

Remaking Relapse Prevention with Sex Offenders: A Sourcebook, edited by D. Richard Laws, Stephen M. Hudson, and Tony Ward (Thousand Oaks, CA: Sage, 2000), 559 pp., \$95.00. [herein Laws et al.]

Practice Standards and Guidelines for Members of the Association for the Treatment of Sexual Abusers (ATSA), by the ATSA Professional Issues Committee (Beaverton, OR: ATSA, 2001), 73 pp., \$25.00. [herein PS&G]

REVIEWED BY

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In 1998 R. Karl Hanson, a prominent Canadian sex offender researcher, addressed the question: What do we know about sexual offender risk assessment? He was neither overly optimistic nor pessimistic. He exercised judicious scientific skepticism to report what we know, and then considered the policy implications of our current state of knowledge.

Two recent books, the subjects of this review, address the implications of a parallel question: What do we know about sex offender treatment? Both books should be read carefully by all sex offender treatment providers, and both are worthy of consideration by policymakers. However, these works do not consistently show the type of skepticism essential to evaluating treatment programs and shaping public policy; consequently, they must not be taken as the definitive word on the subject.

Both works describe relapse prevention (RP) as being the most popular contemporary approach to the treatment of sexual abusers. Adapted from the addictions field, RP uses a variety of techniques to teach clients how to identify cues and situations associated with a greater risk of reoffending and to escape or avoid those risky cues and situations. In their introductory and conclusory chapters, Laws, Hudson, and Ward report that RP has become the treatment of choice even though the original RP model has serious limitations when

applied to sex offenders, the assumption that sex offenders are highly motivated to change is highly suspect, and there is little evidence favoring the actual prevention of relapse. Laws et al. opine that RP may be popular because it reduces anxiety among the treatment *providers* and imbues a sense of optimism.

Laws et al. (p. 503) treat that sense of optimism as a unifying theme in their last chapter: "What do we have to do to keep the faith with the optimism, as well as resources, that has been the result of adopting relapse prevention as the primary model?" Is this optimism warranted? Consider two key questions: Does sex offender treatment reduce recidivism? Do RP components enhance treatment effectiveness?

Does sex offender treatment reduce recidivism? The most careful consideration of this question is found in chapters 2 and 27 of Laws et al.; both chapters are written by Hanson. Hanson notes that there is one completed, published meta-analysis of sex offender treatment outcome (Hall, 1995). Hall found a small but significant treatment effect. However, reanalyses of the 12 studies in that meta-analysis found that the treatment effect could be wholly accounted for by studies that used dropouts/refusers as the comparison group (Harris et al., 1998). Hanson notes that studies have consistently reported lower recidivism rates for those who complete treatment than for those who drop out (Hanson & Bussiere, 1998), but that one cannot conclude from such studies that the treatment made the difference. Some of the same offenders who are at increased risk to sexually reoffend may also be at increased risk to drop out of treatment because of lack of motivation, impulsiveness, or general belligerence. Hanson concludes that "there have been insufficient studies to justify clear conclusions" (p. 491) and maps out a plan for research to address the issue.

Do relapse prevention (RP) components enhance treatment effectiveness (reduce relapse more than programs that do not

have RP components)? One way to address this question would be to conduct studies that randomly assign sex offenders into different types of treatment groups or a no-treatment control group. This type of study is rarely done, perhaps because it is commonly believed that treatment is better than no treatment and that certain types of treatment are better than others. Some sex offenders reoffend, and when a sex offender in a no-treatment control group (or in a type of treatment group hypothesized to be relatively less effective) reoffended, the victim might blame the authorities who authorized not treating (or ineffectively treating) that offender.

An alternative research design compares current treatment programs that use RP techniques with previous treatment programs that did not. Marshall and Anderson's chapter in Laws et al. cites studies of two programs identified as having no RP elements in their treatment programs, which found no effects for treatment, and studies of six programs including RP, which all reported positive effects for treatment. They interpret this as evidence that RP is more effective than other treatment approaches. However, none of these studies utilized a truly randomized design. Treated subjects were compared with a convenience sample of matched, contemporaneous untreated clients from the same setting.

To see how important this methodological issue is, consider the one study to date that has used a randomized design, the Sex Offender Treatment and Evaluation Project (SOTEP; Marques, Nelson, Alarcon, & Day, 2000). This study is presented by the researchers in one chapter in Laws et al., and is described by other authors in at least three other chapters as "most impressive" and "excellent" (p. 490), an "elaborate study" (p. 52), a "rigorously designed study" (p. 250), and "perhaps the most well-known relapse prevention-based sex offender treatment outcome study" (p. 249). SOTEP is a work in progress. Hanson expects that when the SOTEP study is completed, the result will be "highly influential, but it will

not silence the debate on treatment effectiveness. If treatment effects are found, skeptics will point to special features of the program that limit its generalizability. . . . If no treatment effects are found, treatment advocates will similarly suggest that the setting was artificial, clients were difficult, or the treatment was based on the 1989, not the [2000], edition of *Relapse Prevention*" (p. 490).

SOTEP recidivism data

	Completed treatment	Volunteer control	Nonvolunteer control	Dropped out of treatment	Started treatment
Not known to have reoffended	149	194	191	30	179
Reoffended	18	31	29	7	25
Total	167	225	220	37	204
% reoffended	10.8	13.8	13.2	18.9	12.2

Note: Reconstructed from Marques, Nelson, Alarcon & Day (2000, p. 324).

Indeed, the SOTEP project is so well designed that researchers and practitioners are commenting on the data even as they come in. Consider the data presented in the above table. Marques et al. write that their data "showed that after about 5 years at risk, the 167 subjects who completed treatment had a lower sex reoffense rate (10.8%) than did the 225 volunteer control subjects (13.8%) or the 220 nonvolunteer controls (13.2%). This trend . . . has not reached statistical significance. Another consistent finding over our years of follow-up has been that the 37 treatment dropouts have demonstrated the poorest outcomes (18.9% sex reoffense)" [p. 324].

Marshall and Anderson write that the data "do not reveal clear treatment effects. . . . [A]lthough the treated groups have lower recidivism rates, these apparent advantages do not reflect reliable benefits for treatment" (p. 52). Marshall and

Anderson suggest that SOTEP data do not show treatment effectiveness because the treatment program was *too* comprehensive, intensive, and extensive, which "may have convinced the offenders that the program staff did not believe they could manage on their own to avoid relapsing" (p. 52). They therefore "suggest that treatment providers be cautious about making their programs too elaborate and too lengthy for fear they may convey to clients that their problems are essentially beyond their capacity to manage on their own" (p. 52). The editors of Laws et al. find Marshall and Anderson's analysis "intriguing" (p. 504), though they recommend caution in the face of one study. I consider this analysis to be convoluted, guild enhancing, and unlikely.

Although Marshall and Anderson acknowledge that the SOTEP study is better designed than the studies they describe as showing positive treatment effects for programs with RP components, they fail to consider the implications of the SOTEP study on the other studies. They recognize that in studies of the effectiveness of RP components in sex offender treatment "a truly randomized design has not yet demonstrated a treatment effect," but assert that the six studies they review "would methodologically satisfy all but the most ardent enthusiasts of methodological elegance" (p. 51). Marshall et al.'s analysis is focused on the fact that compared with some other studies the SOTEP data show less of a difference between those who completed treatment and controls. What is more important, in my opinion, is that those differences, such as they are, are almost completely accounted for by those who dropped out of treatment.

Thus the question of whether RP components enhance treatment brings us back to the more basic question of whether sex offender treatment reduces recidivism. In each case, some studies suggest a positive effect, but no studies effectively rule out the possibility that lower recidivism rates for treatment completers are achieved primarily by subtracting treatment dropouts/refusers from the analysis. That is, the results

might not show positive effects of treatment at all. It does not take ardent enthusiasm for methodological elegance to recognize that available research studies have not shown what sex offender treatment providers-particularly those who utilize RP, the most popular approach-wish they would show. When we consider available data and approach the questions with healthy scientific skepticism, we must conclude, as Hanson does, that we do not know whether sex offender treatment reduces recidivism, and we do not know whether RP components enhance treatment effectiveness.

Three responses to this lack of knowledge are evident in Laws et al. First, the editors of Laws et al. report: "It is necessary to inform communities that there are interventions that work with sex offenders" (p. 509). Does this "necessity" result from a powerful well-force of clear data spilling forth from the teeming cauldrons of impressive research studies? No. Rather, this "necessity" may be due to the *lack* of clear results. If one desires to influence policymakers to continue or enhance funding of sex offender treatment despite the lack of clear evidence that the treatment works, then one would be inclined to overstate the effectiveness of sex offender treatment(s) and to understate the limitations of the research that shows positive findings.*

Second, Mann and Thornton describe how a sex offender treatment program can adapt to research data as they develop. They conclude: "A commitment to evidence-based treatment is, in our minds, a duty of all sex offender treatment providers. As much as we would like to treat sex offenders according to our whims, our preferences, or our personal theories, we do not serve society responsibly in so doing. As behavioral scientists, our treatment programs must advance on the basis of evidence" (p. 349).

Third, Hanson presents plausible dynamic risk factors for which there is tentative research support (see p. 496) and identifies what has made research of sex offender treatment

effectiveness so challenging. Hanson notes that more than 1,500 treatment programs for sex offenders are currently operating in the United States, and he describes how treatment providers can pool results into the nascent ATSA collaborative research project.

Which of these three responses appears to embody the ATSA standards and guidelines? It is not Hanson's response, which is to recognize current limitations and conduct more research. Although *Practice Standards and Guidelines (PS&G)* is prepared by and for ATSA, I found no mention of the ATSA collaborative research project in *PS&G*. One of the major goals of ATSA is "the dissemination of current information on clinical practice and research, in order to describe best practices for the field" (p. v). Yet the recommended areas of training and experience for ATSA members do not include training in conducting research or even in interpreting it.

Although I expect that the developers of *PS&G* would like to assert that *PS&G* embodies Mann and Thornton's response to the current state of knowledge regarding sex offender treatment, it does not. *PS&G* is replete with statements of fact for which no data are presented or referenced, and at least some of which are not supported by research. For example, *PS&G* introduces RP as follows: "[I]t is very clear that, for people who have engaged in crime, treatments that are structured, skills-oriented, and cognitive-behavioral are more likely to be effective than treatments that are unstructured, insight-oriented, and abstract" (p. 23). This statement is not supported by citing research in *PS&G*, nor does it emerge from careful consideration of the research presented in Laws et al.

I suspect that the developers of *PS&G* share Mann and Thornton's commitment to evidence-based treatment in theory, but in its current form *PS&G* does no more than provide a consensus of whims, preferences, and personal theories. Both Laws et al. and *PS&G* provide clear descriptions of the currently most popular approach to sex offender treatment.

The downside is that Laws et al. (intermittently) and *PS&G* (consistently) overstate the degree to which there is empirical support for the treatment's effectiveness. Relapse prevention is a popular approach to treating sex offenders, but like sex offender treatment generally, its effectiveness has yet to be established. Chapters in Laws et al. (particularly Hanson's chapter 27) provide a framework for the needed research. In the meantime, policymakers should not treat ATSA's *Practice Standards and Guidelines* as a research-based summary of what we know about sex offender treatment.

Note Elsewhere the editors are more forthright in acknowledging limitations of research, but the editors and some other authors have included such overstatements sporadically throughout Laws et al.

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