Part I

Introduction to the International Field of Sexual Offender Assessment and Treatment

Chapter One

Adult Sexual Offender Treatment – Is It Effective?

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This chapter reviews the empirical evidence of the effectiveness of treatments for sexual offenders as regards the reduction of recidivism. There are many studies that claim to have demonstrated respective effects but lack minimal requirements of scientific proof. This is true for all studies reporting antihormonal or surgical treatments, whereas the effectiveness for cognitive behavioral treatments on the first glance seems to be well established. However, even for this kind of intervention there is much debate about its effectiveness. To improve the results of treatment, the risk, needs, and responsivity principles should guide the design of treatment programs and their evaluation.

To this day there is an ongoing discussion about whether empirical proof exists for the effectiveness of sexual offender treatment in reducing relapse rates. William Marshall, IATSO's (International Association for the Treatment of Sexual Offenders) president and one of the most prominent researchers and practitioners in the area of sexual offender treatment belongs to those who hold the view that sexual offender treatment works. In his article about effect sizes in the treatment of sexual offenders (Marshall & McGuire, 2003) he argues that effect sizes for sexual offender treatment are comparable and sometimes better than those for treatment of other offenders, treatment for mental health

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problems and treatment for physical health problems. Of course, he is also aware of the contradictory empirical data base, given the fact that all of the meta-analyses he draws his conclusions from are regarded critically at least in some way, most of them for having included studies with noncomparable control groups. Nevertheless, he has always taken a more optimistic position arguing that there is indeed evidence to encourage the view that sexual offender treatment is effective (Marshall, Jones, Ward, Johnston, & Barbaree, 1991). He calls for a far more careful design of treatment studies including the minimum requirement of at least a matched control group in terms of demographics and features of the offense history. On the other hand, he argues that randomized design studies are problematic on several ethical and practical grounds. Finally, he quotes the results of the ATSA Collaborative Outcome Data Committee (CODC) (Hanson *et al.*, 2002): When only treatment programs meeting the current standards were analyzed, 10% in the treated group, but 17.4% in the untreated group recidivated sexually.

Up to now some authors have reported a positive effect of treatment in as much as that it reduces sexual offender recidivism (Hall, 1995; Hanson *et al.*, 2002; Gallagher, Wilson, Hirschfeld, Coggeshall, & MacKenzie, 1999; Loesel & Schmucker, 2005), but others did not and do not support this view (Furby, Weinrott, & Blackshaw, 1989; Rice & Harris, 2003). The most frequent critique that is brought forward is that positive treatment findings mostly derive from studies which do not meet high levels of study quality.

Hall's metaanalysis (1995), for example, was an endeavour to produce a more optimistic answer to the pessimistic overview published by Furby *et al.* (1989). Hall 1995 investigated 92 studies published since Furby's overview, of which 80 were eliminated for several reasons, mostly because they had no comparison group or they did not report recidivism data. In the remaining 12 studies he found an overall recidivism rate of .19 for treated offenders versus .27 for untreated ones. Furthermore, he found that the combination of cognitive behavioral and hormonal treatments was superior to behavioral treatments on their own. However, when analyzing all the included studies – especially in the light of reviews which were published later – it was found that another four of these studies were not included in the Loesel and Schmucker's metaanalysis (2005), since they did not meet their minimal requirements for inclusion. Of the remaining eight studies one was a study about surgical castration (Wille & Beier, 1989), and although it was included in Loesel and Schmucker's meta-analysis, the authors confirmed that there was no equivalent control group and equivalence could not even have been expected. Of the remaining seven studies four reported on the use of medroxyprogesterone acetate (MPA) (Fedoroff, Wisner-Carlson, Dean, & Berlin, 1992; Maletzky, 1991; McConaghy, Blaszczynski, & Kidson, 1988; Meyer, Cole, & Emory, 1992). They were also included in a later review (Loesel & Schmucker, 2005). However, as illustrated below, only two (Fedoroff *et al.*, 1992; Meyer *et al.*, 1992) of these four studies reported on antihormonal treatment effects. However, in these two remaining

studies no equivalence of the control group was given and therefore the claimed effectiveness might have been confounded by the lower baseline risk in the treatment group (Eher, Gnoth, Birklbauer, & Pfäfflin, 2007).

Of the remaining three studies (Hanson, Steffy, & Gauthier, 1993; Borduin, Henggeler, Blaske, & Stein, 1990; Marshall & Barbaree, 1988) only one (Borduin *et al.*, 1990) used a randomized design. Eight juvenile sexual offenders treated multisystemically were compared with a same-sized control group of juveniles with individual treatment in a random assignment. The treatment group turned out to have a sexual recidivism rate of 12.5% compared to 75% in the control group. However, since an equivalence of groups on risk variables is not even guaranteed in a random assignment, and given the fact that the sample size was very small the possibility of chance findings has to be discussed.

The Hanson *et al.* (1993) study compared 106 child molesters who were released between 1965 and 1973 at a mean age of 32.4, with controls. The treatment group had a previous sexual conviction rate of 63%, which was nearly twice as high as in both controls groups. Although the victim type was comparable to both control groups, the treatment group had a higher baseline risk. After an average follow-up period of 19 years the recidivism rate was not significantly different for the treatment group and the control groups. Controlling for several potential confound variables did not reveal any other result.

However, consistent advantages for the treated over the untreated offender group were reported in a long term evaluation of a behavioral treatment program for child molesters (Marshall & Barbaree, 1988). Although some kind of equivalence was described, no information on the equivalence of relapse relevant variables between treatment and control group was given (previous offences, victim type). Recidivism rates for the treated offenders in the group of nonfamilial child molesters were lower than those of the untreated offenders as regards both female and male victims (42.9% vs. 17.9%, 42.9% vs. 13.3%, respectively). The beneficial effects of treatment were greater the longer the follow-up period. It was concluded that the treated offenders fared better over the long-term follow-up evaluation than the untreated. They were less likely to recidivate, but if they did, they committed almost the same number of sexual offences as the untreated (in average per offender).

In general, Hall's study (1995) was criticized because of the fact that "the strongest treatment effects came from comparisons between treatment completers and dropouts" (Hanson *et al.*, 2002). Also, the apparent effectiveness of antiandrogenic interventions was attributed to just one study on surgical castration – which in turn had no equivalent control group.

All the randomized controlled trials (RCT) on sexual offender treatment were investigated by White, Bradley, Ferriter, & Hatzipetrou (2000). Only one single trial meeting the RCT criteria could be found (McConaghy *et al.*, 1988). However, in this study, the effect of antilibidinal medication (medroxyprogesterone acetate) plus imaginal desensitization turned out to be no better than imaginal desensitization alone. The authors concluded that

it would be disappointing to find that sexual offender treatment was lacking a strong evidence base, particularly in light of the controversial nature of the treatment and the high level of interest in the area.

An impressive example of the whole controversy of the debate is given by the recent withdrawal of a published metaanalysis about randomized controlled treatment (RCT) on psychological interventions with sexual offenders (Kenworthy, Adams, Bilby, Brooks-Gordon, & Fenton, 2008). It was withdrawn in June 2008 and a substantive update of the review was announced. The authors previously had included 9 randomized controlled trials (Kenworthy, Adams, Bilby, Brooks-Gordon, & Fenton, 2004) and came to the result that cognitive behavioral therapy in groups may reduce reoffense rates as measured at the follow-up after 1 year for child molesters when compared with standard methods. According to the authors the limited data did not allow for recommendations to be made, and they warned against experimental treatments for a potentially dangerous group outside a well-designed evaluative study. They announced their review would demonstrate that such studies are possible.

The Collaborative Outcome Data Project on the Effectiveness of Psychological Treatment for Sex Offenders (Hanson *et al.*, 2002) reported a sexual offense recidivism rate of 12.3% for treated and 16.8% for untreated offenders. Analyzing only those studies that met current standards, the difference was even clearer (10% vs. 17.4%). Cognitive behavioral and systemic treatment could be found to be associated with the reduction of sexual and general recidivism, whereas older forms of treatment did not work. Inclusion criteria, for this group of 43 studies in this study, were the availability of recidivism data and a control group. This study included "random assignment," "incidental assignment" and "assignment based on need" studies, as well as comparisons between treated offenders and refusers, completers and refusers, and dropouts versus refusers. In most studies the refusers were allocated to the comparison group, who were assessed and to whom treatment was offered, but which they declined.

However, this report has since been critically reviewed (Rice & Harris, 2003). First, because the results of the random assignment studies, especially for the treatment of adults, could not provide evidence of treatment effectiveness. Only one of the four random assignment studies included reported positive treatment results. However, this study comprised only 8 adolescents treated with "multisystemic therapy" (Borduin *et al.*, 1990), a treatment that would be difficult to apply to adults. The so-called "incidental assignment" studies, which – as concluded by Hanson *et al.* (2002) – would have yielded substantial positive treatment effects, were also criticized, since upon close inspection noncomparable control groups were included and results drawn by such kinds of studies would be too weak to be used to draw inferences about treatment effectiveness. Rice & Harris (2003) came to the conclusion that "in almost every case, the evidence was contaminated by the fact that comparison groups included higher-risk offenders who would have refused or quit treatment had it been offered to them" (p. 428).

The authors also found that any useful information about treatment efficacy would require that the treated group is contrasted with a comparable group receiving a different treatment or no treatment. Although the "golden standard" is the random assignment, even this would not guarantee equivalence of the groups. Nevertheless, the authors demand high quality research and argue that low-standard research in this field would be an unacceptable risk. Weak inference evaluation would simply risk doing harm and wasting resources ("briefly, unless a study measures officially recorded recidivism from at least two distinct groups of sex offenders (at least one of which received treatment), and unless the groups are, except for treatment, comparable, the study has no scientific value in evaluating the treatment"(p. 431). They recommend a random assignment and argue that the only other acceptable way to achieve comparability would be the matching of factors known to be related to risk – this matching should preferably be pairwise. Also, since for refusers and dropouts there seem to be a priori reasons to consider them to be at a higher risk, they should not be included in the control group. Moreover, the authors state that they would not permit the evaluation of treatment by comparing sex offenders who have completed treatment with a group which had not been offered it, since there would not be any information about the comparability regarding the percentage of (possible) refusers in the control group (where treatment was not offered). On the other hand, Hanson *et al.* (2002) defend the "incidental assignment" studies since there was no a priori reason to consider treatment and control groups to differ in risk, as moreover neither the offender nor the therapist determined who was to receive treatment.

One of the most recent meta-analyses (Loesel & Schmucker, 2005), however, indicates a moderate to strong treatment effect (37% less sexual recidivism than controls). However, even this study has to be looked at critically. Out of 69 studies 80 comparisons (74 reporting sexual recidivism) between control and treatment groups have been included in the metaanalysis. Since 60% of the comparisons were on Maryland Scale level 2, the equivalence of the control group could not be assumed (48 had a nonequivalent control group, for 19 equivalence was just assumed, 7 were matched, or statistically controlled, 6 were randomized, but even then equivalence was not controlled). In about 25% of the studies the control group consisted of treatment refusers, who, by definition, are at higher risk. The absolute difference in sexual recidivism between treated and controls amounted to 6.4 percentage points, which was a 37% reduction of the base rate of the controls. The various treatment approaches differed considerably. The highest treatment effect could be found for "physical" treatment, basically meaning antiandrogenic and surgical castration with an odds ratio of 7.4. Eight studies on surgical castration and 6 studies on the use of antiandrogenic medication were included. A further analysis of the studies revealed that an equivalent control group was not included in any of the castration studies. Given the problematic context of surgical castration of sexual offenders in general and for ethical, legal and medical reasons in particular data about a

possible effectiveness of this kind of management based on a quasi null empirical basis should be handled very cautiously.

When extensively reviewing and analyzing all studies on antihormonal treatment (Eher *et al.*, 2007) in the Loesel and Schmucker study one can learn that all papers reporting on the use of Cyproterone Acetate and GnRH Agonists were excluded from further analysis because they did not meet the inclusion criteria for the metaanalysis (no comparison group, no reoffence outcome data) (Schmucker, 2004). The six remaining studies reported on the use of medroxy-progesteron-acetate (MPA), a female sexual hormone, which is not registered in Europe because of its severe side effects. Four of the remaining six studies did not find any effect of MPA on sexual recidivism. Only two studies (Fedoroff *et al.*, 1992; Meyer, Molett, Richards, Arnold, & Latham, 2003) ended up with the statement of effectiveness in terms of sexual offender recidivism reduction. However, these two studies also failed to have an equivalent control group, and a further analysis suggested that the effects might have been confounded by a higher risk in the control groups (Eher *et al.*, 2007).

A positive effect is reported for cognitive-behavioral and classic behavioral approaches. However, since only one-third of the studies fulfilled the minimum requirements of an equivalent control group, and moreover the control groups tended to have a higher risk for reoffending, even these results are still questionable. Also, on analyzing only the randomized studies, a positive effect of the treatment groups could not statistically be proved anymore (Schmucker, 2004).

In conclusion, these results are not overwhelming in respect of the effectiveness of the programs evaluated and their power to reduce sexual recidivism. They rather call for more professional study assignments to be carried out.

Marshall, Marshall, Fernandez, Malcolm, & Moulden (2008) demand a detailed analysis of why a program fails. They list several reasons why programs may fail: the skills and the style of the therapists, the climate of the group, the range and appropriateness of the treatment targets, the duration and the intensity of the treatment, and the adherence of the program to the principles of effective offender treatment. An additional point would be the client's readiness and responsiveness. They describe three "risk populations," the treatment refusers, those who drop out of the program (or are removed by the therapist), and those who remain but do not achieve treatment goals. All three groups are at higher risk than comparable control groups (Marques, Wiederanders, Day, Nelson, & van Ommeren, 2005). To help them to overcome resistance to treatment and to become more ready for effective engagement in treatment Marshall *et al.* (2008) suggest implementing a "preparatory program" in order to enhance treatment motivation. They describe a cognitive behavioral approach with motivational interviewing and treatment strategies derived from several conceptual frameworks, such as positive psychology, hope theory, self-efficacy theory and the good-lives model. The analysis of the effects of this pretreatment program showed that self-efficacy improved as well as the state and the trait hope. The participants also more fully recognized the need for

change and accepted the need for action. When analyzing the outcome of the treatment program carried out in combination with pretreatment those who had completed both programs were at significantly lower risk for sexual and violent reoffense. The authors and treatment providers conclude that after the pretreatment phase the offenders were more hopeful about the future and had more self-efficacy. The participants also were more likely to be moved to lower security and to be deemed to require less intensive treatment, and also to get earlier parole. They also argue that "the early attention to treatment readiness appears to facilitate the internalization of the materials in their resultant comprehensive treatment programs, resulting in better, longer term success" (p. 39).

Since the Marshall *et al.* study (2008) gives a unique example of how readiness for treatment can be enhanced and how this – quite easily achieved – enhanced readiness can lead to a better outcome in a subsequent treatment process, it is also a good example for the validity of the risk, needs, responsivity principles (RNR) of effective correctional treatment (Bonta & Andrews, 2008). Address-ing the readiness for treatment seems to be crucial for any further positive effect. However, improving this readiness is not only a matter of just implementing a pretreatment group but is crucially dependent on the offenders' general psychological functioning and the presence or absence of other disorders. Severe mental illnesses, an increased risk of psychiatric hospitalization (Fazel, Sjostedt, Langstrom, & Grann, 2007), high rates of lifetime disorders like substance abuse, paraphilias, mood disorders and personality disorders (McElroy *et al.*, 1999; Harsch, Bergk, Steinert, Keller, & Jockusch, 2006; Raymond, Coleman, Ohlerking, Christenson, & Miner, 1999) are found in sexual offenders. These findings make it crucial to consider in advance which offender is ready for which treatment program. Offenders meeting criteria of severe paraphilias and psychopathic personality, for example, would understandably need different (pre)treatment settings than neurotic offenders (Olver, Wong, & Nicholaichuk, 2009). Also, one would expect the rate of refusers, dropouts, and nonachievers to be higher in mentally ill offenders due to the defense mechanisms inherent in these disorders (Kernberg, 2006).

Applying the RNR principles therefore, is not only sensible, but also has already shown its effectiveness in successful sexual offender treatment (Hanson, Bourgon, Helmus, & Hodgson, 2009). When analyzing only studies which meet the CODC guidelines of minimum level of study quality (Collaborative Outcome Data Committee, 2007a, 2007b) it is found that the relapse rates for sexual recidivism in those studies in which the treatment programs adhered to the RNR principles were significantly lower than those of the untreated offenders (10.9% vs. 1.92%). This is also true for any kind of recidivism (31.8% vs. 48%). As an aside: None of the surgery or the drug studies was included since none of them met the minimum level of study quality.

There is, however, in spite of the controversial debate, evidence that treatment of sexual offender is effective, as long as the program adheres to the principles of RNR. Although much is still unclear about the criminogenic needs, very clearly

identified dynamic risk factors still exist which should be targeted in treatment programs (Harkins & Beech, 2007). This would make it necessary to carefully plan the treatment program emphasizing targets that are known to be empirically linked to sexual recidivism. If programs are run and designed that carefully, there is good reason to assume that in future more data about high quality studies will be available showing that sexual offender treatment is effective.

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