Because of the enormous human and financial costs society incurs as a result of sexual crimes, any reduction in the recidivism of sex offenders caused by treatment is very worthwhile. Although treatment can be valuable if this reduction is small, the reduction must nevertheless be real. Real effects are statistically significant differences between treated and untreated subjects in controlled studies. In general, statistical significance is a necessary criterion for clinical and economic significance. In the field of sex offender treatment, it is likely to be a sufficient condition as well. In the view of the authors, the effectiveness of treatment in reducing sex offender recidivism has not yet been scientifically demonstrated. To demonstrate the effectiveness of sex offender treatment, more well-controlled outcome research is required that can be evaluated with meta-analytic techniques.

Assessing Treatment Efficacy in Outcome Studies of Sex Offenders

VERNON L. QUINSEY
Queen's University
GRANT T. HARRIS
MARNIE E. RICE
Mental Health Center, Penetanguishene, Ontario
MARTIN L. LALUMIÈRE
Queen's University

Marshall, Jones, Ward, Johnston, and Barbaree (1991) have recently reviewed studies of treatment outcome among sex offenders. They attempted, although not ignoring methodological considerations entirely, to use evaluative criteria that were not so strict as to exclude virtually every study ever done on sex offenders. Using this approach, they concluded (p. 480) that the evidence provides an unequivocally positive answer to the question: "Can sex offenders be effectively treated so as to reduce subsequent recidivism?" More specifically, Marshall et al. argue that comprehensive cognitive-behavioral programs and combined psychological and hormonal treatments are effective with child molesters and exhibitionists but not rapists.

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In this critique, we will argue that the treatment literature does not support these conclusions and, more importantly, that the approach taken in Marshall et al.'s review is unable to provide scientifically satisfactory answers to questions concerning treatment efficacy. We conclude by describing an alternative approach that is more likely to lead to the valid accumulation of scientific knowledge about treatment process and outcome with sex offenders.

METHODOLOGICAL ISSUES

In any review of outcome studies, decisions have to be made about which of them should be excluded or discounted on methodological grounds. Such decisions are of paramount importance in an area where the research is very weak. Marshall et al. (1991) identify important methodological weaknesses in almost every study they review. In this section, we will review the consequences of adopting the methodological criteria used in their review.

In introducing their review, Marshall et al. state that they used official records of rearrest or reconviction as their outcome criteria. They also state that, although the issues of treatment refusal, treatment noncompliance, and treatment dropout might be important, they considered only clients who participate in treatment (presumably until completion). Marshall et al. also argue that, because controlled comparison studies in which treated offenders are compared to untreated offenders cannot be conducted for ethical reasons, the best one can do is to compare the outcome of treated offenders to an estimate of the likely outcome in the absence of treatment based on information about subjects' histories of criminal and sexual offending.

The first problem with Marshall et al.'s criteria for reviewing available data concerns their decision regarding comparison groups. Although everyone would agree that a study in which subjects were randomly assigned to a treatment or control (or pseudotreatment or alternate treatment) group is the scientific ideal, few such studies of sex offenders have been reported. For exceptions, see Marques, Day, and Nelson (in press) and Romero and Williams (1983). Although Marshall et al. (1991) argue that it is unethical to withhold treatment for scientific reasons, we believe the contrary: If the treatment has not yet been shown to be effective or if the comparison/control treatment was the intervention most sex offenders would receive anyway, psychologists have an ethical obligation (cf. Canadian Psychological Association, 1988) to reduce the present ambiguity about the effects of sex offender treatment.

In the presence of such ambiguity, however, Marshall et al. (1991) advocate comparing treatment outcome and an estimate of the likely reoffense rate without treatment. They argue that recidivism rates can be sufficiently narrow in range to be useful for comparison purposes if important factors such as offense history are taken into account, although they note that some authors (e.g., Furby, Weinrott, & Blackshaw, 1989) have concluded that it is impossible to infer untreated recidivism rates from the literature because the rates vary tremendously from study to study. Unfortunately, although comparisons can be made if all risk-related offender characteristics are taken into account, sufficient information is rarely available to ensure the comparability of samples unless the sample is selected specially for the study at hand and, even then, it is questionable whether the treated and untreated groups are comparable on all relevant variables. There are also statistical arguments against matching on more than two or three variables.

Rice, Quinsey, and Harris (1991) yoked (post hoc) subsamples of treated and untreated offenders on variables known to be related to risk of recidivism (because treated men were found to be higher risks before treatment than untreated men) and then examined outcome among the yoked groups. No difference was obtained in outcome between the matched treated and untreated men. The important point here is that, without a specific comparison group, one can have no confidence that the estimated recidivism rates for untreated comparison subjects are accurate. Only truly randomized assignment can allow a strong test to be made in an individual study. Thus we disagree with the position of Marshall et al. (1991) that studies with no specific comparison group can be considered in the evaluation of treatment efficacy.

The second difficulty in making inferences from the outcome literature using Marshall et al.'s criteria involves a potential overestimate of treatment effectiveness caused by not considering those who refuse treatment and dropouts when comparing the outcomes of those who complete treatment with the outcomes of untreated men. There are now several examples in the sex offender treatment literature (Furby et al., 1989; Gordon, Holden, & Leis, 1991) that exemplify the point made more generally in the treatment literature (e.g., Foa & Emmelkamp, 1983) that treatment refusers and treatment dropouts should not be ignored in considering treatment efficacy.

The effects of ignoring treatment dropouts and refusers is especially well illustrated by data from a very well-designed and ambitious California study (Marques et al., in press) of institutionalized sex offenders who volunteered for comprehensive cognitive-behavioral treatment and were then randomly assigned to treatment or nontreatment conditions. An additional comparison group of men who did not volunteer but were matched to the volunteers on a variety of characteristics was also included. Although this study will not be completed for several years, it has been observed that treatment dropouts have about five times the rate of new sexual offenses and about three times the rate of new nonsexual offenses as those who complete treatment, volunteer controls, or matched nonvolunteer controls. Thus this study shows how a comparison between treatment completers and matched untreated controls can be misleading if treatment dropouts are not considered. A lower level of recidivism among treated subjects in comparison to a control group can thus reflect selection rather than treatment effects if treatment dropouts are not considered as part of the treatment group or as a group in themselves. In the absence of data about the numbers and outcomes of treatment refusers, dropouts, and noncompliers, data about the outcome of those who complete treatment, no matter how positive (or negative), *cannot* provide evidence of treatment efficacy.

The third important problem with the approach advocated by Marshall et al. (1991) is that the recidivism data from different groups of subjects who are explicitly matched on risk are only comparable if the groups are sampled from the same jurisdiction and cohort, because there are enormous variations in sex offender legislation and in police, prosecutor, and victim behavior over time and place. These differences are well-known to affect aggregate data, and surely affect recidivism data collected at the individual level as well. Obviously, not selecting a specific comparison group at all, and using instead an estimate gleaned from the literature, is no solution to this problem.

Although we disagree with Marshall et al. about including only those who participate fully in treatment, and about the use of "estimated" recidivism rates for untreated men, we do agree with their decision to use official police records of sexual offenses as the measure of outcome. Although there is widespread agreement that these rates are both conservative and noisy, they are less subject to bias than any other available outcome measure. We do disagree, however, with their assertion that it only makes sense to consider recidivism involving charges for sexual crimes. There is abundant evidence that sex offenses are often subject to plea bargaining or other compromises that result in nonsexual charges being laid for sexual offenses. Because of this plea bargaining, sexual recidivism is best measured from actual offense descriptions rather than simply from arrest or conviction charges. Nonsexual recidivism is important to measure, in addition, because it may be affected positively or negatively by treatment whether specifically targeted or not.

SUBSTANTIVE ISSUES

Having stated the criteria they will use to evaluate the data regarding the treatment of sex offenders, Marshall et al. (1991) go on to review the available literature according to the type of treatment used. In discussing physical treatments, they conclude that there is little reason to recommend psychosurgery or physical castration. They present evidence that physical castration is effective in reducing sexual recidivism, but dismiss it as an acceptable treatment because of its negative side effects, one of which may be an increased rate of recidivism for nonsexual offenses, and because it has fallen out of favor in countries where it was once popular.

Regarding pharmacological interventions, Marshall et al. (1991) favorably evaluate the effectiveness of MPA (medroxyprogesterone acetate) and especially CPA (cyproterone acetate). The evidence for this assertion, however, comes exclusively from uncontrolled clinical trials (cited in Marshall et al.) in which the only outcome measure was self-reported sexual arousal and self-reported sexual behavior. Using Marshall et al.'s own criteria, therefore, there can be no basis for their assertion that "there seems to be no doubt of the value of the combination of CPA and psychological treatment" (p. 474).

For psychological treatments, Marshall et al. discuss treatments according to whether they are nonbehavioral or cognitive/behavioral. For the nonbehavioral treatments, Marshall et al. draw no firm conclusions, presenting some data that show that treatment is effective, some that show it has no effect, and some that indicate that it is detrimental. They describe one program for incest offenders that shows a small positive effect when compared to "expected" rates, but conclude that for such marginal results, treatment resources would better have been invested in a higher risk group.

Marshall et al. (1991) conclude that the evidence shows unequivocally that comprehensive cognitive/behavioral programs for sex offenders can reduce subsequent recidivism. In reaching this conclusion, they divided programs into those that are institution-based and those for outpatients. Because Marshall et al. place so much confidence in the evidence for cognitive/behavioral programs, we will examine these conclusions in more detail. In support of their conclusion that institutionally based programs can be effective, Marshall et al. present evidence from four programs. The first evidence comes from an unpublished evaluation of a program in an Ontario penitentiary (Davidson, 1979, 1984). This program, which ran in the 1970s and early 1980s, included behavioral treatment designed to alter sexual preferences, social skills training, and individual and group insight-oriented psychotherapy. The program accepted rapists and child molesters who expressed a desire for treatment, although many of the men may have volunteered for treatment because they believed it would hasten their release. The outcomes of the treated men were compared with those of a group of men released before the program began and matched to the offender group on victims' sex, age, and relationship to the offender. As Marshall et al. (1991) indicate, Davidson found significantly lower overall and assault-related conviction rates for the treated than for the untreated men. However, although assault-related convictions included sexual offenses, there was unfortunately no significant difference in the number of convictions for *sexual* crimes between treated and untreated men in the total sample or among the child molesters. Moreover, there were significantly more *arrests* for sexual offenses among the treated men.

The second program that Marshall et al. (1991) report as demonstrating the utility of institution-based programs has been described by Gordon (1989). However, there was no comparison group and insufficient data are presented to conclude that the outcome was more positive than the "expected" rate for untreated men.

The third set of data comes from the previously mentioned, rigorously designed evaluation of a program in California (Marques et al., in press). Unfortunately, the data from this program are still preliminary and show no difference in sexual recidivism between men who volunteered for treatment (whether they completed it or not) and control volunteers, although there is some evidence that treatment may delay sexual reoffending.

The fourth program is one operated by the Vermont Agency of Human Services (Pithers & Cumming, 1989). Although the 3% sexual offense reconviction rate for child molesters appears low, there was no comparison group and it appears that only very low-risk offenders were accepted into the program from the outset. No claim is made that the program was effective for rapists.

In contrast to the programs described as having a positive outcome, Marshall et al. (1991) also present data from an institution-based program that found no treatment effect (Rice et al., 1991). The program ran during the 1970s and early 1980s and was very similar to the program reported by Davidson (1984). Although Marshall et al. (1991) state that deviant preferences were the prime targets in treatment, this was not, in fact, the case. Rather, for purposes of evaluation, all patients who had at least participated in one course of laboratory treatment directed toward altering deviant preferences were included. As Rice et al. (1991) report, the program also employed other treatments that evolved over the years, so that by the early 1980s its components included heterosocial skills training, education on sexual values and sexuality, and more cognitively oriented group therapy (Quinsey, Chaplin, Maguire, & Upfold, 1987). Of the 50 (Marshall et al. mistakenly give the number as 136) treated child molesters, 42 received at least one other component and 30 received at least two. Although there were no treatment dropouts per se, there were men who completed just one course, usually of 10 sessions, of laboratory treatment and then declined to participate in other components; these men were included in the treated group.

Outcome data, as Marshall et al. (1991) reported, revealed no therapeutic benefit, with 38% of the treated and 31% of the untreated men being convicted of another sexual offense in the 6.5-year (average) follow-up period. As mentioned above, no treatment effect was observed in a comparison of treated and untreated subjects matched on two risk-related variables. Similarly, when the results of 18 men who had shown a statistically significant improvement in their deviant sexual age preferences were compared to the outcomes of their yoked controls, no positive effects of treatment were found. Similarly, subjects who had received social skills training and/or sex education recidivated as frequently as subjects who had received neither.

In support of their claim of effectiveness for cognitive/behavioral treatment of outpatient sex offenders Marshall et al. (1991) cited two programs. The first is a program in Oregon (Maletzky, 1987). Maletzky found positive results for child molesters and exhibitionists, but not for rapists. There was no untreated control group. The other program was operated by Marshall's own Kingston Sexual Behavior Clinic. In an evaluation of a program for child molesters (Marshall & Barbaree, 1988), the authors matched treated child molesters with untreated men on important risk-related variables and found significantly lower rates of reoffending for the treated men. These data are indeed impressive; however, it is important to keep in mind that the subjects were not randomly assigned to treated and untreated groups, and it is possible that there was some self-selection by the subjects into treatment and nontreatment groups. Men in the untreated group were men who claimed they "lived too far away" to attend treatment regularly or who "changed their minds about the need for treatment" (p. 500). In the outpatient program operated by the second and third authors of this critique, men who appear to have low motivation to attend often drop out before treatment begins or say that they have transportation or other difficulties that prevent their regular attendance. Assigning such men to a control group confounds selection with treatment effects, making the two groups different in important ways other than the presence or absence of treatment. It is also important to note that, despite the claim by Marshall et al. (1991, p. 468) that their review would rely on official police records, the reoffending rates reported by Marshall and Barbaree (1988) were based entirely on unofficial reports. Offical rates for the 1988 study were given by Marshall et al. (1991) but do not yield a statistically significant treatment effect based on the sample sizes reported in the 1988 article.

The other report of the Kingston program (Marshall, Eccles, & Barbaree, 1991) concerns the treatment of exhibitionists and, although Marshall et al. (1991) report that the treated subjects did better than untreated subjects, the difference between groups was not significant in the original paper (the reported χ^2 of 4.54 with 2 *df* is not, as was reported, significant at $p < .05$). In addition, recidivism in both groups was high.

In contrast to the programs described as having a positive outcome, Marshall et al. (1991) also present data from another outpatient program for child molesters in which outcome data were based on self-reported reoffending (Abel, Mittleman, Becker, Rathner, & Rouleau, 1988). Marshall et al. report that, based on the 1-year follow-up data showing a 12.2% recidivism rate, the expected outcome by 5 years is not good. Again, Abel et al. (1988) emphasize the importance of considering treatment dropouts because in their study, the highest-risk offenders were the most likely to drop out of treatment.

In conclusion, we believe that the above discussion shows that the effectiveness of treatment in reducing sexual recidivism remains moot. More importantly, we believe that narrative reviews, regardless of how well conducted, are ill equipped to provide satisfactory answers to questions concerning treatment efficacy. Fortunately, there is now a well-developed alternative.

META-ANALYSIS

Having labored through the methodological issues discussed above, the reader may believe that the principal threats to the validity of the conclusions reached by Marshall et al. (1991) have been enumerated. Unfortunately, there are many others that simply cannot be dealt with by narrative reviews. First, there is the imprecision with which the question of treatment effectiveness is addressed in narrative reviews. A narrative review provides no reliable estimate of whether there is a treatment effect; no estimate of effect size, if one is present; and, in particular, no estimate of the relationship between effect size and type of control group. More importantly, a narrative review provides no quantitative appraisal of whether the variability in outcome over studies is entirely due to sampling error or to moderator variables involving setting, client characteristics, or treatment variables. Narrative reviews allow no way to tell whether variability in outcome is positively or negatively related to methodological characteristics of the studies, whether there is publication bias (more positive studies might get published), how length of follow-up is related to the size of treatment effects, what proportion of subjects are affected by treatment, and so on.

In addition, because narrative reviews provide no estimate of the effect of mediator variables, such as those related to treatment process, it is difficult for them to contribute to an understanding of how in-program changes are related to posttreatment performance.

Carefully conducted meta-analyses can provide quantitative answers to all of these questions. In addition, because meta-analyses typically specify the search procedures used to locate studies for inclusion, we can tell whether the study base is biased or not.

Briefly, meta-analysis is a procedure that organizes and combines quantitative information about a group of studies where each study is considered as a single element (or datum). The information of interest in meta-analyses is the average effect size (reflecting, for example, how much the treated and control groups differ across all studies), the variability of effect sizes, and the study characteristics (moderators, such as length of follow-up or treatment setting) that might create variability in effect sizes. The variability in effect sizes is of particular importance. In meta-analysis, that variability is partitioned into two components: the variability due to sampling error (i.e., sampling fluctuations), and the variability due to moderator variables. When all of the observed variability is due to sampling error, the average effect size of all studies is considered to reflect the real treatment effect (tests are used to determine if the average effect size differs from zero). When a substantial portion of variability cannot be attributed to sampling error alone, the presence of one or more moderator variables is inferred. Moderator variables are sample or study characteristics that systematically change the size of treatment effects. For example, treatment in outpatient settings might be found effective in reducing recidivism, whereas the same treatment in closed institutions might be found ineffective. More recently, meta-analytic techniques have been developed for other complex purposes, such as theory and model development, and the investigation of the role of mediator variables (variables that reflect the process of change) in treatment studies (e.g., Schmidt, 1992; Shadish & Sweeney, 1991). Hunter and Schmidt (1990) provide a detailed exposition of meta-analytic procedures and Durlak and Lipsey (1991) provide a brief practitioner's guide.

We have concluded, after having conducted a number of narrative literature reviews ourselves and writing this critique, that narrative reviews should play a very small, albeit sometimes important, role in drawing substantive conclusions from any literature.

Meta-analyses, however, cannot perform miracles. The treatment studies upon which they are based must have adequate control groups, and conclusions reached through meta-analytic procedures are stronger the greater the proportion of studies that use random assignment to alternate treatments. In the end, there is no substitute for scientific rigor. However, meta-analyses offer the field of sex offender treatment the opportunity of drawing definitive quantitative conclusions by combining the results of many studies, none of which alone would be decisive. The use of meta-analyses should encourage more evaluative research because every experimental or quasi-experimental study helps the field to reach a definitive scientific conclusion. We look forward to the time when there are enough controlled studies of sex offender treatment to permit a convincing meta-analysis. This methodology, more than any other, reminds researchers and practitioners that we are all in this enterprise together.

CONCLUSION

Marshall et al. (1991) argue that even a small reduction in the recidivism of sex offenders due to treatment is very worthwhile because of the enormous human and financial costs society incurs as a result of sexual crimes. We endorse this argument but recognize that it is completely dependent on there being a *real* (i.e., statistically significant) reduction in recidivism associated with treatment. In general, statistical significance is a *necessary* criterion for clinical and economic significance. In the field of sex offender treatment, it is likely to be a *sufficient* condition as well.

Our conclusion that the effectiveness of treatment in reducing sex offender recidivism has not yet been demonstrated is unlikely to make clinicians in the field happier. Clearly, it is more difficult to secure funding and to generate enthusiasm for treatment in the face of such uncertainty. However, in our view, there is no alternative to more and better evaluative research. With the increasing number of sex offenders now receiving treatment, there are unparalleled opportunities for the standardization of measures and pooling of evaluative data.

Marshall and Barrett (1992) have argued that one should not lend support to the reactionary forces who adhere to the idea that, in the treatment of offenders, "nothing works." We agree that, in general, the nothing works doctrine in offender treatment should not be countenanced, but only because there has been enough controlled research in the field of delinquent and adult offender treatment to enable a clinically sophisticated meta-analysis to unequivocally demonstrate strong treatment effects (Andrews et al., 1990; cf. Lipsey, 1992).

Regarding sex offenders, there is a greater danger in promising what may not be deliverable. Lowering our standards of scientific rigor will backfire if it turns out that cognitive/behavioral treatment has little salutary effect for sex offenders. The credibility of claims for the effectiveness of treatments (for which there *is* solid positive evidence in some areas) would inevitably suffer severe and long-lasting damage. As Rothman (1980) has shown, progressive reformers' enthusiasm for the treatment of delinquents, mental patients, and adult offenders in the early part of this century ultimately failed for lack of an effective technology. This failure has left a legacy of mistrust from which we have yet to recover.

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Vernon L. Quinsey received his Ph.D. in experimental psychology from the University of Massachusetts at Amherst in 1970. Currently, he is Professor and Coordinator of Forensic/Correctional Studies in the Psychology Department of Queen's University in Kingston, Ontario.

Marnie E. Rice is the Director of Research at the Mental Health Centre in Penetanguishene, Ontario. She received her Ph.D. in clinical psychology from York University in Toronto.

Grant T. Harris received his Ph.D. in experimental psychology from McMaster University in Hamilton. He is currently a research psychologist at the Mental health Centre in Penetanguishene, Ontario.

Martin L. Lalumière received his M.A. from the University of Montreal. He is currently in the Ph.D. program at Queen's University. His major interests are in quantitative methods and evolutionary theory.