

Treatment of Psychopathy

A Review of Empirical Findings

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Can psychopaths be treated? In this chapter, we evaluate the empirical evidence on the treatment of psychopaths. We concentrate on treatment for criminal psychopaths and intervention strategies in which efforts to reduce criminal and violent behavior are at least part of the protocol. Without denying the importance of other psychopathic characteristics, criminal and violent behaviors are clearly the most important outcomes from a social policy perspective.

We do not discuss treatment for various types of psychopaths, although there has been considerable discussion about the clinical and theoretical significance of psychopathy subtypes. Prototypical (sometimes called primary) psychopaths present as callous and unemotional, whereas secondary psychopaths seem more emotionally labile, angry, or anxious (Poythress & Skeem, Chapter 9, this volume; Skeem, Poythress, Edens, Lilienfeld, & Cale, 2002). It has been hypothesized that one form of psychopathy is primarily a heritable condition while another is due mainly to environmental influences, particularly abuse during childhood (Mealey, 1995). Whether the primary–secondary distinction maps onto the genetic–environmental distinction is unclear. Nevertheless, subtypes of psychopathy might

require different therapies (Skeem, Poythress, et al., 2002). However, until there is more evidence that it matters to prognosis (criminal outcome, response to treatment), the existence of subtypes cannot have much relevance to treatment.

TREATMENT OF PSYCHOPATHIC OFFENDERS AND PSYCHOPATHIC FORENSIC PSYCHIATRIC PATIENTS

The clinical literature has been quite pessimistic about the outcome of therapy for psychopaths. Hervey Cleckley, in his several editions of *The Mask of Sanity* (1941, 1982), described psychopaths as neither benefiting from treatment nor capable of forming the emotional bonds required for effective therapy. In contrast, some early studies claimed positive effects of psychotherapy (Beacher, 1962; Corsini, 1958; Rodgers, 1947; Rosow, 1955; Schmideberg, 1949; Showstack, 1956; Szurek, 1942; Thorne, 1959). However, all these were uncontrolled case reports. Reviewers before 1990 concluded, as had Cleckley, that there was no evidence for the efficacy of treatment with adult psychopaths (Hare, 1970; McCord, 1982).

Therapeutic Communities

One of the most popular treatments for psychopathy has been the therapeutic community. Hare (1970) suggested that the reshaped social milieu of a therapeutic community might alter the basic personality characteristics and social behavior of psychopaths. Although lacking comparative data for untreated psychopaths, there were several early positive reports (Barker & Mason, 1968; Copas, O'Brien, Roberts, & Whiteley, 1984; Copas & Whiteley, 1976; Kiger, 1967). Based on these, Rice, Harris, and Cormier (1992) evaluated an intensive therapeutic community for mentally disordered offenders thought to be especially suitable for psychopaths. It operated for over a decade in a maximum security psychiatric hospital and drew worldwide attention for its novelty. The program was described at length by Barker and colleagues (e.g., Barker, 1980; Barker & Mason, 1968; Barker, Mason, & Wilson, 1969; Barker & McLaughlin, 1977) and elsewhere (Harris, Rice, & Cormier, 1994; Maier, 1976; Nielson, 2000; Weisman, 1995). Briefly, the program was based on one developed by Maxwell Jones (1956, 1968). It was largely peer operated and involved intensive group therapy for up to 80 hours per week. The goal was an environment that fostered empathy and responsibility for peers.

The evaluation (Rice et al., 1992) was quasi-experimental in which 146 treated offenders were matched with 146 untreated offenders on variables related to recidivism (age, criminal history, and index offense). Almost all offenders had a history of violent crime and were scored on the Psychopathy Checklist—Revised (PCL-R; Hare, 1991, 2003). Although the two groups were not explicitly matched on the PCL-R, the average score in each was 19. Because the PCL-R was scored using file information only, the cutoff score for classifying offenders as psychopaths was set at 25 rather than the customary 30. The results of a follow-up conducted an average of 10.5 years after completion of treatment showed that, compared to no program (in most cases, untreated offenders went to prison), treatment was associated with lower violent recidivism for nonpsychopaths but *higher* violent recidivism for psychopaths. Psychopaths showed

poorer adjustment in terms of problem behaviors while in the program, even though they were just as likely as nonpsychopaths to achieve positions of trust and early recommendations for release.

Why did the therapeutic community program have such different effects on the two offender groups? We speculated that both the psychopaths and nonpsychopaths who participated in the program learned more about the feelings of others, taking others' perspective, using emotional language, behaving in socially skilled ways, and delaying gratification. For the nonpsychopaths, these new skills helped them behave in prosocial and noncriminal ways. For the psychopaths, however, the new skills emboldened them to manipulate and exploit others.

In another therapeutic community, Ogloff, Wong, and Greenwood (1990) reported on the behavior of psychopaths and nonpsychopaths defined by criteria outlined in an early version of the Psychopathy Checklist (Hare & Frazelle, 1985). Compared to nonpsychopaths, psychopaths showed less motivation, were discharged earlier (usually because of lack of motivation or security concerns), and showed less improvement. Similar results were reported for a therapeutic community in England's Grendon prison in (Hobson, Shine, & Roberts, 2000), where poor adjustment to the program was likewise associated with higher PCL-R scores. A recent study of a therapeutic community for female substance abusers (Richards, Casey, & Lucente, 2003) reported that, although none of the offenders scored over 30 on the PCL-R, higher psychopathy scores were nevertheless associated with poorer treatment response indicated by failing to remain in the program, rule violations, avoiding urine tests, and sporadic attendance.

Despite evidence that therapeutic communities are ineffective with psychopaths, they remain popular in prisons, secure hospitals, and other institutions in Europe in which some participants are likely to be psychopaths (Dolan, 1998; McMurrin, Egan, & Ahmadi, 1998; Reiss, Meux, & Grubin, 2000). Even in North America, therapeutic communities are advocated for people with substance abuse problems (e.g., Knight, Simpson, & Miller, 1999; Wexler, Melnick, Lowe, & Peters, 1999), some of whom are likely to be psychopaths. Few studies of ther-

apeutic communities outside North America, and only one for substance abusers (Richards et al., 2003), have used PCL measures that would allow estimating the prevalence of psychopathy.

Other Treatment Approaches

Besides therapeutic communities, cognitive-behavioral therapy is often recommended for psychopathic offenders. Andrews and Bonta (1994), Brown and Gutsch (1985), Serin and Kurychik (1994), and Wong and Hare (2005) all suggested that intensive cognitive-behavioral programs targeting “criminogenic needs” (i.e., personal characteristics correlated with recidivism) might be effective. For example, Wong and Hare recommended relapse prevention in combination with cognitive-behavioral programs. However, doubts as to the efficacy of this treatment with psychopaths arose from an evaluation of a cognitive-behavioral and relapse prevention program for sex offenders conducted by Seto and Barbaree (1999). High psychopathy offenders who were rated as having shown the most improvement (as measured by conduct during the treatment sessions, quality of homework, and therapists’ ratings of motivation and change) were more likely to reoffend than other participants, particularly in violent ways. The treatment followed the principles of good correctional treatment (Andrews & Bonta, 1994; Andrews et al., 1990): It was highly structured and cognitive-behavioral, best matching the learning style of most offenders, including psychopaths. Moreover, psychopaths are high-risk offenders with many criminogenic needs (Zinger & Forth, 1998), and thus the program targeted deviant sexual preferences and antisocial attitudes (Barbaree, Peacock, Cortini, Marshall, & Seto, 1998). In view of these features, the results pertaining to psychopaths are especially notable.

Further doubts regarding the efficacy of cognitive-behavioral treatment for psychopaths emerge from other outcome studies. Among participants in a program for mentally disordered offenders in a secure psychiatric hospital, Hughes, Hogue, Hollin, and Champion (1997) found that PCL-R score was inversely correlated with therapeutic gain, even though patients with PCL-R

scores over 30 were excluded. In another study, Hare, Clark, Grann, and Thornton (2000) evaluated cognitive-behavioral prison programs for psychopathic and nonpsychopathic offenders. After short-term anger management and social skills training, 24-month reconviction rates for 278 treated and untreated offenders yielded an interaction between psychopathy and treatment outcome similar to that reported by Rice and colleagues (1992). Whereas the program had no demonstrable effect on nonpsychopaths, treated offenders who scored high on Factor 1 of the PCL-R had significantly higher rates of recidivism than high-scoring but untreated offenders.

In short, the few available empirical results regarding the effectiveness of treatment with psychopathic offenders are dismal, leading some to suggest that one should discuss management rather than treatment for psychopathic offenders (see Lösel, 1998). It may be that the very highest-risk offenders (i.e., psychopaths) might not be treatable even with very intensive and carefully designed and implemented programs. Of even more concern, perhaps, is the possibility that programs that might be beneficial for other offenders actually increase the risk represented by psychopaths.

Meta-Analysis of Research on the Treatment of Psychopathy

Traditionally, in a review of the evidence pertaining to a particular question, commentators summarize studies and derive an informal summary of the state of knowledge. This summary is usually accompanied by speculation about possible sources of apparent conflict in findings across studies. However, a more systematic way to resolve apparent inconsistencies in research findings is to use meta-analysis. This statistical approach allows the combination of research results from many studies, permitting conclusions about the likelihood that a group difference or relationship exists, how large it is, and why some studies find it and others do not. Research on the treatment of psychopathic offenders might seem particularly fruitful for meta-analysis because studies in this area often use small samples, such that effects might go undetected due to low statistical power. Studies also differ in the measures of psy-

chopathy, kinds of treatment provided, criteria by which candidates are assigned to treatments, and procedures used to evaluate outcomes. Meta-analysis offers a solution to the problem of small sample sizes in individual studies, as well as a methodology for testing hypotheses about the sources of differences in findings across studies.

Of course, meta-analysis cannot overcome general deficits. For example, if very few studies of psychopathy treatment used the PCL-R, meta-analysis could not examine it in moderating treatment effects. A meta-analysis also cannot make up for methodological inadequacies in the literature as a whole. For example, one of the most serious problems in this literature is the scarcity of well-controlled studies, especially those using random assignment. By contrast, there is an increasing trend toward evidence-based medicine in the treatment of physical and mental health problems in general, which has resulted in the Cochrane Database of systematic reviews—a collection of methodologically adequate studies on various diseases and conditions (www.update-software.com/cochrane/). Studies using random assignment are heavily weighted in this database and few other designs are considered strong enough to be informative.

A good illustration of the limitations of meta-analysis was afforded by a recent meta-analysis of research findings on the treatment of psychopathy (Salekin, 2002).¹ Salekin provided a quantitative review of 42 studies he identified as having evaluated the effectiveness of some form of therapy for psychopaths. Salekin reported that the mean rate of successful intervention across all treatment studies was .62, $p < .01$. This was the proportion of treatment candidates judged to have “improved”² minus the proportion expected to have improved without treatment (the latter proportion was calculated, according to the author, by averaging the improvement of untreated subjects in eight studies identified as including comparison or control groups). Salekin concluded that the prevailing pessimism about the treatment of psychopaths was unfounded.

Several aspects of this meta-analysis are noteworthy: The mean intensity of treatment was approximately four sessions per week over a year; only four studies employed the Hare PCL-R; only eight studies included

comparison subjects³; few studies (< 20%) assessed outcome in terms of criminal behavior, and even fewer (< 10%) mentioned violence or aggression; the most effective treatment was found to be psychodrama; and the evaluation of effectiveness was most often (> 70%) based on therapists’ impressions. In an effort to improve the rigor of studies without control groups, Salekin stated that he used averaged data from the “controlled” studies to estimate an effect of nontreatment for all studies. However, for reasons articulated later, we consider this method of calculating the improvement of control subjects to be problematic.

Our opinion, based on a variety of considerations, is that no firm conclusions can be drawn from this meta-analysis. In particular, we maintain that only controlled studies can be informative regarding treatment efficacy, and no conclusions can be drawn from uncontrolled studies. Because we consider control groups to be essential, we turn our attention first to the eight studies Salekin identified as controlled. We begin with Rice and colleagues (1992), which was discussed at some length earlier in this chapter, and then consider each of the other seven studies in turn. Rice and colleagues reported that 78% of the treated psychopaths committed a new violent offense during the follow-up compared to 55% of untreated psychopaths. Salekin’s summary of Rice and colleagues stated that 22% of the psychopaths “benefited” from treatment compared to 20% who would have “benefited” without the program, for a net benefit of 2%. Salekin considered that psychopaths who did not violently reoffend during the follow-up “benefited” from treatment even though untreated psychopaths exhibited less violent recidivism. The 20% figure was the weighted average proportion of psychopaths he calculated as having improved without treatment from the eight studies considered to be controlled. For each study in the meta-analysis, he subtracted this 20% figure from the percentage he considered to have benefited from treatment to compute net benefit. We believe the Rice and colleagues study shows why this method is problematic.

Craft, Stephenson, and Granger (1964) compared 50 severely delinquent boys alternately assigned to either a group psychotherapy unit or an “authoritarian” unit. No accepted measure of psychopathy was used.

The former program was new and incorporated many components of Jones's therapeutic community. In the latter, "authoritarian" program, patients were told on admission that "noise and disarray would not be tolerated and peace and quiet would be enforced by putting offenders to bed, fines, [and] deprivation of privileges . . . [combined with] "superficial psychotherapy" (p. 546). It was described as "standard" treatment at the time (1958). The authors had clearly expected that group psychotherapy would emerge as the superior program, but the results favored the authoritarian program. Significantly fewer offenses were committed by boys from that program in the follow-up period than by boys from the group psychotherapy program. Psychological test results also clearly favored the authoritarian program. The authors concluded that no conclusions could be drawn about the effectiveness of either treatment, as there was no untreated control group. They stressed that their study yielded no evidence to support the prevalent view among therapists that psychotherapy was more effective than standard treatment.

Salekin categorized this study as containing two treated groups—"therapeutic community" (the group psychotherapy program) and "cognitive-behavioral" (the authoritarian regime). The term "cognitive-behavioral" did not appear in the original study and cognitive-behavioral therapy was not developed until approximately a decade after this study was completed (Friedman, 1970). Salekin reported that this study showed positive results for both programs because 63% benefited (i.e., had no convictions in the follow-up) from the cognitive-behavioral program, and 43% benefited from the therapeutic community program. However, a different interpretation was given by the study's original authors: "Both treatments may have been better than nothing; both . . . may have worsened the boys—we do not know" (Craft et al., 1964, p. 553). We think a fairer interpretation is that this study yielded results similar to those of Rice and colleagues (1992), inasmuch as the therapeutic community increased recidivism relative to a standard, more custodial, approach.

Ingram, Gerard, Quay, and Levinson (1970) compared 20 juvenile delinquents treated in an "action-oriented" program

with 41 youths admitted either before the program began or after it ended. All were categorized as psychopathic according to an instrument developed by one of the authors. Treated youths had fewer assaultive offenses during the program (.25 per youth) compared to controls (.50 per youth), although the difference was nonsignificant. None of the treated youths were reported to have made a negative institutional adjustment after transfer to another institution, compared to 21% of controls. Salekin reported this study as demonstrating that 75% of treatment participants had benefited in terms of reduction of institutional aggression, and 100% had benefited in terms of improvement in community adjustment.

Korey (1944) studied delinquent boys in a training school. No objective measure of psychopathy was used, although all participants were diagnosed as "constitutional psychopathic inferiors" with "severe delinquent and behavior problems" (p. 127). Seven boys (the experimental group) received benzedine sulfate, and five boys (the controls) received a placebo. Outcome was measured by therapist opinion regarding improvement in various aspects of institutional adjustment. Significantly more ($N = 4$) boys given the drug were judged to have improved than boys given placebo (none of whom were judged to have improved). Korey cautioned that benzedine left the boys' underlying personalities untouched and that it should be part of a more comprehensive treatment. Salekin reported that 57% of the treated boys in this study benefited.

Maas (1966) studied 46 adult female offenders classified as unsocialized on Gough's socialization continuum. Half were assigned to group therapy emphasizing psychodrama, and the others were assigned to an untreated control group. The outcome measure was self-reported ego identity. No actual data were presented indicating how many offenders improved, but the authors stated that there was a significant difference in favor of the psychodrama group. Salekin summarized this study, stating that 63% of the treated subjects improved.

Persons (1965) compared 12 inmates randomly assigned to treatment with 40 inmates randomly assigned to no treatment. Treatment was eclectic counseling twice a week for 10 weeks. All 52 inmates were psy-

chopaths according to a self-report questionnaire. Self-report and therapist ratings showed significantly more improvement for treated offenders. Treated offenders also had significantly fewer disciplinary reports over the 10 program weeks. Salekin reported that 92% of the treated inmates benefited, although (as is the case with the Maas study described previously) it is unclear how this figure was obtained, as no such data were in the original article.

Skolnick and Zuckerman (1979) compared 59 male drug abusers treated in a therapeutic community with 37 untreated male drug abusers of similar IQ who spent an equivalent period in prison. The article neither mentions psychopathy nor how many subjects were classified as psychopaths. The main outcome variables were changes on Minnesota Multiphasic Personality Inventory (MMPI) scales and three other self-report personality measures administered upon admission and again 6 to 8 months later. Although treated subjects decreased significantly more than controls on several measures of psychopathology, Salekin reported a negative effect of treatment in this study, presumably because the number of treated subjects who had 49 or 94 high peak codes on the MMPI increased significantly, whereas there was no increase in the comparison group. The authors pointed out that the increase in treatment participants with 49 or 94 high peak codes was due to decreases in the other scales rather than the result of an absolute increase in four and nine scale scores.

Finally, Woody, McLellan, Luborsky, and O'Brien (1985) studied 30 opium-dependent men diagnosed with personality disorder. Some ($N = 17$) had an additional diagnosis of depression. Some received drug counseling alone while others received counseling plus professional psychotherapy. The outcome variable was change in problem severity measured before and after treatment via structured clinical interviews. Some positive changes were reported for the depressed men, but the other men "showed little evidence of improvement" (Woody et al., 1985, p. 1064). No comparison of the two treatments was reported, and it is unclear how Salekin could have considered this a controlled study. Nevertheless, he reported that

80% of the treated men benefited from treatment.

One study in Salekin's meta-analysis was not classified as controlled, but we believe it should have been. Miles (1969) compared 40 male adolescents admitted to a therapeutic community with 20 control patients in the same hospital (described as a "psychiatric hospital for the subnormal," p. 23) who were not offered the therapeutic community. The two patient groups were similar on age, IQ, and social class. Although Cleckley's work is cited, no mention is made of how many patients were psychopaths. Sociometry was used to measure outcome, and there was a net improvement in acceptance in 70% of the therapeutic community subjects compared to 10% of the comparison subjects. The authors concluded that the therapeutic community "increased the ability of the patients to accept their fellows more than did the traditional treatment" (p. 35). Salekin reported that the therapeutic community benefited 65% of the patients on measures that included "improved empathy," although the authors stressed that they used no measure of empathy.

How can we summarize these "controlled" studies of treatment outcome? We note that only one study (Rice et al., 1992) used the PCL-R, which is the contemporary standard (and most empirically valid) measure of psychopathy. Only two employed objective measures of criminal recidivism (Craft et al., 1964; Rice et al., 1992). Interestingly, our interpretation of both of these is that the treated group exhibited higher rates of recidivism than the control group. Our reading of the "controlled" studies in the Salekin meta-analysis is that there is absolutely no basis for optimism regarding treatment to reduce the risk of criminal or violent recidivism.

Other problematic aspects of the meta-analysis cast further doubt on the author's optimistic conclusion. As mentioned earlier, most studies in the meta-analysis relied on therapists' ratings to measure outcome. We consider this inadequate, especially for psychopaths. Note that Seto and Barbaree (1999) examined the recidivism of sex offenders as a function of psychopathy and progress in treatment, with progress assessed via eight structured therapist ratings. Based

on these ratings, which showed good interrater agreement and were undoubtedly more reliable than unstructured impressions of therapeutic progress, those offenders with better than average progress were more likely to recidivate violently, and this was especially true for psychopaths. In our opinion, therapists' impressions of clinical progress cannot be defended as an index of treatment effectiveness for offenders, especially psychopaths. Independently measured criminal conduct must be at least part of the outcome for an evaluation of treatment for psychopaths. This requirement eliminates all but a handful of the studies in the Salekin meta-analysis.

Several other categorizations in the Salekin meta-analysis were problematic. For example, Salekin categorized a study by Glaus (1968) as involving cognitive-behavioral therapy, with three psychopaths (defined by Cleckley's criteria) all reported to have improved as a function of the therapy. Compared to the 20% Salekin estimated would have improved without treatment, this was reported as a net treatment benefit of 80%. However, a careful reading of Glaus reveals that the author reported on the history and follow-up of 1,000 criminal psychopaths, of which 31 were "fully recovered and socialized" (p. 30). Glaus reported that many more might have improved, but he was unable to find more information (presumably despite follow-up efforts). Glaus described the three aforementioned positive-outcome cases in detail but made no claim that these were representative. Cleckley's criteria were never mentioned, nor was cognitive-behavioral therapy (which was only in its infancy in 1968; see Friedman, 1970); the therapy provided was so briefly described that it is impossible to categorize it. The journal editor noted that "the percentage of favorable results observed is low (over 3 percent), but the author's standards of follow-up and cure are unusually high" (p. 35). There is a huge discrepancy between the original author's report of just above 3% benefit and Salekin's report of 100%. In sum, close scrutiny of the studies in the Salekin (2002) meta-analysis reveals a variety of methodological weaknesses that cast serious doubt on its salutary conclusions. Most important, we think more random as-

signed treatment studies are required before meta-analysis can be informative.

Treatment for Nonforensic Psychopaths

Few studies reviewed by Salekin (2002) included offenders or forensic patients. Even if one could overlook the methodological weaknesses of the meta-analysis and studies included therein and accept its conclusions, it cannot tell us much about the population of primary interest—psychopathic offenders. Nonetheless, to be complete, we describe here findings from a recent evaluation of treatment for nonforensic "potentially psychopathic" patients (Skeem, Monahan, & Mulvey, 2002) not available at the time of the Salekin meta-analysis. Data from the MacArthur Risk Assessment Study were used to examine the interrelationships among psychopathy (assessed by the PCL:SV), self-reported involvement in treatment (mostly unspecified verbal therapy with or without drugs), and serious subsequent violence (almost all of which was undetected by the criminal justice system). The MacArthur methodology entailed interviews conducted every 10 weeks over a period of 1 year during which released civil psychiatric patients were asked about their involvement with treatment and violent behavior in the preceding period. Skeem and colleagues examined the relationship between violence in each target period and self-reported treatment in the previous period. They concluded that, in the first 10 postdischarge weeks, potentially psychopathic patients (> 12 on the PCL: Screening Version [PCL:SV]) who participated in more than 6 sessions of therapy (with an average of 11) exhibited less subsequent violence than those who participated in fewer sessions (the average was 3).

Recognizing that treatment was not assigned at random, the authors attempted to compensate by deriving a multivariate "propensity for treatment score" based on nine variables associated with the likelihood that subjects would report they had attended more treatment. This score was used as covariate in the aforementioned analysis. The inclusion of the "propensity score" attenuated the apparent treatment effect, but it remained statistically significant. While acknowledging several limitations of this study,

Skeem and colleagues (2002) inferred that the results provided evidence of an effect of mental health treatment as usual on reducing the violence associated with psychopathy, thus supporting the conclusions of Salekin's (2002) meta-analysis.

In our view, several methodological problems compromise the conclusions of this study regarding the effectiveness of treatment for psychopaths, despite efforts to correct for nonrandom assignment. First, psychopathy, treatment involvement, and violence were all assessed in the same interviews, leaving open the possibility of unintended measurement bias in all three constructs. A second issue concerns the number of bivariate comparisons performed in seeking evidence of a treatment effect. Skeem and colleagues reported 10 bivariate comparisons (two cutoff scores for psychopathy by four time periods, plus the entire follow-up period, presumably), only one of which yielded a statistically significant ($p < .10$) result⁴ after the incorporation of the "propensity" covariate. One significant result in 10 is exactly as anticipated by chance alone.

Moreover, Skeem and colleagues' (2002) use of a "treatment propensity" covariate is questionable in its own right. Miller and Chapman (2001) critiqued the use of covariance analysis on the grounds that it capitalizes on regression to the mean, and they asserted that its use as a method to equate nonrandomly assigned groups was inappropriate. They did acknowledge that a *propensity score* approach (Rosenbaum & Rubin, 1984) might be of assistance but noted that it could not address unobserved differences between groups. Skeem and colleagues cited Rubin (1997) as a source for "propensity" analysis, but did not employ a key aspect of the method, which involves disaggregating the subjects into subgroups defined by the propensity variable or function.

In our view, the Skeem and colleagues (2002) study probably exhibits "creaming intervention selection bias" (Larzelere, Kuhn, & Johnson, 2004), whereby patients of lower risk are more likely to receive treatment. Moreover, even if one accepts its findings, there are other concerns. Skeem and colleagues acknowledged that the civil patients scoring over 12 on the PCL:SV were only "potentially" psychopathic. The study provided no information about effective

components of treatment, and the conclusion that a dozen hours of unspecified therapy reduced serious violence by psychopaths seems highly questionable. We conclude that this study offers little guidance to those wondering about the efficacy of treatment for psychopathy or what therapy is indicated.

ALTERNATIVE CONCLUSIONS REGARDING THE EFFECTS OF TREATMENT

Given that it is such a serious and long-recognized problem, it is surprising that there has been so little good evaluation research on the treatment of psychopathy. Considering the available treatment literature, several alternative conclusions might be entertained:

- *Alternative Conclusion 1.* There have already been satisfactory demonstrations of effective treatment(s) for psychopaths (i.e., therapy that causes decreases in criminal and violent behavior), and the appropriate course is to provide such treatment(s) with intensity and integrity to as many psychopaths as possible. From this perspective, pressing research questions would pertain to the investigation of the conditions that ensure the successful export and adoption of such treatment(s) throughout the world's criminal justice systems, and modifications required to apply such treatment to noncriminal and youthful psychopaths.

- *Alternative Conclusion 2.* There have not been any satisfactory demonstrations, but only because adequate and persuasive evaluation work has yet to be done. Effective interventions for psychopaths have already been discovered and applied; it is the persuasive demonstrations that are lacking. For example, psychopathic offenders benefit from treatments already shown to be effective for offenders in general, but they require unusually high doses and intensities of such treatments in order for them to be effective. From this perspective, the obvious research priority is for rigorous and persuasive empirical demonstrations of the effectiveness of available treatments with psychopaths (with the next step being broader dissemination; viz. Alternative Conclusion 1).

- *Alternative Conclusion 3.* There have

been no satisfactory demonstrations because an effective clinical intervention is lacking. Psychopaths are fundamentally different even from other serious offenders, so that—despite available knowledge of what methods are effective for getting nonpsychopathic offenders to desist—no effective interventions yet exist for psychopaths. Indeed, some treatments that are effective for nonpsychopaths actually increase the risk of represented by psychopaths. Furthermore, the fact that psychopaths and nonpsychopaths are mixed together in most studies is the main reason why it has been so difficult to demonstrate effective treatment for adult offenders overall (i.e., positive treatment effects for nonpsychopaths are diluted or even negated by null or negative effects for the psychopaths). From this perspective, detailed analysis of the characteristics of psychopaths (inside and outside the laboratory) is needed to inform the design of new and effective interventions tailored to this unique population.

- *Alternative Conclusion 4.* No clinical intervention will ever be effective. Psychopaths are qualitatively different from other offenders but do not have deficits or impairment in any standard clinical sense. From this standpoint, the entire clinical enterprise is fundamentally unsuited to interventions to reduce the harm perpetrated by psychopaths. All that can be hoped for is a set of strategies to limit the harm by psychopaths by constraining their activities and opportunities.

It should be noted that these alternatives are not entirely mutually exclusive. For example, even if one concluded that a dozen sessions of mental health service as usual (Skeem et al., 2002) had actually reduced psychopathic violence (Alternative Conclusion 1), one would be unable specify the operative elements of that treatment, which would necessitate following the implications of Alternative Conclusion 2. Similarly, the enterprise that follows from Alternative Conclusion 3 of finding new therapies founded on an examination of the fundamental features of psychopathy could still be worthwhile even if some effective treatments had already been discovered. However, to the extent that one accepts Alternative Conclusions 1 or 2, one would probably assign lower priority to this task of developing new therapies.

In the final analysis, we adopt a blend of Alternative Conclusions 3 and 4. We believe, as outlined in Alternative Conclusion 4 (and explained further later), that the available evidence implies that psychopaths do not have deficits in the biological or medical sense. We propose that findings from outside the literature on treating psychopathy warrant serious consideration in designing interventions for psychopaths. We believe the evidence favors applying behavioral principles to reducing the harm occasioned by psychopathy. Our belief is based partly on empirical evidence that this approach has worked with some offender and violent populations (although effectiveness with psychopaths remains to be demonstrated). Our belief in the value of behavioral strategies for treating psychopaths also reflects a theoretical perspective that views psychopathy as a nonpathological condition, a reproductively viable life strategy. Next, we outline our evolutionary perspective on psychopathy to highlight implications for interventions.

A NONPATHOLOGICAL, SELECTIONIST ACCOUNT OF PSYCHOPATHY

There is evidence that psychopathy, unlike many psychological constructs, is underlain by a natural discontinuity or taxon (Ayers, 2000; Harris, Rice, & Quinsey, 1994; Haslam, 2003; Skilling, Harris, Rice, & Quinsey, 2002; Skilling, Quinsey, & Craig, 2001). By this view, scores on the best measure of psychopathy, the PCL-R, appear continuous because the identification of indicators and scoring are imperfect. Perfect measurement would, in theory, reveal just two possibilities—an individual either is or is not a true psychopath. Although not unanimous (Marcus, John, & Edens, 2004), the evidence supports the idea that psychopathy is a taxon.

The evidence on taxonicity, our research on treatment and the prediction of recidivism (Harris & Rice, in press; Harris, Rice, & Cormier, 1991; Rice & Harris, 1995) all suggest to us that psychopathy exists because it was a reproductively viable life strategy during human evolution. Adaptations (including those with psychological effects) were selected because they increased inclu-

sive fitness in ancestral environments. For example, being in a cohesive, mutually supportive (“reciprocally altruistic”) group was adaptive and heritable inclinations favoring group solidarity and adherence to rules have been associated with human reproductive success (Dawkins, 1978; Ridley, 1997). However, we (Harris, Skilling, & Rice, 2001; see also Mealey, 1995; Seto & Quinsey, Chapter 30, this volume) hypothesize that such a general strategy created a niche for an alternative cheating (i.e., psychopathic) strategy. When effective, this strategy is especially selfish, callous, manipulative, and lacking in empathy. If many people were cheaters, however, the strategy would lose its effectiveness due to the difficulty finding co-operators to exploit and the increased vigilance of remaining cooperators. Thus, the two strategies are expected to be frequency dependent, with cheating/psychopathy at low prevalence.

We hypothesize that high mating effort (i.e., promiscuous sexual behavior and many short-term marital relationships), and especially the willingness to employ deception and coercion, glibness, and charm, were (and are) also part of the psychopathic life strategy. Belsky, Steinberg, and Draper (1991) argued that a high mating effort life strategy is characterized by insecure attachment to parents and childhood behavior problems, followed by early puberty and precocious sexual behavior, and then unstable adult pair bonding and low parental investment. Psychopathy, we suggest, represents a genetically determined life strategy that has been maintained in the population through its relationship with reproductive success (Barr & Quinsey, 2004; Harris, Skilling, & Rice, 2001; Lalumière, Harris, Quinsey, & Rice, 2005; Rice, 1997).

The evidence on the neurocognitive characteristics of psychopaths (reviewed in this volume) reveals the condition to be an enduring set of traits that can be conceived of as aspects of personality or as differences in the form, manner, and relative speed of processing information. Key for this selectionist account is that these traits endure from situation to situation across the lifespan. Situations vary in the degree to which they differentiate between psychopaths and non-psychopaths, but, by this account, reinforcement and punishment operate for psycho-

paths as they do for everyone else, although what constitute reinforcers and punishers might differ.

Because psychopathy exhibits substantial heritability (reviewed in Waldman & Rhee, Chapter 11, this volume), the most straightforward and parsimonious version of the evolutionary account is that psychopaths have executed a “healthy” (in the biomedical⁵ but not moral sense) obligate strategy. Subtle neuroanatomical and neurochemical differences (without gross lesions) are consistent with this hypothesis. As well, it is expected that special tests would reveal that psychopaths act relatively impulsively, fearlessly, and unempathically and are resistant to punishment under some laboratory conditions but are not grossly disadvantaged. Psychopathy should also be associated with enhanced performance on some tasks. This account of psychopathy is consistent with the observation that it is peculiar for disorders to enhance any ability (such as conning and manipulation, Blair, personal communication, May 2000).

We have tested this account by examining several indicators of neurodevelopmental problems associated with psychiatric disorders (obstetrical and perinatal problems, medical problems in infancy, learning disability, etc.) and found them to be related to violent crime but unrelated or inversely related to violent offenders’ PCL-R scores (Harris, Rice, & Lalumière, 2001; Lalumière, Harris, & Rice, 2001). Although each of neurodevelopmental problems and psychopathy were associated with having had antisocial, negligent, and abusive parents, each appeared to be an independent cause of violent crime. Nonpsychopathic offenders exhibited more fluctuating asymmetry (an index of biomedical health) than psychopaths who themselves were not different from healthy volunteers (Lalumière et al., 2001). Finally, among sex offenders, those who preferentially target “reproductively viable” victims (i.e., postpubertal females) have significantly higher PCL-R scores than those who target all other classes of people (Harris, Hilton, Lalumière, Quinsey, & Rice, 2004). We are unaware of another hypothesis about sex offenders or psychopathy that accounts for this widely known difference.

Thus, there might be two distinct paths to serious, chronic criminality—one associated

with psychopathy and one (associated with less extensive crime and for which some treatments are effective) caused by developmental neuropathology and low embodied capital. If this nonpathological interpretation of psychopathy is correct, there are implications for intervention.

INTERVENTIONS FOR PSYCHOPATHS?

Is psychopathy likely to respond to very intense forms of the treatment that works with nonpsychopaths? The most straightforward implication of a dimensional view of psychopathy is that a high-intensity version of what has been shown to be effective with offenders in general would be effective for psychopaths. This would amount to a cognitive-behavioral program incorporating relapse prevention to combat substance abuse, anger management to control expressive aggression, prosocial modeling to break down antisocial thinking and values, and motivational interviewing to enhance commitment to treatment (Wong & Hare, 2005). The empirical literature supporting this approach for seriously violent adult offenders (Rice & Harris 1997) is as yet quite limited (and non-existent for psychopaths). Thus, this approach needs to be further implemented and evaluated, specifically with psychopathic offenders. However, by our taxonomic, nonpathological account of psychopathy, we believe more success might come from identifying different approaches. These are described in the remaining subsections in this chapter.

Behavior Modification

Meta-analyses of intervention studies have been informative with regard to the treatment of offenders. Lipsey (1992; see also Lipsey & Wilson, 1998) examined almost 400 evaluations of interventions for juvenile delinquency and reported a small statistically significant effect. Effects were larger to the extent that interventions were behavioral and oriented toward building skills. Even more broadly, Lipsey and Wilson (1993) conducted a meta-analysis of over 300 meta-analytic evaluations of human service interventions. Again, there was a moderate significant overall effect size, and, as far as can be

determined, behavioral interventions yielded effects larger than average and larger than the average for medical interventions. That properly implemented behavioral contingencies cause parallel changes in behavior is incontrovertible.⁶ There are debates concerning the mechanisms underlying punishment, the best ways to promote generalization, the effect of reinforcement on intrinsically rewarding behavior, and so on, but there is no doubt that behavior (whether pathological or not) responds predictably to its consequence (e.g., Corrigan & Muesser, 2000; Foxx, 2003; LePage et al., 2003; Lovaas, 1987; Paul & Lentz, 1977; Stein, 1999; Wong, Woolsey, Innocent, & Liberman, 1988; a longer list is available from the authors) while contingencies are in effect.

In no sense are we arguing that any of the foregoing provides evidence for a treatment effect among psychopaths. However, in general, behavioral treatments have the virtue of being explicitly designed for use under conditions in which the cause of the distressing behavior is unknown or cannot be specified (or is known, but cannot be altered). Furthermore, there is a technology that facilitates the implementation of behavioral treatment across an entire facility or agency—namely, the token economy system (Morris & Braukmann, 1987). Unlike other therapeutic approaches, psychopathy does not appear to present special problems for the effectiveness of a token economy (Pickens, Erickson, Thompson, Heston, & Eckert, 1979).

Multisystemic Therapy

The second impressive and persuasive literature on interventions for offenders concerns multisystemic therapy (MST) for juvenile delinquents (Brown, Borduin, & Henggeler, 2001; Brown et al., 1997; Randall & Cunningham, 2003). Theoretically, adolescent criminality is a systems problem: Adolescents engage in crime when responding naturally to the systems in which they operate. Dysfunctional families, ineffective schools, and antisocial peers combine to produce the obvious result—delinquency. MST seeks to alter each system to build functional school and family systems. In practice, MST is very individual and flexible with several general features: building skills, especially

for parents; emphasis on monitoring and consequence both for adolescents and parents; behavioral principles (positive reinforcement; promoting behaviors incompatible with antisociality; emphasizing specific, observable, active behaviors; concern with generalization); and ensuring therapeutic integrity and adherence (Henggeler, Cunningham, Pickrel, Schoenwald, & Brondino, 1996; Henggeler, Melton, Brondino, Scherer, & Hanley, 1997; Henggeler, Schoenwald, & Pickrel, 1995). Most important, MST has yielded large treatment effects in randomized controlled trials (Borduin et al., 1995; Borduin, Schaeffer, Ronis, & Scott, 2003).

For our present purposes, we recognize that the work on MST provides no evidence of a treatment effect for psychopaths or for adult offenders. In fact, its developers acknowledge that it cannot easily be applied to adults (Borduin, personal communication, August 2003). Moreover, our selectionist hypothesis about psychopathy (Harris, Skilling, & Rice, 2001) assigns little direct causal influence to antisocial peers: Psychopaths have more antisocial friends, but as a result of psychopathy, not as a cause. However, our hypothesis does maintain that psychopathic behavior is occasioned by opportunities favorable for its occurrence and that behavioral monitoring and consequence could reduce antisocial conduct by psychopaths by reducing its payoff. Our point here is that the evidence supporting MST as a treatment for delinquency is so promising that we can look past its theoretical underpinnings (Burns, Schoenwald, Burchard, Faw, & Santos, 2000; Huey, Henggeler, Brondino, & Pickrel, 2000) and move on to evaluate its efficacy when applied to offender groups for which it was not specifically designed.

Institutional and Community Programs

Where psychopaths have already committed serious offenses and exhibit evidence of high risk for future violence, we favor the use of selective incapacitation in the form of long-term institutionalization. Regardless of the duration of incapacitation, some organizational system must be in place within the institution. To this end, we favor the application of a sophisticated token economy

incorporating four main features. First, the program is completely explicit and concentrates on reinforcement of behaviors incompatible with psychopathic conduct (i.e., delaying gratification, telling the truth, being responsible, being helpful and cooperative—each tied to an appropriate operational definition) and penalties for impulsive, dishonest, aggressive, irresponsible, and, of course, criminal actions. Second, there is no expectation that the program will be completed or withdrawn; the program is only expected to be efficacious under conditions of continuous administration. Third, contingencies are tightly monitored by institutional staff, based only on observed, overt behavior, and never based on what inmates report about thoughts, feelings, or conduct. Fourth, systems are in place to monitor and consequence performance by front-line and supervisory program staff.

It must be recognized that societal and economic conditions would permit use of this incapacitation strategy with a minority of psychopaths (and a small minority of offenders). For most psychopathic offenders, release to the community in the form of parole or probation is inevitable. In our opinion, greater prospects for effective intervention lie in applying continuing behavioral principles to psychopaths under conditional release. Quite clearly, it will not be easy to design and implement a behavioral program for the institutional management of psychopathic offenders. It is to be expected that psychopathic offenders would resist such a program, break the rules in unexpected ways, seek to undermine institutional security, and engage in attempts to deceive and manipulate staff, supervisors, volunteers, the media, and members of the public.

The challenges associated with operating a program for psychopathic offenders should not be underestimated, but implementing an institutional program will be straightforward compared to delivering a similar behavioral intervention for psychopathic offenders under community supervision. We suggest, however, that the same principles should apply to community-release programs—behavioral monitoring, positive consequence, ensuring program integrity, and an emphasis on observable behavior. Participation in such programs would need

to be a condition for release; otherwise, few psychopaths would volunteer for and persist in such a program. We anticipate that programs of this sort will require more resources than customary parole or probation services, especially because the program is expected to be efficacious only as long as it continues to be administered. Nevertheless, given the broad societal harm caused by psychopaths, we believe an evaluation of such a program could show it to be cost-effective.

Protecting Potential Victims

The aforementioned suggested interventions are expected to reduce the violent and criminal behavior of psychopaths by shrinking the behavioral niche. By our selectionist account, psychopaths (like everyone) are sensitive to the features of the interpersonal environment favoring one behavior over another. To the extent that a particular behavior does not (or appears unlikely to) pay off, we expect its frequency to decline. Because humans exhibit excellent discrimination, we do not expect such behavioral changes to generalize to a postprogram environment because it would be obvious that the niche had changed. However, one might also ask: Rather than simply addressing the behavior of psychopaths, why not change the social environment itself? Some approaches of this kind have been tried with other populations.

Wassermann and Miller (1998; see also Catalano, Arthur, Hawkins, Berglund, & Olson, 1998) evaluated outcome data for several universal programs for preschool and school-age children and concluded that such programs can positively affect outcomes plausibly or empirically related to later antisociality. Programs targeting at-risk adolescents appear to reduce delinquent conduct (e.g., Tolan & Guerra, 1994). Similarly, increasing school supervision, boosting police patrols, installing surveillance cameras, using metal detectors, promoting neighborhood watch and citizen patrols, restricting access to firearms, increasing access to abortion, restricting citizens' freedom to move to relocate, and so on (cf. Catalano et al., 1998) can all be expected to shrink the opportunity for harm due to psychopathy. Of course, no one can say whether the reductions in antisocial conduct achieved by such broad-based

interventions reflect differences in the small minority of youth who become psychopaths. Nevertheless, on theoretical grounds, population-based interventions that (whatever else they do) decrease the opportunity for psychopathic aggression and exploitation can be expected to be worthwhile.

Finally, one might advocate explicit teaching about psychopathy in school and in public education campaigns. Such campaigns do appear to have had salutary effects in improving safety-related behaviors (safe sex, seatbelt use, decreasing smoking, increasing cancer screening, etc.). What is somewhat less obvious, however, is the specific content of training aimed at reducing the harm caused by psychopaths. For example, effectively instructing people to distrust strangers, telling young women that young men only want one thing, and advising everyone that leopards never change their spots are all approaches that might decrease the niche for psychopathy, but at such large social costs that benefits would be outweighed. More focused instructional approaches are probably desirable. Of course, similar concerns apply to tactics described in the previous paragraph. For example, how much police surveillance should law-abiding citizens tolerate in order to diminish the harm caused by psychopaths and other offenders? In our view, there is probably a trade-off in that restrictions on law enforcement agencies' security precautions necessarily increase the niche favorable to psychopathy.

Perhaps the following words of guidance, which we would give to novice forensic clinicians, could be a starting point for all safe relationships:

1. Read Hare (1998).
2. Reputation matters; leopards seldom change their spots.
3. Never take an offender's word at face value; always check his assertions against the record and with other informants.
4. Don't just attend to how he behaves toward you; carefully observe how he treats everyone—peers and other staff.
5. Beware of flattery.
6. Be very suspicious if an offender asks you to break a rule, no matter how minor, or to keep an illicit confidence.
7. Talk to a colleague about your rela-

tionship with him; if your trusted colleague says things don't sound right, beware.

CONCLUSIONS

We believe there is no evidence that any treatments yet applied to psychopaths have been shown to be effective in reducing violence or crime. In fact, some treatments that are effective for other offenders are actually harmful for psychopaths in that they appear to promote recidivism. We believe that the reason for these findings is that psychopaths are fundamentally different from other offenders and that there is nothing "wrong" with them in the manner of a deficit or impairment that therapy can "fix." Instead, they exhibit an evolutionarily viable life strategy that involves lying, cheating, and manipulating others.

Although no therapy has yet been shown to reduce the likelihood of future violence or crime among psychopaths, this does not mean that nothing can help. The best available evidence for effective intervention comes from the application of social learning principles in the form of behavioral programs and from MST. We believe that the strongest evidentiary support exists for institutional incapacitation where practical, and in tightly controlled behavioral programs with contingencies that remain in effect both inside and outside the institution. We can also conceive of societal changes that might reduce the behavioral niche for psychopathy, but such changes inevitably carry some negative impact with respect to the personal liberty of all citizens. Finally, none of these ideas comes close to a solution or cure for the societal harm caused by psychopathy. It is to be expected from our nonpathological, selectionist perspective that psychopaths will attempt to subvert harm reduction strategies employed by nonpsychopaths. In the ongoing arms race, the existing literature only suggests ways to limit psychopaths' advantages. More complete solutions lie in interventions based on future advances in basic neuroscience and molecular genetics (see MacDonald & Iacono, Chapter 19, and Seto & Quinsey, Chapter 30, this volume).

NOTES

1. One other study reported a meta-analysis of treatment for psychopaths (Garrido, Esteban, & Molero, 1995). The authors said there were two separate meta-analyses. The first included 34 studies that examined treatment outcomes for psychopaths compared to nonpsychopaths and purportedly showed that outcomes for psychopaths were worse than those for nonpsychopaths. The second included 19 studies that examined pre- and posttreatment studies of psychopaths and purportedly showed that psychopaths "are able to improve in behavioral and psychological functioning" (p. 59). Because no references were included in the article, there is no way to critically examine the methodology.
2. Salekin's definition of "improved" was somewhat unusual. For example, in the case of criminal behavior, he counted those who did not recidivate in the follow-up period as having "improved" regardless of how long the follow-up period was.
3. Salekin does not name the eight studies he counted as "controlled" in the meta-analysis. In a personal communication (May 2004), he advised that the eight were Craft, Stephenson, and Granger (1964); Ingram, Gerard, Quay, and Levinson (1970); Korey (1944); Maas, (1966); Persons (1965); Rice et al. (1992); Skolnick and Zuckerman (1979); and Woody, McLellan, Luborsky, and O'Brien (1985).
4. Skeem et al. reported a chi-square value of 3.31 as significant, $p < .05$. However, the use of a one-tailed procedure is clearly unwarranted in examining therapy that according to the authors themselves is of doubtful effectiveness, and might even in some instances be harmful.
5. This argument relies on a particular definition of pathology or "disorder" (Wakefield, 1992) which says disorders involve the failure of a mechanism to perform as designed by natural selection. Because this account asserts that it exists because it has been reproductively successful (i.e., it was designed by natural selection) psychopathy is, by definition, not a disorder.
6. Readers might wonder why we consider single case studies persuasive regarding the effects of behavior modification but not with respect to the benefits of psychodrama. The reason is that single-case designs typical of the evaluation of behavioral treatment incorporate considerable methodological control (e.g., objective measurement, multiple baselines, and reversal designs) rarely seen in the informal, impressionistic evaluation of nonbehavioral therapies.

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