

CORRESPONDENCE

Commentary on Englander F, Hodson TJ, Terregrossa RA.
Economic dimensions of slip and fall injuries. *J Forensic Sci* 1996;41(5):733-746.

Sir:

I read with interest the article by Englander et al. reporting on the economic dimensions of slip and fall injuries. Having ventured on similar research here in Canada with respect to the economic cost of falls among Canadian seniors, I well appreciate the difficulties in doing this kind of research. I noted, however, that the authors estimates of the total costs of fall injuries for 1994 of \$64.2 billion (in 1994 dollars) and for 2020 of \$85.37 billion (in 1994 dollars) is inconsistent with my calculations using the same information provided by the author on Tables 2, 3, and 4 (pages 736-737). For example, in both incidences, the authors failed to include the correct adjusted morbidity cost figures. Instead, they erred in indicating to the reader that the adjusted morbidity cost figures were '0' for age groups '0-4' and '5-14', which if calculated would result in considerable differences in the estimations of the total costs for 1994 and 2020. The final figures presented for total costs do appear to be correct as per my estimation of the morbidity costs. I felt that this oversight was significant enough to merit writing you and informing your readers of this miscalculation. As a researcher, I hope that my comments will prompt discussion in regards to this issue.

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Authors' Response

Sir:

We have read the letter from Carl Asche and are embarrassed to report that he is quite correct in his assertions regarding our Table 2. Prompted by his letter, we examined the spreadsheet file from which the table was generated and found that the cells in the first two rows of the "Morbidity Cost" column were improperly entered as names instead of values, to use the Lotus 123 terminology. The cells in the first two rows of the next column, labeled "Morbidity Cost (adjusted)", had the correct formula, but produced zeros because there were no numerical values in the referenced cells. We suspect that we must have inadvertently included an earlier, unrevised print out of Table 2 in the materials we submitted. This is evidenced by the fact that Tables 1, 3, and 4 and the discussion of Table 2 in the text that appeared on page 735 all reflect the same values as appear in the corrected version of Table 2, a copy of which is attached.

We also want to express our thanks to Carl Asche for his alertness in spotting these incorrect cell entries. Typos are ultimately the responsibility of the authors. We wish to convey to you our embarrassment at not spotting these two cells ourselves and to apologize to all who have been inconvenienced by our error.

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Commentary on Geberth VJ, Turco RN. Antisocial personality disorder, sexual sadism, malignant narcissism, and serial murder. *J Forensic Sci* 1997;42(1):49–60

Sir:

Mr. Geberth and Dr. Turco are to be commended for their empirical work concerning serial murder (1). I would like to ask one question for clarification, and then offer several additional comments.

It appears that their subsample of 68 offenders who met criteria for both Antisocial Personality Disorder (ASPD) and Sexual Sadism were also the *only* offenders for which they had enough data to determine whether or not the criteria for these two disorders fit. If this was the case, then are we to assume that virtually all serial murderers who sexually violate their victims are both antisocial personality disordered and sexually sadistic (if sufficient data were available)? I would be interested if the authors would make this inference.

Despite their clarification of the important difference between Antisocial Personality Disorder and psychopathy, the authors do not mention that an instrument is now available for assessing and quantifying the degree of psychopathy in an antisocial individual: the Psychopathy Checklist-Revised (2). This reliable and valid structured interview lets the examiner rate both traits and behaviors of the ASPD subject on a 40-point scale, and has been shown to be an excellent predictor of amenability to treatment, violence risk, and recidivism (3). I would urge the authors to score their subsample of 68 subjects on the PCL-R, since a valid rating can be determined without a clinical interview. This would generate important data for forensic psychological and psychiatric researchers.

I would also like to mention a study in which we psychologically tested and compared samples of sexual homicide perpetrators and psychopaths without a known history of sexual offending (4). Some of our sample of sexual homicide perpetrators ($N = 18$) were serial murderers. Our results confirmed several theoretical and clinical findings that Geberth and Turco summarize from the literature. Our sexual homicide perpetrators, like psychopaths in general, were chronically angry, evidenced borderline (not psychotic) reality testing, were pathologically narcissistic (grandiose and entitled), showed abnormal attachment patterns, and evidenced moderate amounts of formal thought disorder.

Unlike psychopaths, however, the sexual homicide perpetrators were significantly more obsessional and showed some capacity for whole object relatedness and cooperativeness with others. These latter, counterintuitive findings may psychologically contribute to explaining the success of the serial murderer's career—he can be empathic and sociable, and go unnoticed, except when he's killing.

References

1. Geberth VJ, Turco RN. Antisocial personality disorder, sexual sadism, malignant narcissism, and serial murder. *J Forensic Sci* 1997;42(1):49–60.
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3. Meloy JR. Antisocial personality disorder. In: Gabbard G, editor. *Treatments of psychiatric disorders* (2nd edition). Washington, DC: American Psychiatric Press. 1995;2273–2290.
4. Meloy JR, Gacono CB, Kenney L. A Rorschach investigation of sexual homicide. *J. Personality Assessment* 1994;62:58–67.

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Authors' Response

Sir:

Thank you for the opportunity to respond to the letter from Dr. Meloy regarding our paper on antisocial personality disorder and serial murder as published in *J Forensic Sci* 1997;42(1):49–60.

We appreciate Dr. Meloy's kind comments. The answer to his first question is that the subsample of 68 offenders were the only offenders for whom we had *sufficient* data to determine whether or not the criteria for these two disorders fit. (Both ASPD and Sexual Sadism). Within this subsample of 68 offenders are the most infamous serial killers within the United States. In addition to our data and materials, there were numerous newspaper accounts, court records, and other documents available for review as well as biographical information from published trade books on these more publicly recognized serial killers. Had there been sufficient data available of the other serial murderers who sexually violated their victims, we likely would have inferred that most serial murderers are in fact "psychopathic sexual sadists."

The reference to Dr. Robert Hare's "Psychopathy Checklist-Revised" is a point well taken. At the time of our original research (Serial killers in the U.S. up to December, 1993), neither of us (authors) had any specific experience in the application of this instrument. Had we applied the PCL-R to our population we probably would have increased the subsample. We did acknowledge Dr. Hare's work of page 52 of our article and specifically stated "We find this (Hare's) perspective consistent with observable data and applicable to investigation. Hare's work best represents our point of view." We agree that the PCL-R provides researchers and clinicians with reliable and valid assessments of psychopathy.

Our experience "in the field" with sexual homicide perpetrators corresponds to Dr. Meloy's findings regarding the similarity of findings with this population and psychopaths *without* a known history of sexual offending. This includes the chronic anger, borderline but not psychotic reality testing, narcissism (malignant narcissism) and abnormal attachment patterns. We have not specifically

noted any abnormal thought disorder in these groups. We agree that, unlike psychopaths, the sexual homicide perpetrators were more obsessional and showed *some* capacity for whole object relatedness and cooperativeness with others. We also note, however, a play acting, superficial quality to their interactions and sometimes applied the term "cardboard people." We also agree that these (counterintuitive) findings contribute to and partially explain the "success" of the serial murderer's "career" in the context of his sociableness and seeming "empathy" contributing to his going unnoticed in the community at large.

We very much appreciate Dr. Meloy's perspective of our work.

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Reflections on the 1997 AAFS Meeting Plenary Session

Sir:

The New York Plenary Session was focused upon ethics, consistent with the theme of the 1997 annual meeting. The glimpse into the reasoning of ethical philosophers was educational and reassuring. It was delightful for a psychiatrist to see another profession stuck in the mud of ambiguity. The philosophical reflections about the work of forensic scientists showed that the role of an expert witness in American justice system is easily misunderstood.

At times even we forget that scientists are invited into the courtroom to help the decision-makers. Court rules make opinion testimony admissible if it is of assistance to the jury or judge. Our services are needed for the resolution of a dispute. There must be a significant controversy for a case to go to trial. It is therefore not only reasonable but essential that there should be expert witnesses on both sides.

The much maligned "battle of the experts" is not a sin but a virtue. The opposite of an opinion is not a falsehood but another opinion. Expert witnesses naturally disagree which gives some the impression that this is the result of questionable competence or ethics. There are divergent opinions because there is a controversy.

Most forensic practitioners are highly competent and ethical. One of the reasons for this fact is that no other professional activity is under such scrutiny. One ethical or technical error usually ends the career of a forensic expert. Who else has such a Sword of Damocles hanging over his or her head?

A scientist in the courtroom is primarily an educator. Our didactic not ethical shortcoming deserve increased scrutiny.

The philosophical discipline of ethics is fascinating but not a basic science for forensic practitioners. One can devise Rules of Conduct for members of the American Academy of Forensic Sciences without knowledge of the ethical foundations of western culture. I did not need to be knowledgeable in the science of geography to know how to get from Detroit to New York.

A professor of ethics told the assembled forensic scientists that she was asked four times to be an expert witness and she declined every time. One of the requests came from a lawyer representing a tobacco company. "I could never testify on behalf of the tobacco industry," she told the audience. I am a vehement opponent of smoking but I can imagine many situations in which I could testify on behalf of a tobacco company. After all, I testified on behalf of

a Nazi war criminal even though I am a Holocaust survivor. As a physician, I have treated many people whose views and personalities were abhorrent to me.

The mere potential for bias should not disqualify an expert witness from giving testimony. Our esteemed President, Richard Rosner, gave us philosophical reflections on ethics. He was descriptive, never proscriptive. Dr. Rosner avoided labeling anything as unethical. He seemed to advocate ethical teaching which avoids being judgmental.

No such squeamishness was shown by the Chairman of the Academy's Ethics Committee. He relied upon the Code of Ethics and had no trouble labeling some behavior as bad, or even evil. No one on the panel mentioned that, as expert witnesses, we are providers of a service. As such we have ethical obligations to our employers. We should not allow the lawyers to misuse us but, after all, we are hired to do a job.

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The DNA Statistical Paradigm vs. Everything Else

Sir:

Testimony is being given in various courts of law that DNA analysis has "*failed to exclude* the defendant, and consequently the source of this bloodstain is either the defendant, or *another person with the same DNA pattern.*" The analyst will then immediately follow with some qualifying statement that the probability of a random match is one in a squillion, or *somesuch*. DNA analysts seem to be reluctant to positively identify a person, however, even when a conservative estimate of the denominator of the probability of a random match greatly exceeds the population of the Earth.

DNA analysts seem to have embraced the premise that they had best be very, very careful with their statistics, because, if they aren't, their work will be rejected. If this premise is true, and if the DNA paradigm becomes the standard, then most other evidence categories are in deep, deep trouble. We do not, for example, say that "The latent fingerprint is either that of the right ring finger of the defendant, *or else it is that of some other person with exactly the same configuration of ridge minutiae,*" and then go on to state an outside probability that the fingerprint would fall into this rare category. On the contrary, we refuse totally to even acknowledge that there could be such a person, even though most of the probability models proposed for fingerprint individuality do not claim the sort of numbers that are frequently discussed in connection with DNA typing. In a similar fashion, we do not say that "The fatal bullet was fired from the defendant's revolver, *or from some other firearm that marks the bullets in exactly the same fashion,*" and then go on to describe the probability of a chance replication of characteristic striae. We refuse to acknowledge that there could be such a firearm, and yet the statistical underpinning of firearms evidence is much more poorly understood than is the statistical basis of DNA typing.

Perhaps experts in other areas should, no pun intended, bite the bullet, and begin to profess in the courtroom what they so willingly confess in the hallway, that statistics and probabilities are the

foundation of everything that we do, and everything that we opine. We have tended to reject statistics and probability because we generally don't understand them, and to concede their validity forces us to admit to our ignorance as to how they would be implemented. We've gotten away with it for quite a while, but the world is changing and DNA may well be the harbinger of that

Is The Power of Inquest a Valuable Asset?

On Friday, June 4, 1993, I trained to Windsor to attend a coroner's inquest. Actually, the trip involved a car from Ducklington to Oxford, a train from Oxford to Slough, a cab from Slough to Windsor, and then a three-mile walk from the risk manager's office in Windsor to the Guild Hall where the inquest was held. This is immediately adjacent to the front gate of Windsor Castle, and the hearing was interrupted by the changing of the Guard.

Guild Hall is almost as old as the Castle, and the main meeting room is graced with very large and beautiful paintings of current and prior monarchs. At one end of the room is the Judge's bench. In front of that are several tables and chairs. Along one side of the room are rows of chairs where I sat, trying to be inconspicuous because this was one of our cases and the risk manager did not want the coroner or the Press to be aware that we were concerned about it. The coroner took his own notes during the hearing. No court reporter was present, but the Press took notes by shorthand and there was much to scribble about.

The case involved a 40-year-old woman who had been hospitalized urgently because of suicidal tendency. In a matter of hours, however, she improved and did not require constant observation. When she went down to dinner in the psychiatric hospital, she was able to escape without observation, and promptly jumped into a pond next door, drowning herself.

There was no question about the manner and cause of death; this was the sixth time one of the inmates succeeded in drowning in the same pond. In 1992, the fifth case occurred and the coroner held an inquest and proceeded to condemn the hospital authority for not fencing the pond because of the four prior cases.

In the 1993 inquest, the coroner took out his notes from the 1992 inquest and recited his reading of the riot act to the health authority to get something done. Yet, when the fifth patient succeeded in drowning herself, the fence had not been built. (By the time of the inquest, the health authority agreed that the fence be built, even though it maintained doing so was not because of medical necessity, but because of public opinion).

There was some testimony by one of the consulting psychiatrists to the effect that the hospital where the patient drowned is an open institution, the decedent was not in the institution involuntarily, and the pond was a therapeutic environment which should not be fenced. The coroner felt otherwise; or he at least felt public opinion required fencing. He reiterated that criminal problems might arise if a seventh patient succeeded in jumping in the pond.

After hearing the testimony of various witnesses, including the surviving husband, the administrator of the hospital, a consulting psychiatrist, and the attending nurse, the coroner proceeded to sum up his case in the form of a lecture. He repeated his complaints about the dilatory response of fencing. The Press wrote furiously. Then the coroner concluded this woman was going to commit

change. To master statistical models to explain much of our evidence may be a slow, reluctant march through enemy territory, but we must begin to plan for that campaign.

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suicide under any circumstance, and the absence of the fence really made no difference. In two sentences, therefore, the coroner undercut his own case. Still, the fence is now in place.

Is it a good idea to have the facility to hold public hearings? Prosecutors have two such opportunities to help decide the existence of probable cause for criminal liability. The Grand Jury is a public hearing even though in a secret forum. Preliminary hearings are thoroughly public forums. Each serves an evidentiary and a public disclosure function.

Coroners have had the opportunity to perform inquests, a form of public hearing. The purpose is to determine the cause and mode of death, not to determine probable cause for criminal liability. The function is very similar to the forums available to the prosecutor. Is the evidence adequate to come to a conclusion as to the cause and mode of death? Is there some reason to have a public hearing in addition to these evidentiary goals? At Windsor, the answer is yes. I sensed that such hearings were frequent in the U.K., but I do not know how often coroners utilize this forum in the United States. If this forum is not being used, why not? Should the Academy take the position of encouraging the use of inquests? Can medical examiners utilize this forum? If not, should we encourage this also?

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Commentary on Gernandt MN, Urlaub JJ, An Introduction to the Gel Pen. *J Forensic Sci* 1996 May; 41(3):503-4

Sir:

Gel pens are also available in the UK on a fairly limited basis. Having obtained a selection of these I reviewed their ink characteristics. I noted that the blue Pentel Hybrid Roller blue exhibited a faint but distinct fluorescence when viewed using my VSC-4. The fluorescence is promoted by an input radiation of 440-600nm and is seen through the 830 nm camera filter with 0.6 s integration.

The authors of the technical note detected no fluorescence. However, is this a real difference between US and UK pens or merely a lack of sensitivity in the instrument used by the authors? The early version of the VSC-1 does not have the capabilities of the models introduced in the early 1990's to detect faint fluorescence.

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