

**Campbell Systematic Reviews**

2006:13

First published: 31 October, 2006

Last updated: 31 October, 2006

# **The Effects of Custodial vs. Non-Custodial Sentences on Re-Offending: A Systematic Review of the State of Knowledge**

Martin Killias, Patrice Villettaz, Isabel Zoder



**THE CAMPBELL COLLABORATION**

# Colophon

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<b>Title</b>	The effects of custodial vs. non-custodial sentences on re-offending: A systematic review of the state of knowledge
<b>Institution</b>	The Campbell Collaboration
<b>Authors</b>	Killias, Martin Villettaz, Patrice Zoder, Isabel
<b>DOI</b>	10.4073/csr.2006.13
<b>No. of pages</b>	73
<b>Last updated</b>	31 October, 2006

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<b>Citation</b>	Killias M, Villettaz P, Zoder I. The effects of custodial vs. non-custodial sentences on re-offending: A systematic review of the state of knowledge. Campbell Systematic Reviews 2006:13 DOI: 10.4073/csr.2006.13
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<b>Keywords</b>	

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<b>Contributions</b>	None stated.
<b>Support/Funding</b>	Swiss National Science Foundation (grant 101411-101960), Switzerland
<b>Potential Conflicts of Interest</b>	None stated.

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<b>Corresponding author</b>	Martin Killias Institute of Criminology and Criminal Law Ecole des Sciences Criminelles University of Lausanne Lausanne, CH-1015 Switzerland Telephone: +41 21 692 46 40 E-mail: <a href="mailto:martin.killias@unil.ch">martin.killias@unil.ch</a>
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The Campbell Collaboration  
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**The effects  
of custodial vs. non-custodial sentences  
on re-offending.**

**A systematic review of the state of knowledge**

Report to the



SCHWEIZERISCHER NATIONALFONDS ZUR FÖRDERUNG  
DER WISSENSCHAFTLICHEN FORSCHUNG  
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and to

the Campbell Collaboration Crime and Justice Group

by

Patrice VILLET AZ, Martin KILLIAS and Isabel ZODER



**Institute of Criminology and Criminal Law**

Ecole des Sciences Criminelles  
UNIVERSITY OF LAUSANNE  
BCH, CH-1015 Lausanne, Switzerland

*30.09.2006*

This systematic review was supported by Grant 101411-101960 by the Swiss National Science Foundation

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of Custodial vs. Non-Custodial Sentences  
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A Systematic Review of the State of Knowledge*

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## **Preface**

*When, during the second half of 2003, this systematic review has been started, nobody had anticipated that it would take as long to complete it. Two reasons were responsible for this delay, the most important being the overwhelming number of studies in which re-offending was assessed as an outcome of all sorts of possible sanctions. Another reason certainly was the controversial nature of this subject that made preparation of this report, as well as the subsequent reviews and revisions particularly complicated. Although any review, and particularly one on such a controversial topic, never can satisfy all legitimate expectations, we hope providing the reader, beyond the documentation of hundreds of studies, with some guidance on what the current state of knowledge on effects of custodial vs. non-custodial sanctions on subsequent re-offending might be. We further would be happy if the description of shortcomings of current research may prove helpful in the design of future evaluations of new sanctions or correctional programs.*

*The authors wish first to thank the Swiss National Science Foundation that has generously supported the costs of the present project (grant 101411-101960). No less appreciated has been the confidence of the Campbell Collaboration Crime and Justice Group and its Steering Committee whose Chairperson at the time, Professor Dr. David Farrington, has often come forward with most helpful suggestions. Further, we thank all those who have contributed to make this systematic review possible, and in particular to our PA (Professor Dr. Ulla Bondeson), the peer reviewers (Professors Dr. David Wilson and Dr. Hans-Jörg Albrecht) and members of the CCJG Steering Committee who have devoted considerable time and energy to help us in improving the review. Finally, this review has allowed our team to make invaluable contacts with European and American Colleagues involved in similar research and who have assisted us in locating relevant materials covered in this review. Particularly helpful have been the Home Office Research and Statistics Directorate, as well as the Max-Planck-Institute of Criminal Law, International Criminal Law and Criminology at Freiburg (Germany). Both have generously hosted our researchers in their respective libraries where studies could be located that would otherwise not have been available for the present review.*

*Lausanne, July 2006*

*Martin Killias*

## **1. Synopsis**

As part of a broad initiative of systematic reviews of experimental or quasi-experimental evaluations of interventions in the field of crime prevention and the treatment of offenders, our work consisted in searching through all available databases for evidence concerning the effects of custodial and non-custodial sanctions on re-offending. For this purpose, we examined more than 3,000 abstracts, and finally 23 studies that met the minimal conditions of the Campbell Review, with only 5 studies based on a controlled or a natural experimental design. These studies allowed, all in all, 27 comparisons. Relatively few studies compare recidivism rates for offenders sentenced to jail or prison with those of offenders given some alternative to incarceration (typically probation).

According to the findings, the rate of re-offending after a non-custodial sanction is lower than after a custodial sanction in 11 out of 13 significant comparisons. However, in 14 out of 27 comparisons, no significant difference on re-offending between both sanctions is noted. Two out of 27 comparisons are in favour of custodial sanctions. Finally, experimental evaluations and natural experiments yield results that are less favourable to non-custodial sanctions, than are quasi-experimental studies using softer designs. This is confirmed by the meta-analysis including four controlled and one natural experiment. According to the results, non-custodial sanctions are not beneficial in terms of lower rates of re-offending beyond random effects. Contradictory results reported in the literature are likely due to insufficient control of pre-intervention differences between prisoners and those serving “alternative” sanctions.

## **2. Abstract**

### **2.1 Reviewers**

Martin Killias, Patrice Villettaz, and Isabel Zoder, Institute of Criminology and Criminal Law, Ecole des Sciences Criminelles, University of Lausanne, CH-1015 Lausanne, Switzerland. E-mail: [martin.killias@unil.ch](mailto:martin.killias@unil.ch), [patrice.villettaz@unil.ch](mailto:patrice.villettaz@unil.ch), Phone: (0041-21) 692 46 40, Fax (0041-21) 692 46 05.



## **2.2 Background**

Throughout the Western World, community-based sanctions have become a popular and widely used alternative to custodial sentences. There have been many comparisons of rates of reconviction among former prisoners and those who have served any kind of community sanction. So far, the comparative effects on re-offending of custodial and non-custodial sanctions are unresolved, due to many uncontrolled variables.

## **2.3 Objective**

The objective is to assess the relative effects of custodial sanctions (imprisonment) and non-custodial ("alternative" or "community") sanctions on re-offending. By "custodial" we understand any sanction where offenders are deprived of freedom of movement, i.e. placed in a closed residential setting not their home, no matter whether they are allowed to leave these premises during the day or during weekends. Thus, boot camps would be considered "custodial" settings according to the definition adopted here. By "non-custodial", we mean any form of sanction that does not involve any deprivation of liberty, such as community work, electronic monitoring, financial or suspended custodial sanctions. Thus, the category of non-custodial sanctions includes a great variety of punishments that have in common to leave the offender in the community rather than putting him into confinement.

## **2.4 Search strategy**

Relevant published and unpublished studies which meet the eligibility criteria have been identified through multiple sources, including abstracts, bibliographies, and contacts with experts in several countries.

## **2.5 Eligibility criteria**

Randomized or natural experiments have been considered without exception. Quasi-experimental studies, i.e. comparisons between former prison inmates and those who served community sanctions, have been included, provided that variables in addition to those found routinely in registers (age, sex and prior record) have been controlled for (such as attitudes, personal or employment history etc.); in the course of the review, this criterion has been relaxed in the sense that studies were considered if more than three

potentially relevant independent variables have been controlled for. Studies written in any language and prepared between 1961 and 2002 have been considered for inclusion.

## **2.6 Data collection and analysis**

A coding protocol has been prepared, following the guidelines of the Campbell Collaboration.

## **2.7 Main results**

Although a vast majority of the selected studies (see Table 2, page 29) show non-custodial sanctions to be more beneficial in terms of re-offending than custodial sanctions, no significant difference is found in the meta-analysis based on four controlled and one natural experiments.

## **2.8 Reviewers' conclusions**

The review has allowed to identify several shortcomings of studies on this subject:

- (1) Controlled experiments are still rare exceptions, although obstacles to randomisation are far less absolute than often claimed.
- (2) Follow-up periods rarely extend beyond two years. Even in cases of controlled trials where later follow-up studies might be feasible, periods considered never extended to significant parts of subjects' biographies.
- (3) Despite alternative (and presumably more valid) measures of re-offending (such as self-reports), most studies do not include measures of re-offending beyond re-arrest or re-conviction.
- (4) In most studies, only the occurrence (prevalence) of re-arrest or re-conviction is considered, but not the frequency (incidence) of new offences. Some studies have shown, however, that most offenders reduce offending rates after any type of intervention. Thus, the relevant question may be to what extent they improve differently by type of sanction. Therefore, it is urgent to look in future studies at rates of improvement (or reductions in offending) rather than merely at "recidivism" as such.

- (5) Rehabilitation in other relevant areas, such as health, employment, family and social networks, is rarely considered, despite century-old claims that short custodial sentences are damaging with respect to social integration in these other areas.
- (6) No study has addressed the possibility of placebo (or Hawthorn) effects. Even in controlled trials, it is not clear to what extent outcomes that favoured “alternative” sanctions were due to the fact that subjects assigned to non-custodial sanctions may have felt being treated more fairly, rather than to specific effects of “alternative” sanctions as such. Given recent research on neurobiological effects of feelings of fairness (Fehr and Rockenbach, 2003), such a possibility should be envisaged with more attention in future research.

## **2.9 Sources of support**

This review has been funded by the Swiss National Science Foundation (n° 101411-101960). Financial support by the Swiss National Science Foundation has not affected the independence of reviewers.

## **3. Review strategy**

### **3.1 Background**

In the late 19<sup>th</sup> century, leading criminal law teachers (such as Franz von Listz in Germany, Adolphe Prins in Belgium, Bonneville de Marsangy in France, and van Hamel in the Netherlands) promoted the idea that short-term imprisonment is damaging, since inmates are in custody for too short a period to allow any treatment to be beneficial, and for too long to avoid contamination with more severe criminal propensities through the contacts with other prisoners. The basis for this assumption was the idea that crime is a disease which, if not thoroughly treated, will worsen and, ultimately, contaminate others (for a review of the origins of this idea in 19<sup>th</sup> century penology, see Killias 2002, 486; 2001, 480). This led to the call for the substitution of short-term imprisonment by either long sentences, or by “alternative” sanctions such as fines, suspended sentences, or probation (Franz von Listz, 1882). Later, more “modern” alternatives were “invented”, such as community service or electronic monitoring.

Over the decades and throughout the Western World, community-based sanctions have become a popular and widely used alternative to custodial sentences. There have been

many comparisons of rates of re-offending or reconviction among former prisoners and those who have served any kind of community sanction. So far, the comparability of these rates is questionable due to many uncontrolled variables.

## **3.2 Objective**

The main objective of this review has been to compare rates of re-offending after custodial sanctions with those following the execution of non-custodial sanctions. In other words, the question is to know whether custodial *vs.* non-custodial sanctions have different effects on the rates of re-offending. Given the small number of relevant studies that meet the inclusion criteria, studies on adults and juveniles have been considered.

## **3.3 Criteria for considering studies for this review**

### *3.3.1 Preliminary remarks*

The first step was to define what should be considered as custodial and non-custodial sanctions. We considered as custodial all sanctions that imply confinement in a closed institution like prison and jail. Boot-camps and shock incarceration programs are also considered as custodial, although Morris and Tonry (1990) define such punishments as sanctions that can be placed on a continuum of severity between incarceration and probation. Indeed, boot camp prisons (or any sentences involving short terms of incarceration) are similar to a short-term confinement in Europe, for which often alternative sanctions have been developed. All other sanctions have been considered as non-custodial, especially fines or any form of “treatment” or sanction that did not imply placement in any type of facility.

Before we started the search of studies, we established some selection criteria in order to make a later meta-analysis possible. These selection criteria were mainly the following:

- (1) All studies had to include at least two distinct groups: a custodial sanction group and a non-custodial sanction group;
- (2) The sanctions to be compared were imposed following a conviction;
- (3) There was at least one outcome measure of recidivism (new arrests, re-convictions, re-incarceration or self-report data for example);
- (4) The study was completed after 1960 and prior to 2003.

No restriction about type of publication, geographical area, language, type of delinquency, age, or gender has been applied.

Using only the first criterion, we made a rough inventory of more than three thousand studies across the Western countries in which re-offending (mostly reconvictions) has been compared between former prisoners and those who served any type of “alternative” or non-custodial sentence. On the scale developed by Sherman *et al.* (1997), many studies of this kind would be classified at level 3. Usually, the controls were limited to the variables available in official files, such as number and type of previous convictions, sex and age. In other words, the controlled variables are so minimal that no valid comparison is feasible between the different sanctions. Since offenders who receive different types of sanctions tend to differ in many other ways which are likely to be related to a judge’s decision about the sanction, as well as to risks of re-offending, namely attitudes, employment record, drug or alcohol abuse history, any conclusions about “superiority” (in terms of special deterrence) of “alternative” over custodial sanctions in such studies are highly questionable. Since the bias is systematic in all studies of this kind, including them and computing any mean effects would, at best, be misleading.

Therefore and in order to produce a review with reasonably valid conclusions, only studies that met higher methodological standards (level 4 and beyond on the Sherman scale) have been included in this review.

### 3.3.2 *Types of sanctions*

We considered all studies meeting these criteria where “alternative” or community-based sanctions have been compared with custodial sanctions. To qualify for the review, a study had to compare any form of confinement or imprisonment with any of these “alternative” sanctions; on the contrary, comparisons between several community sanctions (e.g. community work vs. electronic monitoring), or several forms of treatment during confinement, have not been included. By “custodial”, we understand any sanction where offenders are placed in a residential setting, i.e. deprived of their freedom of movement, no matter whether or not they are allowed to leave the facility during the day or at certain occasions. Thus, boot camps would, according to this definition, qualify as a form of custodial sanction, just as “community” treatment in a residential setting, as in the Silverlake experiment (Empey and Steven 1971) or in the case of the Californian Youth Authority’s Community Treatment Program (Palmer, 1971 and 1974), would be considered as “custodial” sanctions. This definition led to the exclusion of several randomized experiments where different forms of residential

treatment of juveniles (Empey and Steven 1971, Palmer 1971, 1974) or adults (Lamb and Goertzel 1974) were compared. Whatever the merits of comparing more closed with more “open” facilities for juveniles, or boot camps with traditional prisons, such experiments do not have their place in a review concerned with comparing custodial with non-custodial sanctions. Despite these reservations, protocols summarizing these studies will be included in the *Appendix III* (numbers 1003, 1004, 1006), and reference will be made to these important experimental studies at various occasions throughout this report.

Studies were considered for inclusion regardless of the length of custodial sentences. Indeed some studies have compared prisoners who, after a considerable time in custody, have been paroled (and transferred to a program of electronic monitoring), and those who had to serve their entire sentence (as in the case of the studies by Finn and Muirhead-Steves 2002, and by Bonta J., Wallace-Capretta S., Rooney J. 2000).

Only *sanctions* (following a formal conviction) have been considered. Thus, studies on police cautioning are not included, since such a sanction does not follow a judicial decision, nor are studies on “alternatives” to pre-trial detention. In the same line, studies comparing immediate detention before any judicial hearing (such as in cases of domestic violence in the United States and many other countries) are not included, nor are studies comparing recidivism among defendants in pre-trial detention with those who were bailed out.

### 3.3.3 *Types of offenders*

Initially we included only studies involving adults or young adults aged 17 years at least, because sanctions imposed upon juveniles were covered by a different Campbell Group systematic review (Tammy White & Neil Weiner). However, as we found only two randomized studies with adults, we decided to include also two relevant randomized studies concerning juveniles, despite a risk of overlap with that other review. Finally all types of offenders were included without any limitation. At this point, we noticed that more trials were conducted on juveniles than on adults. Some policy-makers may be less reluctant about accepting random assignment when minors rather than adults are involved.

### *3.3.4 Types of outcome measures*

Most of studies concentrate on reconviction. This is certainly a key variable, but efforts have been made to find more differentiated indicators of re-offending, such as new arrests, contacts with police, or self-reported offences. For example, some studies have shown that the frequency of new convictions is lower after any type of intervention (compared with a pre-conviction period of the same length), and that arrest data may differentiate better between groups of offenders who were treated in different ways. This is particularly true in countries where re-incarceration (for parole violations) is more common than reconviction for a new offence. Some studies have also used self-report data in order to assess the outcome of different interventions.

In smaller trials, the experimental and the control groups often differ in outcome because they differed from the beginning of the experiment, often despite careful randomization. We gave, therefore, priority to comparing relative improvement rather than to comparing absolute levels of re-offending.

In order to assess improvement, we have tried to look not only at prevalence of reconvictions (or percentage of those who re-offend), but also at “incidence” rates (i.e. frequencies of new offences per time unit). Consequently relative improvement can be computed as the standardized mean difference.

### *3.3.5 Types of studies*

First, we selected randomized experiments where re-offending rates among former prisoners (in a broad sense) and those who served any form of community-based sanction have been compared. Such studies would obviously qualify for level 5 according to the scale by Sherman et al. (1997).

Secondly, we included natural experiments where, for example, convicts who were eligible for an “alternative” sanction as part of an amnesty package, were compared with others who were not (and who had to serve their time in prison). In studies of this kind, the criterion for eligibility for an “alternative” sanction was usually a certain date at which the offence had occurred (and which coincided with a significant royal or state event in the country). In such cases, eligibility for an “alternative” sanction was presumably independent of offender characteristics. Such studies may, despite the absence of randomization, eventually qualify for level 5 on the scale by Sherman et al. (1997).

Thirdly, we initially planned to include studies where variables which went beyond information that is routinely found in criminal registers (such as prior convictions) are controlled for. In particular, studies using (interview) data on employment or drug/alcohol abuse history, or on attitudes, qualified, according to our initial plan, for inclusion. However, since few studies met this criterion, we decided to consider all studies with four or more control variables, provided that multivariate methods were used to assess the impact of such control variables. Studies of this kind meet level 4 on the scale by Sherman et al. (1997). However, the line separating eligible (A-) from non-eligible (B-) studies goes through the category 4 of Sherman et al. (1997), the less convincing of these (level 4) studies – i.e. those with three or less control variables, or without multivariate methods - being listed in the bibliography under B. Level 3 studies are excluded and listed in the bibliography under C.

### **3.4 Search strategy for identification of studies**

#### *3.4.1 Search procedure*

After having established selection criteria, we began the search for relevant studies through abstracts, internet, library catalogues, bibliographies of studies and e-mail contacts with research institutes in a number of countries.

We consulted more than 3000 abstracts of studies that, given their title, might have included a comparison of recidivism after custodial and non-custodial sanctions. In most cases, however, this was not the case. As a result, we selected a more refined list with nearly 300 relevant studies.

It was not very difficult to find published studies, especially when there was an article version. However, we had more difficulties in locating unpublished studies. Therefore, it is possible that our review is biased toward studies with statistically significant results, because such studies are probably more likely to be published than those without significant outcomes. In the present case, however, this bias (i.e. an eventual under-representation of studies without significant outcomes) would have produced a conservative error at worst, given that our meta-analysis has failed to demonstrate any significant overall effects of either custodial or non-custodial sanctions..

Relevant studies which met our eligibility criteria have been identified through multiple sources, including Criminal Justice Abstracts, Criminology and Penology Abstracts, bibliographies (in several languages), and databases (such as those listed under the Campbell Crime and Justice Group website). Also consulted were the National Criminal



Justice Reference Service NCJRS, C2-SPECTR that includes more than 10,000 citations of randomised and possibly randomised studies, KRIMDOK of the University of Tübingen, IUSCRIM of the Max-Planck Institute in Freiburg in Germany, and WWW.GOOGLE.CH. We also contacted experts in several countries. Particularly Professor Ulla V. Bondeson from the University of Copenhagen has been helpful in locating two important Scandinavian studies. No eligible study has been found outside of the Western World. We included studies from 1960 onwards, assuming that older studies might no longer be relevant for this review.

Practically speaking, we have selected all studies which compared custodial and non-custodial sanctions under the form of a randomized trial, a natural experiment, a matched pair design, or any non-experimental design where more than three variables have been controlled for. We used keywords covering all types of sanctions (prison, jail, imprisonment, alternative sanctions, electronic monitoring, house arrest, community service, probation, day reporting, fines, shock incarceration, boot camps, etc.) and the more frequent concepts used to define recidivism (re-offending, reconviction, self-reported offences, recidivism, re-arrest, re-incarceration, etc.).

### *3.4.2 Methods of review*

The search method generated nearly 300 citations of potentially eligible studies. We screened these citations and for each study, we assessed its methodological quality.

It has not been always easy to locate evaluation reports. For this reason, a researcher (Dr. Zoder) spent three weeks at the Max-Planck Institute at Freiburg (Germany), and another one (V. Maerki) at the Home Office in London, to try to locate missing evaluation reports. Unfortunately, a small number of evaluation reports could not be located. Four among them belong to the category B (in the attached bibliography), i.e. to the studies that were, after closer examination, not included. All 23 fully eligible studies could be located and abstracted (see appendix).

Each study has been screened for eventual methodological shortcomings:

- (1) In the case of randomized experiments, deviations from the randomization process, or high attrition rates, have been noted. No studies were excluded on such grounds.
- (2) In the case of natural experiments, special attention has been given to the independence of the selection criterion from offender characteristics. The only one natural experiment that has been identified did not present difficulties in this regard.

- (3) In case of non-randomized studies, the theoretical and/or practical relevance of the control variables has been considered. In practice, this criterion was relaxed in order to exclude only studies with less than four control variables. The exact number of studies excluded on this ground has not been recorded, but was obviously large.

At the end of the selection process, we found only four randomized experimental studies and one natural experiment on our subject all over the World. After consultation with the reviewers of the present study, we limited the meta-analysis to these five studies (Table 3a). Studies using a matching assignment, as well as studies using a sufficiently large number of control variables, i.e. beyond variables like age, gender, prior records, and type of offence which are routinely identified through criminal records, are listed in Table 1 and summarily analysed in Table 2. We found, including the five studies included in the meta-analysis, no more than 23 eligible studies (including three studies on juveniles).

### **3.5 Data collection and analysis**

We prepared a coding sheet along the guidelines of the Campbell Collaboration in order to condensate all relevant information presented in the eligible studies. Our coding sheet contains numerous variables such as location, publication year, composition of the samples, type of sanctions and offenders, effect size, type of statistical control, etc. All studies have been coded by I. Zoder under the supervision by P. Villettaz.

Studies differed widely in methodology and research design, types of offenders, sanctions and outcome measures. (Some of these problems will be addressed more in detail in the “Discussion”). Considering this heterogeneity, we first envisaged to give only a descriptive account of the findings of the 23 studies meeting the final inclusion criteria, as already anticipated in the original research protocol. After consultation with the reviewers, however, a meta-analysis limited to the 5 controlled or natural experiments has been conducted.

### **3.6 Comparison with the review by Smith, Goggin and Gendreau (2002)**

We acknowledge having received from Professor Gendreau his and his Colleagues’ review of studies on recidivism after, among other things, a custodial or a community-based sanction (Smith, Goggin and Gendreau 2002) that updates earlier work on the same topic (Gendreau, Goggin, Cullen 1999). After having completed our collection, it

turned out that 49 among the 111 studies included in their meta-analysis were missing in our bibliography<sup>1</sup>. After closer examination, however, it turned out that 47 did not meet our criteria for inclusion, mostly because they concerned a somewhat different topic, such as re-offending after a police arrest in cases of domestic violence, or following probation or several community-based sanctions without a comparison group that served time in prison. Two studies (Walker, Farrington, Tucker 1981, Babst and Mannering 1965) included here, however, have been located thanks to their inclusion in the bibliography by Smith, Goggin and Gendreau (2002). On the other hand, 13 among our (originally) 23 fully eligible studies (A-studies) have not been found in the review by our Canadian Colleagues, among them six from outside the American continent and three of the four randomized experiments. Beyond these differences in coverage, the two reviews differ mostly by inclusion criteria. Whereas our review included only experiments, natural experiments and quasi-experiments with at least four control variables, the Canadian inclusion criteria were far more relaxed in this respect. As we shall see later, most studies on recidivism rates by type of sanction fail to consider sufficiently pre-existing differences between groups of offenders sentenced to custodial vs. non-custodial sanctions and do not, therefore, qualify for inclusion in our review. Possible effects on the outcomes will be under discussion in chapter 5.

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<sup>1</sup> Given the frequent republication of identical (or very similar) results in several publications (or articles following technical reports), we have considered that the two reviews match whenever the same materials have been included.

## 4. Description of the selected studies and findings (A-studies)

### 4.1 Controlled randomized trials

**\*10\*** Barton W.H., Butts J.A. (1990) : *Viable options: intensive supervision programs for juvenile delinquents.*

This study examines the impact on recidivism of a new intensive supervision program developed by the Wayne County Juvenile Court in Detroit (Michigan), compared with the normal institutional placement of juvenile law violators. More than 500 youths were randomly assigned to either intensive supervision (experimental group) or to a control group that was committed to the State for institutional placement. The evaluation focuses on the programs' ability to prevent or reduce delinquent behaviour, taking into account that clients could remain in the community instead of being placed in correctional institutions. This evaluation was limited to males.

On the whole, the findings show mixed differences in recidivism after a two-year follow-up period, either in all official charges or by self-report measures. In particular, the experimental group has significantly more charges than the control group (2.63 *versus* 1.31 per case). Even when status offences and technical violations are excluded, the average number of criminal charges per case still favours the control group (1.17 *versus* 1.85) although the difference is smaller. However, the average seriousness of the control group's charges is significantly higher (4.19) than that of the program youths (3.44). Finally, once all youths are at large for 24 months at least, the average number of criminal charges is always higher for the experimental group than the control group (5.41 *versus* 4.05), but this difference is not statistically significant.

Concerning self-reported delinquency, about 64% of the innovative program youths report having reduced levels of overall delinquency, compared to 50% among those in the control group. On the relatively serious property and violent behaviour indexes, more than 70% of the experimental group juveniles report reductions, compared to about 60% of the control group youths.

Overall, the results indicate that the experimental group is no less effective at curbing recidivism than the control group.

**\*91\*** Bergman G.R. (1976) : *The evaluation of an experimental program designed to reduce recidivism among second felony criminal offenders.*

This study evaluated a pool of second felony offenders who ordinarily were sentenced to prison in Oakland County, Michigan. Offenders from the prison pool were randomly assigned to either an innovative probation program (experimental group) or a traditional prison program (control group). The comparison of these two groups focused on rates of recidivism, and the offenders' change of status in society after treatment.

The results show that offenders randomly diverted from prison and assigned to extensive community treatment had lower failure rates after a 12-month follow-up period than those sent to prison (14% *versus* 33%).

**\*25\*** Killias M., Aebi M., Ribeaud D. (2000) : *Does community service rehabilitate better than shorter-term imprisonment ? : Results of a controlled experiment.*

This study compares the effects of community service *versus* prison sentences through a controlled experiment conducted in the Swiss Canton of Vaud between 1993 and 1995. Community service was used as an alternative to serving unsuspended prison sentences of up to 14 days, with 1 day in jail corresponding to 8 hours of work. The treatment group consisted of 84 adult offenders, and the control group (sent to jail) of 39. The total of 123 offenders were randomly assigned to either condition, the odds being 2 to 1 for community service

The results show that prevalence of re-arrest by the police was slightly, but not significantly higher among prisoners (38.5% *versus* 33.3%). The number of offences known to the police was also higher among prisoners than among those selected for community service after a 24-months follow-up period (2.18 *versus* 0.76). However, during the two-year period, the experimental group improved significantly, in terms of re-offending (incidence rates), whereas the group of former prisoners even deteriorated. Moreover, no difference with respect to later employment history and private life circumstances had been noticed. However, prisoners developed significantly more often unfavourable attitudes towards their sentence and the criminal justice system. As a reader commented (Dr. Frank Vitaro, University of Montréal), the significantly better improvement of those assigned to community service might be due to the fact that they had a choice (and luck), whereas prisoners had not. (A follow-up study, covering 10 years after the intervention and including police, reconviction and tax authority records (on revenue, debts, welfare etc.), is currently in progress.)

**\*66\*** Schneider A.L. (1986) : *Restitution and recidivism rates of juvenile offenders: results from four experimental studies.*

This study examines the impact on recidivism of the restitution programs implemented simultaneously in four communities (Boise, Idaho ; Washington D.C. ; Clayton County, Georgia ; and Oklahoma County, Oklahoma). In these four correctional programs, youths were randomly assigned to restitution or to traditional correction programs (probation or detention). For this review, only the experimental trial in Boise (Idaho) was relevant according to our criteria.

On the whole, the recidivism analysis suggests that the restitution group has fewer re-offences than the detention group during the follow-up period, but the differences in both prevalence and incidence rates are not statistically significant. Specifically, in the 22 months of the follow-up period, 53% of the restitution group had one or more subsequent contacts with the court compared with 59% of the incarcerated group. The post-program annual rate of subsequent contacts per 100 youths (annual incidence rate) was 86 for the restitution group compared to 100 for the incarceration group. However, although the annual offence rate of both groups has decreased after the intervention, the cross-comparison of pre/post rates shows that the drop is slightly smaller for the restitution group than for the detention group. Finally, youths in the restitution group never have higher re-offending rates than those in the control group.

## **4.2 Natural experiment**

**\*124\*** Van der Werff C. (1979) : *Speciale Preventie.*

This study compares the recidivism rates for different offenders sentenced to an unsuspended prison sentence of up to 14 days. Thanks to a royal pardon (at the occasion of the wedding of princess and later Queen Beatrix), people having to serve such a sentence who had committed their offence before a fixed date (January 1, 1966) had automatically their sentence suspended, while sentences for offences committed after that date had to be served. Thus, both groups of offenders could be considered as similar, except for the date on which the offences had been committed.

The results show that the recidivism rates of both groups were similar for traffic (N=1397) and property (N=202) offenders after a 6-year follow-up period (40% versus 40%, and 68% versus 65%, respectively). Among violent offenders (N=321), subjects who had, as a result of the royal pardon, their prison sentence suspended, re-offended significantly less often than those serving a prison sentence (53% versus 63%).

### 4.3 Matched-pair design studies

Although the samples of prisoners and non-custodial convicts in the following studies were matched on factors that research and experience have found to be related to recidivism, there are undoubtedly other factors that can influence both sentencing decision and re-offending. Moreover, there is no way to know how the samples differ on such factors, and consequently whether they are fully comparable.

**\*76\*** Kraus J. (1974) : *A comparison of corrective effects of probation and detention on male juvenile offenders.*

This study investigated the relative efficacy of probation and detention as applied to male juvenile offenders in New South Wales (Australia). The age range of these offenders was 8-18, median 15.2. The matching was done on seven demographic variables to build up a comparable group of offenders from institutions for 223 probationers.

In the five-year follow-up period, the recidivism rate for overall delinquency is significantly higher among the institutional group than among the probation group (74.9% *versus* 67.7%), but the difference is not very large. Among first-time offenders, probation seems to be more effective in reducing property offences than detention (62.6% *versus* 82.4% of re-offenders).

**\*68\*** Muiluvuori M.-L. (2001) : *Recidivism among people sentenced to community service in Finland.*

This study conducted in Finland compared re-offending among offenders sentenced to community service or to prison for a maximum of 8 months. The prison group was selected outside the experimental area. The distribution of sex, age, principal offence, time in prison and length of sentence in the prison group was similar to that of the community service group.

The findings show that recidivism after community service was slightly lower than after prison sentence during the 5-year follow-up period (60.5% *versus* 66.7%). The differences between the groups were not statistically significant.

**\*45\*** Petersilia J., Turner S., and Peterson J. (1986): *Prison versus probation in California: implications for crime and offender recidivism.*

Using a California sample of comparable prisoners and probationers, the authors compared rates of re-offending and estimated the amount of crime that was prevented when felons were imprisoned rather than placed on probation.

After statistical controls, the results show that the prisoners had higher recidivism rates than the probationers. In the two-year follow-up period, 68 percent of the prisoners were rearrested, as compared with 63 percent of the probationers, but this difference was not statistically significant. However, 51 percent of the prisoners were charged with new offences, compared to 38 percent of the probationers; and 47 percent of the prisoners were re-incarcerated, compared to 35 percent of the probationers. These last two differences are statistically significant. However, although prisoners' recidivism rates were higher than those of probationers, their new offences were no more serious.

**\*74\*** Smith L.G., Akers R.L. (1993): *A comparison of recidivism of Florida's community control and prison: a five-year survival analysis.*

This study examines the effectiveness of home confinement compared to imprisonment on recidivism. The re-arrest, reconviction, imprisonment, and recidivism survival of the first cohort of convicted felons sentenced to community control were tracked for nearly five years and compared to the recidivism of a partially matched group of convicted felons released from prison.

The findings show that recidivism rates and survival curves of the two groups are essentially the same. Approximately 4 out of 5 felony offenders sentenced to community control or prison recidivated during the five-year study (77.8% versus 78.6%).

**\*16\*** Weisburd D., Waring E., Chayet E. (1995): *Specific deterrence in a sample of offenders convicted of white-collar crimes.*

This study examines the impact of sanctions on the criminal careers of 742 offenders convicted of white-collar crimes. Using data on court-imposed sanctions and information on subsequent criminal behaviour provided by the Identification Bureau of the FBI, the authors assess the effect of imprisonment upon the official criminal records of these offenders.



Comparing prison and non-prison groups by matching on factors that led to their prison sanction, the results show that prison does not have a specific deterrent effect upon the likelihood of re-arrest over a 10.5-year follow-up period.

#### **4.4 Studies with four or more control variables**

The focus of the following studies is to examine differences in the recidivism of offenders who have received varying correctional sanctions. Most of these studies indicate no statistically significant differences in recidivism of offenders who are sentenced to a custodial *versus* a non-custodial sentence. Furthermore, the question is to know why there is no impact on their subsequent criminal activities. One possibility is that, prior to selection for the study, the offender groups differed in some unmeasured way (different levels of *a priori* risk of recidivism). Therefore, a marginal effect of one sanction may be suppressed. Another explanation for the failure to find an effect could be insufficient statistical power, due either to the small sample size or high variance in the measures of recidivism. Thus, the findings of the following studies must be considered with some caution.

**\*1002\*** Bondeson U.V.(1994 / 2002), Alternatives to imprisonment: intentions and reality.

This study examines the use of community-based sanctions in Sweden. A quasi-experimental design compares groups assigned to ordinary probation (N=138), probation with institutional treatment (considered as a custodial sanction) (N=127), and unsupervised conditional sentences (N=148). Offenders' personal and social backgrounds up to the time they were sentenced are described in detail. Data collection from official records began at the end of 1969 and the beginning of 1970. Information about offenders' prior record (nearly 40 variables in all) as well as their convictions during the follow-up period were collected. Recidivism data were collected from the Central Criminal Register and from the criminal records kept by the National Board of Excise. Comparisons have been made between the penalties taking into consideration the risk categories to which the persons belong.

The findings show that recidivism was more likely for those sentenced to probation with institutional treatment, less so for supervised probation, and least likely for the conditional sentence group, even after controlling for risk scores. Moreover, the effect of supervision varied according to both the degree of support versus control in the supervisor's behaviour, as well as the type of client. All in all, about 40 percent of

variance in reconvictions was explained. Furthermore, supervisors viewed probation as providing help, while offenders regarded it as control.

**\*32\*** Bonta J., Wallace-Capretta S., Rooney J. (2000) : *Can electronic monitoring make a difference? An evaluation of three Canadian programs.*

This Canadian study compares recidivism for three groups of male offenders : a group sentenced to electronic monitoring (EM) programs, a group of prison inmates who were released on parole, and a group sentenced to probation. In addition, EM offenders are compared with inmates and probationers matched for offence risk. Three samples of male offenders include: 262 EM participants; a group of 256 inmates; and 30 probationers. Data were gathered from a self-report questionnaire and correctional files.

The initial findings show that the EM group had significantly lower recidivism rates than both the parole and probation groups : 26.7% vs. 37.9% for parole (prisoners), and 33.3% for probation. Further analysis, however, revealed that these differences could be totally explained by differences in offender risk level. The authors conclude that it is not the EM programs that result in lower recidivism, but the selection of low risk offenders for EM. In other words, EM programs add little value to the more traditional sanctions, in particular to other forms of community control, as far as re-offending is concerned.

**\*20\*** Bonta J., Wallace-Capretta S., Rooney J. (2000) : *A quasi-experimental evaluation of an intensive rehabilitation supervision program.*

This Canadian study evaluates a cognitive-behavioural treatment program within the context of intensive rehabilitative supervision (IRS program) via electronic monitoring (EM). The experimental group consisted of 54 inmates released into the community under EM who were required to attend IRS program. Offenders of this first group were statistically matched on risk and needs factors to 100 inmates who did not receive such a treatment because it was not available in situ. The initial selection of the non-treated inmate group was based on the criteria used for identifying inmates for the IRS program. Data were obtained from prison and program records and questionnaires. This study was part of a larger evaluation of EM programs in Canada. This study does not, strictly speaking, compare re-offending after a custodial and a non-custodial sanction, but rather compares inmates who, after some time in confinement, qualified for non-custodial treatment (with EM), with those who remained in prison up to the end of their term. We decided to include it because the comparison of incarceration with some form of non-custodial supervision seems relevant to our topic, even if both groups shared some common experience with incarceration.

The recidivism rates were 31.5% for the IRS offenders and 31% for the control inmates. Low- and high-risk groups were constructed for both the treated offenders and the control inmates. A statistically significant interaction was found between treatment and risk level. Low-risk offenders who received treatment demonstrated higher recidivism rates than those not treated (32.3% *versus* 14.5%), whereas high-risk treated offenders showed lower recidivism rates, compared to those not treated (31.6% *versus* 51.1%). Findings illustrate the importance of matching treatment intensity to offender risk level, and ensuring that there is a treatment component in intensive supervision programs. (See also our comments in the Discussion chapter.)

**\*1005\*** Börjeson B. (1966), *Om Paföljders Verknningar* (On the effects of sanctions).

This study is an endeavour to elucidate empirically some aspects of the legal system of penalties in Sweden. The author compares the effects of conditional sentences, fines, determinate imprisonment, training school and youth imposed on young law-breakers aged 18 to 20 years. The various sanctions have been classified into two main categories: imprisonment and non-imprisonment. The persons included in this study were selected according to three criteria: (1) they were born in 1937-39, (2) they were sentenced for a serious crime after their eighteenth but before their twenty-first birthday, and (3) a severe sentence must have been meted out by the court. Comparisons between sanctions have been made taking into consideration risk of re-offending (nearly 40 variables in all), the follow-up period being three years. The sample included 101 defendants sentenced to a custodial and 315 to a non-custodial sanction.

The main finding shows a statistically significant difference in favour of non-custodial sanctions in every risk category (about 40 percent variance explained).

**\*23\*** Brennan P.A., Mednick S.A. (1994) : *Learning theory approach to the deterrence of criminal recidivism*.

This study tests a learning theory approach to criminal deterrence. Subjects were drawn from a total birth cohort of men born in Copenhagen (Denmark) between January 1944 and December 1947. The authors compared the effects of prison with those of fine and probation for offenders aged 18 years or older at the time of the arrest. In order to allow for a standard period of risk for the entire cohort, the authors examined only data through age 26 in this study.

The findings show that the type of sanction (prison *vs.* fine) has a significant effect on subsequent arrest rates only at the one-to-two offence level, otherwise, no significant differences in subsequent arrest rates were found at every other level of recidivism risk

(two to three offences, and higher). In the same way, no significant effects of the type of sanction (prison vs. probation) were found at all levels of recidivism risk when age, SES, and time in prison were controlled. Finally, continuous delivery of sanctions is more effective than intermittent delivery of sanctions in reducing future rates of offending. Criminal recidivism resumed if punishment is discontinued.

**\*2\*** DeYoung, D.J. (1997) : *An evaluation of the effectiveness of alcohol treatment, driver license actions and jail terms in reducing drunk driving recidivism in California.*

This study examines the effectiveness of alcohol treatment, driver license actions and jail terms in reducing drunk-driving recidivism. This quasi-experimental study examines the relationships between the sanctions that drivers convicted of driving-under-the-influence (DUI) receive and their subsequent reconviction of DUI, while statistically controlling for pre-existing differences among groups receiving different sanctions. Data were obtained from California motor vehicle agency records of all licensed drivers who were convicted of DUI in the state during 1990 and 1991.

The findings show that for first DUI convictions, combining alcohol treatment with either driver's license restriction or suspension was significantly associated with the lowest DUI recidivism rates during the 18-month follow-up period, compared to jail sanction alone or jail combined with license actions or alcohol treatment. More specifically, the first offender analyses show that subjects receiving jail have, on average, almost double the number of DUI re-convictions as those assigned to first offender treatment programs in addition to license restriction. The author concludes that license actions combined with alcohol treatment are the most effective in reducing DUI recidivism.

**\*31\*** MacKenzie D.L., Shaw J.W. (1993) : *The impact of shock incarceration on technical violations and new criminal activities.*

This study examines the behaviour of shock incarceration releasees during two years of community supervision, and compares their performance with that of similar offenders serving time on probation or parole. The authors compared offenders who were legally eligible for the shock program but who received prison and probation sentences, with those who went to the shock incarceration program.

In general, the shock offenders had significantly lower rates of arrests and convictions for new offences than parolees and probationers. Moreover, shock graduates had lower rates of revocations than parolees. However, the results should be interpreted with

caution because of the possibility of prior differences between the two groups, although there are strong arguments for assuming that the samples were indeed similar.

**\*56\*** MacKenzie D.L. (1991) : *The parole performance of offenders released from shock incarceration (boot camp prisons): a survival time analysis.*

This study compares the parole performance of male offenders who were released after successfully completing a shock incarceration program (N=74), to that of offenders who were serving time on probation (N=108) or parole after a period of incarceration (N=74). Data were gathered from the records of the Louisiana Department of Public Safety and Corrections, and from performance evaluations completed by parole and probation agents.

The findings show that prior incarceration, age, age at first arrest, and risk assessment score were related to recidivism, but type of sentence was not. No evidence was found that shock incarceration reduces recidivism, compared to prison or probation, even if the prevalence rates of arrests after a 12-month follow-up period are higher for the shock incarceration graduates (37.8%) than for the parolees (25.2%) and the probationers (28.2%).

**\*72\*** MacKenzie D.L., Brame R., McDowall D., Souryal C. (1995) : *Boot camp prisons and recidivism in eight states.*

This study examines recidivism among boot camp graduates in eight states (Florida, Georgia, Illinois, Louisiana, New York, Oklahoma, South Carolina and Texas) during community supervision. These recidivism patterns are assessed in light of how one or more comparison groups (probation or parole) in each state perform. Data were gathered for a 12-month period in half the states, and for 24 months in the other half.

The results suggest that those who complete boot camp do not inevitably perform either better or worse than comparison groups (probation or parole). However, re-offending among boot camp releasees was actually higher for those camps that emphasized physical activity and military training without any therapeutic component in their program.

**\*64\*** Roeger L.S. (1994) : The effectiveness of criminal justice sanctions for Aboriginal offenders.

This Australian study compares re-offending among Aboriginal offenders either sentenced to imprisonment or community-based sanctions (probation or community service). Three-and-a-half-year follow-up data were collected from the records of the South Australian Department of Correctional Services and the South Australian Police Department.

The findings show that after controlling for factors associated with recidivism, rates of re-offending do not differ between offenders serving time in prison and those given community-based sanctions.

**\*9\*** Savolainen J., Nehwadowich W., Tejaratchi A., Linen-Reed B. (2002) : *Criminal Recidivism Among Felony-Level ATI Program Participants in New York City*.

The purpose of this study is to evaluate the impact of an alternative-to-incarceration program on recidivism. The analyses examined three dimensions of re-offending: prevalence, incidence, and timing of re-arrest. The follow-up period in this study ranged from 6 to 12 months.

The results showed that the probability of recidivism is significantly higher among those sent to jail than among probationers.

**\*35\*** Spohn C., Holleran D. (2002) : *The effect of imprisonment on recidivism rates of felony offenders: a focus on drug offenders*.

Using 1993 data on offenders convicted of felonies (drug offenders, drug-involved offenders, and non-drug offenders) from the Jackson County Circuit Court (Kansas City, Missouri), recidivism rates for offenders sentenced to prison (N=301) and offenders placed on probation (N=776) have been compared.

The findings show that offenders sentenced to prison have significantly higher rates of recidivism. The four-year recidivism rates for prisoners and probationers were 82% versus 43% for drug offenders, 62% versus 48% for drug-involved offenders, and 57% versus 40% for non-drug offenders. Moreover, offenders sentenced to prison re-offend more quickly than offenders placed on probation. In particular, drug offenders sentenced to prison failed more quickly than drug offenders sentenced to probation throughout the four-year follow-up period, and the difference between the two groups increased over time. Finally, by the end of the follow-up period, about 65% of the

probationers had not been charged with any new offence, compared with only 20% of the prisoners.

**\*43\*** Tashima H.N. Marelich W.D. (1989) : *A comparison of the relative effectiveness of alternative sanctions for DUI offenders.*

This Californian study examined the relationship between various sanctions for driving under the influence of alcohol, and post-treatment driving records, subsequent accidents and convictions. Driving curtailment, through license restriction or suspension and with or without alcohol education, is also in relation to fines, jail days, and blood alcohol concentration (BAC). Whether the impaired driver was a first-time or a repeat offender has also been considered.

The findings show that first and second-time offenders receiving license suspension, either alone or in conjunction with educational alcohol programs, have significantly fewer post-treatment accidents than those receiving no licence suspension. Moreover, groups without licence control actions had the highest subsequent accident and conviction rates. On the contrary, first and second-time offenders sentenced only to short-term imprisonment, had higher subsequent accident and conviction rates than those sentenced to different sanctions, after a two-year follow-up period. For third-time offenders, all types of sanction are equally effective. Finally, for first and second-time offenders, license suspension with a rehabilitative alcohol program seems to be the most effective sanction to reduce driving under influence.

## 4.5 Summary

To facilitate the overview, all fully eligible studies are shortly summarized in the following Table 1. They appear grouped along methodological criteria (randomized controlled trials, natural experiments, matched-pair studies and quasi-experimental evaluations using >3 control variables).

Table 1: Characteristics of 23 fully eligible studies

N°	Study design	Custodial sanction	Non-custodial sanction	Offender type	Standard-limited time served	Specific crime	Follow-up period	Custodial impact	Non-custodial impact	Significant effect (p<.05)	Study Name
<i>Controlled randomized trials</i>											
1		Placement	Intensive supervision	Juveniles	no	no	24 months	0	0	n.s.	Barton W.H., Butts J.A. (1990) (#10)
2		Prison	Probation	Adults	no	no	12 months	0	1	sig.	Bergman G.R. (1976) (#91)
3		Prison	Community service	Adults	14 days	no	24 months	0 0	0 1	- n.s., for prevalence and incidence of arrests and convictions ,  -sig. improvement before/past arrest rate	Killias M., Aebi M., Ribeaud D. (2000) (#25)
4		Correction program	Restitution	Juveniles	no	no	22 months	0	0	n.s.	Schneider A.L. (1986) (#66)



Table 1 *continued*

N°	Study design	Custodial sanction	Non-custodial sanction	Offender type	Standard-limited time served	Specific crime	Follow-up period	Custodial impact	Non-custodial impact	Significant effect	Study Name
<i>Natural experiment</i>											
5		Prison	Suspended sentence	Adults	14 days	no	6 years	0	0	- n.s., for traffic offenders,  - n.s. for property offenders  - sig., for violent crime offenders	Van der Werff C. (1979) (#124)
<i>Matched-pair design studies</i>											
6		Detention	Probation	Juveniles	no	no	5 years	0	1	sig.	Kraus J. (1974) (#76)
7		Prison	Community service	Adults	8 months	no	5 years	0	1	n.s. (p<.10)	Muiluvuori M.-L. (2001) (#68)
8		Prison	Probation	Adults	no	no	24 months	0	1	n.s. (p<.10)	Petersilia J., Turner S., and Peterson J. (1986) (#45)
9		Prison	Home confinement	Adults	no	no	5 years	0	0	n.s.	Smith L.G., Akers R.L. (1993) (#74)
10		Prison	No prison	Adults	no	no	10.5 years	0	0	n.s.	Weisburd D., Waring E., Chayet E. (1995) (#16)

Table 1 *continued*

N°	Study design	Custodial sanction	Non-custodial sanction	Offender type	Standard-limited time served	Specific crime	Follow-up period	Custodial impact	Non-custodial impact	Significant effect	Study Name
<i>Studies with four or more control variables</i>											
11		Probation with institutional treatment	Probation, Conditional prison sentence	Adults	no	no	24-36 months	0	1	sig.	Bondeson U.V. (1994 / 2002) (#1002)
12		Prison	Electronic monitoring and rehabilitation	Adults	no	no	12 months	0	1	sig.	Bonta J., Wallace-Capretta S., Rooney J. (2000) (#32)
13		Prison	Electronic monitoring and rehabilitation	Adults	no	Low- and high-risk offenders	12 months	0 1	1 0	- sig., for high-risk offenders - sig., for low-risk offenders	Bonta J., Wallace-Capretta S., Rooney J. (2000) (#20)
14		Prison	Non imprisonment	Adult	no	no	36 months	0	1	sig.	Börjeson B., (1966) (#1005)
15		Prison	Probation	Adults	no	no	Not clearly defined	0	0	n.s.	Brennan P.A., Mednick S.A. (1994) (#23)
16		Prison	Alcohol treatment and license suspension	Adults	no	Drunk-driving	18 months	0	1	sig.	DeYoung, D.J. (1997) (#2)

Table 1 *continued*

N°	Study design	Custodial sanction	Non-custodial sanction	Offender type	Standard-limited time served	Specific crime	Follow-up period	Custodial impact	Non-custodial impact	Significant effect	Study Name
<i>Studies with four or more control variables</i>											
17		Shock incarceration	Probation	Adults	no	no	24 months	1	0	sig.	MacKenzie D.L., Shaw J.W. (1993) (#31)
18		Shock incarceration	Probation	Adults	no	no	12 months	0	1	n.s. (p<.10)	MacKenzie D.L. (1991) (#56)
19		Shock incarceration	Probation	Adults	8-State comparison	no	12/24 months	0	0	n.s.	MacKenzie D.L., Brame R., McDowall D., Souryal C. (1995) (#72)
20		Prison	Community service	Aboriginal Adults	no	no	3.5 years	0	0	n.s.	Roeger L.S. (1994) (#64)
21		Prison	Probation	Adults	no	no	6-12 months	0	1	sig.	Savolainen J., Nehwadowich W., Tejaratchi A., Linen-Reed B. (2002) (#9)
22		Prison	Probation	Adults	no	Drug offenders and others	4 years	0	1	sig.	Spohn C., Holleran D. (2002) (#35)
23		Prison	Alcohol treatment and license suspension	Adults	no	Drunk-driving	24 months	0	1	n.s. (p<.10)	Tashima H.N., Marelich W.D. (1989) (#43)

In Table 2, the same 23 studies have been grouped according to outcome by methodology. Two studies (#25 and #20) have been counted twice, and one study (#124) has been counted three times, since they provided more than one outcome measure of re-offending. Thus, 27 comparisons have been counted in the following Table 2. Two studies show significantly lower re-offending rates following custodial sanctions, whereas 11 comparisons show significantly better outcomes for non-custodial sanctions. For 14 studies, there was no significant difference, although results were somewhat more favourable to non-custodial sanctions in four cases.

Table 2: Analysis of study outcomes by methodology (N=27 comparisons)

Results of comparison	Study design				Overall
	Controlled randomized trials	Natural experiment	Matched-pair design studies	Studies with four or more control variables	
<i>Favourable to custodial sanction + sig.</i>				2	2
<i>Favourable to custodial sanction + n.s. (.05 &lt; p &lt; .10)</i>					0
<i>No difference</i>	3	2	2	3	10
<i>Favourable to non-custodial sanction + n.s. (.05 &lt; p &lt; .10)</i>			2	2	4
<i>Favourable to non-custodial sanction + sig.</i>	2	1	1	7	11

There seems to be some association between methodological power and outcome, insofar as matched pair studies and, particularly, studies with control of several variables, yielded more results favouring non-custodial sanctions. Indeed, seven out of eleven studies where custodial sanctions were associated with significantly higher rates of re-offending, belong to the weakest category. Given that custodial sanctions are systematically imposed on offenders with higher risks of recidivism, it seems plausible that the less pre-existing differences between groups are being controlled, the more outcomes will favour “alternative” sanctions. If only studies meeting higher methodological standards are considered, the results are more

balanced, even if three in eight comparisons still favour non-custodial sanctions. If three Californian controlled experiments comparing more traditional with more “open” forms of residential treatment (Palmer 1971/1974, Lamb and Goertzel 1974, Empey and Steven 1971) were included, this bias would become even more visible since two out of four comparisons were in favour of more traditional facilities, one favoured treatment in the “open” structure, and one was undecided (see coding protocols \*1003\*, \*1004\*, \*1006\* in *Appendix III*). Despite these methodological reservations, it is true that even among the five strongest (experimental) studies, three in five comparisons resulted in favour of non-custodial sanctions. Current knowledge does not allow deciding whether the impact of methodological rigour is less important than we suspect, or whether some sort of Hawthorn (or placebo) effect is at work here (see below, p. 41). For this reason, it is important to look beyond “vote counts”, as in Table 2, and to consider effect sizes as a more nuanced measure of combined effects of “alternative” versus custodial sanctions.

## 4.6 Meta-analysis<sup>2</sup>

Meta-analysis is an efficient tool to identify combined effects of a certain intervention across a multitude of studies. However, its internal validity never goes beyond the original studies. Therefore, conducting a meta-analysis on studies with systematically biased outcomes can only yield misleading results. If the mission of the Campbell Collaboration Crime and Justice Group, namely to produce and distribute World-wide reliable knowledge about all sorts of interventions, is to be taken seriously, limiting any meta-analysis to high quality studies is essential. In the present case, this implies that only studies can be included where subjects have been randomly assigned to different sanctions, i.e. where the possibility of uncontrolled differences between offenders sent to prison and those sentenced to alternative sanctions is minimal. This also means that quasi-experiments cannot be considered, since the possibility cannot be ruled out that decision-makers (i.e. usually judges) decide using criteria that remain uncontrolled, but that are likely to be related to re-conviction. For these reasons, the following meta-analysis has been limited to the four randomized experiments and the one natural experiment that have been identified. The outcome measure is new offences known to the police or reconviction during the follow-up period, as reported by the authors.

Given the limitations of the available data, we had to transform them before conducting the meta-analysis. As most studies report dichotomous outcomes (proportion of re-offenders), we have first transformed these original outcomes into Odds Ratios (OR), according to recommendations in the literature (Lipsey and Wilson, 2001; Wolf, 1986; Glass, McGaw and Smith, 1981), and then into an appropriate Standardized Mean Difference statistic (our effect size index). Whenever experimental and control groups are contrasted, a positive effect size means that the non-custodial sanction is more effective than the custodial sanction in preventing recidivism. For our purposes, we use both standardized mean differences (Tables 3b and 3e) and odds ratios (Tables 3c and 3f).

In three of the five studies (Barton, Bergman, Schneider) listed in Table 3a, only one effect size has been reported on which a meta-analysis has been feasible. In one study (Killias et al.), two effect sizes are presented, and three in the van der Werff experiment (Table 3a). Since the results of the meta-analysis favoured the null hypothesis, the strongest effect sizes have been used consistently as a conservative way to minimize the chance of obtaining a non-significant outcome. In order to have an uniform definition of outcome in all studies, all these effect sizes are based on new offences known to the police; this led to exclude effect sizes based on re-convictions, as e.g. in the Killias et

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<sup>2</sup> The authors are deeply indebted to Dr. David Wilson for his assistance with the present meta-analysis.

al. study. Beyond what is indicated in Table 3a, the Killias et al. experiment showed also a significant improvement of arrest rates among persons assigned to community work; this effect size could not be used, however, given that no other study presented comparable outcomes. In the case of the van der Werff study, the analysis has been conducted both with all offenders (Tables 3b/3c) and with effect sizes limited to violent offenders (Tables 3d/3e) where a significant effect had been observed. No such special analysis has been conducted with property and traffic offenders since effect sizes were nearly or absolutely zero.

Table 3a: Individual recidivism effect sizes (based on new offences known to the police, unless otherwise indicated) of 5 studies included in the meta-analysis (all types of offenders)

<b>Trial</b>	<b>n/N experimental</b>	<b>n/N control</b>	<b>Biased Effect Size index</b>	<b>Unbiased Effect Size index</b>
BERGMAN	6/42	22/67	0.593	0.589
KILLIAS et al.	28/84	15/39		
- new convictions			0.129	0.129
- new arrests			0.123	0.122
SCHNEIDER	46/86	56/95	0.122	0.122
Van der WERFF:				
- all offenders	426/946	452/974	0.031	0.031
- violent offenders	87/165	98/156	0.229	0.228
- property offenders	64/94	70/108	-0.081	-0.081
BARTON/BUTTS <sup>3</sup>	3.58 (160)	3.69 (326)	- 0.019	- 0.019

<sup>3</sup> For this study, only means are given.

Based on these five studies, the following standardized mean differences were obtained

Table 3b: Comparison of non-custodial versus custodial sanctions on recidivism (all types of offenders), standardized mean differences

No. of obs =	5	Homogeneity Analysis				
Minimum obs =	-.020	Q = 4.65				
Maximum obs =	0.593	df = 4				
Weighted SD =	0.090	p = 0.32541				

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	Mean	-95%CI	+95%CI	SE	Z	P
Fixed effect	0.04263	-0.03958	0.12484	0.04195	1.01626	0.30950
Random effect 1	0.05144	-0.05265	0.15554	0.05311	0.96858	0.33275
Random effect 2	0.04263	-0.03958	0.12484	0.04195	1.01626	0.30950

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1 Random effects variance component (method of moments) = 0.00236  
2 Random effects variance component (full information ML) = 0.00000

Table 3c: Comparison of non-custodial versus custodial sanctions on recidivism (all types of offenders), odds ratios

No. of obs =	5	Homogeneity Analysis				
Minimum obs =	.96437	Q = 4.65				
Maximum obs =	2.933	df = 4				
Weighted SD =	.	p = 0.32541				

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	Mean	-95%CI	+95%CI	SE	Z	P
Fixed effect	1.08039	0.93072	1.25412	.	1.01626	0.30950
Random effect 1	1.09780	0.90892	1.32594	.	0.96858	0.33275
Random effect 2	1.08039	0.93072	1.25412	.	1.01626	0.30950

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1 Random effects variance component (method of moments) = 0.00775  
2 Random effects variance component (full information ML) = 0.00000

Results are the exponent of computed values (i.e., results are odds-ratios)

The results in Tables 3b/3c summarize the results for each of the five studies considered, as well as for all studies together. The results show that custodial and non-custodial sanctions do not differ significantly regarding recidivism beyond a random effect.

Since, among the five studies included in the meta-analysis, the one by van der Werff (#124) used by far the largest sample, we have conducted it also by using only her results on violent offenders (that were significantly positive for non-custodial



sanctions). Individual effect sizes appear in Table 3a, and the results are given in Tables 3d/e.

Based on these five studies, the following standardized mean differences were obtained:

Table 3d: Comparison of non-custodial versus custodial sanctions on recidivism (only violent offenders in the van der Werff study, all offenders in the remaining experiments), standardized mean differences

No. of obs =	5	Homogeneity Analysis					
Minimum obs =	-.020	Q = 5.65					
Maximum obs =	0.593	df = 4					
Weighted SD =	0.153	p = 0.22655					

  

	Mean	-95%CI	+95%CI	SE	Z	P
Fixed effect	0.11164	-0.01482	0.23810	0.06452	1.73026	0.08358
Random effect 1	0.13617	-0.02600	0.29833	0.08274	1.64575	0.09981
Random effect 2	0.13032	-0.02180	0.28244	0.07761	1.67913	0.09313

  

1 Random effects variance component (method of moments) = 0.00987  
 2 Random effects variance component (full information ML) = 0.00667

Table 3e: Comparison of non-custodial versus custodial sanctions on recidivism (only violent offenders in the van der Werff study, all offenders in the remaining experiments), odds ratios

No. of obs =	5	Homogeneity Analysis					
Minimum obs =	.96437	Q = 5.65					
Maximum obs =	2.933	df = 4					
Weighted SD =	.	p = 0.22655					

  

	Mean	-95%CI	+95%CI	SE	Z	P
Fixed effect	1.22445	0.97348	1.54013	.	1.73026	0.08358
Random effect 1	1.28016	0.95394	1.71793	.	1.64575	0.09981
Random effect 2	1.26665	0.96124	1.66909	.	1.67913	0.09313

  

1 Random effects variance component (method of moments) = 0.03247  
 2 Random effects variance component (full information ML) = 0.02193

Results are the exponent of computed values (i.e., results are odds-ratios)

Both mean effect sizes are positive (favouring non-custodial sanctions), but not statistically significant. The mean odds ratios for Table 3c would convert into a percentage difference of 50 percent of recidivism for the custodial group and 48 percent for the non-custodial group; this difference corresponds to an improvement by 2 percentage points. Given the small size of three among the five studies, the overall statistical power at the meta-analytic level is still low. Given the general homogeneity of the results, the fixed and random effects results are essentially the same.

The results in Tables 3d/3e are closer to statistical significance, the odds ratio of 1.22 corresponding to, respectively, 50 and 45 percent recidivism in the two groups. It should be kept in mind, however, that this result is observed only if, in the study of van der Werff, traffic and property offenders are excluded from the analysis. Since the other studies include several categories of offenders (and not just violent ones), considering only violent offenders from the van der Werff data seems questionable, however. Therefore, the safer conclusion seems to be that custodial and non-custodial sanctions do not differ in terms of re-offending beyond random effects, as suggested by Tables 3b to 3e.

Of course, a meta-analysis based on five studies can easily be criticized for being “too” selective. On the other hand, the results of the meta-analysis illustrate also the limits of the “vote count” method, as used in Table 2, since the advantage in “votes” in favour of non-custodial sanctions vanishes in the meta-analysis. Both, however, suggest that differences between custodial and non-custodial sanctions in terms of re-offending are modest, although slightly in the direction favourable to non-custodial sanctions. The results of the meta-analysis are more in line with the “vote-counts” approach used in Table 2, once only controlled and natural experiments are being considered. This match offers further support to the decision to restrict the meta-analysis to studies of high internal validity.

## 5. Discussion

The comparison of the effectiveness of custodial and non-custodial sanctions has been a preoccupation of criminological research over more than one century. Hundreds of studies tried to find out what sanction may be the most effective in reducing recidivism. Although results did not always point in the same direction, it seems that effects of custodial and non-custodial sanctions do differ the less, the more relevant independent variables are being controlled for. For this reason, a systematic review summarizing global knowledge about the effectiveness of sanctions on re-offending was thought to be helpful for policy makers and legislators. The present literature review has been undertaken with the purpose of offering a more balanced account, particularly by locating as many high quality studies on this subject as possible. Unfortunately, only four controlled and one natural experiment could be identified that qualified for a meta-analysis. The results are rather discouraging for those who had argued over years that imprisonment is damaging. Of course, we might have been able to present results that would have been far more positive for alternative sanctions if we had included all those studies where pre-existing differences were, according to current standards applied by the several Campbell Collaboration networks, insufficiently controlled for. In other words, we might have found more convincing evidence of a damaging effect of custodial sentences by including many weaker studies, but at the price of reaching presumably wrong conclusions. Therefore, if a meta-analysis is to be conducted at all (we recognize that this may be debatable, particularly because of the great heterogeneity of the sanctions, programs and groups of offenders involved), the solution could only be to limit it to studies that offer a reasonable guarantee of high internal validity. Such an assumption is, as explained throughout this report, only possible in the case of controlled (randomised) trials and natural experiments where the criterion was close to random (such as the date of a royal pardon). Unfortunately, this leaves only five studies left for the meta-analysis, but we feel that internal validity ought to be a higher priority than statistical power with biased data.

As explained throughout this report and as Walker, Farrington and Tucker (1981) have observed 25 years ago, quasi-experimental studies using statistical control methods are unable to take into account all the variables which could influence sentencing judges as well as later recidivism. This is particularly true if sentencing judges (or bodies) or correctional officers are told to give particular attention to the offender's need for residential treatment, as in the programs evaluated by Bondeson (#1002) and more generally in most continental "alternative" arrangements, since "treatment" or "prison"

groups likely include, under such circumstances, a higher proportion of offenders with risks of re-offending far above average. This may be one of the reasons why only a small proportion of total variance in re-offending has been explained in multivariate analyses. Among the few studies that provide a percentage, the proportion of variance explained usually remained in the range of 20 percent. Bondeson (#1002) and Björeson (#1005) who reached 40 percent are noteworthy exceptions, probably due to the fact that they controlled for an unusually large number of independent variables (about 40). Therefore, more than seventy (or, in the two cases just mentioned, nearly sixty) percent of variance in re-offending may be due to variables that remain unknown or that have not been controlled for.

Moreover, samples of most studies are rather small, i.e. of less than one hundred offenders. Therefore, whenever a researcher tries to control more than two variables, any statistical method loses its power. For this reason, statistical tests are too rarely significant, and the outcomes vary widely. In the same line, most studies have compared post-sanction recidivism rates across different sanctions, but have not compared levels of “improvement”. However, samples of offenders undergoing different sanctions may, despite randomisation, have different offending rates before the intervention. The best way to deal with this problem would, obviously, be to compare relative improvement following the sanction. Only few studies have chosen to do that, among which were Empey and Steven (1971) as well as Killias, Aebi and Ribeaud (2000). Both were able to show that prevalence of offending decreased (even substantially) after any type of sanction or intervention. In sum, sanctions (of whatever kind) may not be “damaging” (in the sense of increasing subjects’ propensity to offend), but simply be more or less helpful in reducing re-offending.

The importance of pre-intervention characteristics is well illustrated by the data presented by McKenzie *et al.* (1995, #72) in her evaluation of boot camps in comparison to other sanctions in seven States (Table 4). During the observation period, the South Carolina boot camp program underwent changes in the selection of persons assigned to it, but no program modifications were noted. Thus, during the first three years of the study, offenders sentenced to regular probation were selected for boot camps (old style). During the last three years however, only prisoners (not probationers) were sent to this program (new style). For the analysis, these two groups were treated separately and recidivism rates were statistically controlled for variables that are known to influence recidivism. The estimated failure rates of the two groups differed substantially after statistical control, although the boot camp program did not change during the whole observation period. Results like this underline that differences in re-

offending may be due to the composition of groups and other uncontrolled variables, rather than to the direct influence of the sanction.

Table 4: Recidivism rates after old boot camp (OBC), new boot camp (NBC) or probation (PROB) in South Carolina

<b>Contrasts of sanction</b>	<b>Recidivism</b>	
	<i>Coefficient</i>	<i>Sig.</i>
<i>NBC vs. OBC</i>	- .660	sig.
<i>PROB vs. OBC</i>	- .387	n.s.
<i>PROB vs. NBC</i>	+ .273	n.s.

The first line in Table 4 indicates that the old boot camp completers (OBC) have significantly higher re-arrest rates than the new boot camp completers (NBC). As indicated in the second and third lines, probationers have a lower recidivism rate than the old boot camp completers, but a higher recidivism rate than the new boot camp completers. Thus, the larger the pre-intervention differences between the groups, the stronger the bias of the effect size. Offenders sentenced to prison may indeed have committed more serious crimes and may have a longer criminal history than those sentenced to probation. Therefore, the former will probably re-offend more often than the latter, regardless of eventual sanction effects.

Finally, our review has offered an opportunity to make a number of methodological observations that may be helpful for future evaluations of “alternative” sanctions or programs and that can be summarized in the following nine points:

- (1) Not all studies have dealt with the same type of offenders. For instance, some studies included traffic offenders, others property offenders, and some also violent offenders or drug users. Of course, risks to re-offend are far from being the same across these groups.

In the McKenzie’s study, it is particularly surprising that prisoners assigned to a boot camp were more successful than probationers (relative recidivism rates were 40.6 percent *versus* 62.8 percent). This suggests that there must have been an interaction at work between type of offenders and type of sanctions.

In their study, Bonta et al. (2000, #20) verified the presence of interaction between type of offenders and type of sanctions in the rehabilitation and intensive supervision programs. As Table 5 shows, intensive supervision (ISP) seems to be useful for high risk offenders, but harmful for low risk offenders.

Table 5: Recidivism rates after intensive supervision program (ISP) *versus* prison

	<b>Sanction</b>	
<b>Risk level</b>	<i>ISP</i>	<i>Prison</i>
<i>Low</i>	32.3%	14.5%
<i>High</i>	31.6%	51.1%

- (2) The length of the observation period differs from study to study. At the same time, we know that recidivism rates do not develop in a linear way. Thus, results can depend on the length of the observation period. In the randomised studies, the observation time varied between 12 and 24 months. Experts in the field of recidivism have always insisted that this was too short an observation period, and that the minimal standard observation period ought to be 36 months at least. Only the van der Werff study (1979, #124) used a follow-up period of six years.
- (3) Outcome measures used to estimate recidivism are not always valid in the same way. For instance, some authors define recidivism as the prevalence of re-incarceration. Under continental law, this indicator of re-offending has the advantage that recidivism is only taken into account when a new offence is serious enough to warrant for a new custodial sentence; in the United States, many offenders, however, are re-incarcerated because of technical violations of parole. In any case, re-incarceration mostly depends on the previous sentence imposed and the criminal history of the offender. In most studies, re-offending has been measured through the prevalence of post-intervention re-convictions or re-arrests. Left alone that questionnaires of self-reported delinquency were very rarely used (Barton and Butts, 1990, #10, being one of the rare exceptions), the simple prevalence (“yes/no”) of arrests or convictions after an intervention may mask important variations in the frequency of offending (“incidence rates”) and relative improvement following different sanctions.

- (4) Custodial sanctions vary greatly in duration and type. On the one hand, custodial sanctions include prison, jail and boot camp programs with inmates serving sentences of very different length. Experimental and most of the A-studies are, however, limited to very short custodial sanctions, since “alternative” sanctions are being envisaged mostly as a substitute for relatively short sentences. Our review, therefore, does not cover longer custodial sentences. Given the century-old dispute about the damaging effects of “short” custodial sanctions, this limitation of our review may be less relevant, however. Intuitively, it seems plausible that “prisonisation” effects are more frequent after custodial sentences of some length. Smith, Goggin and Gendreau (2002) compared recidivism by length of confinement, concluding that the longer the time served in prison, the higher the probability of re-offending. Given the possibility of many confounding factors that were possibly not adequately controlled in many among the reviewed studies, this conclusion may not remain unchallenged, however.
- (5) The diversity of non-custodial sanctions is no less impressive. They include an extended continuum, ranging from fines, community service, probation, intensive probation, and house arrest to electronic monitoring. Some of these sanctions may even have opposite effects on re-offending. Taking into account that many of these non-custodial sanctions have been developed as “alternatives” to incarceration to overcome “damaging” effects of prison experiences, it is not impossible, however, to look whether or not they produce, together, less undesirable side-effects compared to custodial sentences.
- (6) Several sanction programs include rehabilitation services such as social therapy, medical and psychiatric assistance, or extensive general counselling. In the case of short custodial or non-custodial sentences, such as those included in our review, intensive therapeutic components may be exceptional, however.
- (7) Our 23 selected studies have been prepared over a period of 45 years. During all these years, the way sentences are executed has changed about as much as the types of available “alternative” sanctions. Therefore, older studies are of questionable external validity to assess recent programs. In the same way, results obtained in the United States can not automatically be generalized to the rest of the World, particularly when American experts are reluctant about

generalizing outcomes across their own country. Of course, the external validity of European studies is no less questionable.

- (8) Usually, lower re-offending rates among those sentenced to an “alternative” sanction were, whenever observed, attributed to the fact that these offenders were not separated from their work and family life and had, therefore, better opportunities to integrate. However, the evidence is extremely limited in this respect (Lamb and Goertzel 1974, Killias, Aebi and Ribeaud 2000) and does not necessarily confirm this assumption. Given the often extremely short duration of custodial sentences compared to “alternative” sanctions, it seems plausible that any “prisonisation” effect has been limited at worst. In the case of randomised controlled trials, it would be easy, however, to conduct later follow-up studies including, beyond measures of re-offending, any kind of indicators of social integration as they could routinely be found in the files of income revenue services, such as family disruption, unemployment, mental health, support by social welfare agencies, debts, revenues and resources. Such data would be highly relevant in assessing any negative long-term effects on integration of custodial compared do “alternative” sanctions. Given the wide-spread rhetoric on “damaging” effects of custodial sanctions on these levels, it is rather surprising that, apparently, almost no data have been collected on such outcomes.
- (9) To the extent that, in randomised controlled trials, lower re-offending rates have been observed after “alternative” compared to custodial sanctions, it can not be ruled out that something like a Hawthorne or a “*placebo*” effect<sup>4</sup> has been at work. Indeed, persons convicted to a custodial sanction who get the “chance” to serve it under the form of an “alternative” get, in some way, a second chance which, in turn, may favourably affect their attitudes (as observed by Killias, Aebi and Ribeaud 2000). As experiments on cooperation between unrelated individuals (Fehr and Rockenbach, 2003) has shown, the prevailing self-interest approach in the behavioural sciences has serious shortcomings because it overlooks negative effects of sanctions on “altruism”. Indeed, sanctions perceived as fair do not affect subjects’ willingness to cooperate, whereas sanctions resented as unjust or unfair destroy altruistic cooperation almost completely. The sanctions perceived as “fair” (in practice, this probably equals “better than expected”) increase

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<sup>4</sup> It may be debatable whether we are dealing here with a Hawthorn or a placebo effect. We think it is more appropriate to speak about a Hawthorn effect, since subjects in the control group did not get a « placebo ». Since this distinction does not seem to have practical bearings in the present context, we use both terms simultaneously.



willingness to cooperate, matches similar results on reduced rates of re-offending as a result of attitude change. Such outcomes have been observed in studies on attitudes influenced by cognitive-behavioural treatment (Henning and Frueh, 1996; Vennard, Hedderman and Sugg, 1997) or by “fair” procedures (Paternoster, Bachman, Brame and Sherman, 1997).

In order to cope with possible Hawthorn or “placebo” effects, the obvious answer, in the medical field, would be to organize double-blind trials, an option that will be unavailable in the field of criminal justice for obvious reasons. It is surprising, however, that the possibility of such effects has, so far, found very little attention in the criminal justice literature.

- (10) Obviously, the most serious shortcoming of the current body of relevant studies is the lack in experimental designs among evaluations of correctional programs. Researchers in general and particularly the Campbell Collaboration Crime and Justice Group should, over the next years, award high priority to urging Governments to insist on experimental research designs whenever they implement, on an experimental basis, new sanctions or new programs.

## 6. Conclusions

After this literature review, we are, as have previously been Smith, Goggin and Gendreau (2002) after their review, unable to say whether non-custodial sanctions are more effective to prevent re-offending than custodial sanctions. Whether treatment and rehabilitation are more successful than mere surveillance and incarceration, or whether all is a matter of assigning offenders to specific sanctions (Palmer 1974), has been beyond the scope of our review.

In the future, it will be important to develop evaluation standards in the field of research on re-offending in order to improve the quality of trials. Randomised controlled trials ought to be preferred whenever possible, not only by researchers, but also by policy-makers. Without random assignment of offenders to either one of two sanctions to be compared, it will never be possible to conclude that the differential treatment is the cause of differences in offenders' subsequent behaviour. Randomised controlled experiments also allow considering later outcomes beyond re-offending, and even with respect to variables, such as health and social integration, whose relevance had not been anticipated at the time the trial started.

Sceptics tend to reply by pointing to ethical, practical or legal difficulties in conducting randomised controlled trials. Having been associated with experimental trials in the field of corrections over more than a decade in Switzerland, we may reply that, in our experience with correctional services, convicted offenders participating in new programs and policy-makers, random assignment has many advantages not only for researchers, but also for staff and decision-makers operating in the field. Random assignment is often easier to justify than any kind of choice on the grounds of personal characteristics, merits or institutional constraints. As far as legal obstacles are concerned, the Swiss parliament adopted, in 1971, a section in the penal code (article 397bis par. 4) allowing the Government to introduce, on an experimental basis, i.e. for a limited number of offenders and for a certain period of time, innovative sanctions and correctional arrangements beyond what the penal code provides. Thus, offenders who are eligible for an "innovative" program may, at any time, refuse and claim to be treated "according to the law" (and go to prison); however, no one is entitled to claim to become part of an experiment that is, by essence, limited in scope. Therefore, no legal obstacle complicates randomisation among those who are eligible for and who volunteer in any "experiment". Similar provisions have been enacted in many countries where new sanctions have been introduced as a temporary and a more or less "experimental" arrangement. Therefore, experimental evaluations should have been no less feasible.

Finally, ethical arguments seem to be quite odd as long as no evidence has shown that “new” sanctions or programs produce better results than traditional ones, or that they are at least not damaging. No one encourages pharmaceutical firms to sell promising new products before adequate testing through randomised controlled trials. Why should new correctional programs be “sold” to participants as long as their effects have not been adequately tested?

The absence of firm conclusions of our systematic review should not necessarily be taken as bad news. Criminal justice policy makers obviously have to consider many choices and constraints, and it may be good to know that, in terms of rehabilitation, short confinement does not generally fare worse than “alternative” sanctions. Thus, considerations of costs (including for partners and children of offenders), equity (for example, towards victims of violent partners) and consistency in sentencing can be awarded due attention without risk of producing important collateral damages in the biographies of offenders. In the end, criminal law and procedure are searching for equity, and decisions on sentences and correctional arrangements should not be based on treatment considerations as long as there is no evidence of beneficial or damaging effects. Our review suggests that such effects are limited at best (or worst), at least as far as confinement is relatively short in duration.

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## ***Bibliography of the systematic review***

Please note that all studies with a number between **\*asterisks\*** have been abstracted using our coding protocol. They are presented in the appendix.

### **A. The 23 eligible studies (A-studies)**

Studies listed under 1 and 2 reach level 5 on the scale by Sherman et al. (1997). Studies listed under 3 and 4 reach higher methodological standards among the level 4 studies (more than three control variables and higher statistical standards).

#### **1. Controlled randomized trials**

- \*10\* Barton W.H., Butts J.A., "Viable options: intensive supervision programs for juvenile delinquents", *Crime and Delinquency* 36/2 (1990), 238-256
- \*91\* Bergman G.R., *The evaluation of an experimental program designed to reduce recidivism among second felony criminal offenders*, Wayne State University, Detroit (Mich.), PhD dissertation (77-9368) 1976
- \*25\* Killias M., Aebi M., Ribeaud D., "Does community service rehabilitate better than shorter-term imprisonment ? : Results of a controlled experiment", *Howard Journal of Criminal Justice* 39/1 (2000), 40-57
- \*66\* Schneider A.L., "Restitution and recidivism rates of juvenile offenders: results from four experimental studies", *Criminology* 24/3 (1986), 533-552

#### **2. Natural experiment**

- \*124\* Van der Werff C., *Speciale Preventie*, Den Haag (NL): WODC, 1979

#### **3. Matched-pair design studies**

- \*76\* Kraus J., "A comparison of corrective effects of probation and detention on male juvenile offenders", *International Journal of Offender Therapy and Comparative Criminology* 25/2 (1974), 130-138
- \*68\* Muiluvuori M.-L., "Recidivism among people sentenced to community service in Finland", *Journal of Scandinavian Studies in Criminology and Crime Prevention* 2/1 (2001), 72-82
- \*45\* Petersilia J., Turner S., and Peterson J., *Prison versus probation in California: implications for crime and offender recidivism*, 1986
- \*74\* Smith L.G., Akers R.L., "A comparison of recidivism of Florida's community control and prison: a five-year survival analysis", *Journal of Research in Crime and Delinquency* 30/3 (1993), 267-292

- \*16\* Weisburd D., Waring E., Chayet E., "Specific deterrence in a sample of offenders convicted of white-collar crimes", *Criminology* 33/4 (1995), 587-607

#### 4. Studies with four or more control variables

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- \*32\* Bonta J., Wallace-Capretta S., Rooney J., "Can electronic monitoring make a difference? An evaluation of three canadian programs", *Crime and Delinquency* 46/1 (2000), 61-75
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# *Appendixes*

*I) Protocol of the systematic review*

*II) 23 coding protocols of A-studies*

*III) 27 coding protocols of B-studies*

*IV) Comments on 105 B-studies*



# *Appendix I*

*Protocol*

*of the systematic review*

# *Appendix II*

*23 coding protocols  
of A-studies*

# *Appendix III*

*27 coding protocols  
of B-studies*

# *Appendix IV*

*Comments on*

*105 B-studies*