

## Letters to the Editor

### Discussion of "On the Predictability of Violent Behavior: Considerations and Guidelines"

Dear Sir:

Dr. Palermo and his associates have written a generally good article on the predictability of violent behavior [1]. It does contain, however, some conceptual flaws and factual inaccuracies that I would like to address.

First, they use the terms *prediction* of violence and *assessment* of violence risk interchangeably. It is clear from the research literature that we cannot, and will never be able to, predict with reasonable medical certainty future violence. This is mostly due to the statistical phenomena of extremely low base rates for violent acts, regardless of the cause. Assessing violence risk factors, on the other hand, is a more reasonable and attainable task, as they note. I think the research is advanced enough in this area to forego the use of the term *prediction*, as some of us have failed to do in the past [2].

Second, they use the terms *dangerous* and *violent* interchangeably. Although the authors are careful to distinguish between prognosis in medicine and prediction of interpersonal dangerousness, they fail to apply their own semantic care to the important difference between the words dangerous and violent. The former is an adjective with multiple legal meanings which may not imply a violent act and is agonizingly difficult to operationalize. The latter is a word that is much more easily operationalized and measured as an observable behavior.

And third, *violence* and *criminality* are equated. The authors move back and forth between these terms, but unfortunately, they do not necessarily mean the same thing. Even though certain violent behaviors may be criminal, and vice versa, this is often not the case. This confusion is particularly troubling and misleading when risk factors (p. 1440) are cited. Some of their factors apply to violence risk, others only to criminality. For instance, Klinefelter's syndrome, which they note as a biological risk factor for violence, is correlated with criminality, but not with violence risk per se [3].

Although most of the data presented in the article is generally accurate, there are some other notable exceptions. Our study concerning violent behavior in schizophrenics [4], which they cite, measured *neuropsychological*, not neurophysiological, dysfunction. Although they vacillate on race as a factor in violence risk, citing it as a violence variable on p. 1440 and then cautioning against its use in the absence of any other variables on p. 1441, a clearer statement can be made. Race does not contribute to the predictive equation when a violent individual is being clinically assessed for future violence. Swanson et al. [5] also found that race was not a contributor to violence risk when socio-economic status was controlled in their large, epidemiological study of mental disorder and violence.

The data on temporal lobe epilepsy and violence risk is likewise equivocal, and does not deserve notation as a biological risk factor, as they have done. Delgado-Escueta et al. [6] found that the prevalence rate for aggressive behavior during seizures was only 0.13% (7 out of 5400 patients with epilepsy). In the seven patients who were violent, all aggressive acts appeared suddenly, without evidence of planning, lasted an average of 29 seconds, and were stereotyped, simple, unsustained, and never supported by a consecutive series of purposeful movements.

Two important supraordinate variables that are not discussed in the article, but can be easily inferred from their psychological descriptors (p. 1440), are paranoia and psychopathy. Paranoia is most relevant to a violence risk assessment, and may have causes ranging from the ingestion of psychostimulants to the inheritance of a biochemical disorder

such as paranoid schizophrenia. Psychopathy, or psychopathic personality, has been found to be a reliable and valid construct [7], and is significantly associated with an increased risk of violence [8]: a mode of violence which I have called predatory [9], which seems to have distinctive biochemical and neuroanatomical substrates.

The clinical assessment of violence risk, whether we like it or not, is here to stay. Conceptual clarity and knowledge of the limits of our research will allow us to offer data-based opinions to the trier of fact. It can then make the social and legal deprivation of liberty decisions concerning violence risk for which we are asked, as forensic experts, to give an opinion.

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## Authors' Response

Dear Sir:

First of all, we wish to apologize to Dr. J. Reid Meloy and coauthors for the secretarial errors, unfortunately not noted by us, in transcribing Dr. Meloy's name and in inadvertently substituting neuropsychological deficits with neurophysiological dysfunction (page 1438 of our article) when quoting from their article "Neuropsychological Deficits and Violent Behavior in Incarcerated Schizophrenics" [1]. We accept our mistake as a lapsus scribae. That said, we would like to answer Dr. Meloy's personal comments in the order in which he made them.

Dr. Meloy's first comment centers on our use of the word assessment and the word prediction. The 1952 edition of *Webster's New Twentieth Century Dictionary of the English Language*, being as it is devoid of modern "speakthink," was our reference for determining the meaning of these words. *Assessment*, among other definitions, is defined as

“to set, fix, or ascertain; . . .” Prediction, on the other hand, is defined as “a foretelling; a previous declaration of a future event; . . .” Thus, it is clear that the term *assessment* is used as the method by which the result or conclusion is a *prediction*. It seems to us that unless there is some point to the process of evaluation and assessment of patients, such as formulating a prognosis, such efforts are meaningless. This is the whole point of our article.

Ultimately, we believe, we must fall back on an ancient legal axiom: *verba accipienda sunt secundum subjectam materiam*; words are to be understood in reference to their subject matter. Read this way, we do not think the criticism of Dr. Meloy can stand.

Secondly, Dr. Meloy says that we interchange the terms *violent* and *dangerous*. Frankly, we do not understand his point. Citing our old dictionary, the term *dangerous* means “beset with danger” while *danger* means “liability to injury or loss.” *Violent* is defined as “To act, or work with violence” with *violence* defined as “to assault, to injure . . .” Thus the difference is roughly equivalent to the distinction in physics between potential and kinetic energy. The law does not require such fine hair-splitting. For example, a power plant can be prevented from being built without a single clod of earth being turned through an injunction if the *threat* of permanent harm can be shown in advance. This is, again, one of the points to our article and it can be ascertained and understood by a common sense reading of it.

In order to further clarify the semantic issue raised by Dr. Meloy, we would like to cite Siegel [2], who, quoting Shah, wrote, “. . . dangerousness also refers to a propensity to engage in acts that are characterized by the application or overt threat of force and which are likely to result in injury to other persons (Shah, 1978). Thus, dangerous behavior is considered synonymous with violent behavior . . .”

Finally, Dr. Meloy says not all violent acts are criminal. This is true, but the exceptions to the general rule that all violent acts *are* criminal are so well known, that it is hard to believe that these are not understood by Dr. Meloy. We will enumerate the exceptions:

1. **Acts Sanctioned by the Constitution.** Examples of this include declarations of war and issuance of letters of marque and reprisal [3].

2. **Acts Sanctioned by Judicial Decree.** This includes sentences of death in States permitting same.

3. **Acts Rising to Status of Privilege.**

- a. Acts by Law Enforcement Authorities—law enforcement authorities may use all necessary force to execute their duties.
- b. Self-defense—it is well settled in every civilized country on this planet, that individuals may use force to repel similar force unlawfully used against them.
- c. Consent—examples of this are boxing matches, karate tournaments, wrestling matches, other sporting events [4].

One of the most important legal axiom, that any serious man of letters should adopt as a fundamental principal is: *Verba intentioni, non e contra, debent inservire* that is, “Words ought to be subservient to the intent, not the intent to the words.”

On the question of race as a variable factor in the risk of violence, we certainly did not vacillate about it, as stated by Dr. Meloy. Indeed, we clearly stated, (page 1441) “We firmly agree with Thompson that race, *in the absence of any other qualifying or confounding variables*, [emphasis added] may not be a predictive factor for future violence.” Indeed, it seems reasonable to assume that individuals of any race, when exposed to, for instance, particularly poor socio-economic conditions and intense drug or alcohol addiction may, at times, become more prone to violence. Our present social situation testifies to that. In reviewing Dr. Meloy’s article, “The Prediction of Violence in Out-patient Psychotherapy.” [5] we found (page 39–44) a statement that appears to contradict the criticism he made about race in his comments. Dr. Meloy includes race among the

correlates of violence and specifically states the following: "Black males are six times more likely to be the victims or perpetrators of homicide than white males. Blacks accounted for 12% of the population in 1977 and 46% of the arrests for violent crime. This higher proportionality seems to hold true when cross-validated with other minority groups in similar geographic locations." We are certainly at a loss to relate the above statement, originally made by other authors but accepted and reported by Dr. Meloy, to the comments he made referring to our paper. We find no confusion, especially "troubling and misleading," as Dr. Meloy stated, in our list of factors that should be taken into consideration when assessing the probability of an individual's future violent behavior.

Regarding the XYY and XXY populations and their relationship to either criminality or violence, which was questioned by Dr. Meloy on the basis of the results of the Copenhagen study, we have to agree with him that the study does not support a direct relationship between XXY or XYY abnormalities and violence. However, since the first paper by Jacobs, there have been, as often happens, contrasting views regarding the connection between chromosomal aberrations and possible violence/criminality. In reviewing the aggression hypothesis, Witkin, et al., stated, "In this view an extra Y chromosome increases aggressive tendencies and these in turn lead to increased criminal behavior" [6]. It seems that even for them, aggressive tendencies are a prerequisite for criminal/dangerous behavior. However, their interesting and well-documented study revealed that, indeed, the frequency with which the XYY (5 of 12 or 41.7%) and XXY (3 of 16 or 18%) were involved in criminal behavior was higher than the XY controls (9.3%) and statistically significant. They also found that most of the offenses were against property and only case number two of the XYY individuals committed an act described by the authors as "aggression against people" (page 550), and only one of the XXY individuals "assaulted his wife in an extremely brutal way . . ." (page 551). Witkin, et al., end their paper with the following statement, "The data from the documentary records we have examined speak on society's legitimate concern about aggression among XYY and XXY men . . . [even though] no evidence has been found that men with either of those sex chromosome complements are especially aggressive" [6]. In further perusing the literature, we found that the XYY have been described as . . . "highly irresponsible and immature individuals whose waywardness causes concern at a very early age. It is generally evident that the family background is not responsible for their behavior. They soon come into conflict with the law, their criminal activity being aimed mainly against property, although they are capable of violence against persons if frustrated and antagonized . . ." [7]. Lastly, the possibility that a developmental lag, consequent to the chromosomal aberration leads to offensive behavior is supported by clinical and EEG findings. This sequence is as follows: chromosomal aberration leads to a developmental lag which in turn leads to offensive behavior. This can be further simplified as: chromosomal aberration leads to offensive behavior. "As with other immature offenders [XYY, XXY, XXX], they need the safety, time, and education to achieve maturation" [8]. We are well aware of the controversial nature of the subject, and cognizant of not holding the final answer we feel that, regardless of statistical studies pro and con, until that answer is found, the screening for XYY and XXY chromosomes in selected violent individuals should be done in order to be more thorough and objective; and that is, essentially, what we suggested in our paper. Obviously, we do not consider all those persons carrying the XYY and XXY chromosomes to be violent/dangerous individuals. Violent crime is usually due to a combination of variables.

Dr. Meloy's statement, "The data on temporal lobe epilepsy (TLE) and violence risk is likewise equivocal and does not deserve notation as a biological risk factor . . ." really opens a Pandora's Box. After a brief discussion of this controversial topic, we will close with a statement by Dr. Meloy that contradicts his own criticism.

Since the time of Kraepelin, deepened emotionality and viscosity of speech have been considered manifestations of TLE. Already at that time the patient's mood was described as ranging from a good natured one to an angry one, coupled at times with explosive behavior, even to the point of threatening physical violence. The writings of Dostoevsky, who himself suffered from temporal lobe epilepsy, testify to the rapid cycling of the TLE mood. Persons suffering from TLE often, because of their mood swings, alienate themselves from friends and relatives [9].

The study of Delgado-Esqueta, et al., points out that, as a rule, a complex partial seizure (CPS) shows behavior that is short and self-limited, fumbling and purposeless, and with only occasional minimal aggressive behavior during seizures [10]. However, today's clinical investigation has shifted from the ictal seizure to the interictal seizure discharge. "Interictal explosive behavior and irritability are common phenomena in seizure patients with temporal lobe involvement . . . it can produce hardship within the family," and is due to "excessive neuronal activity and its aftermath is associated with the enormous and confusing variety of mental and behavioral features. . . ." [9].

At times, the above patients show moodiness and explosiveness in the form of "intense verbalized anger and threatened physical violence . . . suicide . . . and paranoid ideation" [9]. Further, Ervin and Mark [11], in describing the Episodic Dyscontrol Syndrome and its four characteristic symptoms, lists the first as "a history of physical assault, especially wife and child beating." Monroe [11] characterized the Dyscontrol Syndrome as a highly impulsive and unreflective act, sudden and paroxysmal. Bach-Y-Rita, et al. [11], found EEG abnormalities in 50% of their patients, mostly consisting in spiking in the temporal region and thirteen of 123 patients had undiagnosed temporal lobe epilepsy. Elliot [11] noted significant EEG abnormalities in more than 60 percent of his cases, and Monroe [11] reported abnormalities, both focal and generalized pattern, in 58 percent of 93 aggressive criminals who had two tracings. Bear and Devinsky [12] stated the following: "We and others have observed aggressive behavior in many patients with temporal lobe epilepsy. . . . In selected cases neurosurgical intervention has been shown to be effective in reducing aggression, . . . the severity of any interictal behavior changes including aggressiveness, appear to be independent of, or even inversely correlated to, seizure frequency." As reported by Siegel [13], Minimal Brain Dysfunction (M.B.D.), also is considered an important cause of "wife beating, child abuse, suicide, aggressiveness, and motiveless homicide . . . one perplexing feature of this syndrome is that people who are affected with it often maintain warm and pleasant personalities between episodes of violence," and, "Abnormal EEG readings have been found to be linked to violent behavior." And as a criterion *ex adjuvantibus*, antiepileptic medication is actually used at present in the treatment of aggressive explosive behavior.

After reviewing the study of Delgado-Esqueta, et al. [10], we totally agree with Rickler's statement [11], "Violence occurs in real life settings and the introduction of a laboratory environment seriously alters the naturalistic perspective, particularly as the behaviors are intermittent." And, we would like to add, the EEG findings are also intermittent.

In closing the Pandora's Box of the debated and debatable TLE, a statement by Dr. Meloy in his article, "The Prediction of Violence in Outpatient Psychotherapy" [5] (page 43) confirms our idea that it is both correct and wise to rule out, in the assessment of violent individuals, the possibility of TLE or any other type of seizure discharges. He states, "Although it is premature and too reductionistic to posit neurological dysfunction as a major correlate of violence, in general it is propitious to consider it an important individual difference in the assessment of the intrapsychic experience of a specific patient." Any intrapsychic experience, especially anger, hostility, and paranoid thoughts, may be, at times, directed outwardly against people, usually suddenly.

We subscribe to Stone's statement [14] that patients with limbic epilepsy [or interictal dyscontrol/explosive syndrome] are still persons adapting to a bewildering disorder. The

human being is a biopsychosocialecological unit, and even though it is not our professional opinion that all psychomotor epileptics should be considered potentially violent/dangerous people, it is important, and at times imperative, that forensic clinicians also assume the responsibility to identify organic factors [15]. While there is no doubt that social factors are important variables in the causation of crime, especially violent crime, we also consider constitutional factors, such as impulsive aggressive personality traits or limited intelligence, as important. Could it not be that at the basis of impulsivity and aggressivity lie the vast gamut of brain dysrhythmias, ranging from M.B.D. to the Dyscontrol Syndrome and Interictal Seizures?

Lastly, we would like to respond to the following statement by Dr. Meloy: "Two important supraordinate variables that are not discussed in the article but can be easily inferred from their psychological descriptors are paranoia and psychopathy." We preferred to describe the psychological components of paranoia, psychopathy, schizophrenia, and depressive reaction, as he well perceived, instead of listing the usual disease entities. We thought that our readers would benefit more from a list of the qualifying variables and, based on that, better recognize the specific disease entities as well. Clarity should be based on meaningful, practical, and common interpretation and description of words, concepts, and behavior, and not primarily on a theoretical, semantic disquisition that at times eventuates in more confusion of opinions rendered and actions taken.

We realize that we have indulged in a rather lengthy response, but because Dr. Meloy raised some questions about the objectivity of some of the statements in our paper, we thought that it was necessary for us to react in as complete a manner as possible so that our readers will not be deprived of that objectivity and thoroughness that should be part of any good scientific communication. Even though we do not agree with Dr. Meloy's comments, we are happy that our paper has stimulated his critical appraisal and has been rated by him as "a generally good article." Further, we would like to thank him for his comments, which gave us the opportunity to further expound on important factors which are probably at the basis of violent behavior.

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### Using Pacemakers to Identify Decedents

Dear Sir:

The Medical Examiner-Coroner in addition to determining the cause and manner of death is also responsible for the identification of a decedent. We recently had a case at the Los Angeles County Department of Coroner of a male caucasian who appeared to be in his 70s, who was found down on the street in Los Angeles. He was taken to a local hospital where he was pronounced.

Cause of death at autopsy was found to be atherosclerotic cardiovascular disease. He had severe left anterior descending coronary artery disease. Fingerprints could not be compared because there was no record. He had no teeth, there were only dentures present so identification could not be done by this method. However, during the autopsy we found that he had a pacemaker.

As is our procedure at the Coroner's Office the pacemaker was removed from the decedent at autopsy and submitted as evidence. Model and serial number were duly recorded on Coroner's forms. The investigator responsible for identification, contacted me regarding identification by means of the pacemaker. We found the pacemaker was made by Medtronic Incorporated. The investigator, by a few phone calls, found a local number in Los Angeles and through their computer records found that the pacemaker was placed in one Mr. J. D. in 1988. It was placed when he was 81 years old. They also indicated one Dr. M. H. installed the unit. Further investigation by us revealed that Dr. M. H. was no longer in practice and Dr. C. S. was now the owner of the practice. We got the record from Dr. C. S.'s office. The pacemaker serial number and model number matched with the pacemaker taken from our decedent. Hence the decedent was positively identified as one Mr. J. D.

We are writing this letter to emphasize that pacemakers present in decedents should be removed at autopsy for the following reasons:

1. To have them tested to confirm that they are functioning adequately.
2. May be helpful in identification of the decedent as was proved in our case.

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**Discussion of "Effects of the Taser in Fatalities Involving Police Confrontation"**

Dear Sir:

In their review of 16 taser-related deaths in the March 1991 issue of the *Journal of Forensic Sciences* [1] Drs. Kornblum and Reddy hasten to conclude "that the Taser in and of itself does not cause death." At the same time the authors entirely ignore a more logical conclusion, namely that certain medical conditions, including drug use and heart disease, may increase the risk that the taser will be lethal.

The authors do not report their methods of study. As a former deputy medical examiner from Los Angeles, this raises my suspicions about the quality of the information. How did they find their cases? Were any cases excluded in the time period reported? How did they know that all 16 had a history of chronic drug abuse? Did the medical examiner attend the death scene? Were police and paramedical reports reviewed by the authors? Did the autopsies include careful neck dissection and examination of the spinal cord? Was there a complete microscopic examination? Were there any findings in microscopic sections of heart and blood vessels? Was the cardiac conduction system examined? If Case 2 had idiopathic cardiomyopathy and acute myocarditis then why not report the heart weight and nature of the pathology more specifically?

Since Drs. Kornblum and Reddy are experienced in the investigation of electrical deaths, it is curious that they did not mention some of the more important points of information in this regard. The location of the taser barbs or wounds on the body was not reported. This could aid in determining whether an electrical current may have passed through the heart region. The number and duration of taser shocks per taser barb also were not reported. This is important because the risk of ventricular fibrillation increases not only with the amount of current but also with the duration of its application.

Drs. Kornblum and Reddy tell us that "Because of the level of drugs found, the cause of death could be attributed to an overdose in 13 of 16 cases." Apparently then, at least six cases officially determined by the medical examiner to be homicides were in fact accidental drug overdoses. Doesn't this reflect more than a "considerable variation in wording" that the authors suggest?

In fact, the authors do not even report the drug concentrations observed in their case series. In my experience drug concentrations by themselves rarely determine that death was due to overdose. It would be pertinent to compare the drug concentrations in these supposed overdose deaths associated with taser use to other known overdose cases. Are the mean drug concentrations similar in these two groups? This type of statistical comparison is readily available at the large facility in Los Angeles and might have supported the authors contention that the taser-associated deaths were actually drug overdoses.

A very important point in the diagnosis of electrical deaths is the close temporal relation between the exposure to electricity and the onset of ventricular fibrillation. In this regard, the time interval between tasing and pronouncement of death reported by Drs. Kornblum and Reddy has very little significance. It is the time interval between tasing and collapse that is pertinent. This interval is not given. Information about what happened to the victim immediately after tasing is also important to the determination of the cause of death. Was there a recovery period? Were paramedics on scene? Was ventricular fibrillation or asystole observed?

I was the deputy medical examiner assigned to investigate Case 6 in the Los Angeles series. This individual was acutely psychotic from use of cocaine and caused several inches of standing water to flood his motel room. Seven tasers were fired delivering multiple shocks but seeming to have no observable effect until finally the victim collapsed in the



water immediately after the last shock. Paramedics at scene rushed in but could not resuscitate the individual. Therefore, to list the 45 minutes of resuscitation time, from the last taser injury until pronouncement of death, as being the time interval between the shock and death gives a misleading impression that the taser was not responsible. In fact, death was an immediate and direct result of the taser.

Also misleading is the authors claim that in Case 6 "No other cause of death was found at autopsy; therefore, this death fits into the cocaine category." The diagnosis of electrocution is not excluded when only cutaneous electrical burns are observed at autopsy. I have seen cases of electrocution in which there were no findings at autopsy but additional scene investigation showed that death was caused by electrical energy.

It is reported that Case 1 died from "cardiac dysrhythmia/acute PCP intoxication" within 15 min of tasing. Are we to believe that this individual coincidentally had a drug-associated arrhythmia during arrest that was not related to the tasing? Is the taser completely excluded as a possible contributing factor in the death?

Why is an emergency medical complication (esophageal airway obstruction) listed in Case 16 as the second most important cause of death? Was the blunt force trauma to the neck caused by a choke hold? And since death was only 30 min after tasing, shouldn't the taser be considered at least a possible contributing factor?

I was apparently one of only two medical examiners in the Los Angeles office to list the taser on a death certificate. This was because pathologists in Los Angeles were under pressure from law-enforcement agencies to exclude the taser as a cause of death. In Case 6, the autopsy was performed in the presence of six upper-level law enforcement agents who were confrontational and argumentative in their attempts to persuade me that death was caused by drowning in a few inches of water. I was not allowed to attend the death scene. I insisted that the cause of death would not be determined until all tests were complete. My opinion was widely and prematurely misquoted by the officers. Likewise, I was called into Dr. Kornblum's office to defend my investigation in something more akin to a disciplinary hearing than a scientific conference. In the end, Dr. Kornblum seemed to agree that the tasing was the immediate cause of death. Yet, in his article it is stated that the "death clearly fits into the cocaine category."

Obviously, if a person is shot with a taser and then immediately killed with bullets, we are not in a position to draw a conclusion about whether the tasing was fatal. A similar consideration applies when forceful restraint or choke holds, which can also result in fatalities are used. My point is that, with more than one type of injury, we are not free to exclude the taser as potentially contributing to death. At the very least the 16 taser-related deaths in Los Angeles indicate a failure of the taser as a non-lethal weapon since its use did not prevent fatal results.

If we eliminated from Table I those deaths in which gunshot wounds, blunt force trauma, or physical restraint were deemed important factors, then we have nine individuals who were alive and active, collapsed on tasing, and did not survive. In my opinion, the taser contributed to at least these nine deaths. There is no evidence that any of these individuals did in fact recover from the tasing and later died of drug effects. This is in spite of the author's assurances that "Under ordinary circumstances this process (tasing) is temporary and completely reversible."

As pathologists, we should warn law-enforcement agencies that tasers can cause death. It seems only logical that a device capable of depolarizing skeletal muscle can also depolarize heart muscle and cause fibrillation under certain circumstances.

Furthermore, while the use of tasers may be generally safe in healthy adults, pre-existing heart disease, psychosis, and the use of drugs including cocaine, PCP, amphetamine and alcohol may substantially increase the risk of fatality. Since tasers are most

likely to be used on psychotic or intoxicated individuals, in whom the medical history is unknown, the priorities for use of the taser among law enforcement's "nonlethal" armamentarium must be carefully considered.

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**Reference**

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