

The role of psychopathy in assessing risk for violence: Conceptual and methodological issues

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Purpose. There is growing evidence that psychopathic (dissocial) personality disorder is associated with violence. The purpose of this paper is to consider the role of psychopathy in clinical assessments of risk for violence.

Arguments. Risk assessments are conducted for the purpose of preventing, not predicting, violence. Yet, most research on risk for violence is conducted and interpreted within the framework of a simplistic prediction paradigm, thus underestimating the practical importance of risk factors. Despite this bias, violence predictions based on psychopathy are only slightly less accurate than predictions that cognitive behavioral therapy will reduce symptoms of depression or cardiac bypass surgery will reduce angina pain; and more accurate than predictions that smaller class sizes will lead to improved academic achievement or cardiac bypass surgery will reduce mortality.

Conclusion. Information about psychopathy can be used to make relatively accurate predictions of violence. Of course, decisions concerning if and how such information should be used are another matter. The paper concludes with recommendations concerning the appropriate role of psychopathy in violence risk assessments and avenues for future research.

There is a robust association between psychopathy and violence, a conclusion reached in several recent qualitative and quantitative reviews of the empirical literature (e.g. Hart, in press; Hart & Dempster, 1997; Hart & Hare, 1996, 1997; Patrick & Zempolich, in press; Salekin, Rogers & Sewell, 1996), including the excellent paper by Hemphill, Hare & Wong (1998) in this Special Section. Readers interested in details should consult the reviews; however, I think it is fair to summarize their findings as follows:

- (1) *Psychopaths are more likely than non-psychopaths to have a history of community and institutional violence.* Retrospective research indicates that, when at risk to do so, psychopaths committed more violence, and more types of violence (including sexual violence), than did non-psychopaths. Unlike most offenders, the violence of psychopaths often was 'impulsively instrumental', motivated by factors such as material gain, opportunism, and sadism.

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- (2) *Psychopaths are more likely than non-psychopaths to engage in future community and institutional violence.* Prospective research indicates that psychopaths are more likely than non-psychopaths to commit violence *after* assessment. This increased likelihood of violence is observed in a variety of populations, including adult correctional offenders, delinquents, sex offenders, forensic psychiatric patients, and civil psychiatric patients, and is evident even after controlling for potential confounding factors such as demographics or criminal history.

The association between psychopathy and violence has generated much theoretical interest. Several authors have speculated about the mechanism(s) responsible for it (e.g. Blair, Jones, Clark & Smith, 1995, 1997; Hart, in press; Hart & Dempster, 1997; Hart & Hare, 1996; Newman & Wallace, 1993; Patrick & Zempolich, in press). There seems to be growing consensus that the violence of psychopaths may stem at least in part from a deficit in affective functions, although there is disagreement concerning the generality and aetiology of the deficit.

The association between psychopathy and violence may have important practical implications with respect to the assessment, treatment and management of patients and offenders. In this contribution, I will focus on the role of psychopathy in the clinical assessment of risk for violence. Although I and others have touched on this issue elsewhere (e.g. Hart, in press; Quinsey, 1995; Serin & Amos, 1995), it is complex and worth revisiting. My own views have evolved rapidly in recent years as a result of collaborations with a number of colleagues (e.g. Boer, Hart, Kropp & Webster, 1998; Hart & Hare, 1996; Kropp, Hart, Webster & Eaves, 1995; Lyon, Dempster, Sullivan & Hart, 1997; Webster, Douglas, Eaves & Hart, 1997), and as a result of forays into the clinical realm (conducting violence risk assessments and providing expert testimony in various criminal and civil courts, training correctional and mental health professionals). I begin by providing a general definition of risk assessment, which will serve as a conceptual foundation for the rest of the commentary. Next, I review some of the major methodological factors that influence and complicate research on risk factors for violence and evaluate psychopathy as a risk factor for violence. I end by making recommendations concerning the role of psychopathy in the process of violence risk assessments.

Conceptual issues: The nature of risk assessment

Definition

Risk assessment is a central or organizing concept in many fields, including law, business, medicine, and engineering (Menzies, Webster & Hart, 1995). Most generally, risk assessment is the process of identifying and studying hazards to reduce the probability of their occurrence. Social scientists and mental health professionals interested in violence tend to define risk assessment as *the process of evaluating individuals to (1) characterize the likelihood they will commit acts of violence and (2) develop interventions to manage or reduce that likelihood* (e.g. Monahan, 1981/1995; Monahan & Steadman, 1994).

The process of risk assessment sometimes is referred to as 'assessing dangerousness' or 'predicting violence'. I try to avoid these other terms, however. With respect to the former, 'dangerousness' is a rather vague term—danger for what, and to whom?—that connotes a dispositional construct (Monahan & Steadman, 1994). At the present time, there is no evidence of a single, global trait that predisposes individuals to all kinds of violence across all kinds of contexts. With respect to 'predicting violence', this term implies that professionals are passive evaluators, interested in observing rather than influencing outcomes (Litwack & Schlesinger, in press). Nothing could be further from the truth. Clinicians are bound—morally, ethically and legally—to prove themselves wrong when they 'predict' violence; they must take every reasonable action to ensure that those at high risk for violence do not act violently. If I could predict future violence with perfect accuracy on the basis of an assessment at discharge from hospital, then it would mean that any community-based interventions based on my assessment were totally ineffective at preventing violence. I would consider myself to be perfectly useless, as my knowledge of patients' risk at discharge could not prevent a single violent act. Similarly, I would consider myself to be an excellent clinician even if my predictive accuracy was zero, as long as this was because I intervened appropriately with everyone I perceived to be at risk and thus prevented violence in every case. To put it simply, the clinical task is violence *prevention*, not violence *prediction*.

Approaches to risk assessment

Two major decision-making approaches have dominated the field of risk assessment (e.g. Menzies *et al.*, 1995; Monahan, 1981/1995). Historically, the most common used has been unstructured clinical (i.e. professional) judgment. The hallmark of this approach is that there are no constraints concerning how evaluators make a decision based on the information available to them (Meehl, 1954/1996), although it is also generally the case that evaluators have complete discretion concerning how assessment information is gathered and which information is considered. It comes as no surprise that unstructured clinical judgment is also described as 'informal, subjective, [and] impressionistic' (Grove & Meehl, 1996, p. 293).

The unstructured clinical judgment approach to risk assessment has been criticized on a number of grounds, which are very familiar to most readers and have been described in detail elsewhere (Monahan, 1981/1995; see also Litwack & Schlesinger, 1987, in press; Monahan & Steadman, 1994; Webster, Harris, Rice, Quinsey & Cormier, 1994). First, there tends to be lack of consistency or agreement across evaluators with respect to how evaluations are conducted and what decisions are reached (i.e. low inter-rater reliability). Second, there is little evidence that decisions made using this approach are accurate, or at least that they improve much over chance (i.e. low validity). Third, evaluators may fail to specify why or how they reached a decision, making it difficult for others to question that decision or determine the reason for disagreement among professionals. The clinical approach has advantages, as well, perhaps the most important being flexibility and a focus on violence prevention.

The second major approach is actuarial risk assessment. The hallmark of this approach is that, based on the information available to them, evaluators make an ultimate decision according to fixed and explicit rules (Meehl, 1954/1996). It is also generally the case that actuarial decisions are based on specific assessment data, selected because they have been demonstrated empirically to be associated with violence and coded in a predetermined manner. The actuarial approach also has been described as 'mechanical' and 'algorithmic' (Grove & Meehl, 1996, p. 293).

There is little doubt that the actuarial approach is superior to unstructured clinical judgment with respect to decision making in general, and more specifically with respect to assessing risk for violence (Monahan, 1981/1995). Most importantly, it improves the consistency and accuracy of risk assessment. But it is important to remember that the actuarial approach also has limitations. I will discuss these in some detail, not because they are more serious than the limitations of unstructured clinical judgment but because they may be less familiar to readers (despite excellent reviews by Gottfredson & Gottfredson, 1986 and Monahan, 1981/1995). First, it tends to focus the evaluation on a small number of risk factors that are thought to predict violence across individuals and settings, thus ignoring factors that may be important but idiosyncratic to the case at hand. Second, it tends to focus attention on (relatively) static or stable features of individuals, such as demographics and criminal history. As a result, actuarial assessments are often passive predictions of limited practical use. How is one to intervene if an individual is at risk because he is male? How can one assess changes in risk over time if the decision is based on past convictions? Third, due to their primary emphasis on empiricism, actuarial assessments may include risk factors that are unacceptable on legal grounds and exclude factors that are entirely logical but of unknown validity (Hart, 1996). For example, it is possible to construct an empirically valid actuarial instrument that is discriminatory (e.g. includes risk factors such as age, race, and sex), includes factors that are not causally related to violence (e.g. shoe size; men are more likely to be violent than women, and men have bigger feet than women), and excludes factors that any reasonable person would consider germane (e.g. homicidal ideation or intent). Decisions based on such an instrument would be difficult to justify before courts or tribunals. Fourth, actuarial instruments constructed using an empirical approach are 'high-fidelity' predictors: They are optimized to predict a specific outcome, over a specific period of time, in a specific population (Gottfredson & Gottfredson, 1986). Use of the instrument in other assessment contexts may lead to non-optimal and sometimes even bizarre decisions (Hart, 1996; Lyon *et al.*, 1997). Finally, it has been my experience that the use of actuarial risk assessment tends to disengage evaluators from the evaluation process, because actuarial instruments are so structured that they require minimal professional judgment. Consequently, unless they are sufficiently schooled in psychometric theory to have a healthy respect for (and scepticism of) test data, professionals may tend to grossly over- or underutilize actuarial data when making decisions about individuals.

To illustrate some of the concerns raised above, consider the Violence Risk Appraisal Guide (VRAG; Rice & Harris, 1995; Webster *et al.*, 1994). The VRAG is one of the best-constructed and best-validated actuarial instruments developed to

date. It was designed to predict general violence among mentally disordered offenders released into the community. Despite the scale's considerable utility, it may be quite inappropriate for use in other decision-making contexts. For example, although schizophrenia has a robust positive association with violence (e.g. Douglas & Hart, 1996), in the VRAG it is *negatively* associated with risk. Similarly, the VRAG is significantly worse at predicting sexual violence than it is at predicting general violence (Rice & Harris, 1997). This means that the VRAG may not be useful for evaluating violence risk in, for example, community residents, civil psychiatric patients, or sex offenders. Previously (Hart, 1996), I used the VRAG to assess violence risk in the case of a man who had committed serial rapes (≥ 75) and serial sexual homicides (≥ 3)—a sex offender who in all likelihood will never be released from prison, and thus is not representative of the VRAG construction sample. Somewhat surprisingly, the test results indicated that this person was only an average or moderate risk for violent recidivism over periods of seven to 10 years. This is due in part to the fact that, all else being equal, killing women on the VRAG makes one a lower risk for violence than does, for example, threatening men; and because the VRAG fails to consider homicidal ideation or intent (e.g. sexual sadism).

An alternative to the clinical and actuarial approaches described above is known as structured clinical judgment. Here, decision making is assisted by guidelines that have been developed to reflect the 'state of the discipline' with respect to empirical knowledge and professional practice (Borum, 1996). Such guidelines—sometimes referred to as clinical guidelines, consensus guidelines, or clinical practice parameters—are quite common in medicine, although used less frequently in psychiatric and psychological assessment (Kapp & Mossman, 1996). More germane to the present discussion, my colleagues and I have developed clinical guidelines for assessing violence risk in mentally disordered populations (Webster *et al.*, 1997), risk for spousal violence (Kropp *et al.*, 1995), and risk for sexual violence (Boer *et al.*, 1998). In each case, the guidelines attempt to define the risk being considered, discuss necessary qualifications for conducting an assessment, recommend what information should be considered as part of the evaluation and how it should be gathered, and identify a set of core risk factors that, according to the scientific and professional literature, should be considered as part of any reasonably comprehensive assessment. Despite their structure and grounding in empirical research, though, clinical guidelines do *not* specify how an ultimate decision is reached and therefore cannot be considered actuarial.

More about structured clinical judgment

It is my opinion that structured clinical judgment is a more appropriate method for assessing violence risk than is actuarial decision making. It promotes systematization and consistency, yet it is flexible enough to handle the diversity of human beings and the contexts in which assessments are conducted; it promotes transparency and accountability, yet encourages the appropriate use of professional discretion; and it is based on sound scientific knowledge, yet is practically relevant.

In psychometric terms, this 'goodness-of-fit' between structured clinical judgment and the clinical reality of risk assessment is known as *method-function match*.

My enthusiasm for structured clinical judgments would be rejected by many. For example, Grove & Meehl (1996) recently stated that the use of anything other than the actuarial approach to decision making 'is not only unscientific and irrational, it is unethical' (p. 320). Notwithstanding my regard for these authors and my considerable sympathy for almost all of their arguments, I must respectfully disagree with their conclusion. Reliance—at least, complete reliance—on actuarial decision making by professionals is unacceptable, for the following reasons. First, actuarial decisions are made on the basis of a fixed set of factors, determined without reference to the case at hand. This failure to exercise discretion by considering the 'totality of the circumstances' means that actuarial assessments may be considered arbitrary and thus a violation of the individual's legal rights. Second, actuarial decision making is inconsistent with the ethical and legal fact that professionals are responsible for the decisions they make. Put another way, professionals make decisions; tests do not. A professional may place considerable weight on the results of a given test, in effect letting the test make a decision, but he or she is still responsible for having selected that particular test and for judgments made about the weight given to the results of that particular test in that particular case. Similarly, a professional may decide, quite properly, that the test results are not meaningful in a given case and should be ignored. Recall the earlier example of the VRAG. Does it matter at all what an offender's total score is on the VRAG, how many risk factors are present or whether he scores above a specific cut-off, if he also expresses genuine homicidal intent?¹

Methodological issues: Identifying and evaluating risk factors for violence

Identifying risk factors

There exists a rather substantial empirical literature that examines the demographic, clinical and criminal history factors associated with risk for violence (e.g. Monahan & Steadman, 1994). Perhaps the single most commonly used research design in this literature is the cohort study (e.g. Ahlbom & Norell, 1984): A cohort of offenders or psychiatric patients is assessed and followed up over some period of time to determine which members were or were not violent; a statistical index of the predictive power of the risk factor is then calculated. As I have noted previously, if this design appears simple, then appearances are deceiving (Hart, 1996, in press; see also the excellent paper by Jackson, 1997). The interpretation of findings is made difficult by the nature of the risk factors, the outcome (i.e. violence), the follow-up, and even the statistical indices of accuracy.

¹This is analogous to what Meehl (1954/1996) refers to as the 'broken leg' problem in actuarial decision making (see also Grove & Meehl, 1996; Monahan, 1981/1995). Suppose that, according to a well-validated actuarial scale, a certain professor is assessed as being very likely to go to a movie this Friday night. Would you still use the actuarial scale if you knew the professor had a broken leg and was immobilized in a hip cast? However, Meehl warns that such 'broken leg' scenarios are rare and cautions against using similar arguments (what others call 'clinical overrides'; Monahan, 1981/1995) to supercede actuarial judgment on a routine or regular basis.

Risk factors. There is tremendous variability across studies with respect to how risk factors are defined and assessed. The risk factors being investigated may be empirical markers, consequences, or causes of the outcome. They may be defined with varying degrees of precision and using any number of standardized tests or diagnostic criteria. Measurements may be scaled in a metric that is categorical (e.g. present versus absent) or continuous (e.g. severity rated on a 10-point scale; age, expressed in years). Those measuring the risk factors may be clinicians, criminal justice professionals, or researchers with varying degrees of training and experience, working alone or as part of a team. In some studies, the inter-rater or test-retest reliability of measurements is evaluated, to aid the interpretation of predictive power estimates; in most, it is not. The measurements may be made at any point in time between patients' admission to and discharge from a facility, or may be made at regular intervals during the follow-up.

To illustrate, Kevin Douglas and I recently conducted a meta-analysis of studies that examined the link between psychosis and violence (Douglas & Hart, 1996). The meta-analysis included studies in which the risk factor was defined in terms of symptoms or diagnoses, which in turn were defined according to a variety of criteria (e.g. *DSM* versus *ICD*), evaluated according to different time frames (e.g. current versus lifetime), and evaluated at different times (e.g. admission versus discharge). Not surprisingly, the predictive power of the risk factor varied systematically as a function of how it was defined.

Violence. There is no simple way to define and measure violence (Jackson, 1997; Monahan, 1981/1995). Violence may be assessed via official records (e.g. police reports, hospital records), by self-report, or by reports from collateral sources. The definition may or may not include property damage, verbal threats, and self-directed aggression. It is quite common to differentiate among violent acts on the basis of acquaintanceship with the victims (e.g. family members versus friends versus strangers) or the severity of harm suffered by the victims (e.g. psychological harm versus physical injury versus death). Some researchers make a distinction between sexual and non-sexual violence; others do not. The context of the violence may be examined to identify the motivation of the perpetrator or situational precipitants of the violence, such as intoxication, psychosis, or victim provocation. Finally, it is worth noting that violence can be coded in a metric that is categorical (e.g. yes versus no, severe versus minor), continuous (e.g. total number of incidents), or time dependent (e.g. time to first violence, incidents per unit of time at risk).

Follow-up. The most obvious way in which the follow-up differs across studies is length. Some studies look at predictions over the short term, with time periods as brief as one or two weeks; others have follow-ups of moderate length, ranging from several months to a year; and still others have long-term follow-ups, ranging from three or four years to 20 years or longer. Given this variability, it is not surprising that studies also differ in the degree to which they record and control for events that occur during the follow-up, including changes in mental health status, institutionalization, level of supervision by criminal justice agencies, treatments received from mental health agencies, changes in socio-demographic status

(divorce, unemployment)—even injury and death. Although some factors are static in nature, and are expected to change little or even not at all over long periods of time, many are inherently dynamic and are in a near-constant state of flux; yet, few studies routinely monitor these latter variables during the follow-up period.

Statistical indices. The statistical indices chosen to evaluate the predictive power of a risk factor depends largely on the study's design and the measurement of risk factor and outcome. However, it also depends on which aspect of predictive efficiency is of most interest to the researchers. For example, researchers may be interested in the absolute or the relative degree of risk associated with a factor; they may be interested in false positive, false negative, or overall errors; or they may be interested in evaluating predictive efficiency after controlling for the effects of other variables. They may focus on the probability that the event will occur over a fixed time period, or they may be interested in the extent to which the hazard rate changes over time. The magnitude or effect size of the statistic may be interpreted relative to chance (p level; that is, the probability of obtaining an effect size of magnitude x in a sample of size y if, in fact, there existed no association between prediction and outcome) or relative to some other criterion, such as perfection or some other, established risk factor. Finally, indices of predictive efficiency may or may not be corrected (more properly, disattenuated) for unreliability in the measurement of the predictor or outcome.

Research example. How have scientists responded to the bewildering complexity of research on violence predictions? The answer is illustrated in Fig. 1, the basic structure of which is familiar to anyone who has read a paper on violence prediction (Hart, Menzies & Webster, 1993). As the figure indicates, most studies cleave the risk factor and outcome into simplistic dichotomies. Notice that the follow-up, a crucial aspect of the basic research design, is not apparent in the figure at all. The simplistic manner in which the data are summarized allows one to define easily 'prediction successes', which are identified with a happy face, and 'prediction failures', identified with a sad face. It is now a relatively trivial matter to assign arbitrary numeric values to the predictor (say, 0 = risk factor absent, 1 = risk factor present) and the outcome (0 = not violent, 1 = violent) and calculate one's preferred statistical index of association.

		Outcome:	
		Not Violent	Violent
Prediction:	Low Risk	☺	☹
	High Risk	☹	☺

Figure 1. Science responds to complexity.

There are two important points I want to make about Fig. 1. The first is that this analytic strategy makes good sense only if one views risk assessment as passive prediction. If one prefers to view risk assessment as violence prevention, then it is not at all clear that there should be a sad face in the lower left quadrant of Fig. 1. According to the passive prediction paradigm, this cell is a false positive prediction failure; but according to the violence prevention paradigm, it may well be a preventive success! Similarly, the lower right quadrant of the figure is a true positive prediction success, but quite possibly a violence prevention failure. These ambiguities in the definition of success versus failure complicate the interpretation of statistical indices of predictive efficiency. (The only case in which Fig. 1 makes perfect sense, and in which the interpretation of statistical indices is unambiguous, is when decisions are made completely independent of the risk factor. This means that one would assess, release, manage, and follow up a cohort of patients without regard to the risk factor, be it psychosis, psychopathy, employment problems, etc.)

The second point to be made is that a tremendous amount of potentially valuable information is lost—not aggregated or simplified, but gone forever—when the data are summarized as in Fig. 1. The crucial point is this: *Lost information results in the underestimation of predictive accuracy.* I cannot imagine any circumstance in which artificially dichotomizing risk factor and outcome, ignoring time or nuisance variables or measurement error, and so forth will systematically increase the apparent predictive power of a risk factor.

Despite these problems, it is quite common to discover risk factors that predict violence reliably better than chance when data are summarized as in Fig. 1. Take, for example, a study conducted by Harris, Rice & Cormier (1991), which examined the association between psychopathy and violence in a cohort of forensic psychiatric patients who completed an institutional treatment programme. Psychopathy was assessed using the Hare Psychopathy Checklist-Revised (PCL-R; Hare, 1991); violence was coded from police and hospital records; and the follow-up period averaged about 10 years, with no regular monitoring. PCL-R scores and follow-up data were available for 166 patients. Patients with scores of 25 and greater were considered psychopathic (risk factor present); those with scores of 24 and lower were considered non-psychopathic (risk factor absent). Everyone who had an arrest for offences against persons, or whose hospital files indicated that they committed an act that could have resulted in such an arrest, was deemed violent. No attempt was made to control for the unreliability of measures or for post-release interventions.

The association between the PCL-R and violence during follow-up in the Harris *et al.* (1991) study is presented in Fig. 2. That there is an association between psychopathy and violence in the figure is apparent even to the naked eye. Here are some statistical indices of the magnitude of the association: overall accuracy, 78 per cent; accuracy of positive predictions, 77 per cent; accuracy of negative predictions, 79 per cent; relative improvement over chance, .62; chance corrected agreement (Cohen's κ), .53, $p < .001$; correlation (ϕ), .53, $p < .001$; and odds ratio, 12.5, $p < .001$. Optimists will note these findings indicate that, relative to chance, predictions of violence based on a single risk factor, the PCL-R, were reliably accurate—that is, reliably better than nothing. To pessimists, this is hardly a strong

		Outcome:	
		Not Violent	Violent
Prediction:	Low Risk	90	24
	High Risk	12	40

Figure 2. Association between psychopathy and violence in Harris *et al.* (1991).

endorsement. They are likely to point out that, relative to perfection, the predictions leave much to be desired. Technically, of course, both positions are correct. But I would like to make a few observations.

First, for the reasons discussed earlier, the statistical indices in the paragraph above are based on a gross simplification of the original data. More sophisticated and appropriate analyses are likely to reveal a stronger association between psychopathy and violence. For example, receiver operating characteristic analysis appropriate for examining the association between a continuous risk factor and a dichotomous outcome over a fixed time period (e.g. Mossman, 1994; Rice & Harris, 1995). If there is a dichotomous outcome and several predictors, or a single predictor and several nuisance variables, then logistic regression may be indicated (e.g. Hart, Kropp & Hare, 1988). If violent incidents are analysed as a count or rate variable, then non-linear regression analysis is preferable (Gardner, Mulvey & Shaw, 1995). If time to violence is an important consideration, survival analysis (e.g. Hart *et al.*, 1988; Rice & Harris, 1995) can identify variables that predict the rapidity of violence, defined dichotomously, during follow-up. Survival analysis is well suited for situations where the length of the follow-up is variable across participants or when there is a need to control for nuisance variables; it can also be used to analyse or control for variables that change status during the follow-up (i.e. time-dependent covariates).

Second, the original data themselves do not capture fully the complexity of the risk factor, the violence, or the follow-up. For example, if self-reports or collateral reports of violence were used in addition to official records, it is likely that the rate of violence during follow-up would have increased substantially—indeed, the rate of violence among psychopaths may have approached 100 per cent. Also, the association between psychopathy and violence was observed despite any supervision or treatment delivered during the follow-up. Interventions may have reduced the overall rate of violence, particularly among those at high risk (as those at low risk are subject to a floor effect), thereby reducing the magnitude of the association.

Third, the magnitude of the association is large in terms of effect size. Perhaps the simplest effect size indicator is the familiar Pearson correlation, r . The r

between psychopathy and violence in the Harris *et al.* (1991) study is .53, which is considered large in absolute terms (e.g. Cohen, 1988). It is somewhat unusual to find a large effect size in research on psychopathology, even in highly controlled laboratory research. To find a large correlation between psychopathy and violence—despite measurement problems, despite a 10-year follow-up, despite intervening life events—is, to me, quite amazing.²

Evaluating psychopathy as a risk factor for violence

Using meta-analysis, it is possible to summarize the predictive validity of psychopathy with respect to violence in terms of a single number, known technically as an 'effect size'. Two such meta-analyses have been conducted, the first by Salekin *et al.* (1996) and the second by Hemphill *et al.* (1988), both focusing on the PCL-R. The results of these meta-analyses are not identical, as they used different procedures to select studies and estimate effect sizes; readers interested in details should consult the original sources. Hemphill *et al.*, using very conservative procedures, obtained an effect size of $r = .27$, which corresponds to a Cohen's d of about .56. Salekin *et al.*, using less conservative procedures and with access to a larger database of studies, obtained a Cohen's d of .79, which corresponds to a Pearson r of about .37. For the purpose of the discussion below, let us assume that the predictive validity of the PCL-R with respect to violence, across a wide variety of contexts and populations, is about $r = .35$ or $d = .75$. We can conclude from these meta-analyses that psychopathy is a significant risk factor for violence: PCL-R scores predict violence significantly better than chance and, in absolute terms, moderately well. This makes psychopathy one of the strongest and most robust predictors of violence identified to date in the empirical literature. (For example, in comparison, psychosis is a less robust and weaker predictor, correlating on average about $r = .20$ with violence.)

Another way of looking at this issue is to compare the accuracy of violence predictions using the PCL-R with the accuracy of other predictions made by human beings. Lipsey & Wilson (1993) conducted a review of 302 meta-analyses that evaluated the impact of various psychological, correctional, and educational interventions; they also compared these with meta-analyses of medical interventions. The interventions reviewed typically were moderately effective: The median d was .47, which corresponds to about $r = .23$. In another paper (Hart, in press), I selected examples of good (i.e. relatively effective), bad (i.e. relatively ineffective), and typical interventions within the fields of psychotherapy, education, and medicine; the effect sizes (d and r) associated with these interventions are presented

²It is also worth noting here that the maximum value of the correlation between two dichotomous variables is often substantially less than unity (1.0). Unity is the maximum only when the base rates of the predictor and the outcome are identical. To illustrate, I will use an example provided by Professor Lösel at the NATO Advance Studies Institute on Psychopathy (Lösel, 1996). Suppose we assign 200 offenders at random either to a correctional treatment or to a no-treatment control group. The treated group is deemed 'low risk', and the untreated group 'high risk'. All offenders subsequently are released from prison. During the follow-up, half of the untreated offenders (50 of 100, or 50 per cent) recidivate. In contrast, virtually none of the treated offenders (1 of 100, or 1 per cent). Treatment almost eliminated violence, reducing the recidivism rate by nearly 50 percentage points. Yet, the correlation between treatment and reduction in recidivism is only .56!

Table 1. Average effect sizes for various interventions

Intervention	Effect size	
	<i>d</i>	<i>r</i>
<i>Psychological</i>		
Cognitive behavioural therapy, effects on depression (Dobson, 1989)	.99	.44
Psychotherapy, effects on all outcomes (Smith, Glass & Miller, 1980)	.85	.39
Correctional treatment-adults, effects on all outcomes (Lösel & Kofler, 1989)	.25	.12
<i>Educational</i>		
Student tutoring, effects on academic achievement (Cohen, Kulik & Kulik, 1982)	.40	.20
Small class size, effects on achievement (Hedges & Stock, 1983)	.20	.10
Mass media campaigns, effects on seatbelt use (Moore, 1990)	.14	.07
<i>Medical</i>		
Cardiac bypass surgery, effects on angina pain (Lynn & Donovan, 1980)	.80	.37
Cyclosporine, effects on organ rejection (Rosenthal, 1991)	.39	.15
Cardiac bypass surgery, effects on mortality (Lynn & Donovan, 1980)	.15	.07

Note. From Lipsey & Wilson (1993).

in Table 1. As the table indicates, the accuracy of violence predictions using the PCL-R is only slightly lower than the accuracy of predictions that cognitive behavioural therapy will reduce symptoms of depression, psychotherapy will improve general well-being, or cardiac bypass surgery will reduce angina pain. Violence predictions using the PCL-R are more accurate, often substantially more accurate, than predictions that smaller class sizes will lead to improved academic achievement, correctional treatment will improve the well-being of offenders, cardiac bypass surgery will reduce mortality, or public education will increase seatbelt use.

Based on a strict statistical criterion, then, it would be hypocritical to support cardiac bypass surgery, small class sizes, or correctional treatment programmes as promoting social good without also supporting violence predictions.³ When I have brought up this example in the past, some people have objected to sole reliance on a statistical criterion to judge importance, saying that it is impossible to compare studies with such diverse outcomes and that even small effects can be practically

³Interestingly, I don't know anyone who has turned down cardiac bypass surgery when it was recommended by a physician, but I know many people who still claim it is impossible to predict violence reliably.

important when the outcome is serious (e.g. as in medical research). My response is that I am trying to compare the accuracy of predictions, not the nature of the outcomes. Also, I think it is quite reasonable to compare the outcome of violence predictions with that of many medical interventions; in both cases, prediction errors may result in suffering or death. I think the general public would view the occurrence of criminal violence to be as important a social outcome as improved classroom climate or increased use of seatbelts.

Conclusion: The role of psychopathy in violence risk assessments

Recommendations for practice

To this point, I have discussed the nature of risk assessment and research on psychopathy as a risk factor for violence. I will now attempt to integrate these two discussions and make some general recommendations about the proper role of psychopathy in violence risk assessments. Of necessity, I will stray outside the boundaries of science, for science tells us what is possible, not what is preferable. Decisions about how we *should* conduct risk assessments must be influenced as much by legal and pragmatic concerns as they are by empiricism (Hart, 1996; Ogloff & Lyon, in press).

(1) *Psychopathy is a necessary, but not sufficient, factor in the assessment of violence risk.* Psychopathy must be considered a central part of any comprehensive assessment of risk for violence because it is empirically related to future violence, it is theoretically important in the explanation of violence, and it is pragmatically relevant in making decisions about risk management. Indeed, psychopathy is such a robust and important risk factor for violence that failure to consider it may constitute professional negligence. However, for reasons discussed in the first part of this paper, it would also be negligent to base a risk assessment on a single risk factor such as psychopathy.

(2) *Psychopathy may be sufficient, but not necessary, to conclude that an individual is a high risk for violence.* The rate of violent recidivism in psychopaths is so high (> 80 per cent over five years; see Hemphill *et al.*, 1998) that one may reasonably conclude that all psychopaths are a high risk for future violence.⁴ However, the converse is not true: Not all those at high risk for violence are psychopaths. It is quite possible for non-psychopathic individuals to be a high risk for violence as a consequence of other risk factors, such as expressed homicidal ideation/intent, sexual sadism, and so forth. This is particularly true when assessing risk for specific forms of violence, such as sexual or spousal assault, in which psychopathy may play a rather circumscribed role.

⁴I should emphasize here that the level of psychopathic traits should be high in absolute terms, not in relative terms (e.g. Litwack & Schlesinger, in press; Salekin *et al.*, 1996). Some researchers may define 'psychopathy' as scores above the sample mean on the PCL-R, which may be as low as 15 or 20. In contrast, the diagnostic cut-off for psychopathy specified in the PCL-R manual is 30 (Hare, 1991). Taking into account the standard error of measurement on the PCL-R, it seems that extremely high levels of psychopathic traits should be defined as PCL-R scores of 34 and greater, a score that would place the individual in the top 5 per cent of all offenders and forensic patients in the PCL-R normative samples (Hare, 1991).

(3) *When conducting violence risk assessments, psychopathy should be assessed by appropriately qualified and trained personnel using standardized procedures.* Personality disorder, including psychopathy, is a form of mental disorder. As is true for all mental disorder, the assessment and diagnosis of personality disorder requires considerable training and experience. Most jurisdictions restrict practice of assessing and diagnosing mental disorder to specific professions. The use of standardized tests, such as the PCL-R, may improve the reliability and validity of psychopathy assessments; however, the use of psychological tests also requires special qualifications and training, and may also be restricted to specific professions (see Ogloff & Lyon, in press).⁵

Implications for research

There is still much we need to know about the association between psychopathy and violence. No doubt, researchers will continue to conduct follow-up studies to determine the risk for violence associated with psychopathy in various contexts, as well as the co-factors that may increase or attenuate this risk. We are particularly in need of studies that closely monitor individuals over time, so that we can identify important dynamic risk factors. (Some of the statistical procedures discussed earlier, such as survival analysis with time-dependent covariates, offer great potential in this regard.) We also need to determine why a small subgroup of psychopaths, comprising about 10 to 20 per cent of those diagnosed, appear to spontaneously desist from further criminal and violent behaviour after release from prison. It may be that these individuals have found a prosocial (or less overtly antisocial) alternative to violence. Alternatively, it may be that they continue to act violently, but that this violence is not noted in formal records (e.g. because they have moved to another jurisdiction or learned how to avoid arrest). Finally, it may be that they are dead, but that this event has escaped the notice of researchers (e.g. researchers failed to search death records, or death records were filed under an alias).

It would be a mistake to over-focus on follow-up studies, however. They are inherently descriptive, and are of limited value in identifying or testing causal mechanisms. Research should explore further the proximal affective, cognitive, and behavioural factors that lead psychopaths to act violently. It also should include intervention studies that target these (putative) factors in psychopaths; evidence from well-controlled studies that intervention systematically decreases risk for future violence is strong evidence that an important causal mechanism has been identified.

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⁵Almost all research supporting the predictive validity of psychopathy has used the PCL-R. Also, it is clear from research that other procedures for the assessment of psychopathy are, at best, moderately correlated with the PCL-R; at worst, they may be completely uncorrelated with the PCL-R (Hare, 1991). Therefore, the use of standardized instruments other than the PCL-R in risk assessment may be problematic.

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