body temperature on breath alcohol (BrAC) analysis. We were aware that in Canada criminal charges are not normally laid until the BAC is at least  $100 \text{ mg} \cdot \text{dL}^{-1}$ , despite the statutory limit for impairment being  $80 \text{ mg} \cdot \text{dL}^{-1}$ . Where such latitude exists, we agree there is little "practical need" to measure mouth temperature to overcome a potential inaccuracy of up to approximately +20% (hyperthermia) or -20% (hypothermia) in determination of BAC from BrAC. However, for jurisdictions where the statutory limit is interpreted more rigorously, we would like to comment on the inference in Hodgson's second paragraph that hypothermia would not be a factor in BAC determination if the person being tested spent "at least 30 min at room temperature." Cold-exposed persons who become hypothermic demonstrate a well-documented "afterdrop" of core body temperature which occurs *after* the cold stress has been removed. For example, the data presented in Fig. 1 of our paper show that after 30 min of aggressive rewarming in a hot bath, the afterdrop of our subjects was just completed and core temperature was approximately equivalent to that seen when cold exposure had ended and rewarming had just begun. Therefore, it should be made clear that 30 min at room temperature may be sufficient to rewarm the *skin* of a significantly cold-exposed person, but not necessarily core tissues such as the lungs.

It is also true, as Mr. Hodgson points out, that hypothermia would act to the benefit of an accused undergoing BrAC analysis, since it would result in an underestimation of BAC. Although our paper reported the effect of hypothermia on BrAC, we pointed out that *hyperthermia* (for example, fever, heat exposure, post-exercise) should, in theory, cause BrAC to overestimate BAC. In this case, such error would be to the disadvantage of an accused.

In summary, we realize that obtaining maximum and consistent accuracy of prediction of BAC from BrAC is an ideal that must meet practical realities. However, we do not feel that measuring mouth temperature in the way we suggested is significantly impractical in terms

of equipment, time, or expertise required. For the sake of enhanced analytical accuracy, the least that should be done is to provide the means to measure and record mouth temperature *if* circumstances indicate the *possibility* of hypo- or hyper-thermia.

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**Discussion of "Reliability of the Scoring System of the American Board of Forensic Odontology for Human Bite Marks"**

Dear Sir:

In the Oct. 1986 issue of the *Journal*, we published an article entitled "Reliability of the Scoring System of the American Board of Forensic Odontology for Human Bite Marks."

It was felt that this article would generate discussion and feedback relative to the Board's scoring guide. Subsequent discussion and review have led the authors to the conclusion that much more work and consideration will be needed before a stable and accurate index is developed that can be widely applied. The presence of voluminous "statistics" in the article may have led eager readers to form conclusions that are unwarranted by the data at this time. We therefore urge all the professionals involved in forensic odontology to regard the summary and descriptive statistics in the referenced article as preliminary results only.

While the Board's published guidelines suggest use of the scoring system, the authors' present recommendation is that all odontologists await the results of further research before relying on precise point counts in evidentiary proceedings. This does not mean that the investigator should not use the scoring system or other method of analysis that he or she may find helpful. It does mean that the authors believe that further research is needed regarding the quantification of bite mark evidence before precise point counts can be relied upon in court proceedings.

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