CHAPTER 1

INTRODUCTION

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The professional approach to sexual deviance involves the scientific study of the paraphilias and related sexual misbehaviors (e.g., rape), as well as the clinical assessment and treatment of these domains. In this chapter we briefly overview the major issues involved in the scientific study and clinical treatment of sexual deviance.

DEFINITIONAL ISSUES

Problems with Defining Sexual Deviance as Mental Disorder

There are continuing controversies about what constitutes "sexual deviance." The present book generally follows the *Diagnostic and Statistical Manual of Mental Disorders*, fourth edition, text revision (DSM-IV-TR; American Psychiatric Association, 2000), although admittedly this is ultimately an institutional rather than a scientific resolution to the definitional problem. That is, the American Psychiatric Association—by working committee, and ultimately by a vote of its membership—decides both what kinds of sexual behaviors are considered mental disorders, and the specific criteria to be used in attempting to define these demarcations. This is in stark distinction to the chemists' periodic table of elements, which is a taxonomy that carves nature at its joints. There are no votes by the American Chemical Association to determine whether oxygen or hydrogen is inside or outside this taxonomy. Committees, of course, are not carving nature at its joints. They instead are subject to political, personal, and other extrascientific considerations. Various editions of the DSM have included as paraphilias different entities (such as homosexuality), and have used different diagnostic criteria. In addition, controversies continue. For example, there is no explicit paraphilia that directly covers rape; some who commit rape may meet the current diagnostic criteria for sexual sadism, but many do not. Whether this is a gap is debatable.

The DSM-IV-TR offers a general definition of "mental disorder," which presumably all paraphilic diagnoses must meet:

> A clinically significant behavioral or psychological syndrome or pattern that occurs in an individual and that is associated with present distress (e.g., a painful symptom) or disability (i.e., impairment in one of more important areas of functioning) or with a significantly increased risk of suffering death, pain, disability, or an important loss of freedom. In addition, this syndrome or pattern must not be merely an expectable and culturally sanctioned response to a particular event, for example, the death of a loved one. Whatever its original cause, it must currently be considered a manifestation of a behavioral, psychological, or biological dysfunction in the individual. Neither deviant behavior (e.g., political, religious, *or sexual*) nor conflicts that are primarily between the individual and society are mental disorders unless the deviance or conflict is a symptom of a dysfunction in the individual, as described above. (American Psychiatric Association, 2000, p. xxxi; emphasis added)

This definition raises the question of whether deviant sexual behavior "is a symptom of a dysfunction in the individual, as described above." Apparently, the American Psychiatric Association thinks so, as it includes in the DSM-IV-TR a number of paraphilias. However, nowhere does it explicitly state exactly how the included categories meet this standard, or why some categories such as rape or homosexuality do not. Thus the principles used to make decisions regarding what should be included and what should be excluded are unclear. This is unfortunate, because if they were explicated not only would the taxonomy seem more open and reasoned, but it could be criticized and thus potentially improved.

Value Judgments and Sexual Liberation Movements

We also need to recognize that a somewhat complex issue involves value judgments that a type of sexual behavior is disordered or deviant. Certain value judgments have at times flown and continue to fly in the face of various "sexual liberation" movements that have been prominent in the past century. To some extent, these have probably been inspired by what happened with the diagnostic category of homosexuality. In some earlier editions of the DSM, homosexuality was regarded as sexual deviance, but after some effective political campaigning (and some rather weak scientific study) it was removed. In the first edition of this book, the authors of one chapter took issue with the view of sexual masochism as deviant (Baumeister & Butler, 1997).

One of the best-known current sexual liberation movements is the pedophilic one. It has previously received endorsements from such prominent sex researchers as Alfred Kinsey and John Money. For example, Kinsey, Pomeroy, Martin, and Gebhard (1953) stated:

> When children are constantly warned by parents and teachers against contacts with adults, and when they receive no explanation of the exact nature of the contacts, they are ready to become hysterical as soon as any older person approaches, or stops and speaks to them in the street, or fondles them, or proposes to do something for them, even though the adult may have had no sexual objective in mind. Some of the more experienced students of juvenile problems have come to believe that the emotional reactions of the parents, police officers, and other adults who discover that the child has had such a contact, may disturb the child more seriously than the sexual contacts themselves. The current hysteria over sex offenders may very well have serious effects on the ability of many of these children to work out sexual adjustments some years later. . . . (p. 122)

John Money (1991), the prominent Johns Hopkins sex therapist (although see Colapinto, 2000), said:

> If I were to see the case of a boy aged ten or eleven who's intensely erotically attracted toward a man in his twenties or thirties, if the relationship is totally mutual, and the bonding is genuinely totally mutual . . . then I would not call it pathological in any way. (p. 3)

Groups such as the North American Man/Boy Love Association (NAMBLA) and the René Guyon Society describe themselves as representing the most recent wave of sexual liberation. Typically they assert that the first wave of sexual liberation was women's sexual liberation, that the second was liberation associated with the acceptance of premarital sex, and that the third was gay liberation. For example, a speech posted first on the NAMBLA website and then elsewhere on the Internet asserts the following regarding "cross-generational love":

> The issue of love between men and boys has intersected the gay movement since the late nineteenth century, with the rise of the first gay rights movement in Germany. In the United States, as the gay movement has retreated from its vision of sexual liberation, in favor of integration and assimilation into existing social and political structures, it has increasingly sought to marginalize even demonize cross-generational love. Pederasty—that is, love between a man and a youth of 12 to 18 years of age—say middle-class homosexuals, lesbians, and feminists, has nothing to do with gay liberation. Some go so far as to claim, absurdly, that it is a heterosexual phenomenon, or even "sexual abuse." What a travesty! (Thorstad, 1998)

Mary DeYoung (1989) has analyzed the literature produced by pedophile organizations, and has found the use of the following persuasion strategies:

1. Adoption of value-neutral terminology.
2. Redefining the term "child sexual abuse" (to terms such as "adult–child sex" or even "intergenerational intimacy").
3. Promoting the idea that children can consent to sex with adults.
4. Questioning the assumption of harm.
5. Promoting "objective" research (as opposed to the research produced by "biased" researchers).
6. Declassification of pedophilia as mental illness.

This sort of thinking has also received some agreement from a few current researchers, such as Theo Sandfort of the Netherlands. Sandfort was one of the editors of the Dutch journal *Paidika: The Journal of Paedophilia*, which advocated adult–child sexual contact. Sandfort (1982) has stated that when he gave a screening questionnaire to a small group of boys who reported sexual contact with adults, "the question was whether a sexual contact with an adult could be a positive experience for a child. To the extent to which this research material can give a definite answer, the question must be answered in the affirmative" (p. 84). O'Donohue (1992) has criticized Sandfort's research and conclusions, on the grounds that both the psychometrics of the clinical screening scales he used and his reasoning are problematic. Sandfort's research is based on utilitarian ethics: One has to

show harm to render a negative moral evaluation. Sandfort seems oblivious to duty-based ethics or voluntariness-based ethics. O'Donohue has argued that (1) Sandfort's methodology is insufficient to detect all possible harm; (2) children do not by definition have the cognitive capacity to enter into negotiations with adults regarding sexual contact; and (3) adults have the duty to protect and not to harm children, and sexual contact can be harmful to children in a variety of ways. Based on these three considerations, O'Donohue has argued that there is no "sexual liberation" associated with pedophilia, but rather just a problematic argument that has the potential to do much harm. Nevertheless, in 2003 Sandfort was elected president of the International Academy of Sex Research.

In conclusion, professionals in this field need to be aware of these "liberation" movements and these debates, as their arguments and evidence at first blush can seem to have some merit. There is certainly no consensus regarding such definitional issues. However, much is at stake concerning these issues, and open consideration and clarity are important.

Problems with the Current Diagnostic Criteria

O'Donohue, Regev, and Hagstrom (2000) criticized the 1994 DSM-IV diagnostic criteria for pedophilia—and, by extension, the criteria for all the paraphilias, because of their similar structure—on a number of grounds. In DSM-IV, Criterion B for all paraphilias was "The behavior, sexual urges, or fantasies cause clinically significant distress or impairment in social, occupational, or other important areas of functioning" (American Psychiatric Association, 1994, p. 523). O'Donohue and colleagues suggested that this would allow the contented pedophile who has not acted on his urges[1] to avoid the diagnoses. Although the DSM-IV-TR was supposed to make no changes in diagnostic criteria, the editors *have* changed the diagnostic criteria for pedophilia and all the other paraphilias that involve a nonconsenting person, so that acting on the urges alone currently meets Criterion B. Although this change is a significant improvement, many problems still remain:

1. The interdiagnostician reliability of all the paraphilic diagnoses is still unknown.
2. There is still much vagueness in the criteria (e.g., what is meant by "recurrent" and "intense" in Criterion A for these diagnoses?).
3. There is significant arbitrariness in the Criterion A specification that the person must experience a paraphilia for 6 months before a diagnosis is made. What is the argument or evidence that this time frame is reasonable?

MEASUREMENT ISSUES

Measurement is a fundamental process and skill in both research and clinical endeavors. The task of accurate measurement is not easily achieved in any domain, but it may be particularly difficult with regard to sexual deviance.

Sensitivity and Specificity

Scientists rely on measurement for several basic goals. First, scientists want to be able to accurately detect the presence or absence of something (e.g., does this person experience

violent fantasies?). An accurate instrument is characterized by both "sensitivity" and "specificity." Sensitivity is a quality metric that addresses the question "If *X* is present, to what extent will the measurement operation detect *X*?" Another way to look at sensitivity is that it is an index of false negatives. Specificity is the converse; it addresses the question "If *X* is *not* present, to what extent will the measurement process indicate that it is not present?" Specificity is a measure of false positives. We want our measurements to detect as accurately as possible—that is, with no false negatives and no false positives. Often, although not always, there is a tradeoff between these two attributes. When we make our measure more sensitive, we also "buy" more false positives.

Detection of phenomena related to sexual deviance may be difficult because the target may be covert (e.g., fantasies), and/or because a person may have an interest in providing distorted information (as is usually the case when a person has been arrested for a sexual offense). In our field, we need to know through careful scientific psychometric studies the specificity and sensitivity of our measures, as much can ride on false positives or false negatives. In too many cases, this information is missing; despite this important gap, such instruments are often still used.

Quantification

In addition to presence or absence, some phenomena allow for quantification. Height is not simply present in people; it can be quantified. Thus another measurement task is to accurately measure quantity. Sometimes, though, it can be difficult to discern what the underlying scale would be. Sex drive seems to be not simply present or absent; it seems to have magnitude. But what scale is to be used? This is a complex question (e.g., would men and women use the same scale, or are their sex drives so different in some basic way as to require separate scales?). Quantification is important because many of the questions we are interested in depend on it. Clinically, we are often interested in reducing (or eliminating) some phenomena and thus are interested in quantity.

Evidence-Based Assessment

It is axiomatic in psychometrics that all measures contain error. The keys are to try to estimate or understand the size of the error term, and to consider this in all inferences and decisions based on the assessment. These are among the aims of a recent movement called "evidence-based assessment." Hunsley and Mash (2005) define this movement as follows:

> First, research findings and scientifically viable theories on both psychopathology and normal human development should be used to guide the selection of constructs to be assessed and the assessment process.
>
> Second, as much as possible, psychometrically strong measures should be used to assess the constructs targeted in the assessment. Specifically, these measures should have replicated evidence of reliability, validity, and, ideally, clinical utility. Given the range of purposes for which assessment instruments can be used (e.g., screening, diagnosis, treatment monitoring) and the fact that psychometric evidence is always conditional (based on sample characteristics and assessment purpose), supporting psychometric evidence must be available for each purpose for which an instrument or assessment strategy is used. Psychometrically strong measures must also possess appropriate norms for norm-referenced interpretation and/or replicated supporting evidence for the accuracy (i.e.,

> sensitivity, specificity, predictive power, etc) of cut-scores for criterion-referenced interpretation.
>
> Third, although at present little evidence bears on the issue, it is critical that the entire process of assessment (i.e., selection, use and interpretation of an instrument, and integration of multiple sources of assessment data) be empirically evaluated. In other words, a critical distinction must be made between evidence-based assessment methods and tools, on the one hand, and evidence-based assessment processes, on the other. (p. 251)

The question then arises: How do these general principles apply to measurement instruments used for the paraphilias? The chapters that follow can help in the first task: identifying key constructs to be measured. However, the reader will see that there are no well-corroborated accounts of the psychopathology involved in the paraphilias. In addition, too little is known about "normal" sexuality and sexual development. Thus, again, there are gaps in what constructs ought to be measured. The reader will also see key gaps in the second defining characteristic of evidence-based assessment: Much psychometric information regarding accuracy is missing for our existing measures, and in particular the clinical utility and incremental validity of measures are missing. Finally, there are few guidelines or studies showing how multiple pieces of assessment data ought to be integrated and interpreted. Much work needs to be done regarding this, but we strongly recommend that the field embrace the value of moving toward evidence-based assessment and begin to do so.

A Diagnostic Example of Measurement Difficulties

The diagnostic criteria for exhibitionism are as follows (American Psychiatric Association, 2000, p. 569):

> A. Over a period of at least 6 months, recurrent, intense sexually arousing fantasies, sexual urges, or behaviors involving the exposure of one's genitals to an unsuspecting stranger.
> B. The person has acted on these sexual urges, or the sexual urges or fantasies cause marked distress or interpersonal difficulty.

Note all the constructs that must be measured with accuracy just to make this diagnosis:

1. Incidence during a 6-month period.
2. Recurrence (of "sexually arousing fantasies, sexual urges, or behaviors . . . ").
3. Intensity (of "sexually arousing fantasies, sexual urges, or behaviors . . . ").
4. Fantasies.
5. Sexual urges.
6. Sexual behaviors.
7. Exposure of one's genitals.
8. Unsuspectingness of the stranger.
9. "Stranger" status.
10. Whether the person has acted on these urges.
11. Marked distress (including causal nexus).
12. Interpersonal difficulty (including causal nexus).

The reader is referred to the relevant chapter in this volume (see Morin & Levenson, Chapter 5), but one can already perceive that even the diagnostic task (let alone the as-

sessment tasks involved with treatment or comorbidity) is difficult, given the present psychometric status of our assessment instruments.

TREATMENT STATUS

Continuing Problems

We would very much like to be optimistic about the future of diagnosing and treating sexual deviance. As a basis for some such optimism, we believe that this volume is in many ways a considerable improvement over the first edition. However, many of the deficiencies we noted in 1997 remain. As editors of a popular book that will be widely read and frequently consulted, it is our duty to point some of these out.

If we examine the results of treatment outcome research (such as it is), we find that there are three outstanding problem areas. First and foremost, there are what we term "orphaned treatment problems." These are problems in outcome that result from the fact that there is simply not enough information to make a judgment about what does and does not work. Outcome research in this field can be very tough to conduct. Obtaining an adequate sample size can be difficult, as can training therapists and ensuring treatment fidelity. Following up patients for several years (5 is good) also contrives to make this quite difficult. But, most of all, many paraphilias are orphaned because there is little federal or private money to conduct this research. At times there is a conflict between the apparent societal importance of overcoming these problems, and the actual amount of funding available to accomplish this goal. It might be useful for an influential organization such as the Association for the Treatment of Sexual Abusers (ATSA) to publish total dollars available for this type of research and where it is going. Advocacy, in which we clearly inform decision makers about this issue is critical. If this is not done, and done effectively, then 10 years from now when the third edition of this volume comes out, we will find the same orphans.

Second, there are far from enough positive findings regarding treatment outcome that are encouraging. The most optimistic of these findings come from meta-analytic studies (e.g., Hanson & Harris, 2001; Hanson et al., 2002; Lösel & Schmucker, 2005), although specific information on various treatment interventions is available (e.g., Marshall, Anderson, & Fernandez, 1999; Marshall, Fernandez, Marshall, & Serran, 2006; Ward et al., 2004; Ward, Yates, & Long, 2006). However, much information is still missing. Paul (1967) posed the great question: "What treatment, by whom, is most effective for this individual, with that specific problem, under which set of circumstances, and how does it come about?" We can identify several gaps in our knowledge base:

1. What treatments have sufficient evidence even to be viable candidates for "evidence-based practice"? (This admittedly presupposes the question of what standards of evidence ought to be employed.)
2. What therapist variables are critical? Is a graduate degree necessary? Is competence in some circumscribed skill set required? Are the "nonspecifics" important, and if so, how important? Does therapist competence "drift" or degrade over time? What do we know about efficacy trials versus effectiveness trials?
3. What about individual differences and treatment prescriptiveness? Is comorbidity a particular problem? What do good case formulation and treatment planning re-

quire when there are other problems present (e.g., substance abuse, depression, anger, attention-deficit/hyperactivity disorder [ADHD], social skill deficits)?
4. What other circumstances are relevant (legal status, social support, prison vs. outpatient treatment, etc.)?
5. What exactly are the key active ingredients in our treatment successes? We are still at the stage of too little outcome research, and we have virtually no process research.

In addition, there are important pragmatic questions. First, what is the financial case for treatment? Can we show that it is the most economical alternative? How much exactly does successful treatment cost? We want someone (usually a third party such as the government) to pay the bill, but we have been none too clear about what the bill is, or what value the payer receives. Finally, there is an important question of access: How many competent treatment programs are there now, and how can we disseminate these so more (or all) sex offenders have access to competent treatment? This is a very tough problem.

Third, there are also some very important negative findings about treatment outcome (e.g., Berliner, 2002; Marques, Nelson, Wiederanders, & Day, 2002; Marques, Wiederanders, Day, Nelson, & van Ommeren, 2005; Schweitzer & Dwyer, 2003), and these have had a major impact upon the field. The long-term implications of these findings have, at this writing, been largely ignored or too quickly explained away. Science must weigh negative findings with positive findings (see, e.g., Karl Popper's work on falsification; Popper, 1959). Science is, in essence, an attempt to mitigate our confirmation biases. When well-controlled negative research such as that of Marques and colleagues (2005) is published, it must be scrutinized honestly. This very elegantly designed research gives rise to this question: Is relapse prevention still to be considered an empirically supported treatment?

Examination of the clinical chapters in this volume subtitled "Psychopathology and Theory" shows that the etiology of most paraphilias remains informative but largely speculative. Examination of the chapters subtitled "Assessment and Treatment" tells the tale of where we stand today in these areas. Anyone who has worked in this business for a number of years knows that most clinical attention in sexual deviance treatment is devoted to the three (supposedly) major forms of sexual deviance: pedophilia, rape, and exhibitionism. The remaining paraphilias that we have chosen to include in this volume are the ones that we term "orphans": fetishism, frotteurism, sadism, masochism (considered by some not to be a paraphilia, as noted earlier; see Baumeister & Butler, 1997), transvestism, voyeurism, and the catch-all category of paraphilia not otherwise specified. This was essentially the state of affairs in 1997 when the first edition of this volume appeared, and not much has changed in the ensuing years.

How Should Outcome Research Be Guided?

The problems described above can be solved, although not easily, and probably not in the very near term. If we were to attack the problem of not knowing enough about how to treat either the major or minor paraphilias across the board, we would have to resort to a structure such as the Chambless and Hollon (1998) criteria for "empirically supported therapies" (ESTs). The criteria were described by their authors as "a scheme . . . for determining when a psychological treatment for a specific problem or disorder may be considered to be established in efficacy or to be possibly efficacious" (p. 7). Treatments so es-

tablished are referred to as ESTs and are defined by Chambless and Hollon (1998, p. 7) as "clearly specified psychological treatments shown to be efficacious in controlled research with a delineated population." These criteria were designed for application to more common clinical syndromes (e.g., anxiety, depression, phobia) than sexual deviance, and might have to be slightly modified to be made applicable to the paraphilias. Nevertheless, it is worth summarizing these criteria at some length.[2] The Chambless and Hollon criteria consider the evaluation of treatment outcome research in terms of two major characteristics: (1) "treatment efficacy" (can the treatment be shown to work in controlled research?) and (2) "treatment effectiveness" (does it work in actual clinical practice?).

Efficacy

1. *Overall research design*. Treatment efficacy must be demonstrated in controlled research where we can conclude that the benefits observed are due to the treatment and not to chance or confounding factors. This is best accomplished in randomized clinical trials or carefully controlled single-case design experiments. Replication is critical, particularly by an independent research team. If the treatment is found to be efficacious in at least two studies by independent research terms its efficacy is said to be established. If efficacy is supported by only one study, the findings are considered to be promising.
2. *Comparisons with no treatment*. A treatment is compared with a type of minimal or no-treatment condition (waiting list or assessment only control). If two or more studies conducted by independent research teams show that the treatment is more beneficial than no treatment (and these findings are not contradicted by others) the treatment is considered to be efficacious. Evidence of specificity of effect is typically not required.
3. *Comparisons with other treatments or placebo*. Treatments found to be superior to conditions that control for nonspecific processes (e.g., attention) or to another treatment are said to be efficacious and specific in their mechanism of action.
4. *Combination treatments*. A typical study of combination treatments might involve comparison of a multiple component treatment and one or more of its individual parts.
5. *Sample description*. If outcome research is to be informative, it is essential that researchers clearly define the population for which the treatment was designed and tested. The question here is not just whether the treatment is efficacious, but whether it is efficacious for a specific problem or population.
6. *Outcome assessment selection of instruments*. These tools must evaluate significant dimensions of the problem. They must have demonstrated validity and reliability in previous research. Reliance solely on self-report should be avoided. Some indices lack psychometric properties but have high face validity (e.g., number of arrests, days in jail). It would be desirable if instruments could go beyond merely the effects of treatment and examine more general measures of functioning and quality of life.
7. *Follow-up*. Information on the long-term effects of treatment is rarely found. It is important to know if different treatments differ with respect to their stability over time. It is not clear how long follow-ups should be maintained.
8. *Clinical significance*. It is not enough that treatment effects be shown to be statistically significant. They must be large enough to be clinically useful and meaningful.
9. *Treatment implementation/Treatment manuals*. Outcome research is not informative if clinicians do not know what treatment was tested. Undefined treatment interventions cannot be replicated. At minimum, manuals should clearly and explicitly

describe the kinds of techniques and strategies that constitute the intervention. These may be highly detailed or provide general but clear guidelines.

10. *Therapist training and monitoring*. A particular treatment may fail to impress not because it lacked efficacy but because it was poorly implemented as a result of inadequate training or supervision. When there are specific interventions to learn, training matters. Measurement of the *quality* of implementation is not an area that is well understood.
11. *Investigator allegiance*. Any given therapy will tend to do better in comparison with other interventions when it is conducted by people who are expert in its use. For comparison purposes, allegiances cannot be eliminated but they can be balanced.
12. *Single-case experiments*. The principles listed above all apply to single-case designs. However, there are some special issues applying only to these approaches.
    a. *Establishing a stable baseline*. The baseline serves as the comparison condition that controls for the effects of assessment and the passage of time and that, depending on the nature of the baseline (no intervention vs. control intervention), may control for expectancy, attention, and the like.
    b. *Acceptable designs*. There are two.
        i. *ABAB design*. A is the baseline, B is the treatment.
        ii. *Multiple baseline*. Variations include multiple baselines across behaviors, settings, and participants.
    c. *Efficacy in single-case experiments*. Treatment is considered to be efficacious if it has proved beneficial to at least three participants in research by a single group. Multiple replications (a least three each) by two or more independent research groups would be needed to consider the efficacy of the treatment to be established.
    d. *Interpretation of results*. Data are usually interpreted visually because effects are often so striking that they are readily convincing to the naked eye.
    e. *Conflicting results*. If a group of well-designed studies point to one conclusion and poorly designed studies point to another, the former will be chosen. In a group of studies with roughly comparable methodological rigor, the question is whether the preponderance of the studies point to efficacy. Meta-analyses are useful but they can obscure qualitative differences. Therefore it is necessary to know something about the standards for the quality of studies included in the analysis.

Effectiveness

1. *Generalizability across populations*. Can the results of randomized controlled trials really be trusted? It is widely believed that participants in stringently controlled outcome research are less complex and easier to treat because they have been selected to have specific characteristics and do not resemble the ordinary client seen in typical clinical settings.
2. *Generalizability across therapists and settings*. Therapists working in controlled trials may have received a level of training and supervision not typically available to the average practitioner. Another problem is that the act of controlling treatment alters its nature and threatens the practical utility of the obtained results. Those studies that, as closely as possible, reproduce the conditions found in actual clinical practice are most likely to produce findings that generalize to those settings. Efficacy is one thing; whether it works in a naturalistic setting is another.
3. *Patient acceptance and compliance*. When treatment options are offered, clients may be expected to choose the easier, less demanding option rather than the more effec-

tive but arduous intervention. Many clients do not benefit from treatment because they are unable or unwilling to stick to the required regimen.
4. *Ease of dissemination*. Treatments that are straightforward and easy to learn are more likely to be disseminated to the larger practice community.
5. *Cost effectiveness*. Those treatments that cost the least are likely to be preferred if there is no great difference in outcome.

We have presented the Chambless and Hollon (1998) criteria in some detail for a very good reason: The status of treatment outcome research in the field of sexual deviance, with very few exceptions, is quite discouraging. Almost none of the studies published would begin to meet these quite reasonable criteria.

Positive Outcome Research Findings

There are unquestionably numerous reports in the literature that show positive effects for the psychological treatment of sex offenders—far too many to attempt to summarize here. What can be said about them is that they typically are not large-scale investigations and rarely are able to show that a specific intervention is effective with a specific population.

For decades, reports have been appearing piecemeal that describe the success (as well as the indifferent performance) of sex offender treatment programs. These typically take the form of comprehensive reviews (e.g., Craig, Browne, & Stringer, 2003; Furby, Weinrott, & Blackshaw, 1989), reports on single programs (Marshall et al., 1999; McGrath, Cumming, Livingston, & Hoke, 2003; Pithers, Martin, & Cumming, 1989), or reports on multisite programs (Friendship, Mann, & Beech, 2003). Craig and colleagues (2003) make two essential points regarding evaluation of sex offender treatment:

1. Methodological differences in treatment and recidivism research make it difficult to assess treatment efficacy, ultimately affecting predictive accuracy of recidivism.
2. There is a small but increasing number of treatment and meta-analytic studies using robust methodologies that demonstrate positive treatment effects.

Craig and colleagues are referring to a meta-analysis performed by Hanson and colleagues (2000; Hanson & Harris, 2001). The Hanson and colleagues analysis evaluated a combined sample of rapists and pedophiles (N = 9,454). Average follow-up time was 4–5 years. According to Hanson and colleagues (2002), treatments provided prior to 1980 were not shown to be effective. Current treatments were associated with a significant reduction in sexual recidivism (from 17% to 20%). Cognitive-behavioral treatments (undefined) proved to be the most effective with adult offenders. Programs delivered in institutions were as effective as community programs. Hanson (2002) listed the policy implications of this meta-analysis:

1. Treatment programs contribute to public safety by reducing the risk of sexual recidivism.
2. Not all treatment programs are effective. Before deciding that an offender's risk has been reduced by treatment, evaluators need to consider the nature and quality of the treatment provided.
3. Training and supervision efforts should focus on those treatments that have the strongest evidence for effectiveness (i.e., cognitive-behavioral).

4. No treatment program can guarantee a complete cessation of offending. They are simply one element in a comprehensive risk management strategy.

More recently, Lösel and Schmucker (2005) examined 69 studies ($N > 22{,}000$). They used a broad definition of "recidivism," ranging from incarceration to lapses in behavior. Their analysis revealed a 11% recidivism rate for treated offenders and a 17.5% rate in comparison groups. They concluded that treatment had a substantial effect upon sexual recidivism. As in the earlier Hanson and colleagues (2002) meta-analysis, Lösel and Schmucker found that cognitive-behavioral interventions produced the greatest impact.

Negative Outcome Research Findings

Despite the positive findings noted above, many researchers are quite pessimistic regarding the efficacy of sexual offender treatment. They argue that there is simply no good evidence that treatment of any sort is effective with sex offenders, and in particular that the effectiveness of psychological treatment for sex offenders remains to be demonstrated (see, e.g., Camilleri & Quinsey, Chapter 11, this volume; Rice & Harris, 2003).

There is certainly evidence that might discourage the most optimistic among clinicians and researchers.[3] Perhaps the outstanding example is the Sex Offender Treatment and Evaluation Project (SOTEP) (Marques, Day, Nelson, & Miner, 1989; Marques, Nelson, Alarcon, & Day, 2000), funded by the state of California from 1985 to 1995. This was thought by many to be the gold standard of sex offender treatment. Using a relapse prevention model (see Laws, 1989, 2003), this was a major attempt at empirical validation. SOTEP followed treatment completers, dropouts, and nonparticipants for nearly a decade after the program terminated. By 1999 a treatment effect had not yet emerged. Marques and colleagues (2000, 2002) continued to report this fact; they termed it "unanticipated." By 2005 it was all over. Marques and colleagues (2005), in a final report, stated that no significant differences were found among the three groups in their rates of sexual or violent offending over an 8-year follow-up period. The researchers found this null result for both rapists and child molesters, using time to reoffense as the outcome and controlling for static risk differences across the groups. The best face that could be put on these disappointing results was that those offenders who had closely adhered to the program's relapse prevention model had lower reoffense rates than those who did not.

Over the past 20 years, SOTEP has been imprinted upon the literature. Rice and Harris (2003, p. 443) stated that SOTEP was "the most well-designed and executed study the sex offender field has ever seen or is likely to see for some time." Berliner (2002) wrote the epitaph for SOTEP and perhaps for the classical model of relapse prevention as well:

> It should not be overlooked that the most well designed and executed study, the California . . . study that used random assignment, that included volunteers and nonvolunteers, that implemented a state of the art intervention program, that had a follow-up treatment component, and that calculated recidivism in the most comprehensive way found no significant effects for treatment. (pp. 196–197)

Given these appalling results, it is difficult not to conclude that something is very wrong here. The most stringent experimental design—the vaunted randomized controlled group

comparison that is repeatedly recommended—proved unable to demonstrate a treatment effect over a period now exceeding 10 years.

This is not a one-off, unexplainable event. In 2003 Schweitzer and Dwyer reported a very similar outcome evaluation in Australia. The design was very like that of SOTEP; it contained the typical cognitive-behavioral components, including relapse prevention. The Australian study examined outcome for three groups: completers, dropouts, and nonparticipants. As in SOTEP, no differences in recidivism were found over an average at-risk period of 5 years.

When it became apparent that SOTEP was going to fail, one of us (D. Richard Laws) contacted R. Karl Hanson and simply asked him whether he really believed that sex offender treatment was effective. He replied in so many words that there was evidence that treatment was effective, but that the evidence was not strong. Laws contacted Hanson again in late 2006 and asked the same question. Hanson replied (personal communication, October 10, 2006):

> My reading of the literature is that certain forms of psychological intervention can be effective in reducing the sexual and general recidivism rate of sexual offenders. The evidence is stronger now than it was five years ago but it is still not strong. Reasonable people can read the literature and come to different conclusions. I think they are mistaken, but I don't think they are irrational.

And, of course, another question can be asked: What is the magnitude of the effect? We want the effects of therapies not just to be statistically significant (i.e., reliably different from those of some comparison treatment), but to exhibit a large magnitude of difference. This issue has been dealt with mainly in the terms of the harm reduction model. Although it is true that if the offense rate at baseline is 10 per year and if at some subsequent point of time it is 8 per year, harm has been reduced, most would also agree that the intervention is relatively weak and could be substantially improved. O'Donohue and Fisher (2006) have suggested that treatments should have benchmarks (what is the standard amount of improvement?), and that transparent quality improvement processes should be used consistently in efforts to meet or exceed these benchmarks.

In explaining away negative or unpromising results, we often hear senior clinicians state, "This is a young field." This is absolute nonsense. Marshall and Laws (2003) and Marshall and colleagues (1999) have noted that Eysenck's (1952) evaluation of traditional psychotherapy was the event that set off the behavior therapy revolution, first in the United Kingdom and later in the United States. These procedures eventually filtered into sex offender treatment and formed the initial foundation for many of the treatments used today. We now have a 50-year history of such treatments, and it is entirely reasonable to ask: What have we got to show for it? The answer, sadly, is very little. If other sciences had proceeded as glacially as ours, the 21st century would indeed be in a bad way. Saying that ours is a soft, social science is no excuse. We *can* do better, and we must. The small amount of progress in the years since the first edition of this book was published appeared points to the fact that we are not doing better. Again, we ought to look at a macro-organizational level and plan strategies (as opposed to letting this remain a *laissez-faire* process) so that in the coming years more progress will be made. Closing the books and abandoning sex offender treatment do not constitute a likely option; nor should it be based on the negative outcome of two major studies, even given their eerily similar out-

comes. What *is* an option is starting to look for other ways to perform these interventions, or perhaps looking at options other than treatment.

New Interventions

The Self-Regulation Model

The Ward and Hudson self-regulation model is the obvious candidate to replace relapse prevention (Laws, 2003; Laws, Hudson, & Ward, 2000; Ward & Hudson, 2000; Ward et al., 2004, 2006). This model (admittedly a work in progress) postulates four possible pathways to relapse and interventions for responding to each. It has now been formalized in two manuals (Ward et al., 2004, 2006) and comes very close to meeting the Chambless and Hollon (1998) criteria. It has been adopted as a major feature of the nationwide sex offender treatment program operated by Correctional Service Canada (P. M. Yates, personal communication, March 17, 2003; see also Yates et al., 2000).

The Good-Lives Model

The good-lives model is gradually being incorporated into the self-regulation model. In the Chambless and Hollon (1998) criteria is the statement that beyond the assessment of changes in symptoms, outcome evaluations should ideally focus on more general issues of human functioning and quality of life. Ward and Stewart (2003) argue that beyond attempts to control recidivism, sex offender treatment must also focus on positive enhancement of an offender's life—the pursuit of what they call "primary human goods" (states of affairs, states of mind, personal characteristics, activities or experiences sought for their own sake), which are likely to promote psychological well-being. These are the things that most of us take for granted and consider our due. It is our belief (and hope) that the good-lives model is going to achieve a paramount place in treatment.

Desistance

The desistance literature comes to us from sociology, criminology, and various writings on probation-related subjects (Farrall & Calverley, 2006; Laub & Sampson, 2001; Maruna, 2001; Maruna & Immarigeon, 2004). Although interest in it is developing, desistance has not yet found a place in the management of sex offenders.

Numerous sociological/criminological theories have been advanced to account for desistance from crime. Many of these theories agree that a series of rather ordinary human events—what Ward and Stewart (2003) would call the pursuit of primary human goods—can often account for desistance. These include getting married; having children; renewing bonds with close family and friends; obtaining stable and rewarding employment; redefining oneself as a worthy, noncriminal person; feeling in control of one's life; or just getting tired of a criminal career. Maruna (2001) argues that to really understand offenders, we must examine their life course narratives and must listen to the stories that they tell, because in this process lies the capacity to transform the self and desist from crime. Lest this sound overly simplistic, Maruna describes the transformative process as a rough one. A more familiar perspective would be to see it as the process of preventing relapse originally proposed by Marlatt and Gordon (1985): a process of starts and stops, replete with relapses and recoveries, until the final desired state is achieved.

No one at present knows the extent to which this model applies to sex offenders. Surely there are offenders who, over time, gradually achieve a state of desistance. And surely there are those who do not. This is entirely consistent with the criminological description of persisters and desisters. Although this approach is certainly not antithetical to treatment, it does not make treatment a centerpiece, but rather one of the building blocks. It is tantalizing to speculate that the treatment effect about which we hear so much may serve not so much to instruct people on the errors of their ways as to awaken new possibilities for growth. R. E. Mann (personal communication, June 2006) has speculated that a good place to begin, and an approach that might prove sufficient in itself, would be the use of motivational interviewing (Ginsburg, Mann, Rotgers, & Weekes, 2002; Miller & Rollnick, 2002). It is certainly striking how much Maruna's findings agree with the propositions of the good-lives model of rehabilitation (Ward & Stewart, 2003), described above. Since the good-lives approach is already being incorporated into the Ward and colleagues (2006) self-regulation model, perhaps this is the initial path to take in dealing with desistance from sexual crime.

Information on Best Practices

Finally, the field also needs to be concerned with the issue of dissemination of best practices. Many have realized that simply discovering that a treatment is efficacious is not sufficient to ensure that it is widely adopted and competently practiced. The Substance Abuse and Mental Health Services Administration, and the Agency for Healthcare Research and Quality, are two U.S. government organizations that attempt to sponsor research and provide support for those struggling with this problem. We know that there are disparities not only around the world but within a single country in health care quality (e.g., Elko, Nevada, has fewer good-quality alternatives than does San Francisco, California). The question for our field is not only what best practices are, but how these best practices can be disseminated across all treatment settings. This is a complex problem. The problem involves agreeing on what best practices are, finding the right managers and clinicians, training them, maintaining their competencies, keeping up with innovations, and supporting managers and clinicians in a variety of ways. There are certainly centers of excellence in our field, but the question of how the other treatment facilities can be brought up to these standards remains. Thus dissemination research and a dissemination agenda also need to be part of the focus of this field.

FORENSIC ISSUES

The use of risk assessment instruments has proliferated enormously in the past decade. These are typically of two varieties. Actuarial risk assessments use complex algorithms based on static risk factors to make rather precise predictions of reoffense at specific time intervals (usually 5, 10, or 15 years). These are exemplified by the Static-99 (Hanson & Thornton, 1999) and by the Violence Risk Appraisal Guide (VRAG) and Sex Offender Risk Appraisal Guide (SORAG) (Quinsey, Rice, Harris, & Cormier, 1998). Because of their apparent predictive power, these instruments have become extremely popular and are widely used in forensic evaluations (see Doren, 2002). Although we acknowledge their scientific basis and obvious statistical power, the problem with these instruments is that they tell one very little. Clinicians are not very interested in an unspecified event that

might or might not happen in 5 or 10 years. They want to know what might happen tomorrow, next week, or 3–6 months from now. Actuarial instruments are unable to provide this information.

The second type of risk assessment instrument is a structured professional guidelines assessment, exemplified by the Sexual Violence Risk–20 (SVR-20; Boer, Hart, Kropp, & Webster, 1997) and the Risk for Sexual Violence Protocol (RSVP; Hart et al., 2003). These instruments evaluate static as well as dynamic risk factors. Detailed descriptors instruct the user on how to rate the items. As a summary, there is no total score or probability statement, but rather an evaluation of whether the individual presents a low, moderate, or high risk of reoffending. The most recent of these instruments, the RSVP (Hart et al., 2003), provides considerable information beyond the simple evaluation of low, medium, or high risk. Possible scenarios for reoffense are constructed, and evaluations of how to deal with any potential threat(s) are described in detail. This approach (explained more fully by Hart & Kropp, Chapter 29, this volume) most closely meets the need for immediate information and suggestions that clinicians require.

PREVENTION ISSUES

Virtually all of this volume is devoted to traditional ways of thinking about the psychopathology, assessment, and treatment of sexual deviance. We have added a final chapter (Chapter 32) that describes how we might approach sexual deviance through a public health model of prevention. Briefly, there are three levels of prevention in public health: (1) "primary," preventing an undesired event from ever occurring; (2) "secondary," quickly intervening in a problem when it shows its early stages; and (3) "tertiary," containing a well-established problem and keeping it from happening again. Chapter 32 argues that most of our efforts have been devoted to the secondary and tertiary levels of prevention, because this is where we find most of the clients with whom we work. The chapter presents a detailed example of a community prevention program that gives strong emphasis to the primary level but actually works at all three. Public health approaches have been shown to work successfully against smallpox, tuberculosis, and polio, but less successfully against drunk driving or smoking. It has not worked against sexual deviance because it has not been tried.

STRUCTURE OF THIS BOOK

The first edition of this book (Laws & O'Donohue, 1997) proved successful beyond our expectations. As nearly as we can tell, it is considered a solid piece of work that is widely consulted and cited. When we were approached by the publisher to compile a second edition, we were at first reluctant to undertake the task. Both of us have long experience in the field, and we are painfully aware of how slowly things change in the sexual deviance business. However, new areas of study have emerged in the past decade, and we decided to prepare the work.

Readers will notice similarities and differences between the two editions. We have retained the structure we used previously for dealing with the major clinical syndromes (present Chapters 4–23). Each is again addressed in two chapters: one dealing with psychopathology and theory, and one dealing with assessment and treatment. To avoid

overlap between the two chapters in each pair, we have provided authors with detailed guidelines for structuring their chapters.

Much in the present volume is new. We have added a chapter on the etiology of sexual deviance (Chapter 2). Since there is a forensic concern regarding the decrease of risk with age, we have included a chapter on sexual deviance across the lifespan (Chapter 3). New areas of interest include online sexual offending (Chapters 24 and 25), sexual deviance in females (Chapters 26 and 27), multiple paraphilias (Chapter 28), legal issues with sexual offenders (Chapter 29), and the public health approach (Chapter 32).

We are pleased with the new structure and believe that it substantially broadens the reach of the book. It is our hope that we have preserved what was best in the first edition and extended that information. The inclusion of the new material should increase interest in this field. Overall, we feel that this volume clearly shows that we are moving ahead in dealing with the problems presented by sexual deviance in our society.

NOTES

1. Both in the O'Donohue and colleagues (2000) paper and in most of the first edition of the present volume (except for the chapter on female sexual deviance), only masculine generic pronouns—"he," "his," and "him"—were used, because most individuals with paraphilias are males. The same will be done in this edition of this book, except for the two chapters on female sexual deviance (Chapters 26 and 27).
2. The criteria are from Chambless and Hollon (1998, pp. 7–16).
3. Some of what follows is a revision of Laws and Ward (2006).

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