

Outcome Evaluations of Offender Programs in Sweden

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ABSTRACT

The Research and Evaluation unit of Swedish Prison and Probation Service published its first outcome evaluation of a program in 2008. To this date, reports on eleven evaluations have been published in Swedish. In this article, we will describe how these evaluations are conducted and the context in which they take place. The primary aim of the studies is to investigate whether offender programs seem to reduce criminal recidivism. This is achieved by comparing participants in a specific program with clients who have not participated in the program, but who otherwise may be considered equal based on baseline characteristics handled in the study.

INTRODUCTION

Like several other West European countries, Sweden has a long history of probation, with early precursors beginning in the 19th century in the form of volunteer organizations helping prisoners prepare for release. Probation has been a state matter in the country since the early 20th century (Svensson, 2010). Unlike in many other countries, the prison and probation system belong to the same government agency. The Swedish Prison and Probation Service has been a single national authority since 2006, but used to consist of many local government authorities (Swedish Ministry of Justice, 2005). On an average day 13,000 clients are on probation in one of 34 probation offices, where about 1,200 employees work. There are also around 7,500 volunteers who work with probation clients. This may be compared with 46 prisons and 31 remand centres, where there are 4,000 prison inmates and 1,500 pre-trial detainees on an average day, and around 5,000 corrections officers work (Swedish Prison and Probation Service, 2014).

Most treatment programs in Swedish Prison and Probation Service are based on cognitive behavioral therapy (CBT) and many originate from the U.K. (e.g. One-to-One) or Canada (e.g. Moderate Intensity National Substance Abuse Program). However, some programs were also created in the Nordic countries (e.g. Vinn). Most of the programs are focused on preventing general reoffending or substance misuse. There are also specific programs for clients convicted of violent crime, sexual crime, domestic violence, as well as a gender-specific program for female offenders, a motivational interviewing program, and a

problematic gambling program. Besides treatment programs, there are other interventions, for example a community-based employment program.

Most treatment programs are offered to both prison inmates and probationers. Participation in treatment programs is voluntary for prison inmates, but community sentences may be combined with a requirement to participate in treatment. If a probationer with a requirement to go to a program fails to do so, the sentence may be converted to prison by the court. Based on the risk principle (Andrews and Dowden, 2006; Lowenkamp, Latessa, and Holsinger, 2006), the policy within Swedish correctional service is to offer medium and high intensity programs to clients with at least a medium risk of reoffending (Swedish Prison and Probation Service, 2008). But even though the risk of criminal recidivism is assessed for all clients, structured risk assessment instruments are currently only used in some circumstances, such as for clients with a prison sentence of four years or more. Selection into treatment is therefore currently not systematically based on structured risk assessment instruments.

All program deliverers receive education in the specific program. In order to be allowed to continue delivering a program, one must become certified after giving two rounds of the program (or after one year in the case of individual programs). To achieve certification, one must fully attend the education in the program and deliver two rounds of the program that are approved by a supervisor according to criteria for the program. Program deliverers also receive regular guidance by an advisor.

Evaluations of correctional programs were sporadically initiated in the 1990s and early 2000s, often by external researchers or other governmental agencies. Several outcome evaluations of programs for driving under influence offenders were conducted by external researchers (Andrén, Bergman, Schlyter, and Laurell, 2002; Törnros, 1992, 1993, 1998). The National Council for Crime Prevention conducted an evaluation of a solution-focused network therapy and another on the Reasoning and Rehabilitation program (RandR) (Berman, 2004; Lindforss and Magnusson, 1997). External researchers also began an evaluation of a Swedish program for Motivational Interviewing in 2004 (Forsberg, 2006). Because these studies were initiated by different organizations, they differ in terms of both study aim and design. A few of the evaluations used a randomized controlled trial design, where study participants are randomly allocated to either the program or a control group (Forsberg, 2006; Lindforss and Magnusson, 1997). However, most of the evaluations were observational, meaning that the evaluators had no influence over which clients the programs were provided to.

In 2002, an accreditation system was created based on criteria used by the Correctional Service Accreditation Panel in England and Wales (Lipton, Thornton, McGuire, Porporino, and Hollin, 2000). One of the criteria for accredited programs is that there are routines for evaluating the program. Among other things, this means that their effect on criminal recidivism shall eventually be evaluated (Swedish Prison and Probation Service, 2007). So when the Research and Evaluation (RandE) unit¹ was created, one of its prioritized missions was to evaluate the effect of programs on reoffending. To this day, reports only eleven outcome evaluations have been published, and more are on their way. However, because the reports are published in Swedish, information on the evaluations seldom reach other countries. In this article, we will describe how these outcome evaluations are conducted.

¹ Originally, it went under the name of Research and Development (RandD) and consisted of several units.

INITIAL OUTCOME EVALUATIONS

The first outcome evaluation conducted by the R and E unit was published by Danielsson, Dahlin, and Grann (2008). Participants in a specific treatment program were compared with clients who did not start any treatment. To merely compare treated and non-treated clients straight-off would probably lead to biased results, because they differ on important baseline characteristics. Perhaps clients who did not participate in any treatment did not have need of treatment, were unmotivated or did not have a long enough sentence to complete treatment, factors that could all be associated with the risk of reoffending. Confounding factors that are associated both with participation in treatment and with reoffending, should therefore be handled, for example by controlling them in a regression model.

When the Swedish Prison and Probation Service started to conduct outcome evaluations, only the information contained in our registers was available to us. This limited our ability to handle baseline differences between program participants and the comparison group. At the time, this meant that we could control for:

- Basic demographics: age, gender and Nordic citizenship
- Type of criminal sentence
- Number of days in prison (including pre-trial remand)
- Criminal history based on the types of crimes and number of previous sentences within Swedish correctional services the last five years
- Substance misuse

This is of equal, or better, methodological quality than most outcome evaluations of offender treatment programs (Lösel and Schmucker, 2005; Mitchell, Wilson, and MacKenzie, 2012) However, there are, of course, other important baseline factors, such as age at first criminal conviction, and the results could therefore still be biased.

Our evaluations include intent-to-treat analysis, where all program starters are compared with the control group. There are several reasons to include program starters regardless of whether they complete the program or not. Firstly, when offering a program it is not known which clients will complete the program. Secondly, experiencing adverse effects or lack of effect can lead to drop-out. Thirdly, program participants who manage to complete a program tend to be in a better position to stop committing crime even before treatment. For instance, program completers tend to be more motivated (Olver, Stockdale, and Wormith, 2011). Motivation could lead to both a higher tendency to complete programs and a reduced risk of criminal recidivism. To only include program completers could thus increase the risk of biased results.² However, taking part in the full program could be essential in order to achieve positive effects. If so, an intent-to-treat analysis will underestimate the effect of the program. For this reason, we also conduct per-protocol analyses, where program completers are compared with a control group. Usually, we also conduct a separate analysis on program-dropouts. This is of relevance because program dropouts tend to reoffend more than untreated control groups. It is also currently unknown whether this is caused by baseline differences or if program disruption in itself increases criminal recidivism (McMurran and Theodosi, 2007; Olver et al. 2011).

² The risk of biased results is especially high if program completers are compared with a control group consisting of dropouts, which is thus not recommended (Landenberger and Lipsey, 2005).

Initially our definition of reoffending was limited to reconviction that led to a penalty within Swedish Prison and Probation Service because we only had access to our own register data. Our evaluations usually contain both prison inmates and probationers. For clients who received a program in prison, follow-up begins at the date of release from prison. For probationers, the control group is followed from the start-date of the probation sentence and the program group from the end-date of the program. Because these dates vary between clients, the length of possible follow-up time differs. This can be handled by either deciding on a fixed follow-up time for all clients, such as one or two years, or by using a statistical method called survival analysis, which takes differences in follow-up time into account. We wanted to use all available information on reoffending and thus chose to use a method of survival analysis called Cox regression. We also handled confounders by adjusting for them in the Cox regression model.

EFFORTS TO IMPROVE METHODOLOGY

The Swedish Prison and Probation Service do not register all factors relevant to criminal recidivism. Therefore, we started to use data from other national registers. Since 1947 everyone who is registered as living in Sweden gets a unique national identification number. This number is used for many administrative purposes, such as taxation, healthcare, social security and salary payment. During the second half of the 20th century, several national administrative registries evolved and eventually came to replace the survey-based census used back then. Data from different national registers can be linked for research purposes by using the national identification number as key. In order to protect individual integrity, the data is often de-identified before researchers are given access to it. These population registries have been used extensively in epidemiology and sociology (Bauer, 2014; Lyngstad and Skardhamar, 2011; Rosén, 2002). We were able to use de-identified data from several population registers thanks to collaboration with the university Karolinska Institutet in Stockholm. This meant that we could handle a lot more background characteristics than before (see Table 1). The first report using register data from other government authorities was published in 2013 (Nordén, Fors, and Damsten, 2013).

Table 1. Register-based baseline factors

Register	Government authority	Examples of baseline factors
Total Population Register	Statistics Sweden	Age, gender, immigrant background, residential area
Conviction Register	National Council for Crime Prevention	Age at first criminal conviction, number of criminal convictions, any violent conviction, drug conviction, DUI conviction, mother, father or sibling ever convicted of crime *
LISA register	Statistics Sweden	Educational level, work experience, marital status
Patient Register	National Board of Health and Welfare	Alcohol dependence, drug dependence, ADHD, personality disorder, parental substance dependence *
Swedish Prison and Probation Service Administrative Register	Swedish Prison and Probation Service	Prison or probation client, previous participation in general offending/violent crime program, substance misuse program, domestic violence/sex crime program or motivational program
The Population and Housing Register	Statistics Sweden	Childhood socio-economic status

* Created by combining information from the register with data on family relationships from the Multi-Generation Register.

Our extended access to register-based data also has other benefits. Before, a client who died or emigrated could be registered as not reoffending, when in fact he or she could no longer commit crimes in Sweden. But with access to data from the Cause of Death Register and the Migration Register, we are now able to end the follow-up at time of death or emigration. Also, we are no longer limited to defining reoffending as new convictions with a prison or probation sentence. It is now possible for us to define reoffending as any new criminal conviction (Conviction Register) or any new reasonable suspicion of crime (Suspicion Register).³ Statistical power, the probability of detecting an effect statistically, is affected by the incidence rate. Hence, these more inclusive definitions of reoffending have resulted in increased statistical power. Besides reoffending, we were also able to add some other outcomes that are sometimes considered important program goals. A register-based indicator of substance misuse can now be used as a proxy outcome for substance misuse programs. The indicator is based on data from the Patient Register on acute alcohol/drug intoxication or on entry into an inpatient substance dependence clinic⁴. This only captures some, probably severe, substance misuse and is therefore not an ideal measurement. But hopefully the indicator can give a hint on whether a program has an effect on substance misuse.

³ The suspicion register contain information on suspicions that are at least of the third degree of suspicion on a five degree scale, called reasonably suspected of crime. This suspicion level can under some circumstances provide ground for arrest or detention ("The Swedish code of judicial procedure," 1998). It is common to define criminal recidivism as re-arrest in evaluations (Lipse, Landenberger, and Wilson, 2007), but to our knowledge there is no Swedish register on arrests. The suspicion register is therefore probably the closest thing to registered arrest in Sweden.

⁴ Inpatient substance dependence clinics are used for detoxification and withdrawal treatment. The treatment can be voluntary or involuntary.

One of the issues with using regression models is that it is difficult for many practitioners to interpret the results. Group differences in percent reoffending are a lot more intuitive. However, a simple proportional comparison of criminal recidivism in treated and untreated clients is likely to be biased by background differences. But by using matching instead of regression, one can produce figures on percent reoffending that are adjusted for background differences. This is one of the reasons that we started to use propensity score matching. In this context, a propensity score can be described as the likelihood of participating in a program, based on the baseline factors that are controlled for. Consequently, propensity score matching means that program participants are matched with clients in the comparison group who did not participate in the program, but should have been about as likely to do so based on their characteristics. If successful, propensity score matching will result in an equivalent distribution of baseline characteristics between the groups (for an example, see Table 2).

Table 2. Excerpt from an evaluation illustrating the result of propensity score matching on group differences in baseline factors

Baseline factor	Program participants	Unmatched comparison group	Matched comparison group
Female gender, %	11.4	12.1	10.0
Age, m	31.6	41.6	31.1
Criminal convictions, m	8.8	11.2	9.4
Any conviction for violent crime, %	72.0	61.7	73.8
Any conviction for drug crime, %	70.6	46.4	70.8
Any conviction for DUI, %	32.0	47.3	30.8

Another methodological change is that the control group is now allowed to have previous experience of other programs, as long as it is equivalent with the experience of program participants *before* the current program. As program participation becomes more common, completely untreated clients become a less available control group. Also, since many program participants have previous experiences of treatment, a comparison with completely untreated clients will often reflect effects of several programs, not only the current one.

LIMITATIONS

Our evaluations can be considered effectiveness trials ('pragmatic' trials), which means that we evaluate the effect of programs as they are carried out in practice. In contrast, efficacy trials ('explanatory' trials) aim towards studying if a program *can* affect the outcome of interest (Treweek and Zwarenstein, 2009). Researchers try to achieve this by studying the program under ideal circumstances, for example through strict control over client selection and program fidelity. Because we make no such efforts, our results can only answer if the program seems to reduce reoffending with current implementation, not whether it is possible for the program to reduce reoffending. However, the strict control over implementation in efficacy trials is seldom possible to achieve when programs are rolled-out in ordinary practice, and our results are thus of more practical relevance.

Even though we have taken steps to reduce the risk of bias in our evaluations, there are still important baseline variables that we do not control for, such as a motivation and pro-criminal

attitude. A structured risk assessment instrument for use on all clients is currently being implemented in the Swedish Prison and Probation Service. This instrument will make it possible for us to handle additional risk and protective factors in future evaluations, such as pro-criminal attitude, antisocial peers and involvement in pro-social activities. However, regardless of how many background factors you control for, there is always a risk that some important factor has been left out. There might even be important factors that are currently unknown. The only way to be sure that the results are not affected by systematic baseline differences, in both known and unknown factors, is to randomize clients into either the program or control group in a Randomized Controlled Trial (RCT) (Farrington and Welsh, 2005). This design, however, tends to be more expensive and time-consuming and may encounter resistance because of ethical or practical considerations (Farrington and Welsh, 2005).

IMPACT ON PRACTICE

Outcome evaluations have had several implications on practice in the Swedish Prison and Probation Service. One of the most obvious consequences is that they have been used as guidance on which programs to continue using. However, a single outcome evaluation indicating negative results has not been reason enough itself to discontinue a program. Outcome evaluations of the same program can have differing results. Even for interventions that show positive effect in several high-quality studies, a single study may indicate null or negative effect (see Buscemi et al. 2007; Spek et al. 2007). For this reason, the results of a single outcome evaluation must be interpreted in light of previous studies on the same or similar programs. When negative results have inspired program withdrawal, several previous studies of acceptable methodological quality also indicated that the program did not have intended effects or there were other considerations that led to that decision. Another reason could for example be that the program was not developed for use with adult correctional clients.

As in many other studies (McMurrin and Theodosi, 2007; Olver et al. 2011), our results almost exclusively show that program dropouts have an increased risk of reoffending compared to the control group. This has led to increased measures to retain clients in treatment for programs with a high drop-out rate.

On one occasion, the results of an evaluation indicated that the intervention might not be as effective as some had hoped. However, guidelines on the intervention were very sparse, which meant that there could be local differences in both content and implementation. We therefore concluded that the results reflect the average effect of the intervention, but that some local versions may more effective than other. Based on this, development of clearer guidance for the intervention was initiated.

CONCLUSIONS

Outcome evaluations can be conducted at small cost by using data that is already collected routinely for administration or other purposes. Besides being cost-effective, register-based outcome evaluations can be conducted with good methodological quality. However, the most valid conclusions on the effect of programs are drawn from well-conducted RCTs (Farrington and Welsh, 2005; Weisburd, 2003). The Nordic countries have especially favorable conditions for register-based outcome evaluations. There are already examples of register-based outcome evaluations in other Nordic countries, for example in Denmark (Nielsen and

Kyvsgaard, 2007). In countries without population registers that use national identification numbers, outcome evaluations can still be based on administrative data collected by their correctional services. Also, without national identification numbers, client data can be linked with population registers using information such as name and birth date (see Ministry of Justice, 2013).

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