Report prepared for Brå by Patrice Villettaz, Gwladys Gilliéron and Martin Killias

The effects on re-offending of custodial versus non-custodial sanctions

National Council for Crime Prevention

The effects on re-offending of custodial versus non-custodial sanctions

An updated systematic review of the state of knowledge

Brå – a centre of knowledge on crime and measures to combat crime

The Swedish National Council for Crime Prevention (Brottsförebyggande rådet – Brå) works to reduce crime and improve levels of safety in society by producing data and disseminating knowledge on crime and crime prevention work and the justice system´s responses to crime.

This document is an update of a systematic review published in 2006 on the website of the Campbell Collaboration Crime and Justice Group
Authors of the first report: Patrice Villettaz, Isabelle Zoder, Martin Killias

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Foreword

Many aspects need to be taken into consideration when criminal sanctions are designed and implemented. One recurring challenge is that of finding the right balance between on the one hand ensuring a sufficient response to crime and, on the other, the risk for negative consequences associated with incarceration. One core issue in this regard is the tendency to reoffend found among those convicted of offences. This systematic review examines the best available research in order to answer the question: are custodial or non-custodial sanctions more beneficial in terms of their effects on reoffending?

There are never sufficient resources to conduct rigorous evaluations of all the crime prevention or criminal justice measures employed in an individual country such as Sweden. Nor are there resources to conduct scientific studies of all the effects produced by e.g. different measures intended to combat and respond to crime. For these reasons, the Swedish National Council for Crime Prevention (Brå) has commissioned distinguished researchers to conduct a series of international reviews of the research published in these fields.

This report presents a systematic review of the effects of custodial versus non-custodial sanctions on reoffending. The work has been conducted by Dr. Patrice Villettaz of the University of Lausanne, Dr. Gwladys Gilliéron of the University of Zürich, and Professor Martin Killias of the University of St. Gallen.

The study follows the rigorous methodological requirements of a systematic review. The analysis combines the results from a substantial number of studies that are considered to satisfy a list of empirical criteria for measuring effects as reliably as possible. Even a comprehensive review of this kind cannot provide a final and definitive answer to the important question in focus. However, the study nonetheless provides one of the most accessible and far reaching overview of this issue that has been produced to date.

Stockholm, November 2014

Erik Wennerström
Director-General

Introduction by David P. Farrington

This systematic review addresses a very important topic: What is the relative effectiveness of custodial sentences compared to non-custodial sentences in reducing recidivism? Sentences have many different aims, including incapacitation, deterrence (individual and general), rehabilitation and retribution. Custodial sentences are clearly more incapacitating and retributive, but it is less clear that they have greater individual or general deterrent effects. It is even more doubtful that they would be more rehabilitative than non-custodial sentences, although this would depend on the effectiveness of the rehabilitation programs included in the different types of sentences. It is important for policy makers and practitioners to know the relative effectiveness of different types of sentences, so that they can make the best possible decisions about how to achieve penal aims.

The main problem in comparing offenders who receive custodial sentences with offenders who receive non-custodial sentences is that the former group are usually worse to start with. Generally, more frequent and serious offenders are more likely to receive a custodial sentence. This means that, irrespective of the effects of sentences, those who receive custodial sentences will probably have more frequent and serious criminal careers afterwards than those who receive non-custodial sentences. Paradoxically, however, those who receive custodial sentences will generally show a greater improvement afterwards compared with before, because of the well-known problem of regression to the mean.

It is important to disentangle the effects of the sentences from the effects of pre-existing differences between offenders. The best way of doing this is to randomly assign offenders to either custodial or non-custodial sentences. Providing that a sufficiently large number of offenders are assigned, this ensures that the custodial group are equivalent to the non-custodial group before the sentences on all possible measured and unmeasured variables. However, because of ethical, practical and legal problems, it is very difficult to carry out this kind of a randomized experiment. The next best method of dealing with selection effects is to statistically match custodial and non-custodial offenders before the sentences.

Nowadays, this matching is achieved most effectively by using propensity score matching. Briefly, the custodial and non-custodial groups are matched case-by-case on their prior probability of receiving a custodial sentence (based on variables that predict whether an offender receives a custodial sentence). This mimics randomization by ensuring that both offenders in each matched

pair have the same prior probability of receiving a custodial sentence compared to a non-custodial sentence. However, whereas randomization ensures equivalence on all measured and unmeasured variables, propensity score matching can only ensure equivalence on all measured variables. Nevertheless, propensity score matching is often the most convincing method that can be used in practice to investigate the effectiveness of custodial sentences compared to non-custodial sentences.

The authors of this systematic review should be congratulated for their very thorough searches for relevant evaluations and for their sophisticated meta-analyses. They have discovered many new studies in the last 10 years. Very few randomized experiments were found, but rather more quasi-experimental analyses using propensity score matching have been carried out, especially in recent years. Unfortunately, results obtained in the two types of evaluations are not concordant. While the quasi-experimental analyses show very convincingly that custodial sentences are followed by higher recidivism rates, the randomized experiments do not. The most defensible conclusion is that more research on this topic is needed.

More randomized experiments are especially needed. It seems to me that they could be feasible if offenders who would normally be given a custodial sentence were randomly assigned to receive either a custodial sentence or a non-custodial sentence. This would be especially appropriate where a new non-custodial sentence was introduced as an alternative to custody. For example, 40 years ago in England and Wales, community service orders were introduced as an alternative to short prison sentences. At the time, I advocated that the effects of these orders should be evaluated in a randomized experiment, but they never have been.

This type of an experiment would be feasible because no offender would be receiving a worse sentence than he or she would normally receive. Also, this type of an experiment would be ethical because the effects of the new order (compared with the alternative of short prison sentences) are not known. Just as the effects of a new drug must be assessed in randomized controlled trials, the effects of new sentences should be assessed in randomized experiments. Martin Killias and his colleagues should be congratulated for carrying out one of the few randomized experiments that compared the effects of custodial and non-custodial sentences.

Many recommendations can be made (and indeed have been made, in many cases, by the authors) about how to improve future research on the effectiveness of custodial sentences compared to non-custodial sentences. Ideally, long-term follow-ups are needed, to investigate whether effects persist or wear off. Ideally, self-report as well as official measures of reoffending are needed. Ideally, more sensitive measures of future criminal careers are needed, focussing on the frequency, seriousness and cost of crimes and

the duration of criminal careers. Ideally, measures of life success should be included in follow-up interviews, to investigate effects of custodial sentences versus non-custodial sentences on many aspects of people's lives, including accommodation, relationships, employment, mental health and substance use. Ideally, key features of sentences (e.g. types of rehabilitative programs) that may be effective or damaging should be investigated. Ideally, research should try to specify what works with whom, or the interaction between types of people and types of effects.

In conclusion, the authors of this systematic review should be congratulated for completing a very high quality review that greatly advances knowledge about the effectiveness of custodial sentences compared to non-custodial sentences and for their recommendations about how to carry out more adequate evaluations in the future.

Aknowledgements

Almost eight years ago, the preceding systematic review has been published on the website of the Campbell Collaboration Crime and Justice Group. The nearly 100 new relevant studies located through our searches for the present update amply justify the effort to present the state of knowledge in the light of additional studies. The new studies were published between 2003 and 2013. While the first review published in 2006 had been supported by the Swiss National Science Foundation, the update benefitted from a grant by the Swedish National Council of Crime Prevention (Brottsförebyggande rådet) at Stockholm. The search for and coding of new studies has been in the hands of Dr. Gwladys Gilliéron, assisted by Dr. Nora Markwalder (University of Zurich), during her internship at the University of Minnesota and the Max-Planck-Institute of Comparative Criminal Law and Criminology at Freiburg/Germany, as well as by Julien Chopin of the Lausanne University School of Criminal Justice. As with the previous review, Dr. Patrice Villettaz (School of Criminal Justice, University of Lausanne) has analysed the data and supervised the data collection between 2011 and 2013. Martin Killias (University of St. Gallen Law School) has coordinated the update and drafted the report. Contrary to the first review, the meta-analysis has been extended, beyond randomized controlled trials, to quasi-experimental studies that meet high standards of quality.

The authors wish to express their gratitude to all those who have supported them by providing relevant materials, sharing their experience or helpful suggestions on earlier drafts of this review. Our thank goes in particular to Dr. David Wilson for invaluable advice during the data analysis, to Dr. David Farrington for his support during the preparation of the report and for drafting a very helpful introduction to this document. A special thank goes to Erik Wennerström, General Director of the National Council for Crime Prevention, for making this report available to a larger audience. He and the staff of this centre have competently and efficiently followed the project throughout the production process.

St. Gallen/Lenzburg, June 2014

Martin Killias
Gwladys Gilliéron
Patrice Villettaz

1. Executive summary

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, in 2006, more than 3,000 abstracts, and identified more than 300 possibly eligible studies. For the update, nearly 100 additional potentially eligible studies published or completed between 2003 and 2013 have been identified. For the update, 10 matched-pair design studies and one RCT have been abstracted. One study (Bergman 1976) that, in 2006, had been classified as an RCT turned out, after closer examination, to have been quasi-experimental with respect to the comparison of the custodial and the non-custodial groups. As a result, it has been "downgraded" and included among the quasi-experimental studies in this update.

The findings of the update confirm one of the major results of the first report, namely that the rate of re-offending after a non-custodial sanction is lower than after a custodial sanction in most comparisons. However, this is true mostly for quasi-experimental studies using weaker designs, whereas experimental evaluations and natural experiments yield results that are less favourable to non-custodial sanctions. It can be concluded that results in favour of non-custodial sanctions in the majority of quasi-experimental studies may reflect insufficient control of pre-intervention differences between prisoners and those serving "alternative" sanctions.

2. Summary

Reviewers of the update

Gwladys Gilliéron, University of Zurich, Patrice Villettaz, School of Criminal Justie, University of Lausanne, and Martin Killias, University of St. Gallen. E-mail: gwladys.gillieron@yahoo.com, patrice.villettaz@unil.ch, martin.killias@unisg.ch, .

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 largely unknown, due to many uncontrolled variables.

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 over weekends. Thus, jails and 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 leaving the offender in the community rather than putting him into confinement.

Search strategy

Relevant published and unpublished studies which meet the eligibility criteria have been identified, during the first as well as for the updated review, through multiple sources, including Abstracts, bibliographies, and contacts with experts in several countries. In particular, the following sources have been searched for abstracts: Criminal Justice Abstracts, Criminology and Penology Abstracts, National Criminal Justice Reference Service (NCJRS), Library Catalogues (University of Minnesota), http://www.google.ch. The following keywords have been used to identify relevant studies: Prison, jail, imprisonment, alternative sanctions, house arrest, electronic monitoring, community service, probation, day re-

porting, fines, shock incarceration, boot camps; further keywords: re-conviction, re-offending, self-reported offenses, recidivism, re-arrest and re-incarceration.

Eligibility criteria

Randomized or natural experiments, as well as quasi-experimental comparisons between former prison inmates and those who served community sanctions have been included without exception, provided that propensity score matching methods were used. Other quasi-experimental studies have been included, for the updated as well as the first review, if subject were matched or if three or more potentially relevant independent variables had been controlled for. Studies written in any language and prepared between 1961 and 2013 have been considered for inclusion. For the update, ten studies have been identified and considered that used propensity scores in order to control for pre-existing differences between custodial and non-custodial groups.

Data collection and analysis

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

Main results

Although a majority of the selected studies (see Table 2) 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. It should be noted that offences prevented through incapacitation of incarcerated offenders have not been considered in this assessment.

Reviewers' conclusions

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

(1) Controlled experiments are still rare exceptions, although obstacles to randomisation are often less formidable than 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 rarely extended to significant parts of subjects' biographies.
(3) Despite alternative (and presumably more valid) measures of re-offending (such as self-reports) have become widely available, 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 improvement differs by type of sanction. Therefore, future studies should look 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 treated more fairly, rather than to specific effects of "alternative" sanctions as such. Given experimental research on neurobiological effects of feelings of fairness (Fehr and Rockenbach, 2003), such a possibility should be envisaged with more attention in future research.

Sources of support

The update has been supported by a grant of the Swedish National Council for Crime Prevention. The original review, published in 2006, was supported by a grant of the Swiss National Science foundation.

3. Review strategy

Background

In the late 19th century, leading criminal law teachers (such as Franz von Listz in Germany, Adolphe Prins in Belgium, 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. These ideas go back to a French magistrate, Arnould Bonneville de Marsangy (1802-1894) who, in several writings (Normandeau, 1969) and especially in his most prominent publication (Bonneville de Marsangy 1847), expressed the idea that crime is a disease which, if not thoroughly treated, will worsen and, ultimately, contaminate others, especially if offenders are brought to prison where they will live in proximity with other criminals. Particularly the idea that short-term imprisonment is damaging led many teachers of criminal law ever since to the call for the replacement of short prison sentences by either long sentences, or by "alternative" sanctions such as fines, suspended sentences, or probation (Franz von Listz, 1882) – often without paying credit to Bonneville de Marsangy. 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.

Objective

The main objective of this review was to compare rates of re-offending after custodial sanctions versus 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. The objective of the update is to see whether new studies have become available in the mean-time that might challenge the former review's conclusions.

Criteria for considering studies for this review

Preliminary remarks

The criteria of inclusion and exclusion, as developed for the first review, have remained unchanged during the update. 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, including temporary confinement over night or during weekends in half-way houses. Boot-camps, jails 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. However, 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.

In order to be eligible studies also had to meet the following criteria:

(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 for a criminal offense;
(3) There was at least one outcome measure of recidivism (new arrests, re-convictions, re-incarceration or self-report data);
(4) The study was completed after 1960 and 2002, and between 2003 and 2013 for the update.

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

Looking for studies that compared some sort of custodial and non-custodial sanctions, the original review allowed identifying more than three thousand studies across the Western countries in which re-offending (mostly reconvictions) has been compared between former prisoners and those who experienced any kind 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 only control variables included information available in official files, such as number and type of previous convictions, gender and age. Since offenders who receive different types of sanctions tend to differ in many other ways which are likely to be related to judicial disposal 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 sanc-

tions in such studies are highly questionable. Further, under all systems defendants with higher odds of re-offending are more likely to receive a custodial sentence than those with more promising outlooks. Thus, the bias is systematic in all studies of this kind and in all countries. Comparing later outcomes of sanctions with such different populations will lead to flawed conclusions. Thus, including low-quality studies in a meta-analysis and computing mean effects cannot be the solution.

Therefore and in order to reach reasonably valid conclusions, only studies that meet higher methodological standards have been included in this review. In the update, we have extended the meta-analysis beyond RCTs to quasi-experimental studies using propensity score methods, but analysed them separately.

Types of sanctions

Any studies meeting these criteria where "alternative" or community-based sanctions have been compared with some form of custodial sanctions have been included. 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 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 have been included in the *Appendix III* (numbers 1003, 1004, 1006) of the first review.

Studies were considered for inclusion regardless of the length of custodial sentences. Some studies have compared prisoners who, after a considerable time in custody, have been paroled (and transferred to a program of electronic monitoring), with 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.

Types of offenders

Given the scarcity of RCTs in this domain, studies involving adult and juvenile offenders and any type of offenses were included during the original review as well as during the update.

Types of outcome measures

Most of the included 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-report measures. For example, some studies have shown that the frequency of new offences decreases following any type of intervention (compared with an equivalent pre-intervention period), 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 in case of a new offence, or in continental countries where a multitude of offences leads eventually to one single rather than several convictions (that will be recorded under the most serious offence). Some studies have also used self-report data in order to assess the outcome of different interventions.

In order to assess improvement, we have tried to look not only at prevalence of reconviction (or percentage of those who re-offend), but also at "incidence" rates (i.e. frequencies of new offences per time unit).

Types of studies

In a first step, randomized experiments have been selected where re-offending rates among former prisoners (in a broad sense) had been compared with those among convicted persons who served any kind of community-based sanction.

In a second step, 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 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).

As a next step, studies using propensity scores matching methods have been considered. Such studies are preferable over conventional matching studies (Nagin, Cullen and Jonson, 2009) which were included in the next category, along with quasi-experimental studies with control of several potentially confounding variables (age, gender, prior record, offence type etc.). Finally, level 3 (on the Sherman et al. scale) studies are excluded and listed in the bibliography under C.

Search strategy for identification of studies

Search procedure

Potentially relevant studies were sought through abstracts, internet, library catalogues, bibliographies of studies and e-mail contacts with research institutes in a number of countries.

For the update, some 100 additional potentially relevant studies have been located (published between 2003 and 2010) and added to the list of nearly 300 relevant studies from the first review. For experimental and quasi-experimental studies using propensity score methods, the update extends up to 2013.

It was not very difficult to find published studies, especially when there was an article version. However, unpublished studies turned out to be more difficult to locate. This is unfortunate given the frequent bias among the scientific community and editors of journals. There are indeed anecdotal indications that studies showing no difference or, worse, unfavourable outcomes for "alternative" sanctions might less likely be published than those showing successful outcomes. Provided it is real, this bias would have produced a conservative error, however, given that our meta-analysis has failed to demonstrate any significant overall effects of either custodial or non-custodial sanctions.

For the update as well as for the original review, 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, KRIMDOK of the University of Tübingen (Germany), IUSCRIM of the Max-Planck Institute in Freiburg in Germany, and WWW.GOOGLE.CH. All new eligible studies have been published in English.

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 usual definitions of recidivism (re-offending, reconviction, self-reported offences, recidivism, re-arrest, re-incarceration, etc.).

Methods of review

The search method generated nearly 300 citations of potentially eligible studies, plus some 100 citations for the update. We screened these citations and for each study, we assessed its methodological quality. For the update, most of this work was accomplished during the second author's assignment to the Max-Planck-Institute for Criminal Law and Criminology at Freiburg (Germany). All code sheets were controlled by a different person from the staff of the Zurich University Institute of Criminology (Dr. Nora Markwalder). The search for propensity score studies completed between 2011 and 2013 was accomplished by a researcher of the University of Lausanne (Julien Chopin).

Each study has been screened for eventual methodological short comings:

(1) The only new RCT was a study on long-term effects (over 11 years) of a RCT included already in the original review.
(2) In the case of natural experiments, special attention has been given to the independence of the selection criterion from offender characteristics. Of the two natural experiments that have been identified, one did not present difficulties in this regard, whereas another did not to qualify for inclusion.
(3) In case of non-randomized studies, the theoretical and/or practical relevance of the control variables has been considered. Compared with the first review, the methodological rigour of quasi-experiments has seen some important improvements. Subjects undergoing different sanctions have been matched more carefully and propensity scores matching has been used in several studies. Studies using propensity scores have been meta-analysed. On the other hand, studies that did not meet higher standards and controlled only for gender, age and previous offense record were not abstracted, nor meta-analysed. As an example, Bartles (2009) who compared, without matching, reconvictions after several types of sanctions in Tasmania (Australia), such as wholly and partially suspended or unsuspended sentences, has not been abstracted, nor meta-analysed. The number of relevant studies World-wide is appallingly week, namely no more than 14. This is to say that, despite strong rhetoric about the damaging effects of incarceration, the empirical base to assess such claims is appallingly weak.

Especially deplorable is the near-absence of RCTs and natural experiments.

Data collection and analysis

As during the first review, a coding sheet along the guidelines of the Campbell Collaboration has been used to extract all relevant information from the eligible studies. All studies completed by the end of 2010 have been coded by G. Gilliéron, supported by Nora Markwalder of the Institute of Criminology of the University of Zurich. Studies completed between 2011 and 2013 have been identified and coded by Julien Chopin (University of Lausanne, School of Criminal Justice).

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 section. As in the original review, mixing the few randomized or natural experiments with studies of different quality has been avoided. Studies using propensity score methods have, thus, been meta-analysed separately.

Comparison with other reviews

At the time of the first review, the authors have received from Professor Gendreau his and his Colleagues' review of studies on recidivism after custodial or community-based sanctions (Smith, Goggin and Gendreau 2002) that updated earlier work on the same topic (Gendreau, Goggin, Cullen 1999). There was no complete overlap, mostly due to the differences in scope of the two reviews, but higher quality studies were covered more completely in our first review. For the update, the authors have been inspired by Nagin, Cullen end Jonson (2009) review that included the higher quality studies from our first review. For the update, all studies using propensity score methods have been included in both reviews – with the obvious exception of studies that have been completed in the mean-time.

4. Description of the eligible studies and results

In this section, we first describe the randomized studies included in this review. Most of them have already been included in the previous review. In the following section, we summarize two natural experiments. Next, studies using propensity matching will be described. In the fourth section, other matching studies and those controlling for several control variables will be listed. Some of the studies described here have not been eligible, despite an apparently higher quality. The reasons are indicated in the summaries.

Studies liste under 4.1, 4.2 and 4.3 are, with a few exceptions, included in the meta-analysis.

*In the meta-analysis (below 4.5), effects on several outcomes will often be combined. In these cases, the results discussed in the following summaries go beyond what will be considered in the meta-analysis. The numbers of the studies refer to the bibliography. Studies identified during the update are marked with U. An * designs studies included in the meta-analysis.*

Controlled randomized trials

*01 Barton W.H., Butts J.A. (1990): Viable options: intensive supervision programs for juvenile delinquents. *Crime and Delinquency* 36/2 (1990), 238–256

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 home-based intensive supervision (experimental group, N=326) or to a control group that was committed to the State for institutional placement (N=185) that lasted on average about 13 months. 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 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 marginally not statistically significant ($p < .07$).

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 (under home-based intensive supervision) re-offends no less than the control group (placed in an institution).

Reviewers' comment: Nagin, Cullen and Jonson (2009) and McGrath and Weatherburn (2012) criticize that the non-institutionalized group actually spent on average about six months in custody during the two-year follow-up period, and that parts of the preventive effect may be due to incapacitation rather than special deterrence. The authors, however, have taken time spent in custody into account, and weighted re-offending rates for time spent at large by subjects of both groups (p. 244). Further, the authors have excluded from the analysis all subjects who spent the entire 2-years follow-up period in custody. Therefore, the comparison remains valid, in the reviewer's view.

*02 Bergman G.R. (1976): *The evaluation of an experimental program designed to reduce recidivism among second felony criminal offenders*. Wayne State University, Detroit (Mich.), PhD dissertation (77-9368).

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 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 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%).

Reviewers' comment: After a new examination of the selection process, as described by Bergman in his unpublished dissertation (pp. 204-208), it turned out that subjects were not randomly assigned to custodial and non-custodial sanctions. The 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. However, the description of the randomization process being not entirely clear, the author's explanations lead us to assume that randomization was limited to program variations within the prison and the probation condition and did not include the assignment either to prison or probation. Given this uncertainty and despite the fact that it was included among RCTs in our former review as well as in the Nagin, Cullen and Jonson (2009) review, this study has been considered as a quasi-experiment rather than an RCT for the purposes of this review.

*03 Killias M., Aebi M., Ribeaud D. (2000): Does community service rehabilitate better than shorter-term imprisonment? Results of a controlled experiment. *Howard Journal of Criminal Justice* 39/1, 40-57.

This study compares the effects of community service versus short 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 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. 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.

*U01 Killias M., Gilliéron G., Villard F., Poglia C. (2010): How damaging is imprisonment in the long-term? A controlled experiment comparing long-term effects of community service and short custodial sentences on re-offending and social integration. *J. of Experimental Criminology* 6/2, 115-130.

This is a follow-up study of the previous RCT (see above, *25). The observation period (new convictions) was extended from 2 years (as in the previous study) to 11 years. Social integration was assessed through data from the Internal Revenue Service on marital status, employment history, income, financial circumstances (including debts) and compliance with tax regulations (i.e. filing an income tax form). Few significant differences were found, but former prisoners fared slightly better in terms of social integration. Reconviction rates were nearly the same in both groups after 11 years. However, only 2 former prisoners (or 5%) were convicted during both the first 5 years and again later during the following six years, whereas this was the case for 16 subjects (20%) among those who had served community service.[1] This study is one of the few that looked, beyond reconviction, at social integration. In contrast to mainstream thinking since 1850, it found no difference of social integration. Two objections were raised by Wermink et al. (2010), namely that (1) offenders assigned to community service kept the right to opt for imprisonment, and that (2) the correctional service retained the right to override random assignment and send offenders to prison. However, dropping-out concerned actually 2 subjects among the 41 assigned to prison (1 died, 1 emigrated) and 16 among the 100 assigned to community service (2 died, 4 emigrated, 3 were excluded from the program due to serious offences during the program, 2 fine-defaulters paid their fine before executing community work, and 5 opted finally for short-term imprisonment instead). As the authors (Killias et al. 2010) observe, it is rather surprising that 39 of 41 among those assigned to prison, and 84 among the 100 sent to community work were still available for analysis. The objections made by Wermink et al. (2010) concern actually 3 and 5 subjects, respectively.

*04 Schneider A.L. (1986): Restitution and recidivism rates of juvenile offenders: results from four experimental studies. *Criminology* 24/3, 533-552

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 Boise (Idaho) trial is relevant. 95 subjects aged 15 on average were randomly assigned to weekend detention, while 86 had been allocated to a restitution program. Detention lasted on average 8 days that juveniles often spent during several successive weekends. On the whole, the recidivism analysis suggests that the restitution

[1] This (significant) difference had not been noticed by the authors, but came to the attention of the reviewers through Dr. Farrington).

group had 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 number of offences dropped from 137 to 100 among the detention group and from 103 to 86 among the restitution group. This difference points to the fact that the detention group was somewhat more delinquent prior to the intervention than the experimental group.

Reviewers' comment: McGrath and Weatherburn (2012) observe that the random process was somewhat disturbed because the judge overruled it in 3 per cent of cases assigned to the custodial and in 11 per cent of cases assigned to the restitution condition. Schneider (1986) analysed these cross-overs in a first time "as assigned" and in a second time "as treated" and did not find a difference in outcome (pp. 538, 543-545). Therefore, the reviewer's do not think these cross-overs invalidate this RCT.

Natural experiments

05 Gottfredson D.C., Barton W. H. (1993). Deinstitutionalization of juvenile offenders. *Criminology* 31/4, 591-610.

In 1988, the State of Maryland closed one of its two juvenile training schools, Montrose. This natural experiment offered the opportunity to follow three groups of juvenile offenders over 32 months after release at most (on average, 2.5 years). The first group (N=318) consisted of juveniles who had passed their full time (of 8.5 months on average) at Montrose and who were released shortly before the decision to close this institution. The second (i.e. the "transition" group, N= 355) were juveniles who had passed some time (on average, 6.9 months) at Montrose and who, at the moment of its closure, were transferred to community treatment. The third group (N=256) consisted of young offenders who would normally have had a high probability to be placed at Montrose but who, having been disposed of after the decision to close, spent little or no time in placement. Records on re-offending showed rates of the two confined groups to be lower compared to the third group. The differences were consistent and significant for property offences, but not for drug and violent or more serious offences, however. Self-report measures did not show significant differences across groups, probably due to the inclusion of many

trivial offences (which resulted in generally very high offending rates in all three groups).

Reviewers' comment: Unfortunately, the groups differed in some important respects (age, prior record, place of residence). Thus (and as declared by the authors), they are not equivalent. Therefore, this study has not been included in the meta-analysis.

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

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%).

Matched-pair design studies using propensity score methods

*U02 Apel R., Sweeten G. (2010a) The impact of first-time incarceration on criminal behaviour during transition to adulthood. Unpublished. (Note: The authors have published a related paper based on the same data: Apel R. & Sweeten G. (2010b). The impact of incarceration on employment during the transition to adulthood. *Social Problems* 57/3, 448-479.

This study evaluated the impact of incarceration (mostly jail) during late adolescence and early adulthood on later criminal history. Based on the National Longitudinal Survey of Youth 1997, it looked at first-time convictions and incarcerations prospectively and assessed both short and long-term effects on later crime over up to six years on samples of 315 incarcerated and 508 non-incarcerated adolescents. The results indicated that incarceration increases (self-reported) criminal involvement as well as the risk of re-conviction and re-incarceration during the first four years following imprisonment, with particularly high risks during the first year following confinement. Systematic differences between individuals sentenced to prison and those receiving non-custodial sanctions are controlled for through propensity score match-

ing and fixed-effects modelling for unobserved differences. Due to major attrition problems, only the first-year follow-up has been considered, with 307 incarcerated adolescents and 307 matched-pairs (Table 3, p. 43). Confinement lasted on average 4.3 months. According to the second paper (Apel & Sweeten 2010b) incarceration affects negatively employment opportunities over the following years.

*U03 Bales W., Piquero A. (2012). Assessing the impact of imprisonment on recidivism. *Journal of Experimental Criminology*, 8/1, 71-101.

This study compares over 65,000 probationers (the Community control group) with 79,000 prisoners in Florida sentenced between 1994 and 2002. Using propensity score matching, precise matching and logistic regression, the authors conclude that prison exerts a consistent criminogenic effect. The follow-up time was 36 months.

*U04 Loughran T.A., Mulvey E.P., Schubert C.A., Fagan J., Piquero A.R., Losoya S.H. (2009). Estimating a dose-response relationship between length of stay and future recidivism in serious juvenile offenders. *Criminology* 47/3, 699-738.

A large longitudinal sample of serious juvenile offenders from two large cities has been followed over four years. Among the sample, 419 had experienced incarceration and 502 were sentenced to non-custodial sanctions. Many potentially relevant variables and propensity scores were used to control for systematic differences between the two groups. The study found a null effect of institutional confinement overall, as assessed through re-arrest or self-reported offending. Regarding the dose-response effect, it was found that longer lengths of stay in institutional settings had little or marginal effect on later re-offending.

*U05 Lulham R., Weatherburn D., Bartels L. (2009). The recidivism of offenders given suspended sentences: A comparison with full-time imprisonment. *Crime & Justice Bulletin*, 136, 1-16.

Using propensity score matching, the preventive effect of prison was assessed, over a three-year follow-up time, through a comparison of 6,825 offenders receiving a suspended sentence with 7,018 offenders sentenced to custody in New South Wales (Australia). Through survival analysis, differences in "free time" between the groups were taken into account (subjects sentenced to custody spent on average about one year and nine months or 648 days in the community). Among offenders without prior record, no effect of imprisonment was found. Among those with prior experience of custody, however, a significant criminogenic effect has been observed.

Reviewer's comment: Given the authors' explanations, the custodial and the non-custodial groups may have differed more on

some unmeasured variables. Indeed, the odds of a defendant to receive a suspended sentence decrease with prior convictions or incarcerations. In many jurisdictions, offenders with substantial criminal records who still receive a suspended sentence may benefit from particularly positive prognostic outlooks.

*U06 McGrath A., Weatherburn D. (2012). The effect of custodial penalties on juvenile reoffending. *Australian & New Zealand J. of Criminology* 45/1, 26-44.

Young offenders who appeared in the New South Wales (Australia) Children's Court (N=6,196) and who were sentenced to control orders (i.e. a custodial sanction of on average 8 months and ranging from 2 days to 24 months, N=376) were compared to a group of matched offenders receiving community-based sanctions. Propensity scores matching was used, based on demographic variables (gender, indigenous status), living in an economically disadvantaged area, urbanization, and criminal history (age at first court appearance, concurrent offences, number of counts of the principle offence, offence seriousness, prior convictions, prior imprisonment, and prior violent offences). Prior to matching, the survival analysis revealed a significantly higher re-offending probability among subjects sentenced to detention compared to those receiving community-sanctions. However, after matching (using propensity scores), the trend reversed and juveniles sent to detention had a slightly (though not significantly) longer median survival time (i.e. number of days to reoffend), namely 359 compared to 325 days. The follow-up period was 21 months.

Reviewers' note: Due to missing information in the published paper, no effect sizes could be computed. It has not been possible to obtain the missing information in time. Therefore, this study had to be dropped from the meta-analysis.

U07 Mears D.P., Cochran J.C., Bales William D. (2012). Gender differences in the effects of prison on recidivism. *Journal of Criminal Justice* 40/5, 370-378.

Using propensity score matching, this study compared male and female offenders, sentenced either to custody or to probation. The authors differentiated reoffending by types of new offenses (violent, property, drug and other recidivism). As it turned out, new offenses are more common among those sentenced to prison, regardless of the type of offenses, although this effect is strongest for drug offences, followed by property recidivism and minimal for violent and other offenses. This result holds for men and women. Interestingly, the "criminogenic" effect of prison (i.e. State correctional facilities) is strongest in the comparison with intensive probation, followed by traditional probation and jail.

Reviewer's comment: The difference in size of "criminogenic" effects across type of "alternatives" to prison may reflect selection

of offenders to different sanction types – jail inmates may be more similar to prisoners in (unmeasured) respects than probationers. This study has not been retained for the meta-analysis because the "counterfactual group" (i.e. the non-custodial condition) included jail and, thus, another custodial sanction.

*U08 Nieuwbeerta P., Nagin D., Blokland A. (2009). Assessing the impact of first-time imprisonment on offenders' subsequent criminal career development: A matched samples comparison. *Journal of Quantitative Criminology* 25, 227-257.

This study has assessed re-convictions among 575 incarcerated and 1,111 non-incarcerated first-time offenders (aged 18-38) in The Netherlands over three years. The non-custodial sanctions included fines, prosecution wavers and suspended sentences. The relatively large sample allowed considering over 70 independent variables. Using propensity score methodology, it was found that re-convictions were nearly twice as frequent after (mostly short) prison sentences. Interestingly, this (negative) effect of imprisonment leveled off after age 26. As the authors observe (p. 251), no controls have been possible for subjects' histories of alcohol and drug abuse, employment, marital status and family circumstances. These variables are likely to be known to sentencing judges and, in turn, affect probabilities of re-conviction.

*U09 Nirel R., Landau S.F., Sebba L. and Sagiv B. (1997). The effectiveness of service work: An analysis of recidivism. *J. Quantitative Criminology* 13/1, 73-92.

This study analyses reconvictions over 14 months among 950 offenders sentenced to imprisonment and 407 offenders sentenced to service work (community service) in Israel. Propensity score methodology was used to assess the odds of recidivism by (a) estimating the probability of assignment to a particular sanction given a set of confounding variables, and (b) estimating the conditional probability of recidivism, given cofounders and probability scores. Before adjustment for the systematic differences between the two groups, the odds of re-conviction was 2.4 times higher for prisoners compared to those assigned to service work. After adjustment, it was reduced to 1.7. (This study has been deemed non-eligible in the first review, but after re-examination, it has been decided to include it among the meta-analysed matched-pair studies.)

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

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 prisoners (368) and those sentenced to non-custodial sanctions (374) that were matched on factors related to their criminal history, 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.

Reviewers' note: The authors have re-analysed the data in a later publication, using propensity scores: Weisburd D., Waring E., Chayet E. (2001), White-collar crime and criminal careers. Cambridge: Cambridge University Press.

*U10 Wermink H., Blokland A., Nieuwbeerta P., Nagin D., Tollenaar N. (2010). Comparing the effects of community service and short-term imprisonment on recidivism: a matched samples approach. *J. of Experimental Criminology* 6/3, 325-349.

Longitudinal official record data on adult offenders (n=4,246) from The Netherlands have been used to compare re-convictions after community service and short-term imprisonment. To account for possible bias due to selection of offenders into these types of sanctions, a large set of confounding variables have been controlled for through a combined method of "matching by variable" (gender, age, length of incarceration) and "propensity score matching". Re-offending was significantly less frequent after community service, both in the short-term and in the long run. The total post-intervention observation period was 8 years.

Other matched-pair studies and studies with several control variables

08 Bondeson U.V. (1994). *Alternatives to imprisonment: intentions and reality*, Transaction Publishers / Westview Press, London / Boulder (2nd edition 2002)

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 was collected. Recidivism data were collected from the Central Criminal Register and from the criminal records kept by the National Board of Excise.

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. All in all, about 40 percent of variance in reconvictions was explained.

09 Bonta J., Wallace-Capretta S., Rooney J. (2000a): Can electronic monitoring make a difference? An evaluation of three Canadian programs. *Crime and Delinquency* 46/1, 61-75

This Canadian study compares recidivism for three groups of male offenders, namely a group sentenced to electronic monitoring (EM, n=262), a group of prison inmates who were released on parole (n=256), and a group sentenced to probation (n=30). In addition, EM offenders are compared with inmates and probationers matched for offence risk. Re-offending was assessed through self-report measures 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. In the further analysis, however, these differences could be entirely accounted for 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 into EM.

10 Bonta J., Wallace-Capretta S., Rooney J. (2000b): A quasi-experimental evaluation of an intensive rehabilitation supervision program. *Criminal Justice and Behaviour* 27/3, 312-329

This Canadian study evaluates a cognitive-behavioural treatment program within the context of intensive rehabilitative supervision (IRS program) based on 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 compared 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 had 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.

U11 Bontrager R.S., Winokur E.K., Hand G., Chapman S. (2013). Juvenile justice interventions: System escalation and effective alternatives to residential placement. *Journal of Offender Rehabilitation*, 52/5, 358-375

This study included all juveniles sentenced either to probation (N= 2,823) or to institutional placement (N=269) in Connecticut between 2005 and 2007. The results suggest that juveniles placed on probation re-offend less than comparable subjects placed in a closed setting.

11 Börjeson B. (1966). *Om Påföljders Verkningar (On the effects of sanctions). En undersökning av prognosen för unga lagöverträdare efter olika slag av behandling*, Almqvist & Wiksell, Stockholm

This study compares the effects of conditional sentences, fines, determinate imprisonment, training school and youth imposed on young law-breakers aged 18 to 20 years in Sweden. The various sanctions have been classified into custodial (n=101) and non-custodial sanctions (n=315). Subjects 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 had been imposed by the court. Nearly 40 variables have been taken into account. Over a follow-up period of three years, non-custodial sanctions were followed by less reconviction in every risk category. About 40 per cent of total variance was explained.

12 Brennan P.A., Mednick S.A. (1994): Learning theory approach to the deterrence of criminal recidivism. *Journal of Abnormal Psychology* 103/3, 430-440.

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 sanctions in reducing future rates of offending. Criminal recidivism resumed if punishment is discontinued.

U12 Cid J. (2009). Is imprisonment criminogenic? A comparative study of recidivism rates between prison and suspended prison sentences. *European J. of Criminology* 6/6, 459-480.

Cases from Barcelona courts were used to assess recidivism (i.e. re-incarceration over a period of 8 years) among defendants sentenced to unsuspended (N=179) and suspended imprisonment (N=304). Offenders were not matched, but demographic and offense-related variables (type, previous convictions and previous imprisonment) have been controlled for. The defendant's drug dependency and difficult financial circumstances have also been taken into account. The results showed that re-admissions to prison were much higher among those sentenced to an unsuspended custodial sanction.

Reviewers note: Since the criterion was re-incarceration rather than re-offending (or a proxy like re-convictions), the results could also be taken as evidence that former prison experience increases the odds of being sentenced to immediate custody at a next court appearance.

13 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. *Addiction* 92/8, 989-997.

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 analyses on first offenders show that subjects receiving jail have, on average, almost twice as many DUI re-convictions as those assigned to first offenders' treatment

programs in combination with license restriction. The author concludes that license actions combined with alcohol treatment are the most effective in reducing DUI recidivism.

14 Kraus J. (1974): A comparison of corrective effects of probation and detention on male juvenile offenders. *British Journal of Criminology* 14/1, 49-62.

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, with a median of 15.2. Matching was done on seven demographic variables to build up two comparable groups of offenders.

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. The difference is larger among first-time offenders and for property offenses (with 62.6% *versus* 82.4% of re-offenders).

U13 Kraus J. (1978). Remand in custody as a deterrent in juvenile jurisdiction. *Brit. J. of Criminology* 18/3, 285-289.

Two groups of 90 male juvenile first offenders each, aged between 11 and 17 (16 on average), were remanded either in custody (up to 3 weeks) or at home by a metropolitan children's court in Sidney (Australia). The follow-up period was 24 months. Groups were matched on age and type and number of charges. However, no information on families was available. Offenders remanded in custody had far higher re-offending rates (assessed through number and seriousness of "proven charges".

15 MacKenzie D.L. (1991): The parole performance of offenders released from shock incarceration (boot camp prisons): a survival time analysis. *Journal of Quantitative Criminology* 7/3, 213-236.

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. To the contrary, 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%).

16 MacKenzie D.L., Shaw J.W. (1993): The impact of shock incarceration on technical violations and new criminal activities. *Justice Quarterly* 10/3, 463-487.

This study 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, 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.

17 MacKenzie D.L., Brame R., McDowall D., Souryal C. (1995): Boot camp prisons and recidivism in eight states. *Criminology* 33/3, 327-358.

This study compares recidivism among boot camp graduates in eight states (Florida, Georgia, Illinois, Louisiana, New York, Oklahoma, South Carolina and Texas) during community supervision with re-offending among control groups (probation or parole). 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 perform 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.

18 Muiluvuori M.-L. (2001): Recidivism among people sentenced to community service in Finland. *Journal of Scandinavian Studies in Criminology and Crime Prevention* 2/1, 72-82.

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 (N=160) was slightly lower than after a prison sentence (N=157) during the 5-year follow-up period (60.5% *versus* 66.7%). The differences between the groups were not statistically significant.

19 Petersilia J., Turner S., and Peterson J. (1986): Prison versus probation in California: Implications for crime and offender recidivism. Santa Monica (Ca.): The RAND Corporation.

Using a California sample of 511 comparable prisoners and probationers, the authors compared rates of re-offending and esti-

mated the amount of crime that was prevented when felons were imprisoned rather than placed on probation.

After statistical controls (of 12 variables), the results show that prisoners had higher recidivism rates than probationers. In the two-year follow-up period, 68 percent of the prisoners were re-arrested, 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 offenses, 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 former prisoners' recidivism rates were higher than those of probationers, their new offences were no more serious.

20 Roeger L.S. (1994): The effectiveness of criminal justice sanctions for Aboriginal offenders. *Australian and New Zealand Journal of Criminology* 27/3, 264-281.

This Australian study compares re-offending among Aboriginal offenders who were sentenced either 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.

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

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.

22 Smith L.G., Akers R.L. (1993): 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, 267-292.

This study examines the effectiveness of home confinement compared to imprisonment on re-offending. 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 partial-

ly 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 re-offended during the five-year follow-up period (77.8% *versus* 78.6%).

U14 Spohn C. (2007). The Deterrent Effect of Imprisonment and Offenders'Stakes in Conformity. *Criminal Justice Policy Review* 18/1, 31-50

In Jackson County (Kansas City, Missouri), drug offenders (N=342, mostly convicted of drug possession), offenders convicted of property or other nun-drug offences with a history of drug use or previous convictions of a felony drug offense (N=274), and non-drug offenders (N=461) were followed over a period of 4 years. Recidivism (defined as a scale, ranging from filing of a new charge up to re-incarceration) was more frequent among those in every group who were sentenced to unsuspended custodial sanctions. Among drug offenders, those who were sent to prison re-offended less often than those receiving suspended sentences, see Table 1). The criminogenic effect of prison held even when stakes in conformity (marital status) were taken into account. (Subjects were not matched, but demographic background, marital and employment status and drug abuse history, have been controlled.)

23 Spohn C., Holleran D. (2002): The effect of imprisonment on recidivism rates of felony offenders: a focus on drug offenders. *Criminology* 40/2, 329-357.

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.

24 Tashima H.N. Marelich W.D. (1989): *A comparison of the relative effectiveness of alternative sanctions for DUI offenders*. Sacramento, CA: California Department of Motor Vehicles.

Table 1: Characteristics of included eligible studies (* included in the meta-analysis)

N°	Author Name	Study design, country	Custodial sanction	Non-custodial sanction	Offender type	Follow-up period	Results favour… (number of comparisons, *= p <.05 at least)	Observations
01*	Barton, Butts (1990)	RCT (Michigan, USA)	Institutional placement of 13 months on average	Intensive (home-based) supervision	Juveniles	24 months	3 custodial (2*) 3 non-custodial (2*)	Non-institutionalized group spent on average 6 months in custody, but authors weighted for time spent at large by subjects
02	Bergman (1976)	RCT (Michigan, USA)	Prison	Probation	Adults	12 months	Non-custodial	Subjects have not been randomly assigned to custody vs. probation
03*	Killias, Aebi, Ribeaud (2000)	RCT (Switzerland)	Prison up to 14 days	Community service	Adults	24 months	5 non-custodial (1*)	
U01*	Killias, Gilliéron, Villard, Poglia (2010)	RCT (Switzerland)	Prison up to a4 days	Community service	Adults	132 months	1 custodial (1* for social integration) 1 non-custodial	Follow-up of study 3. Meta-analysis combines 3/U01 (11 years)
04*	Schneider (1986)	RCT (Idaho, USA)	Weekend detention of 8 days (on average), served during successive weekends	Restitution	Juveniles aged 15 on average	22 months	2 non-custodial (prevalence and incidence rates) 1 custodial (change in post- vs. pre-intervention offense rates)	Randomization process disturbed by judicial interference but outcomes did not change when cross-overs were analysed “as assigned” rather than “as treated”.
05	Gottfredson & Barton (1993)	Natural experiment (Maryland, USA)	Placement in Training School (8.4 months on average)	Treatment in the community (with little or no placement time)	juveniles	30 months (average)	1 institutionalized, 3 none	Three groups were compared (institutionalized, before closing of Montrose Training School): a pre-closure and a post-closure group plus a “transition” group. Groups were not equivalent (not included in meta-analysis).
06*	Van der Werff (1979) (#124)	Natural experiment (Netherlands)	Prison up to 14 days	Suspended custodial sentence	Adults	72 months	Traffic offenders: none Property offenders: none Violent offenders: non-custodial (1*)	
U02*	Apel & Sweeten (2010a)	Propensity score matching (USA)	Prison (jail) of 4.3 months (on average)	Any non-custodial alternative	Adolescents	12 months	Non-custodial (*)	Imprisonment negatively affects employment opportunities
U03*	Bales and Piquero (2012)	Propensity score, precise matching and logistic regression (Florida, USA)	Prison	Community control	Adults	up to 36 months	Non-custodial*	Unusually large sample (prison group: n=79,022; community control group: n=65,394)
U04*	Loughran et al. (2009)	Propensity score matching (USA)	Prison	Any non-custodial sanctions	Juveniles	48 months	Non-custodial (3)	Length of imprisonment was negatively correlated with later offending
U05*	Lulham, Weatherburn, and Bartels (2009)	Propensity score matching (New South Wales, Australia)	Prison	Suspended sentences	Adults	up to 36 months	Non-custodial* (except for first-time offenders)	Difference in “free time” between the groups was corrected through survival analysis.
U06*	McGrath & Weatherburn (2012)	Propensity score matching (New South Wales, Australia)	Prison of 8 months on average (from 2 days to 24 months)	Community-based sanctions	Adolescents	21 months	Non-custodial (1, before matching) Custodial (1*, after matching)	Not included in the meta-analysis because essential information has not been available (as a result, effect sizes could not be computed)

N°	Author Name	Study design, country	Custodial sanction	Non-custodial sanction	Offender type	Follow-up period	Results favour… (number of comparisons, *= p <.05 at least)	Observations
U07	Mears et al. (2012)	Propensity score matching (Florida, USA)	Prison (State prisons)	Traditional probation, intensive probation, jail	Adults	36 months	Non-custodial* (few exceptions), for male and female releasees	Not included in the meta-analysis because the alternative condition included jail (i.e. another custodial sentence)
U08*	Nieuwbeerta et al. (2009)	Propensity score matching (Netherlands)	Prison (short)	Fines, suspended prison terms, prosecution wavers	Adults aged 18-38 years	36 months	Noncustodial (2*)	
U09*	Nirel et al. (1997)	Propensity score matching (Israel)	Prison	Community sanctions	Adults	14 months	Non-custodial (1*)	Propensity score reduced results in favor of community service
07*	Weisburd, Waring, Chayet (1995) (#16)	Propensity score matching (USA)	Prison	Any non-custodial sanctions	Adult white-collar offenders	126 months	No significant difference	Data were re-analyzed in Weisburd, Waring and Chayet (2001).
U10*	Wermink et al. (2010)	Propensity score matching (Netherlands)	Prison (60 days on average)	Community service	Adults aged 18-50	96 months	Non-custodial (OR 1.7 after matching)	
08	Bondeson (1994, 2002) (# 1002)	QE (Sweden)	Placement with institutional treatment	Suspended custodial sentence, probation	Adults	24-36 months	Non-custodial*	
09	Bonta, Wallace-Capretta, Rooney (2000) (#32)	QE (Canada)	Prison	Electronic monitoring and rehabilitation	Adults	12 months	None (after controls)	
10	Bonta, Wallace-Capretta, Rooney (2000) (#20)	QE (Canada)	Prison	Electronic monitoring and rehabilitation	Adult low vs. high-risk offenders	12 months	Custodial* (high-risk group) Non-custodial* (low-risk group)	
U11	Bontrager et al. (2013)	QE (Connecticut, USA)	Institutional placement	probation	juveniles		Non-custodial*	
11	Börjeson (1966) (#1005)	QE (Sweden)	Prison	Non-custodial	Adults	36 months	Custodial*	
12	Brennan & Mednick (1994) (#23)	QE (Denmark)	Prison	Probation	Men born in Denmark between 1944-1947	Subjects followed up to age 26	No effect if SES, age and time spent in custody are controlled for. Preventive effect for first- and second-time offenders	Effects are complex and suggest different effects of custody in various sub-groups.
U12	Cid (2009)	QE (Barcelona, Spain)	Prison	Suspended sentences	Adults	96 months	Non-custodial*	Reviewers' note: The criterion was re-incarceration. Thus, the results do not establish a criminogenic effect of prison, but could indicate that former incarceration increases the odds of being sentenced to prison again at a new court appearance.
13	DeYoung (1997) (#2)	QE (California, USA)	Prison	Alcohol treatment and license suspension	Adult convicted of drunk-driving	18 months	Custodial*	
14	Kraus (1974)	Matching (New South Wales, Australia)	Prison	Probation	Juveniles	60 months	Non-custodial*	
U13	Kraus (1978)	Matching (Sidney, Australia)	Pre-trial detention (up to 3 weeks)	Other disposition	Juveniles	24 months	Non-custodial*	

N°	Author Name	Study design, country	Custodial sanction	Non-custodial sanction	Offender type	Follow-up period	Results favour… (number of comparisons, *= p <.05 at least)	Observations
15	MacKenzie (1991) (#31)	QE (USA)	Shock incarceration	Probation	Adults	12 months	custodial*	
16	MacKenzie & Shaw (1993) (#31)	QE (USA)	Shock incarceration	Probation	Adults	24 months	Non-custodial*	
17	MacKenzie, Brame, McDowall, Souryal (1995) (#72)	QE (USA)	Shock-incarceration	Probation	Adults	12/24 months	none	
18	Muiluvuori (2001)	Matching (Finland)	Prison (8 months on average)	Community service	Adults	60 months	Non-custodial (p <.10)	
19	Petersilia, Turner, and Peterson (1986)	Matching (California, USA)	Prison	Probation	Adults	24 months	Non-custodial (p <.10)	
20	Roeger (1994)	QE (Australia)	Prison	Community service	Aboriginal Adults	42 months	none	
21	Savolainen J., Nehwa-dowich W., Tejaratchi A., Linen-Reed B. (2002) (#9)	QE (New York, USA)	Prison	Probation	Adults	6-12 months	Non-custodial*	
22	Smith and Akers (1993) (#74)	Matching (Florida, USA)	Prison	Home confinement	Adults	60 months	none	
U14	Spohn (2007)	QE (Missouri, USA)	Prison	Suspended custodial sentences	Adult drug (N=342), non-drug (N=461) and property offenders with a history of drug abuse (N=274)	48 years	Offenders sentenced to unsuspended sentences had higher re-offending rates (however measured) even when marital status was controlled.	Subjects were not matched, but marital status, demographic background and drug history were controlled for. Reviewers' note: drug offenders sent to prison seemed to reoffend less often.
23	Spohn C., Holleran D. (2002) (#35)	QE (Missouri, USA)	Prison	Probation	Adult drug and other offenders	48 months	Non-custodial*	
24	Tashima H.N. Marelich W.D. (1989) (#43)	QE (California, USA)	Prison	Alcohol treatment and license suspension	Adults convicted of drunk-driving	24 months	Non-custodial (p<.10)	

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 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. First and second-time offenders sentenced to short-term imprisonment only 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.

Summary

To facilitate the overview, all studies summarized above are shortly summarized in the following Table 1. They appear grouped along methodological criteria: randomized controlled trials (RCT), natural experiments, matched-pair studies using propensity scores and quasi-experimental evaluations (QE) using several control variables (usually gender, age, type of current offense and prior record).

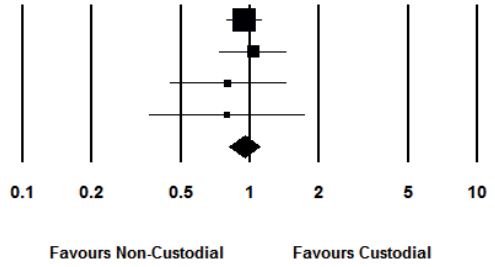
Meta-analysis[2]

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 is to be taken seriously, namely to produce and distribute World-wide reliable knowledge about all sorts of interventions, 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 either randomly assigned to different sanctions or where, through propensity score matching methods, the possibility of unmeasured differences between offenders sent to prison and those sentenced to alternative sanctions is more than minimally controlled. This

[2] The authors are deeply indebted to Dr. David Wilson for his assitance with the present meta-analysis.

Table 2a: Comparison of non-custodial versus custodial sanctions on re-offending (all types of offenders), odds ratios (fixed effects), RCTs and one natural experiment only
Fixed effect

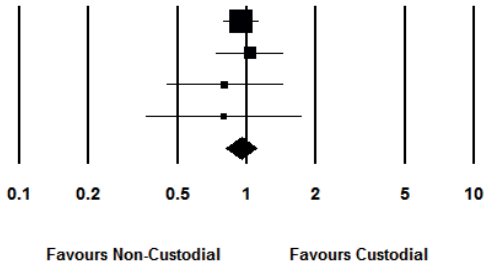
Study name	Subgroup within study	Time point	Odds ratio	Lower limit	Upper limit	Z-Value	p-Value	Relative weight
Van der Werff 1979	Combined	Blank	0.947	0.788	1.137	-0.588	0.557	69.61
Barton 1990	Blank	Blank	1.034	0.733	1.457	0.189	0.850	19.87
Schneider 1986	Blank	Blank	0.801	0.445	1.443	-0.739	0.460	6.75
Killias 2010	Blank	Combined	0.795	0.361	1.747	-0.572	0.567	3.77
			0.946	0.812	1.103	-0.709	0.478	



Model	Number Studies	Point estimate	Lower limit	Upper limit	Z-value	P-value	Q-value	df (Q)	P-value	I-squared	Tau Squared	Standard Error	Variance	Tau
Fixed	4	0.946	0.812	1.103	-0.709	0.478	0.752	3	0.861	0.000	0.000	0.032	0.001	0.000
Random	4	0.946	0.812	1.103	-0.709	0.478								

Table 2b: Comparison of non-custodial versus custodial sanctions on re-offending (all types of offenders), odds ratios (random effects), RCTs and one natural experiment only
Random effect

Study name	Subgroup within study	Time point	Odds ratio	Lower limit	Upper limit	Z-Value	p-Value	Relative weight
Van der Werff 1979	Combined	Blank	0.947	0.788	1.137	-0.588	0.557	69.61
Barton 1990	Blank	Blank	1.034	0.733	1.457	0.189	0.850	19.87
Schneider 1986	Blank	Blank	0.801	0.445	1.443	-0.739	0.460	6.75
Killias 2010	Blank	Combined	0.795	0.361	1.747	-0.572	0.567	3.77
			0.946	0.812	1.103	-0.709	0.478	



also means that quasi-experiments using weaker methods of control 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 risks of re-conviction. Further, this possibility is undoubtedly better controlled by randomized controlled experiments than by any other design, including matching using propensity scores. For these reasons, the following meta-analysis has been conducted in two different steps, first with the four randomized trials and one of the two natural experiments that have been identified, and next for the studies using propensity score matching. The outcome measure is new offences known to the police or reconvictions during the follow-up period, as reported by the authors and summarized above.

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) (our effect size index), according to recommendations in the literature (Lipsey and Wilson, 2001; Wolf, 1986; Glass, McGaw and Smith, 1981). 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 for fixed and random effects, odds ratios, logarithmic odds ratios, and standardized mean differences (respectively Tables 2a/2b and Tables 3a/3b, *and in Appendix-I, Tables 2c/2d/2e/2f and Tables 3c/3d/3e/3f))*.

In two of the four studies (Barton, Schneider) listed in the following Tables, only one effect size has been reported on which a meta-analysis has been feasible. In the two studies by Killias et al., only re-convictions over the entire period (of 11 years) are considered, and the combined effects for all three offences in the van der Werff natural experiment. As explained above, the Bergman study has been excluded since it turned out to be quasi-experimental as far as the comparison of custodial and non-custodial subjects is concerned. For all studies included in the meta-analysis and given the fact that the overall results favoured the null hypothesis, the strongest effect sizes have been used as a conservative way to minimize the chance of obtaining a non-significant outcome. The updated Killias et al. experiment showed also partially significant improvement of social integration among persons assigned to short-term imprisonment. This effect size could not be used, however, given that no other meta-analysed study presented comparable outcomes.

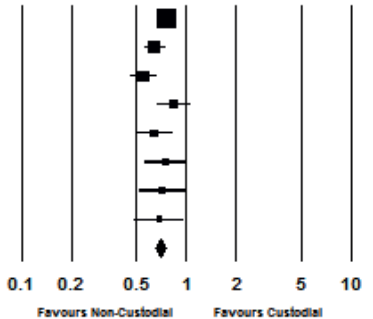
Meta-analysis of RCTs and similar natural experiments

As mentioned above, the four RCTs (or three, the Killias et al. 2010 study being an update) that were located and the van der Werff (1979) natural experiment have been meta-analysed in a

Tables 3a: Comparison of non-custodial versus custodial sanctions on re-offending (all types of offenders), odds ratios (fixed effect), studies using propensity score matching only

Fixed effect

Study name	Odds ratio	Lower limit	Upper limit	Z-Value	p-Value	Relative weight
Wermink	0.759	0.681	0.847	-4.936	0.000	38.36
Lulham	0.640	0.548	0.748	-5.603	0.000	18.85
Nieuwbeerta	0.546	0.455	0.656	-6.449	0.000	13.56
Loughran	0.827	0.654	1.047	-1.577	0.115	8.27
Nirel	0.635	0.493	0.817	-3.525	0.000	7.16
Apel	0.740	0.555	0.986	-2.055	0.040	5.54
Weisburd	0.706	0.514	0.970	-2.151	0.031	4.55
Bales	0.680	0.478	0.966	-2.154	0.031	3.71
	0.693	0.648	0.742	-10.618	0.000	



Model	Number Studies	Effect size and 95% interval: Point estimate	Lower limit	Upper limit	Test of null (2-Tail): Z-value	P-value	Heterogeneity: Q-value	df (Q)	P-value	I-squared	Tau-squared: Tau Squared	Standard Error	Variance	Tau
Fixed	8	0.693	0.648	0.742	-10.618	0.000	12.993	7	0.072	46.124	0.009	0.011	0.000	0.096
Random	8	0.684	0.618	0.758	-7.276	0.000								

Table 3b: Comparison of non-custodial versus custodial sanctions on re-offending (all types of offenders), odds ratios (random effect), studies using propensity score matching only

Random effect

Study name	Odds ratio	Lower limit	Upper limit	Z-Value	p-Value	Relative weight
Wermink	0.759	0.681	0.847	-4.936	0.000	22.15
Lulham	0.640	0.548	0.748	-5.603	0.000	17.55
Nieuwbeerta	0.546	0.455	0.656	-8.449	0.000	15.14
Loughran	0.827	0.654	1.047	-1.577	0.115	11.52
Nirel	0.635	0.493	0.817	-3.525	0.000	10.52
Apel	0.740	0.555	0.986	-2.055	0.040	8.86
Weisburd	0.706	0.514	0.970	-2.151	0.031	7.69
Bales	0.680	0.478	0.966	-2.154	0.031	6.58
	0.684	0.618	0.758	-7.276	0.000	

first step. The results are given in Tables 2a using odds ratios for fixed effects, followed by random effects (Table 2b). The Q-test being non-significant, the random as well as the fixed effect models can be considered as valid.

The results in Tables 2a-2b summarize the results for each of the four studies considered, as well as for all studies together. The results show that custodial and non-custodial sanctions do not differ significantly with regard to their effect on re-offending.

Of course, a meta-analysis based on four studies can easily be criticized for being "too" selective. Further, differences between

custodial and non-custodial sanctions in terms of re-offending are modest at best, although slightly in the direction favourable to non-custodial sanctions.

Meta-analysis of quasi-experimental studies using propensity score matching

In all, 10 studies using propensity scores have been located. Eight of them are included in the following meta-analysis. One study (by McGrath and Weatherburn, 2012) has not been included because essential information to compute effect sizes has not been available. A second study (by Mears et al., 2009) has not been included because the non-custodial condition includes, beyond "alternative" sanctions in the usual sense, also jail (and, thus, a custodial sanction in our definition).

The meta-analytic methods follow the same line as for the RCTs. The effect sizes have been computed based on the log odds ratios, the standardized differences of means and the odds ratios. Effect sizes have been assessed using the fixed as well as the random models. Given the significant Q-tests, the studies are not homogeneous and only the random effects should be considered in interpreting the outcomes.

As we can see from the analyses in Tables 3a-3b, the studies using propensity score matching show, taken together, a significant "criminogenic" effect of imprisonment. Some studies (Wermink et al., Loughran et al., McGrath and Weahterburn, not included) found non-significant effects, suggesting that the difference may not be too substantial. All in all, however there is no doubt that the two parts of the meta-analysis lead to contradicting conclusion, i.e. namely a "null effect" if the analysis is limited to RCTs and one RCT-like natural experiment, and a significant effect in favor of non-custodial sanctions if the analysis is conducted with studies using propensity score matching. To our knowledge, this contradiction has not attracted much attention so far, as e.g. in the review of the evidence by Nagin, Cullen and Jonson (2009). A possible explanation may be that RCTs are in a better situation to control for potentially influential variables, such as some unrecorded personal characteristics of offenders that may affect sentencing decisions as well as risks of re-offending. That some studies based on propensity score matching reach similar ("neutral") conclusions may be related to a more complete control of such confounding variables. In sum, the safe conclusion may be that studies that have controlled for a more complete assemblage of independent variables provide results that are close to a "null effect", whereas studies with a more limited number of controlled variables produce results that are consistently in favor of non-custodial sanctions.

5. Discussion

What did we learn through this updated systematic review?

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, an overwhelming majority of the studies summarized here as well as in other reviews (Nagin, Cullen and Jonson 2009, Smith, Goggin and Gendreau 2002, Gendreau, Goggin and Cullen 1999) point in the direction of criminogenic effects of imprisonment. However, it has also been generally recognized that any difference between custodial and non-custodial conditions are smaller when more relevant independent variables are controlled for. Given that more recent studies have tried to improve control of extraneous variables through new statistical techniques (such as, for example, propensity score matching), an update of the Campbell systematic review published in 2006 was thought to be helpful for researchers, policy makers and legislators. The present update has been undertaken with the purpose of taking these more recent and presumably more convincing studies into account

Unfortunately, the number of randomized trials has not increased over the last ten years. The only new RCT which came to our attention was indeed an update of an earlier trial (Killias, Ribeaud and Aebi, 2000, #03) that extended the post-intervention period from two to eleven years and used more complete measures of social integration (Killias et al. 2010, #U01). New examination of the original reports of all the RCTs led to the exclusion of one formerly included study (Bergman 1976, #02) because subjects had not been assigned randomly to the custodial or to the non-custodial condition, as previously presumed (by our team as well as by Nagin, Cullen and Jonson 2009). The first meta-analysis, conducted with four RCTs and one natural experiment, confirmed the results published in 2006, namely a zero-effect of imprisonment over its non-custodial alternatives. As in the previous systematic review, it has been decided to analyse these high-quality studies separately from those where the similitude of groups is more questionable. The internal validity is a higher priority than statistical power with biased data.

As explained throughout this report and as Walker, Farrington and Tucker (1981) 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 may be one of the reasons why the proportion of total variance in re-offending that is explained in multivariate regression analyses remains relatively modest. Bondeson (1994/2002, #08) and Börjeson (1966, #11), who reached 40 per cent, are noteworthy exceptions, probably due to the fact that they controlled for an unusually large number of independent variables (about 40). Even so, a very high proportion of variance in reoffending may be due to variables that remain unknown or that have not been controlled for. This was one of the arguments Weisburd (2010) invoked to criticize the reliance on correlational studies rather than RCTs in assessing the effectiveness of certain policies.

Already at the moment of the first systematic review, a few matched studies have been identified that controlled for a certain number of critical independent variables affecting risks of reoffending and, at the same time, sentencing judges' decisions over the type of sanction to be imposed. In this update, we have focused on studies that used propensity score matching. This method provides clear advantages over classical matching due to the so-called dimensionality problem, i.e. the fact that perfect matching (that does not reduce continuous scales to ordinal scales or dichotomies) does not allow controlling beyond very few variables. Over the last decade, eight studies using propensity score matching have been located, while two further studies were published in the 1990ies (Weisburd, Waring and Chayet 1995, #07; Nirel et al. 1997, #U09). Nine of these studies qualified for inclusion in the meta-analysis. One study (Mears et al. 2012, #U07) has been excluded because the "non-custodial" group includes jail which, according to our definition, is to be considered as a custodial sanction.

The studies using propensity score matching point, taken together, into the direction of a criminogenic effect of imprisonment, although the differences are sometimes small and not always significant. Does this mean that, in terms of rehabilitation, community service, fines, suspended sentences, electronic monitoring and all the other feasible non-custodial sanctions produce better results than imprisonment? We believe not.

The problem is that comparisons between custodial and non-custodial sanctions are systematically biased because, as Bales and Piqueiro (2012) phrased it, *"the main problem in this area of research is that individuals sentenced to prison differ in fundamental ways from those individuals who receive a non-custodial sanction"* (p. 97). Propensity score matching is certainly a powerful tool in controlling for a given set of variables. Nagin, Cullen and Jonson (2009) advocate that five critical variables should, through propensity score matching, be taken into account, namely gender,

age, race, current offense and prior record. But why should just these five variables be controlled for, why not many more or why all of them? As Bales and Piqueiro (2012) observe, *"…there may be other covariates not represented in the data that are related to both the imprisonment and recidivism variables"* (p. 77). Feasible candidates might be, among other variables, a history of alcohol or drug abuse, employment history, involvement in and quality of a marital relationship, having dependent children to care for, addiction to gambling and possibly many more that all affect risk of reoffending as well as judge's sentencing preferences (Wermink et al. 2010, 344; Nieuwbeerta et al. 2009, 251). In addition, some non-verbal cues such as the defendant's dressing style, his body-language and the attitude he/she displays towards the victim, the offence and the court, may further be worth being controlled for. Some evidence indicates that, for example, attitudes have an impact not only on judicial decision-making, but also on the risks of re-offending (Henning and Frueh 1996). Unfortunately, none of the studies located so far has ever included variables like these in the propensity scores or other matching procedures. The truth probably is that judges are less simple human beings and their reasoning may be more complex than what the research has been able to model.

The only way how the shortcoming of uncontrolled variables can be overcome are randomized controlled trials. Therefore, whatever the appeal of propensity scores and other matching procedures, no method has come into play so far that could claim to be a full-fledged substitute to random assignment of subjects (Weisburd 2010). Unfortunately, it has become all too comfortable to dismiss in advance RCTs on ethical grounds or because they are more difficult to "sell" to funding agencies. In the literature, several statements can be found about the few RCTs conducted in the present field that may not be justified. For example, McGrath and Weatherburn (2012, #U06) observe that Schneider's (1986, #04) experiment was flawed because random assignment was overridden by judicial interventions in a few cases. Although, through judicial decisions, a few subjects became cross-overs (serving non-custodial sanctions instead of being placed in a juvenile institution, or the other way around), the truth is that Schneider correctly analysed these subjects "as assigned" as well as "as treated". (Note that no difference was found between these two ways of analysing the data.) By any standard, this is a conservative procedure that minimizes all excessive conclusions (in either way). Further, the RCT by Barton and Butts (1990, #01) has been criticized for having ignored the fact that incarcerated subjects had a shorter post-intervention period than those receiving a non-custodial sentence (McGrath and Weatherburn 2012). However, Barton and Butts (1990) have well taken this difference into account by standardiz-

ing post-intervention periods. Finally the Killias et al. (2000, #03) experiment has been criticized for having overlooked that the correctional service kept the right to revoke community service (and replace it by prison), or that a small number of defendants finally opted, despite having been randomly assigned to community service, for (halfway) prison (Nieuwbeerta et al. 2009). These cases have been correctly treated as drop-outs and documented (Killias et al. 2010, #U01). The truth is that no RCTs have ever been conducted without some subjects dropping out of the condition they were assigned to. As long as the proportion of drop outs remains low and the statistical analyses of these cases remains conservative, there is no reason to dismiss the findings.

The only alternative to RCTs may be natural experiments (as the one described by van der Werff 1979, #06) where it can be assumed that subjects have been sentenced to prison or a non-custodial sanction for reasons related to the date of a royal pardon, i.e. an element that can be presumed to be unrelated to the sentence and to later re-offending. That is not to say that quasi-experiments could not be improved beyond controlling for variables that usually can be found in criminal records, such as offence type, prior record, gender, age and ethnic/racial background. As the third authors has suggested during several workshops where future studies were to be planned, one could imagine, for example, that researchers follow court hearings as observers and rate, independently of the judge's later decision, what sentence they would predict given the offender's background and the merits of the case. During such hearings, researchers could observe also non-verbal cues, such as the way the defendant dresses, his body-language and the attitude he displays towards the victim, the offence and the court. Whenever the judge's sentencing decision deviates from what the researcher would have expected, an interview might help to understand even better the subtle mechanisms of judicial decision-making. No study that would have used direct observation as a method has come to our attention, however.

There are a few more ways how quasi-experiments could be improved, whatever the matching method they use. For example, most studies have compared post-sanction recidivism rates across different sanctions, but have not compared levels of "improvement" during the post-intervention period compared to an identical period before the intervention. As in the study by Schneider (1986, #04), even randomly assigned samples of offenders undergoing different sanctions may 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, as done by Schneider (1986, #04), Empey and Steven (1971) and Killias, Aebi and Ribeaud (2000, #03). All three 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 future offending.

Given the quantity of studies that have been meta-analysed in this updated review (that covers studies up to 2013), what conclusion can we draw? Ignoring methodological rigor, the apparent conclusion would be that imprisonment is increasing the risk of re-offending among those who experience it, compared to those who "benefit" from a non-custodial sanction. If we take into account the methodological rigor, the conclusion is that imprisonment is damaging according to a majority of quasi-experimental studies, while the randomized or natural trials located for this update do not warrant such a conclusion. Rather, the highest quality evidence suggests no difference between these sentencing options. We think it is disturbing that the potential "criminogenic" effect of prison receives the strongest support from studies that have the least successfully managed controlling relevant third variables. Although a few quasi-experimental studies found comparable outcomes for custodial and non-custodial sanctions, anti-prison conclusions prevail particularly among the studies that have not been integrated in the meta-analysis, but summarized in the text. The most credible interpretation of the evidence is that any difference between prison and alternative sanctions is a wash.. There are obviously a number of caveats, among them the fact that some possibly important variables (such as the length of imprisonment) have not been considered in this review. These will be discussed below.

Beyond "criminogenic" effects of imprisonment

If prison does not have much effect in either way relative to non-custodial alternatives, what does this mean considering other aspects of punishment and social control? Why, despite the evidence showing that imprisonment is not reducing re-offending relative to a community sanction, do virtually all societies across the World continue using it, and often even in growing proportions? Considering that prisons are expensive to build and to operate, policies supporting its continued use would seem to be perfectly irrational. Some scholars may indeed subscribe to this claim without hesitation.

Beyond rehabilitation, prisons have always been considered useful as means of incapacitating (at least temporarily) offenders who otherwise might continue doing harm to other people. It is not the purpose of this review to look into this matter (see Nagin 2013 for a recent review of the evidence). However, the issue is not only whether incapacitation works as a general crime control strategy – an idea that has widely contributed to mass-incarcera-

tion in the United States and elsewhere. Even short-term imprisonment has the obvious advantage of stopping temporarily offenders from doing harm to others. Since offenders are often arrested at a time of frequent offending (when they actually faced the highest risk of arrest), stopping them from pursuing an intensive period of delinquency may well have beneficial effects for public safety. In research on recidivism it has been an obvious requirement to start the observation period when prisoners are released, and to compare following re-offending rates with those of defendants sentenced to a non-custodial sanction. The decision to start with "time at risk" is obviously a heritage of medical thinking in criminology where the issue is recovery or rehabilitation. Looking at the matter from a public safety angle, however, a correct assessment of potential benefits of imprisonment should follow prisoners and those receiving alternative sanctions from the time of disposition, and compare re-offending rates of both groups from that moment. In studies included in this literature review, this has not been the generally followed approach however – leading to a further and serious caveat about the apparently detrimental effect of imprisonment.

Beyond incapacitation, prisons may serve also purposes of deterrence in other ways. Some recent studies have shed new light on this question. For example, Helland and Tabarrok (2007) found evidence of a deterrent effect in their analysis of data from California and other states having adopted Three-Strikes Laws. They compared re-offending among offenders who were released from prison in 1994 after having experienced two strikeable convictions, with those released after two trials for strikeable offenses, but only one "second-strike" conviction. The results suggest a reduction of re-offending due to the immediate threat of a third strike by about 17 per cent.

In a similar way, Drago, Galbiati and Vertova (2009) have analysed a brilliant natural experiment conducted in Italy in 2006. With the purpose to reduce prison overcrowding, the Parliament voted an amnesty that reduced the remaining sentence of all prisoners uniformly by three years. About 22,000 inmates who had no more than three years left to serve in prison were all released on August 1st, 2006. The law provided that offenders would be immediately re-incarcerated to serve the remaining time in case of a new offence. Thus, released offenders had to loose, in case of re-offending, between one day and three years. Further, this threat does not vary with the offenders' profile since all prisoners benefitted, regardless of the length of their sentence, of a reduction by up to three years. Drago, Galbiati and Vertova (2009) analysed the outcomes over a period of 7 months and found that re-offending dropped by 0.16 percentage points for every additional month an

offender risked losing in case of a new offence.[3] Along with Drago et al. (2009) who made a similar statement before, Nagin et al. (2009) noticed, however, that according to this study the length of imprisonment was positively correlated with re-offending, and they concluded that longer periods of incarceration may be "criminogenic". This interpretation may be questionable, however, since prisoners serving longer sentences almost certainly differ in profile from those who were incarcerated for shorter periods. The same reservation seems warranted with respect to the finding that prisoners having served their time in particularly harsh prisons re-offend slightly more often (Drago, Galbiati and Vertova 2011). It is at least plausible that prisoners are assigned to Italy's highly variable facilities in line with their profile. Noteworthy is an indirect (or general) deterrent effect that Drago and Galbiati (2012) found with respect to peers of those released who lived in the same area/region. The effect on peers was of comparable size to what had been observed on released prisoners themselves.

A further randomized controlled experiment with several interventions to push defaulters to pay their fines showed that the threat of immediate incarceration was more efficient than milder admonitions (Weisburd, Einat and Kowalski, 2008).

These findings support the conclusion that perhaps not prison as such, but the threat of prison may be a powerful means to reduce future offending. Similar conclusions had been reached by Gottfredson and Barton (1993) in their analysis of the closing of one of the State of Maryland's training school for juvenile delinquents (Montrose). There, as well as after the deinstitutionalization experience in Massachusetts (evaluated at that time by Ohlin et al. 1977 and Coates et al. 1978, quoted by Gottfredson and Barton 1993), closing of training schools was followed by higher rates of offending among those who, otherwise, would have been placed there but remained in the community, compared to those who had spent their full sentence in confinement at Montrose, or at least part of it (the "transition" group).[4]

The threat (or its removal) of experiencing custody may, thus, have some effect on people's behaviour. Perhaps the wide use of suspended sentences is justified because the threat of incarceration in case of a new offence may work as a powerful deterrent. Since defendants receiving suspended sentences are hardly comparable to those going to prison, a direct comparison of re-offending rates among those with suspended and unsuspended sentences will not allow clearing this question any further. However, it might

[3] This post-intervention period may seem exceptionally short, but work in progress by the authors apparently confirms the results over a period of now 17 months (correspondence with Professor Drago, 21 March 2014).

[4] Due to problems of comparability among the three groups, this natural experiment did not qualify for inclusion in the meta-analysis.

be worth studying recidivism rates before and after legal change among those who, as a result of a new law, receive a non-custodial rather than a prison sentence, with those who, before the legal change, received a custodial sentence but would otherwise have qualified for a non-custodial sanction. Unfortunately, no study who took advantage of legal change in this area has been located.[5]

Finally, punishment in general and prison in particular may be needed to show the public that justice is really being done. As Fehr and Gaechter (2002) have shown in a laboratory experiment, people have a strong desire that those who deceive their normative expectations will undergo some form of punishment. This desire for "equity" does not necessarily mean that mass incarceration as in the USA will be needed, as the example of many European countries with similar crime rates and far lower rates of incarceration illustrates. On the other hand, some European countries have reduced an offender's odds to be sentenced to an immediate custodial sentence to a point where social cohesion may become problematic. Usually, the Scandinavian countries are mentioned in this context, but the real "prison-free" society is Switzerland where the probability of a convicted defendant to receive (even a short) unsuspended prison sentence is below 30 per cent in case of robbery and even below 10 per cent in cases of sexual assault or aggravated assault (European Sourcebook 2010, Tables 3.2.3.6–3.2.3.10). American readers who are familiar with mandatory and long sentences should not forget that extremes can also go in an opposite direction and possibly produce similar problems of legitimacy and control.

Observations on methodological issues

Our review allows a number of methodological observations that may be helpful for future evaluations of "alternative" sanctions or programs. These observations 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 equal across these groups. Therefore, efforts (particularly in more recent studies) to differentiate across types of offenders are to be welcome.

(2) The length of the observation period differs from study to study. At the same time, we know that recidivism rates do

[5] The Swiss Federal Office of Statistics published a study on re-offending under the old and the new criminal code, www. /www.bfs.admin.ch/bfs/portal/de/tools/search.html (Neues Sanktionenrecht und strafrechtlicher Rückfall, BfS Aktuell, November 2011). Under the new law, recidivism did not decrease, although "detrimental" prison sentences were used far less often.

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 132 months. Experts in the field of recidivism have always insisted that one year or less was too short an observation period, and that the minimal observation period ought to be 36 months at least.

(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, however, many offenders are re-incarcerated because of technical violations of parole. Under both systems, re-incarceration mostly depends on the previous sentence and the criminal history of the offender. Since defendants with previous incarceration face higher risks of being sentenced to a custodial sentence than those having experienced non-custodial sanctions only, higher re-incarceration rates following imprisonment compared to alternative sanctions does not support the conclusion of a "criminogenic" effect of imprisonment, but rather reflects selection (i.e. sentencing) patterns of sentencing judges. Cid (2009) and McGrath and Weatherburn (2012) both concluded that prison was more damaging in case of previously incarcerated offenders or among those with more previous convictions; however, they may have overlooked the role of previous on future judicial decisions. Less problematic are, in this respect, data on re-arrest and re-convictions. Under continental sentencing systems, however, judges impose one sentence for all offences that come to their attention in one procedure. Therefore, one new conviction can be pronounced for just one or for a multitude of new offences. Therefore, beyond prevalence rates of post-intervention re-convictions or re-arrests, measures of the number of new offences as registered by the police (i.e. "incidence" rather than simple "prevalence" rates) may be more helpful to assess changes in the intensity of offending. So far, not all too many studies have attempted to measure recidivism through incidence rates of offending. Even less have used questionnaires of self-reported delinquency (Barton and Butts, 1990, #10, being among the few exceptions) that, beyond offering an opportunity to validate official counts, provide a unique opportunity to learn more about criminal activities that may otherwise remain unnoticed. Self-reports, if sufficiently adapted to measure serious and not just trivial offending, allow to assess, beyond the simple prevalence ("yes/no") of arrests or convictions after an intervention, variations

in the frequency of offending ("incidence rates") before and after an intervention or, in plain terms, subjects' relative improvement following different types of 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 studies using propensity scores 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 detrimental effects of "short" custodial sanctions, this limitation of our review may be less relevant, however. Intuitively, it seems plausible that "prisonization" effects are more frequent after custodial sentences of some length (Bushway 1998). 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. Beyond the fact that previous and, in particular, long-term incarceration is an important factor in future decisions by judges, this conclusion may not be warranted given the many confounding factors that were not adequately controlled for in many among the reviewed studies.

(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 supervised by electronic monitoring. Some of these sanctions may have opposite effects on re-offending (see 8, below). 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 have been exceptional, however. In other words, it is not known whether interventions beyond incarceration or "alternative" sanctions are able to produce any rehabilitative effect.

(7) The 5 randomized and natural experiments and the nine studies using propensity score matching that have been eli-

gible for the meta-analysis extend over a period of 45 years. During all these years, the custodial sentences have 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 cannot 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. While some studies find a detrimental effect of incarceration on future employment opportunities (as for example Apel and Sweeten, 2010b) and social integration, others reach mixed conclusions in this respect (Lamb and Goertzel 1974, Killias, Aebi and Ribeaud 2000, Killias et al. 2010). Given the often extremely short duration of custodial sentences compared to "alternative" sanctions, it seems plausible that any "prisonization" effect has been limited at worst. In the case of randomised controlled trials, it would be easy, however, to conduct later follow-up studies considering, beyond measures of re-offending, indicators of social integration as they could routinely be found in the files of internal revenue services, such as family disruption, unemployment, income, welfare revenues, debts, assets and mental health. 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 about "detrimental" effects of custodial sanctions on these levels, it is rather surprising that, apparently, almost no data have been collected on such outcomes. For example, in an experiment comparing electronic monitoring with community service, it was found that offenders in home-confinement re-offended less and did better, in economic and social respects, than those who experienced community service (Killias et al. 2010b). This experiment also illustrates that outcomes may differ substantially across different "alternative" sanctions.

(9) To the extent that lower re-offending rates have been observed after "alternative" compared to custodial sanctions, it cannot be ruled out that something like a Hawthorne or

a "*placebo*" effect [6] has been at work. Indeed, persons convicted to a custodial sanction who get the "chance" to serve it under the form of an "alternative" obtain, in some way, a second chance which, in turn, may favourably affect their attitudes (as observed by Killias, Aebi and Ribeaud 2000, or perhaps violent offenders in the van der Werff (1979) natural experiment). As experiments on cooperation between unrelated individuals (Fehr and Rockenbach, 2003) have 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 willingness to cooperate. This finding matches similar results on effects of attitude change on re-offending, often as a result of cognitive-behavioural treatment (Henning and Frueh, 1996; Vennard, Hedderman and Sugg, 1997) or "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) In assessing the criminogenic or treatment effects of imprisonment on those who experienced this kind of sanction, more attention should be given to factors beyond the prison setting. Where do prisoners return after their release, and what kind of criminal or legitimate opportunities do they find? In another natural experiment, Kirk (2009, 2012) found that prisoners who, as a result of Hurricane Katrina, had left their native area in and around New Orleans and settled in other states, re-offended considerably less than those who returned home and, thus, presumably to their old networks of delinquent associates. Similar observations were made by Sampson and Laub (1995) in their re-analysis of the Glueck data, or by Sharkey and Sampson (2010) among prisoners who had left Chicago. Perhaps prison as such is

[6] 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.

less important for re-offending than what environment prisoners find after release.

(11) 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, urge Governments to insist on experimental designs whenever they test new sanctions or new programs.

6. Conclusions

Consistent with Smith, Goggin and Gendreau (2002) and Nagin, Cullen and Jonson (2009), this updated systematic review leads to ambiguous findings. A majority of quasi-experimental studies suggest that prison is followed by higher re-offending rates than non-custodial sanctions, whereas experimental studies show no difference. Given that the studies with the lower risk of selection bias (i.e., randomised controlled trials) find no difference, it seems that the outcomes of the quasi-experimental studies are for the most part a matter of assigning offenders with different profiles to specific sanctions (Palmer 1974).

In the future, it will be important to develop more rigorous evaluation standards in the field of research on re-offending. Randomised controlled trials or natural experiments ought to be preferred, whenever possible, by researchers and policy-makers. 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 may not have been anticipated at the start of a trial. At the very least, future analyses should go beyond controlling (under whatever form) those few variables that can be found in criminal records, such as age, gender, offence type and criminal history, but include data on alcohol and drug abuse, employment history, family and other private-life networks, income and offenders' attitudes toward the offence, the victim and the court. If possible, direct (participating) observation, e.g. in a court room, could be used to assess some of these variables. They are likely to affect judicial decision-making as well as re-offending.

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 two decades in Switzerland, we may reply that, in our experience with correctional services, convicted offenders 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 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 allowing the Government to introduce, 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 to participate, and claim to be treated "according to the law" (and go to prison). No one, however, 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 other countries where new sanctions have been introduced as a temporary and more or less "experimental" arrangement. Therefore, experimental evaluations should have been no less feasible. Finally, ethical reservations are not justified as long as no evidence has shown that "new" sanctions or programs produce better results than traditional ones, or that they at least are not damaging.

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 offenders' partners and children, Murray et al. 2009, 2014), 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 detrimental 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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Appendix I: Additional Tables

A-1 Complementary meta-analysis on the three RCTs and the van der Werff natural experiment with log-odds ratio and standardized mean differences

Tables 2c: Comparison of non-custodial versus custodial sanctions on re-offending (all types of offenders), RCTs and one natural experiment only

Fixed effect

Study name	Subgroup within study	Time point	Log odds ratio	Standard error	Statistics for each study: Variance	Lower limit	Upper limit	Z-Value	p-Value	Relative weight
Van der Werff 1979	Combined	Blank	-0.055	0.094	0.009	-0.238	0.128	-0.588	0.557	69.61
Barton 1990	Blank	Blank	0.033	0.175	0.031	-0.310	0.376	0.189	0.850	19.87
Schneider 1986	Blank	Blank	-0.222	0.300	0.090	-0.811	0.367	-0.739	0.460	6.75
Killias 2010	Blank	Combined	-0.230	0.402	0.162	-1.018	0.558	-0.572	0.567	3.77
			-0.055	0.078	0.006	-0.208	0.098	-0.709	0.478	

Log odds ratio and 95% CI

-1.00 -0.50 0.00 0.50 1.00

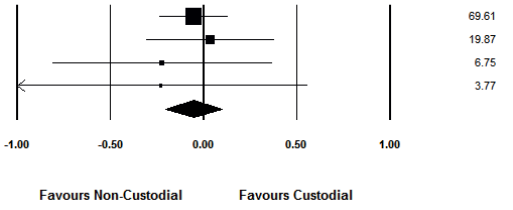
Favours Non-Custodial Favours Custodial

Model	Number Studies	Effect size and 95% confidence interval: Point estimate	Standard error	Variance	Lower limit	Upper limit	Test of null (2-Tail): Z-value	P-value	Heterogeneity: Q-value	df (Q)	P-value	I-squared	Tau-squared: Tau Squared	Standard Error	Variance	Tau
Fixed	4	-0.055	0.078	0.006	-0.208	0.098	-0.709	0.478	0.752	3	0.861	0.000	0.000	0.032	0.001	0.000
Random	4	-0.055	0.078	0.006	-0.208	0.098	-0.709	0.478								

Table 2d: Comparison of non-custodial versus custodial sanctions on re-offending (all types of offenders), log odds ratio (random effect), RCTs and one natural experiment only

Random effect

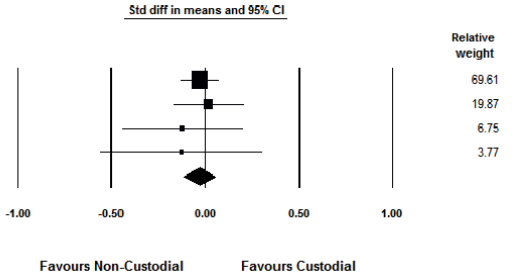
Study name	Subgroup within study	Time point	Log odds ratio	Standard error	Variance	Lower limit	Upper limit	Z-Value	p-Value	Relative weight
Van der Werff 1979	Combined	Blank	-0.055	0.094	0.009	-0.238	0.128	-0.588	0.557	69.61
Barton 1990	Blank	Blank	0.033	0.175	0.031	-0.310	0.376	0.189	0.850	19.87
Schneider 1986	Blank	Blank	-0.222	0.300	0.090	-0.811	0.367	-0.739	0.460	6.75
Killias 2010	Blank	Combined	-0.230	0.402	0.162	-1.018	0.558	-0.572	0.567	3.77
			-0.055	0.078	0.006	-0.208	0.098	-0.709	0.478	



Tables 2e: Comparison of non-custodial versus custodial sanctions on re-offending (all types of offenders), standardized mean differences (fixed effects), RCTs and one natural experiment

Fixed effect

Study name	Subgroup within study	Time point	Statistics for each study							Relative weight
			Std diff in means	Standard error	Variance	Lower limit	Upper limit	Z-Value	p-Value	
Van der Werff 1979	Combined	Blank	-0.030	0.052	0.003	-0.131	0.071	-0.588	0.557	69.61
Barton 1990	Blank	Blank	0.018	0.097	0.009	-0.171	0.207	0.189	0.850	19.87
Schneider 1986	Blank	Blank	-0.122	0.166	0.027	-0.447	0.202	-0.739	0.460	6.75
Killias 2010	Blank	Combined	-0.127	0.222	0.049	-0.561	0.307	-0.572	0.567	3.77
			-0.031	0.043	0.002	-0.115	0.054	-0.709	0.478	

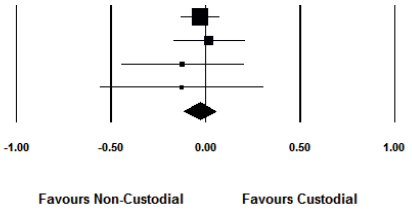


Model		Effect size and 95% confidence interval					Test of null (2-Tail)		Heterogeneity				Tau-squared			
Model	Number Studies	Point estimate	Standard error	Variance	Lower limit	Upper limit	Z-value	P-value	Q-value	df (Q)	P-value	I-squared	Tau Squared	Standard Error	Variance	Tau
Fixed	4	-0.031	0.043	0.002	-0.115	0.054	-0.709	0.478	0.752	3	0.861	0.000	0.000	0.010	0.000	0.000
Random	4	-0.031	0.043	0.002	-0.115	0.054	-0.709	0.478								

Table 2f: Comparison of non-custodial versus custodial sanctions on re-offending (all types of offenders), standardized mean differences (random effects), RCTs and one natural experiment only

Random effect

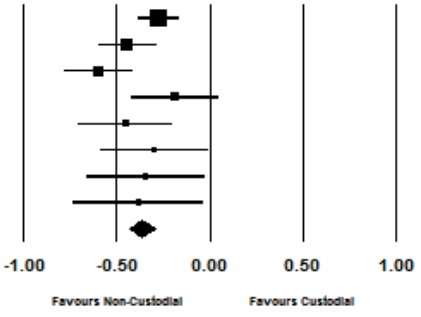
Study name	Subgroup within study	Time point	Std diff in means	Standard error	Variance	Lower limit	Upper limit	Z-Value	p-Value	Relative weight
Van der Werff 1979	Combined	Blank	-0.030	0.052	0.003	-0.131	0.071	-0.588	0.557	69.61
Barton 1990	Blank	Blank	0.018	0.097	0.009	-0.171	0.207	0.189	0.850	19.87
Schneider 1986	Blank	Blank	-0.122	0.166	0.027	-0.447	0.202	-0.739	0.460	6.75
Killias 2010	Blank	Combined	-0.127	0.222	0.049	-0.561	0.307	-0.572	0.567	3.77
			-0.031	0.043	0.002	-0.115	0.054	-0.709	0.478	



Tables 3c: Comparison of non-custodial versus custodial sanctions on re-offending (all types of offenders), log odds ratio (fixed effect), studies using propensity score matching only

Fixed effect

Study name	Log odds ratio	Standard error	Variance	Lower limit	Upper limit	Z-Value	p-Value	Relative weight
Wermink	-0.275	0.056	0.003	-0.384	-0.166	-4.936	0.000	38.36
Lulham	-0.446	0.080	0.006	-0.602	-0.290	-5.603	0.000	18.85
Nieuwbeerta	-0.605	0.094	0.009	-0.788	-0.421	-6.449	0.000	13.56
Loughran	-0.189	0.120	0.014	-0.425	0.046	-1.577	0.115	8.27
Nirel	-0.455	0.129	0.017	-0.708	-0.202	-3.525	0.000	7.16
Apel	-0.301	0.147	0.022	-0.589	-0.014	-2.055	0.040	5.54
Weisburd	-0.348	0.162	0.026	-0.665	-0.031	-2.151	0.031	4.55
Bales	-0.386	0.179	0.032	-0.738	-0.035	-2.154	0.031	3.71
	-0.367	0.035	0.001	-0.434	-0.299	-10.618	0.000	

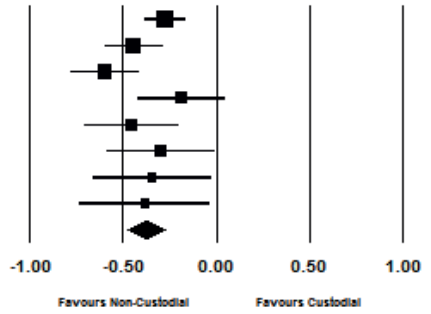


Model	Effect size and 95% confidence interval					Test of null (2-Tail)		Heterogeneity				Tau-squared			
Model	Number Studies	Point estimate	Standard error	Variance	Lower limit	Upper limit	Z-value	P-value	Q-value	df (Q)	P-value	I-squared	Tau Squared	Standard Error	Variance	Tau
Fixed	8	-0.367	0.035	0.001	-0.434	-0.299	-10.618	0.000	12.993	7	0.072	46.124	0.009	0.011	0.000	0.096
Random	8	-0.379	0.052	0.003	-0.481	-0.277	-7.276	0.000								

Table 3d: Comparison of non-custodial versus custodial sanctions on re-offending (all types of offenders), log odds ratio (random effect), studies using propensity score matching only

Random effect

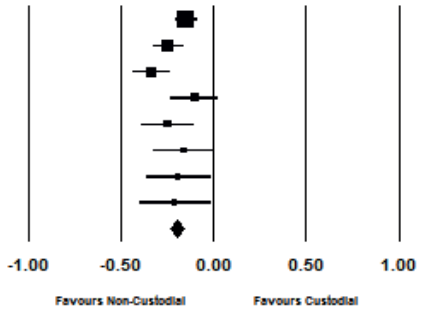
Study name	Log odds ratio	Standard error	Variance	Lower limit	Upper limit	Z-Value	p-Value	Relative weight
Wermink	-0.275	0.056	0.003	-0.384	-0.166	-4.936	0.000	22.15
Lulham	-0.446	0.080	0.006	-0.602	-0.290	-5.603	0.000	17.55
Nieuwbeerta	-0.605	0.094	0.009	-0.788	-0.421	-6.449	0.000	15.14
Loughran	-0.189	0.120	0.014	-0.425	0.046	-1.577	0.115	11.52
Nirel	-0.455	0.129	0.017	-0.708	-0.202	-3.525	0.000	10.52
Apel	-0.301	0.147	0.022	-0.589	-0.014	-2.055	0.040	8.86
Weisburd	-0.348	0.162	0.026	-0.665	-0.031	-2.151	0.031	7.69
Bales	-0.386	0.179	0.032	-0.738	-0.035	-2.154	0.031	6.58
	-0.379	0.052	0.003	-0.481	-0.277	-7.276	0.000	



Tables 3e: Comparison of non-custodial versus custodial sanctions on re-offending (all types of offenders), standardized mean differences (fixed effect), studies using propensity score matching only

Fixed effect

Study name	Std diff in means	Standard error	Variance	Lower limit	Upper limit	Z-Value	p-Value	Relative weight
Wermink	-0.152	0.031	0.001	-0.212	-0.091	-4.936	0.000	38.36
Lulham	-0.246	0.044	0.002	-0.332	-0.160	-5.603	0.000	18.85
Nieuwbeerta	-0.333	0.052	0.003	-0.435	-0.232	-6.449	0.000	13.56
Loughran	-0.104	0.066	0.004	-0.234	0.025	-1.577	0.115	8.27
Nirel	-0.251	0.071	0.005	-0.390	-0.111	-3.525	0.000	7.16
Apel	-0.166	0.081	0.007	-0.325	-0.008	-2.055	0.040	5.54
Weisburd	-0.192	0.089	0.008	-0.367	-0.017	-2.151	0.031	4.55
Bales	-0.213	0.099	0.010	-0.407	-0.019	-2.154	0.031	3.71
	-0.202	0.019	0.000	-0.239	-0.165	-10.618	0.000	

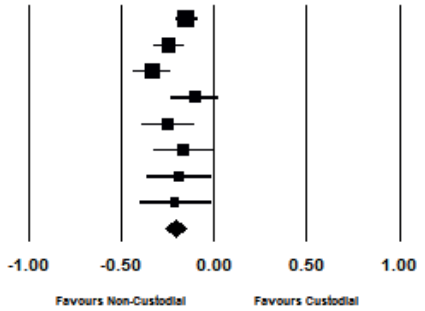


Model		Effect size and 95% confidence interval					Test of null (2-Tail)		Heterogeneity				Tau-squared			
Model	Number Studies	Point estimate	Standard error	Variance	Lower limit	Upper limit	Z-value	P-value	Q-value	df (Q)	P-value	I-squared	Tau Squared	Standard Error	Variance	Tau
Fixed	8	-0.202	0.019	0.000	-0.239	-0.165	-10.618	0.000	12.993	7	0.072	46.124	0.003	0.003	0.000	0.053
Random	8	-0.209	0.029	0.001	-0.265	-0.153	-7.276	0.000								

Table 3f: Comparison of non-custodial versus custodial sanctions on re-offending (all types of offenders), standardized mean differences (random effect), studies using propensity score matching only

Random effect

Study name	Std diff in means	Standard error	Variance	Lower limit	Upper limit	Z-Value	p-Value	Relative weight
Wermink	-0.152	0.031	0.001	-0.212	-0.091	-4.936	0.000	22.15
Lulham	-0.246	0.044	0.002	-0.332	-0.160	-5.603	0.000	17.55
Nieuwbeerta	-0.333	0.052	0.003	-0.435	-0.232	-6.449	0.000	15.14
Loughran	-0.104	0.066	0.004	-0.234	0.025	-1.577	0.115	11.52
Nirel	-0.251	0.071	0.005	-0.390	-0.111	-3.525	0.000	10.52
Apel	-0.166	0.081	0.007	-0.325	-0.008	-2.055	0.040	8.86
Weisburd	-0.192	0.089	0.008	-0.367	-0.017	-2.151	0.031	7.69
Bales	-0.213	0.099	0.010	-0.407	-0.019	-2.154	0.031	6.58
	-0.209	0.029	0.001	-0.265	-0.153	-7.276	0.000	



Appendix II: Bibliography of the systematic review

A. The 38 eligible studies

Studies listed under 1, 2 and 3 and with an * have been meta-analysed. Studies under 4 are summarized in Table 1 because they reach higher methodological standards (more than three control variables and higher statistical standards), though not to the extent of studies included in the meta-analysis.

1 Controlled randomized trials

*01 Barton W.H., Butts J.A. (1990).Viable options: intensive supervision programs for juvenile delinquents. *Crime and Delinquency* 36/2, 238-256.

02 Bergman G.R. (1976). *The evaluation of an experimental program designed to reduce recidivism among second felony criminal offenders*. Wayne State University, Detroit (Mich.), PhD dissertation (77-9368). (This study is listed here because it was included among the RCTs in the first edition of this review. It has not been retained among RCTs in the update, however)

*03 Killias M., Aebi M., Ribeaud D. (2000). Does community service rehabilitate better than shorter-term imprisonment ? : Results of a controlled experiment. *Howard Journal of Criminal Justice* 39/1, 40-57.

*U01 Killias M., Gilliéron G., Villard F., Poglia C. (2010a) How damaging is imprisonment in the long-term? A controlled experiment comparing long-term effects of community service and short custodial sentences on re-offending and social integration. *J. of Experimental Criminology* 6/2, 115-130.

*04 Schneider A.L. (1986), Restitution and recidivism rates of juvenile offenders: results from four experimental studies. *Criminology* 24/3, 533-552

2 Natural experiments[7]

05 Gottfredson D.C., Barton W. H. (1993). Deinstitutionalization of juvenile offenders. *Criminology* 31/4, 591-610. (Not included in the meta-analysis)

[7] Unless otherwise indicated, studies listed here have been included in the meta-analysis.

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

3 Matched-pair design studies using propensity score methods [8]

*U02 Apel R., Sweeten G. (2010a). The impact of first-time incarceration on criminal behaviour during transition to adulthood. Unpublished.

*U03 Bales W.D., Piquero A.R. (2012). Assessing the impact of imprisonment on recidivism. *J. of Experimental Criminology* 8/1, 71-101.

*U04 Loughran T.A., Mulvey E.P., Schubert C.A., Fagan J., Piquero A.R., Losoya S.H. (2009). Estimating a dose-response relationship between length of stay and future recidivism in serious juvenile offenders. *Criminology* 47/3, 699-738.

*U05 Lulham, Rohan; Weatherburn, Don; Bartels, Lorana (2009). The recidivism of offenders given suspended sentences: A comparison with full-time imprisonment. *Crime & Justice Bulletin*, 136, 1-16.

U06 McGrath A., Weatherburn D. (2012). The effect of custodial penalties on juvenile reoffending. *Australian & New Zealand J. of Criminology* 45/1, 26-44.

U07 Mears D.P., Cochran J.C., Bales W.D. (2012). Gender differences in the effects of prison on recidivism. *J. of Criminal Justice* 40/5, 370-378. (Not included in the meta-analysis)

*U08 Nieuwbeerta P., Nagin D., Blokland A. (2009). Assessing the Impact of First-Time Imprisonment on Offenders' Subsequent Criminal Career Development: a matched samples comparison. *Journal of Quantitative Criminology* 25, 227-257.

*U09 Nirel R., Landau S.F., Sebba L. and Sagiv B. (1997). The effectiveness of service work: An analysis of recidivism. *J. Quantitative Criminologe* 13/1, 73-92.

*07a Weisburd D., Waring E., Chayet E. (1995). Specific deterrence in a sample of offenders convicted of white-collar crimes", *Criminology* 33/4, 587-607.

*07b Weisburd D., Waring E., Chayet E. (2001). White-collar crime and criminal careers. Cambridge: Cambridge University Press.

[8] Unless otherwise indicated, studies listed here have been included in the meta-analysis.

*U10 Wermink H., Blokland A., Nieuwbeerta P., Nagin D., Tollenaar N. (2010). Comparing the effects of community service and short-term imprisonment on recidivism: a matched samples approach. *J Exp Crim* 6/3, 325-349.

4. Other matched-pair studies and studies with several control variables (not included in the meta-analysis)

08 Bondeson U.V. (1994). *Alternatives to imprisonment: intentions and reality*, Transaction Publishers / Westview Press, London / Boulder (2nd edition 2002)-

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

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Systems of criminal sanctions around the world comprise both custodial and non-custodial sanctions. But what impact do custodial versus non-custodial sanctions have on the re-offending of those convicted of offences? Does a custodial sentence produce more or less re-offending than a non-custodial sanction? What does the research tell us?

Systematic reviews are one means of helping people to pick their way through the jungle of research findings. Systematic reviews combine a number of studies that are considered to satisfy a list of empirical criteria for measuring effects as reliably as possible. The results of these studies are then used to calculate and produce an overall picture of the effects associated with a certain phenomenon. In this way, systematic reviews systematically produce a more reliable overview based on the best knowledge available.

The Swedish National Council for Crime Prevention (Brå) has therefore initiated the publication of a series of systematic reviews, in the context of which distinguished researchers have been commissioned to perform the studies on our behalf. In this study, the authors have carried out a systematic review, including a meta-analysis, of 24 studies from different part of the world, on the effects of criminal sanctions on re-offending.

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