

January 2006

EVIDENCE-BASED ADULT CORRECTIONS PROGRAMS: WHAT WORKS AND WHAT DOES NOT[‡]

In recent years, public policy decision-makers throughout the United States have expressed interest in adopting “evidence-based” criminal justice programs. Similar to the pursuit of evidence-based medicine, the goal is to improve the criminal justice system by implementing programs and policies that have been shown to work. Just as important, research findings can be used to eliminate programs that have failed to produce desired outcomes. Whether for medicine, criminal justice, or other areas, the watchwords of the evidence-based approach to public policy include: outcome-based performance, rigorous evaluation, and a positive return on taxpayer investment.

This report to the Washington State Legislature summarizes our latest review of evidence-based adult corrections programs. We previously published a review on this topic in 2001.¹ In this study, we update and significantly extend our earlier effort.

The overall goal of this research is to provide Washington State policymakers with a comprehensive assessment of adult corrections programs and policies that have a proven ability to affect crime rates.

We are publishing our findings in two installments. In this preliminary report, we provide a systematic review of the evidence on what works (and what does not) to reduce crime. In a subsequent final report, to be published in October 2006, we will extend this analysis to include a benefit-cost estimate for each option.

[‡] Suggested citation: Steve Aos, Marna Miller, and Elizabeth Drake. (2006). *Evidence-Based Adult Corrections Programs: What Works and What Does Not*. Olympia: Washington State Institute for Public Policy.

¹ S. Aos, P. Phipps, R. Barnoski, and R. Lieb (2001). *The Comparative Costs and Benefits of Programs to Reduce Crime*, Olympia: Washington State Institute for Public Policy.

Summary

This study provides a comprehensive review of evidence-based programs for adult offenders. We asked a simple question: What works, if anything, to lower the criminal recidivism rates of adult offenders? To provide an answer, we systematically reviewed the evidence from 291 rigorous evaluations conducted throughout the United States and other English-speaking countries during the last 35 years.

We find that some types of adult corrections programs have a demonstrated ability to reduce crime, but other types do not. The implication is clear: Washington’s adult corrections system will be more successful in reducing recidivism rates if policy focuses on proven evidence-based approaches.

Washington’s Offender Accountability Act

This research was undertaken as part of our evaluation of Washington’s Offender Accountability Act (OAA). Passed in 1999, the OAA affects how the state provides community supervision to adult felony offenders. In broad terms, the OAA directs the Washington State Department of Corrections to do two things:

- 1) Classify felony offenders according to their risk for future offending as well as the amount of harm they have caused society in the past; and
- 2) Deploy more staff and rehabilitative resources to higher-classified offenders and—because budgets are limited—spend correspondingly fewer dollars on lower-classified offenders.

When the Legislature enacted the OAA, it defined a straight-forward goal for the Act: to “reduce the risk of reoffending by offenders in the community.”² To determine whether the OAA results in lower recidivism rates, the Legislature also directed the Washington State Institute for Public Policy (Institute) to evaluate the impact of the Act.³

Whether the OAA is able to affect crime rates will depend, in part, on the policy and programming choices made to implement the Act. As we show in this report, there are some adult corrections programs that have a demonstrated ability to reduce crime, but there are other types of programs that fail to affect crime rates. Given these mixed results, it is reasonable to conclude that the OAA (or any other adult corrections policy initiative) will be successful in reducing crime only if it encourages the implementation of effective approaches and discourages the use of ineffective programs. The purpose of this report is to assist policymakers in sorting through the many evidence-based choices.

The Evidence-Based Review: The Basic Question

The goal of the present study is to answer a simple question: Are there any adult corrections programs that work? Additionally, in order to estimate costs and benefits, we seek to estimate the magnitude of the crime reduction effect of each option.

To answer these fundamental questions, we conducted a comprehensive statistical review of all program evaluations conducted over the last 40 years in the United States and other English-speaking countries. As we describe, we found 291 evaluations of individual adult corrections programs with sufficiently rigorous research to be included in our analysis. These evaluations were of many types of programs—drug courts, boot camps, sex offender treatment programs, and correctional industries employment programs, to name a few.

It is important to note that only a few of these 291 evaluations were of Washington State adult

corrections programs; rather, almost all of the evaluations in our review were of programs conducted in other locations. A primary purpose of our study is to take advantage of all these rigorous evaluations and, thereby, learn whether there are conclusions that can allow policymakers in Washington to improve this state’s adult criminal justice system.

Research Methods

The research approach we employ in this report is called a “systematic” review of the evidence. In a systematic review, the results of *all* rigorous evaluation studies are analyzed to determine if, on average, it can be stated scientifically that a program achieves an outcome. A systematic review can be contrasted with a so-called “narrative” review of the literature where a writer selectively cites studies to tell a story about a topic, such as crime prevention. Both types of reviews have their place, but systematic reviews are generally regarded as more rigorous and, because they assess all available studies and employ statistical hypotheses tests, they have less potential for drawing biased or inaccurate conclusions. Systematic reviews are being used with increased frequency in medicine, education, criminal justice, and many other policy areas.⁴

For this report, the outcome of legislative interest is crime reduction. In particular, since the programs we consider in this review are intended for adult offenders already in the criminal justice system, the specific outcome of interest is reduction in recidivism rates. Therefore, the research question is straightforward: *What works, if anything, to lower the recidivism rates of adult offenders?*

As we describe in the Appendix, we only include rigorous evaluation studies in our review. To be included, an evaluation must have a non-treatment comparison group that is well matched to the treatment group.

² RCW 9.94A.010.

³ The Institute’s first five publications on the Offender Accountability Act are available for downloading at the Institute’s website: www.wsipp.wa.gov. The final OAA report is due in 2010.

⁴ An international effort aimed at organizing systematic reviews is the Campbell Collaborative—a non-profit organization that supports systematic reviews in the social, behavioral, and educational arenas. See: <http://www.campbellcollaboration.org>.

Researchers have developed a set of statistical tools to facilitate systematic reviews of the evidence. The set of procedures is called “meta-analysis,” and we employ that methodology in this study.⁵ In the Technical Appendix to this report (beginning on page 9) we list the specific coding rules and statistical formulas we use to conduct the analysis—technical readers can find a full description of our methods and detailed results.

Findings

The findings from our systematic review of the adult corrections evaluation literature are summarized on Exhibit 1.⁶ We show the expected percentage change in recidivism rates for many types of evaluated adult corrections programs. A zero percent change means that, based on our review, a program does not achieve a statistically significant change in recidivism rates compared with treatment as usual.

We found a number of adult corrections programs that have a demonstrated ability to achieve reductions in recidivism rates. We also found other approaches that do not reduce recidivism. Thus, the first basic lesson from our evidence-based review is that some adult corrections programs work and some do not. A direct implication from these mixed findings is that a corrections policy that reduces recidivism will be one that focuses resources on effective evidence-based programming and avoids ineffective approaches.

As an example of the information on Exhibit 1, we analyzed the findings

⁵ We follow the meta-analytic methods described in: M. W. Lipsey and D. Wilson (2001). *Practical meta-analysis*. Thousand Oaks: Sage Publications.

⁶ Technical meta-analytical results are presented in Exhibit 2.

Exhibit 1 Adult Corrections: What Works? Estimated Percentage Change in Recidivism Rates (and the number of studies on which the estimate is based)		
Example of how to read the table: an analysis of 56 adult drug court evaluations indicates that drug courts achieve, on average, a statistically significant 10.7 percent reduction in the recidivism rates of program participants compared with a treatment-as-usual group.		
Programs for Drug-Involved Offenders		
Adult drug courts	-10.7%	(56)
In-prison “therapeutic communities” with community aftercare	-6.9%	(6)
In-prison “therapeutic communities” without community aftercare	-5.3%	(7)
Cognitive-behavioral drug treatment in prison	-6.8%	(8)
Drug treatment in the community	-12.4%	(5)
Drug treatment in jail	-6.0%	(9)
Programs for Offenders With Co-Occurring Disorders		
Jail diversion (pre- and post-booking programs)	0.0%	(11)
Programs for the General Offender Population		
General and specific cognitive-behavioral treatment programs	-8.2%	(25)
Programs for Domestic Violence Offenders		
Education/cognitive-behavioral treatment	0.0%	(9)
Programs for Sex Offenders		
Psychotherapy for sex offenders	0.0%	(3)
Cognitive-behavioral treatment in prison	-14.9%	(5)
Cognitive-behavioral treatment in the community	-31.2%	(6)
Behavioral therapy for sex offenders	0.0%	(2)
Intermediate Sanctions		
Intensive supervision: surveillance-oriented programs	0.0%	(24)
Intensive supervision: treatment-oriented programs	-21.9%	(10)
Adult boot camps	0.0%	(22)
Electronic monitoring	0.0%	(12)
Restorative justice programs for lower-risk adult offenders	0.0%	(6)
Work and Education Programs for the General Offender Population		
Correctional industries programs in prison	-7.8%	(4)
Basic adult education programs in prison	-5.1%	(7)
Employment training and job assistance in the community	-4.8%	(16)
Vocational education in prison	-12.6%	(3)
Program Areas in Need of Additional Research & Development		
<i>(The following types of programs require additional research before it can be concluded that they do or do not reduce adult recidivism rates)</i>		
Case management in the community for drug offenders	0.0%	(12)
“Therapeutic community” programs for mentally ill offenders	-27.4%	(2)
Faith-based programs	0.0%	(5)
Domestic violence courts	0.0%	(2)
Intensive supervision of sex offenders in the community	0.0%	(4)
Mixed treatment of sex offenders in the community	0.0%	(2)
Medical treatment of sex offenders	0.0%	(1)
COSA (Faith-based supervision of sex offenders)	-31.6%	(1)
Regular parole supervision vs. no parole supervision	0.0%	(1)
Day fines (compared to standard probation)	0.0%	(1)
Work release programs	-5.6%	(4)

from 25 well-researched cognitive-behavioral treatment programs for general adult offenders. We found that, on average, these programs can be expected to reduce recidivism rates by 8.2 percent. That is, without a cognitive-behavioral program we expect that about 49 percent of these offenders will recidivate with a new felony conviction after an eight-year follow-up. With a cognitive-behavioral treatment program, we expect the recidivism probability to drop four points to 45 percent—an 8.2 percent reduction in recidivism rates.

It is important to note that even relatively small reductions in recidivism rates can be quite cost-beneficial. For example, a 5 percent reduction in the reconviction rates of high risk offenders can generate significant benefits for taxpayers and crime victims. Moreover, a program that has no statistically significant effect on recidivism rates can be cost-beneficial if the cost of the program is less than the cost of the alternative. Jail diversion programs are examples of this; even if research demonstrates that diversion programs have no effect on recidivism, the programs may still be economically attractive if they cost less than avoided jail costs. In the final version of this report, to be delivered to the Legislature in October 2006, we will present full benefit-cost estimates for each of the programs shown in Exhibit 1.⁷

Findings by Type of Program

We organized our review of the adult corrections evidence base into eight categories of correctional programming (as shown in Exhibit 1). A brief discussion of our findings for each of these categories follows.

Programs for Drug-Involved Offenders. We analyzed 92 rigorous evaluations of drug treatment programs. These programs are for drug-involved adult offenders in a variety of prison and community settings. We found that, on average, drug treatment leads to a statistically significant reduction in criminal recidivism rates. We examined adult drug courts, in-prison therapeutic communities, and other types of drug treatment including cognitive-behavioral approaches.

Adult Drug Courts. Specialized courts for drug-involved offenders have proliferated throughout the United States, and there are several adult drug courts in Washington. We found 56 evaluations with sufficient rigor to be included in our statistical review. We conclude that drug courts achieve, on average, a statistically significant 10.7 percent reduction in the recidivism rates of program participants relative to treatment-as-usual comparison groups.

In-Prison Therapeutic Communities. Programs for drug offenders in a prison or jail setting are typically called “therapeutic communities” when they contain separate residential units for the offenders and when they follow group-run principles of organizing and operating the drug-free unit. Some evaluations of the effectiveness of in-prison therapeutic community programs have also included community-based aftercare for offenders once they leave incarceration. Based on our review of the evaluation literature, we found that the average therapeutic community reduces recidivism by 5.3 percent. The community aftercare component, however, produces only a modest additional boost to program effectiveness—to a 6.9 percent reduction. Thus, most of the recidivism reduction effect appears to stem from the prison-based therapeutic community experience for these offenders.

Other Types of Drug Treatment. As shown in Exhibit 1, we also studied the effects of three other types of drug treatment modalities: prison-based drug treatment that employs a cognitive-behavioral approach, general drug treatment approaches in the community, and general drug treatment programs in local jails. We found that each of these approaches achieve, on average, a statistically significant reduction in recidivism.

Jail Diversion Programs for Offenders With Mental Illness and Co-Occurring Disorders.

There is young but growing research literature testing the effectiveness of jail diversion programs for mentally ill adults and for offenders with co-occurring mental health and substance abuse disorders. Some of these are pre-booking programs implemented by the police, and some are post-booking programs implemented by court personnel, such as mental health courts. We found 11 evaluations with sufficient research rigor to be included in our review. Eight of these programs were part of a recent federally-funded

⁷ An overview of what will be included in the October 2006 report can be found at www.wsipp.wa.gov/ Steve Aos (2006). *Options to Stabilize Prison Populations in Washington State, Interim Report*, Olympia: Washington State Institute for Public Policy.

effort (Broner et al., 2004). On average, these approaches have not demonstrated a statistically significant reduction in the recidivism rates of program participants. This null finding does not mean the programs are not valuable; since they are typically designed to divert offenders from costly sentences in local jails, they may save more money than the programs cost. As mentioned earlier, we will review the economics of all programs in the present study in our October 2006 final report.

Treatment Programs for the General Offender Population.

Cognitive-Behavioral Treatment. We found 25 rigorous evaluations of programs for the general offender population that employ cognitive-behavioral treatment. This type of group therapy addresses the irrational thoughts and beliefs that lead to anti-social behavior. The programs are designed to help offenders correct their thinking and provide opportunities to model and practice problem-solving and pro-social skills. On average, we found these programs significantly reduce recidivism by 8.2 percent. We identified three well-defined programs that provide manuals and staff training regimens: *Reasoning and Rehabilitation (R&R)*, *Moral Reconation Therapy (MRT)*, and *Thinking for a Change (T4C)*. Effects of R&R and MRT are significant and similar to each other and to the other cognitive-behavioral treatment programs in our review. Only a single evaluation of T4C is currently available. Since, on average, all of these programs produce similar results, we recommend the state choose any of the three well-defined programs for implementation in Washington.

Programs for Domestic-Violence Offenders

Education/Cognitive-Behavioral Treatment. Treatment programs for domestic violence offenders most frequently involve an educational component focusing on the historical oppression of women and cognitive-behavioral treatment emphasizing alternatives to violence. Treatment is commonly mandated by the court. Based on our review of nine rigorous evaluations, domestic violence treatment programs have yet, on average, to demonstrate reductions in recidivism.

Programs for Sex Offenders.⁸ We found 18 well-designed evaluations of treatment programs for sex offenders. Some of these programs are located in a prison setting and some are in the community. Sex offenders sentenced to prison are typically convicted of more serious crimes than those sentenced to probation. We found that cognitive-behavioral treatments are, on average, effective at reducing recidivism, but other types of sex offender treatment fail to demonstrate significant effects on further criminal behavior.

Psychotherapy/Counseling for Sex Offenders.⁹

These programs involve insight-oriented individual or group therapy or counseling. We found only three rigorous studies of this approach to treatment. The results indicate that this approach does not reduce recidivism in sex offenders.

Cognitive-Behavioral Treatment of Sex Offenders in Prison.

Sex offenders sentenced to prison are typically convicted of more serious crimes than those sentenced to probation. We examined five rigorous studies of these specialized cognitive-behavioral programs that may also include behavioral reconditioning to discourage deviant arousal, and modules addressing relapse prevention. Among the five programs in this category was a randomized trial¹⁰ with an eight-year follow-up showing small but non-significant effects on recidivism. On average across all five studies, however, we found that cognitive-behavioral therapy for sex offenders in prison significantly reduces recidivism by 14.9 percent.

Cognitive-Behavioral Treatment of Sex Offenders on Probation.

Offenders sentenced to probation have usually been convicted of less serious crimes than sex offenders sentenced to prison. Cognitive-behavioral programs for sex offenders on probation are similar to the programs in prisons, and may also incorporate behavioral reconditioning and relapse prevention. We found

⁸ The categories of sex offender treatment listed here are based on those outlined in two recent reviews of sex offender treatment literature: R. K. Hanson, A. Gordon, A. J. Harris, J. K. Marques, W. Murphy, V. L. Quinsey, and M. C. Seto (2002). First report of the collaborative outcome data project on the effectiveness of psychological treatment for sex offenders, *Sexual Abuse: A Journal of Research and Treatment*, 14(2): 169-194; F. Losel, and M. Schmucker (2005). The effectiveness of treatment for sexual offenders: A comprehensive meta-analysis, *Journal of Experimental Criminology*, 1: 117-146

⁹ Psychotherapy and counseling are not currently used as stand-alone treatment for sex offenders (Hanson, et al., 2002).

¹⁰ J. K. Marques, M. Wiederanders, D. M. Day, C. Nelson, and A. van Ommeren (2005). Effects of a relapse prevention program on sexual recidivism: Final results from California's Sex Offender Treatment and Evaluation Project (SOTEP), *Sexual Abuse: A Journal of Research and Treatment*, 17(1): 79-107.

six rigorous studies and conclude that cognitive-behavioral therapy for sex offenders on probation significantly reduces recidivism. As a group, these programs demonstrated the largest effects observed in our analysis.

Behavioral Treatment of Sex Offenders. Behavioral treatments focus on reducing deviant arousal (using biofeedback or other conditioning) and increasing skills necessary for social interaction with age appropriate individuals. The two rigorous studies of programs using only behavioral treatment failed to show reductions in recidivism.

Intermediate Sanctions. In the 1980s and 1990s a number of sanctioning and sentencing alternatives were proposed and evaluated. Interest in developing additional alternatives continues. We found studies that center on five types of these “intermediate” sanctions.

Intensive Supervision With and Without a Focus on Treatment. We found 24 evaluations of intensive community supervision programs where the focus was on offender monitoring and surveillance. These programs are usually implemented by lowering the caseload size of the community supervision officer. This approach to offender management has not, on average, produced statistically significant reductions in recidivism rates. On the other hand, intensive supervision programs where the focus is on providing treatment services for the offenders have produced significant reductions; we found 10 well-researched evaluations of treatment-oriented intensive supervision programs that on average produced considerable recidivism reductions. The lesson from this research is that it is the treatment—not the intensive monitoring—that results in recidivism reduction.

Adult Boot Camps. Boot camps are intensive regimens of training, drilling, and some treatment. We found 24 rigorous evaluations of adult boot camps and, on average, they do not produce a statistically significant reduction in re-offense rates. As with our comment on jail diversion programs, however, it is possible that boot camps are economically attractive if they cost less to run than the alternative. Our October 2006 report will analyze the economics of adult boot camps.

Electronic Monitoring. Supervision of offenders in the community that is aided with electronic monitoring devices has been the focus of some rigorous evaluation efforts. We found 12 control-group studies; on average they indicate that electronic monitoring does not reduce recidivism.

Restorative Justice for Lower-Risk Adult Offenders. Restorative justice approaches have been tried for both juvenile and adult offenders. Offenders placed in restorative justice programs are often, but not always, lower risk compared with offenders processed through the usual court procedures. Restorative justice typically involves a form of victim-offender mediation, family group conferences, or restitution. We found six rigorous evaluations of these programs for adult offenders. On average, they did not result in lower recidivism rates. Our October 2006 report will also report on restorative justice programs for juvenile offenders. Unlike our findings for the restorative justice programs for adult offenders, our preliminary findings indicate that restorative justice programs do achieve significant reductions in recidivism rates of lower-risk juvenile offenders.

Work and Education Programs for General Offenders. We found 30 rigorous evaluations of programs that attempt to augment the educational, vocational, and job skills of adult offenders. Some of these programs are for offenders in prison and some are in community settings. On average, we found that employment- and education-related programs lead to modest but statistically significant reductions in criminal recidivism rates. We examined the following five categories of these programs.

In-prison Correctional Industries Program. Most states run in-prison correctional industries programs, yet only a few have been evaluated rigorously. We located only four outcome evaluations of correctional industries programs. On average, these programs produce a statistically significant reduction in recidivism rates. Our updated economic analysis of this finding will be presented in October 2006.

Basic Adult Education Programs in Prison. We found seven rigorous evaluations of programs that teach remedial educational skills to adult offenders when they are in prison. On average, these programs reduce the recidivism rates of program participants.

Employment Training and Job Assistance Programs in the Community. We analyzed the results of 16 rigorous evaluations of community-based employment training, job search, and job assistance programs for adult offenders. These programs produce a modest but statistically significant reduction in recidivism.

Vocational Education Programs in Prison. We found only three quality studies of vocational training programs for offenders while they are in prison. On average, the programs appear to reduce recidivism, but additional tests of this tentative finding is necessary.

Programs Requiring Further Study. In our review of the adult corrections literature, we were unable to draw conclusions about recidivism reduction for a number of programs. In Exhibit 1, we list these inconclusive findings at the bottom of the table. For each of these approaches, further research is required before even tentative conclusions can be drawn.¹¹

Case Management in the Community for Drug Offenders. These types of programs typically involve an outside third-party agency that provides case coordination services and drug testing. The goal is to provide the coordination of other existing monitoring and treatment services for offenders in the community. We found 12 rigorous tests of this approach. Our statistical tests reveal that while, on average, these programs have no significant effect on recidivism, some case management programs do have an effect and some do not. This inconclusive result means that additional research is required on this class of programming in order to identify the aspects of case management that are effective or ineffective. In other words, additional research may indicate that some forms of case management reduce recidivism.¹²

“Therapeutic Community” Programs for Mentally Ill Offenders. A relatively new approach to providing treatment to mentally-ill offenders follows a modified version of the therapeutic community approach to drug offenders described earlier. This approach appears to show promise in reducing recidivism rates.

¹¹ Technical Note. As we explain in the technical appendix, we employ “fixed effects” and “random effects” modeling to derive meta-analytic estimates of program effectiveness. Sometimes, a collection of evaluations of similar programs has significant recidivism when judged with fixed effects modeling, but the same set of programs has insignificant findings when a random effects model is used. This situation provides an indication that additional meta-analytic research is needed to identify the factors that produced the heterogeneity in the outcomes. Several of the programs listed here fall into this category. For more information, see the technical appendices.

¹² As a technical note, Exhibit 2 shows that case management services produce a marginally significant ($p=.114$) effect on recidivism in a fixed effects model but the model indicates significant ($p=.000$) heterogeneity. The random effects model indicates non significance ($p=.48$). Thus, a multivariate meta-analysis of this literature may isolate the factors that were associated with successful approaches among the 12 studies.

However, this is based on only two rigorous studies, and they involved small samples of offenders. Thus, this is an approach that requires additional research.

Faith-Based Programs. These Christian-based programs provide religious ministry, including bible study, to offenders in prison and/or when offenders re-enter the community. The faith-based offender programs that have been evaluated to date do not significantly reduce recidivism.¹³ Rigorous evaluations of faith-based programs are still relatively rare—we found only five thorough evaluations—and future studies may provide evidence of better outcomes.

Domestic Violence Courts. These specialized courts are designed to provide effective coordinated response to domestic violence. Domestic violence courts commonly bring together criminal justice and social service agencies and may mandate treatment for offenders. The two courts included here differed—one was exclusively for felony cases and the other for misdemeanors. In the misdemeanor court, recidivism was lowered, while the felony court observed increased recidivism. Thus, this is an area that requires additional research.

Intensive Supervision of Sex Offenders in the Community. The programs included in the analysis were all developed in Illinois and varied by county. All involve a specialized probation caseload, frequent face-to-face meetings with offenders, and home visits and inspections. Supervision programs may also include treatment. The recidivism results in the four counties vary widely, suggesting that some of the programs may be effective while others are not. Additional research is needed to identify these characteristics.

Mixed Treatment of Sex Offenders. Two rigorous studies evaluated community sex offender treatments employed across geographic areas (Washington State and British Columbia). In each case, the individual treatment programs varied widely. On average, these mixtures of treatments significantly reduced recidivism; however, while the treatments in Washington were significant and large, those in British Columbia were very small and non-significant. Controlling for the variation, the overall effect was zero.

¹³ Similar findings were recently published in a review of faith-based prison programs: J. Burnside, N. Loucks, J. R. Addler, and G. Rose (2005). *My brother's keeper: Faith-based units in prison*, Cullompton, Devon, U.K.: Willan Publishing, p. 314.

Medical Treatment of Sex Offenders. Several medical approaches to treating sex offenders have been tried. These include castration and two types of hormonal therapy. Ethical considerations have made it difficult to conduct rigorous evaluations of these types of treatment. The single study we used in our analysis compared men who volunteered for castration to another group who volunteered but did not receive the surgery. Recidivism was significantly less among castrated offenders.

Circles of Support and Accountability (COSA/ Faith-Based Supervision of Sex Offenders). This program originated among members of the Mennonite church in Canada. Volunteers provide support to sex offenders being released from prison. Five lay volunteers visit or contact the offender every week. The volunteers are supported by community-based professionals, typically psychologists, law enforcement, correctional officers, or social service workers; the full circle meets weekly. The single evaluation of this program showed a significant reduction in recidivism of 31.6 percent.

Regular Parole Supervision vs. No Parole Supervision. The Urban Institute recently reported the results of a study that compared the recidivism rates of adult prisoners released from prison with parole to those released from prison without parole. The study used a large national database covering 15 states. It found no statistically significant effect of parole on recidivism. This null result is consistent with our results for surveillance-oriented intensive supervision programs versus regular levels of supervision (reported above). We would like to see additional treatment and comparison group tests of the parole vs. no-parole question before drawing firm conclusions.

Day Fines (compared with standard probation). We found one rigorous study of “day fines.” These fines, which are more common in Europe than the United States, allow judges to impose fines that are commensurate with an offender’s ability to pay and the seriousness of the offence. This approach has been evaluated for low-risk felony offenders and was used to divert these offenders from regular parole supervision. The approach had no effect on recidivism rates but additional research is needed to estimate whether this sentencing alternative is cost-beneficial.

Work Release Programs. We found only four quality studies of work release programs. While, on average, these programs appear to reduce recidivism, more rigorous outcome research is needed on this type of adult corrections program.

Technical Appendices

Appendix 1: Meta-Analysis Coding Criteria

Appendix 2: Procedures for Calculating Effect Sizes

Appendix 3: Institute Adjustments to Effect Sizes for Methodological Quality, Outcome Measure Relevance, and Researcher Involvement

Appendix 4: Meta-Analytic Results—Estimated Effect Sizes and Citations to Studies Used in the Analyses

Appendix 1: Meta-Analysis Coding Criteria

A meta-analysis is only as good as the selection and coding criteria used to conduct the study. The following are the key choices we made and implemented for this meta-analysis of adult corrections programs.

- 1. Study Search and Identification Procedures.** We searched for all adult corrections evaluation studies conducted since 1970. The studies had to be written in English. We used three primary means to identify and locate these studies: a) we consulted the study lists of other systematic and narrative reviews of the adult corrections research literature—there have been a number of recent reviews on particular topics; b) we examined the citations in the individual studies; and c) we conducted independent literature searches of research databases using search engines such as Google, Proquest, Ebsco, ERIC, and SAGE. As we describe, the most important inclusion criteria in our study was that an evaluation have a control or comparison group. Therefore, after first identifying all possible studies using these search methods, we attempted to determine whether the study was an outcome evaluation that had a comparison group. If a study met these criteria, we then secured a paper copy of the study for our review.
- 2. Peer-Reviewed and Other Studies.** We examined all program evaluation studies we could locate with these search procedures. Many of these studies were published in peer-reviewed academic journals, while many others were from government reports obtained from the agencies themselves. It is important to include non-peer reviewed studies, because it has been suggested that peer-reviewed publications may be biased to show positive program effects. Therefore, our meta-analysis included all available studies regardless of published source.
- 3. Control and Comparison Group Studies.** We only included studies in our analysis if they had a control or comparison group. That is, we did not include studies with a single-group, pre-post research design. This choice was made because we believe that it is only through rigorous comparison group studies that average treatment effects can be reliably estimated.
- 4. Exclusion of Studies of Program Completers Only.** We did not include a comparison study in our meta-analytic review if the treatment group was made up solely of program completers. We adopted this rule, because we believe there are too many significant unobserved self-selection factors that distinguish a program completer from a program dropout, and that these unobserved factors are likely to significantly bias estimated treatment effects. Some comparison group studies of program completers, however, contain information on program dropouts in addition to a comparison group. In these situations, we included the study if sufficient information was provided to allow us to reconstruct an intent-to-treat group that included both completers and non-completers, or if the demonstrated rate of program non-completion was very small (e.g. under 10 percent). In these cases, the study still needed to meet the other inclusion requirements listed here.
- 5. Random Assignment and Quasi- Experiments.** Random assignment studies were preferred for inclusion in our review, but we also included non-randomly assigned control groups. We only included quasi-experimental studies if, and only if, sufficient information was provided to demonstrate comparability between the treatment and comparison groups on important pre-existing conditions such as age, gender, and prior criminal history. Of the 291 individual studies in our review, about 20 percent were effects estimated from well implemented random assignment studies.
- 6. Enough information to Calculate an Effect Size.** Following the statistical procedures in Lipsey and Wilson (2001), a study had to provide the necessary information to calculate an effect size. If the necessary information was not provided, the study was not included in our review.
- 7. Mean-Difference Effect Sizes.** For this study we coded mean-difference effect sizes following the procedures in Lipsey and Wilson (2001). For dichotomous crime measures, we used the arcsine transformation to approximate the mean difference effect size, again following Lipsey and Wilson. We chose to use the mean-difference effect size rather than the odds ratio effect size because we frequently coded both dichotomous and continuous outcomes (odds ratio effect sizes could also have been used with appropriate transformations).
- 8. Unit of Analysis.** Our unit of analysis for this study was an independent test of a treatment in a particular site. Some studies reported outcome evaluation information for multiple sites; we included each site as an independent observation if a unique and independent comparison group was also used at each site.

9. **Multivariate Results Preferred.** Some studies presented two types of analyses: raw outcomes that were not adjusted for covariates such as age, gender, criminal history; and those that had been adjusted with multivariate statistical methods. In these situations, we coded the multivariate outcomes.
10. **Broadest Measure of Criminal Activity.** Some studies presented several types of crime-related outcomes. For example, studies frequently measured one or more of the following outcomes: total arrests, total convictions, felony arrests, misdemeanor arrests, violent arrests, and so on. In these situations, we coded the broadest crime outcome measure. Thus, most of the crime outcome measures that we coded in this analysis were total arrests and total convictions.
11. **Averaging Effect Sizes for Arrests and Convictions.** When a study reported both total arrests and total convictions, we calculated an effect size for each measure then took a simple average of the two effect sizes.
12. **Dichotomous Measures Preferred Over Continuous Measures.** Some studies included two types of measures for the same outcome: a dichotomous (yes/no) outcome and a continuous (mean number) measure. In these situations, we coded an effect size for the dichotomous measure. Our rationale for this choice is that in small or relatively small sample studies, continuous measures of crime outcomes can be unduly influenced by a small number of outliers, while dichotomous measures can avoid this problem. Of course, if a study only presented a continuous measure, then we coded the continuous measure.
13. **Longest Follow-Up Times.** When a study presented outcomes with varying follow-up periods, we generally coded the effect size for the longest follow-up period. The reason for this is that our intention for this analysis is to compute the long-run benefits and costs of different programs. The longest follow-up period allows us to gain the most insight into the long-run effect of these programs on criminality. Occasionally, we did not use the longest follow-up period if it was clear that a longer reported follow-up period adversely affected the attrition rate of the treatment and comparison group samples.
14. **Measures of New Criminal Activity.** Whenever possible, we excluded outcome measures that did not report on new criminal activity. For example, we avoided coding measure of technical violations of probation or parole. We do not think that technical violations are unimportant, but our purpose in this meta-analysis is to ascertain whether these programs affect new criminal activity.
15. **Some Special Coding Rules for Effect Sizes.** Most studies in our review had sufficient information to code exact mean-difference effect sizes. Some studies, however, reported some, but not all of the information required. The rules we followed for these situations are these:
 - a. **Two-Tail P-Values.** Some studies only reported p-values for significance testing of program outcomes. When we had to rely on these results, if the study reported a one-tail p-value, we converted it to a two-tail test.
 - b. **Declaration of Significance by Category.** Some studies reported results of statistical significance tests in terms of categories of p-values. Examples include: $p \leq .01$, $p \leq .05$, or “non-significant at the $p = .05$ level.” We calculated effect sizes for these categories by using the highest p-value in the category. Thus if a study reported significance at “ $p \leq .05$,” we calculated the effect size at $p = .05$. This is the most conservative strategy. If the study simply stated a result was “non-significant,” we computed the effect size assuming a p-value of .50 (i.e. $p = .50$).

Appendix 2: Procedures for Calculating Effect Sizes

Effect sizes measure the degree to which a program has been shown to change an outcome for program participants relative to a comparison group. There are several methods used by meta-analysts to calculate effect sizes, as described in Lipsey and Wilson (2001). In this, we use statistical procedures to calculate the *mean difference effect sizes* of programs. We did not use the odds-ratio effect size because many of the outcomes measured in this study are continuously measured. Thus, the mean difference effect size was a natural choice.

Many of the outcomes we record, however, are measured as dichotomies. For these yes/no outcomes, Lipsey and Wilson (2001) show that the mean difference effect size calculation can be approximated using the arcsine transformation of the difference between proportions.¹⁴

$$(A1) \quad ES_{m(p)} = 2 \times \arcsin \sqrt{P_e} - 2 \times \arcsin \sqrt{P_c}$$

In this formula, $ES_{m(p)}$ is the estimated effect size for the difference between proportions from the research information; P_e is the percentage of the population that had an outcome such as re-arrest rates for the experimental or treatment group; and P_c is the percentage of the population that was re-arrested for the control or comparison group.

A second effect size calculation involves continuous data where the differences are in the means of an outcome. When an evaluation reports this type of information, we use the standard mean difference effect size statistic.¹⁵

¹⁴ Lipsey and Wilson, *Practical meta-analysis*, Table B10, formula (22).

¹⁵ Ibid., Table B10, formula (1).

$$(A2) \quad ES_m = \frac{M_e - M_c}{\sqrt{\frac{SD_e^2 + SD_c^2}{2}}}$$

In this formula, ES_m is the estimated effect size for the difference between means from the research information; M_e is the mean number of an outcome for the experimental group; M_c is the mean number of an outcome for the control group; SD_e is the standard deviation of the mean number for the experimental group; and SD_c is the standard deviation of the mean number for the control group.

Often, research studies report the mean values needed to compute ES_m in (A2), but they fail to report the standard deviations. Sometimes, however, the research will report information about statistical tests or confidence intervals that can then allow the pooled standard deviation to be estimated. These procedures are also described in Lipsey and Wilson (2001).

Adjusting Effect Sizes for Small Sample Sizes

Since some studies have very small sample sizes, we follow the recommendation of many meta-analysts and adjust for this. Small sample sizes have been shown to upwardly bias effect sizes, especially when samples are less than 20. Following Hedges (1981),¹⁶ Lipsey and Wilson (2001)¹⁷ report the “Hedges correction factor,” which we use to adjust all mean difference effect sizes (N is the total sample size of the combined treatment and comparison groups):

$$(A3) \quad ES'_m = \left[1 - \frac{3}{4N - 9}\right] \times [ES_m, \text{or}, ES_{m(p)}]$$

Computing Weighted Average Effect Sizes, Confidence Intervals, and Homogeneity Tests

Once effect sizes are calculated for each program effect, the individual measures are summed to produce a weighted average effect size for a program area. We calculate the inverse variance weight for each program effect, and these weights are used to compute the average. These calculations involve three steps. First, the standard error, SE_m , of each mean effect size is computed with:¹⁸

$$(A4) \quad SE_m = \sqrt{\frac{n_e + n_c}{n_e n_c} + \frac{(ES'_m)^2}{2(n_e + n_c)}}$$

In equation (A4), n_e and n_c are the number of participants in the experimental and control groups and ES'_m is from equation (A3).

Next, the inverse variance weight w_m is computed for each mean effect size with:¹⁹

$$(A5) \quad w_m = \frac{1}{SE_m^2}$$

The weighted mean effect size for a group of studies in program area i is then computed with:²⁰

$$(A6) \quad \overline{ES} = \frac{\sum (w_{m_i} ES'_{m_i})}{\sum w_{m_i}}$$

Confidence intervals around this mean are then computed by first calculating the standard error of the mean with:²¹

$$(A7) \quad SE_{\overline{ES}} = \sqrt{\frac{1}{\sum w_{m_i}}}$$

Next, the lower, ES_L , and upper limits, ES_U , of the confidence interval are computed with:²²

$$(A8) \quad \overline{ES}_L = \overline{ES} - z_{(1-\alpha)}(SE_{\overline{ES}})$$

$$(A9) \quad \overline{ES}_U = \overline{ES} + z_{(1-\alpha)}(SE_{\overline{ES}})$$

In equations (A8) and (A9), $z_{(1-\alpha)}$ is the critical value for the z-distribution (1.96 for $\alpha = .05$).

The test for homogeneity, which provides a measure of the dispersion of the effect sizes around their mean, is given by:²³

$$(A10) \quad Q_i = \left(\sum w_i ES_i^2\right) - \frac{\left(\sum w_i ES_i\right)^2}{\sum w_i}$$

The Q-test is distributed as a chi-square with $k-1$ degrees of freedom (where k is the number of effect sizes).

Computing Random Effects Weighted Average Effect Sizes and Confidence Intervals

When the p-value on the Q-test indicates significance at values of p less than or equal to .05, a random effects model is performed to calculate the weighted average effect size. This is accomplished by first calculating the random effects variance component, v .²⁴

$$(A11) \quad v = \frac{Q_i - (k - 1)}{\sum w_i - \left(\sum w_i q_i / \sum w_i\right)}$$

This random variance factor is then added to the variance of each effect size and then all inverse variance weights are recomputed, as are the other meta-analytic test statistics.

¹⁹ Ibid., 49, equation 3.24.

²⁰ Ibid., 114.

²¹ Ibid., 114.

²² Ibid., 114.

²³ Ibid., 116.

²⁴ Ibid., 134.

¹⁶ L. V. Hedges (1981). Distribution theory for Glass's estimator of effect size and related estimators. *Journal of Educational Statistics*, 6: 107-128.

¹⁷ Lipsey and Wilson, *Practical meta-analysis*, 49, formula 3.22.

¹⁸ Ibid., 49, equation 3.23.

Appendix 3: Institute Adjustments to Effect Sizes for Methodological Quality, Outcome Measure Relevance, and Researcher Involvement

In Exhibit 2 we show the results of our meta-analyses calculated with the standard meta-analytic formulas described in Appendix 2. In the last column in Exhibit 2, however, we list “Adjusted Effect Sizes” that we actually use in our benefit-cost analysis of each of the programs we review. These adjusted effect sizes, which are derived from the unadjusted results, are always smaller than or equal to the unadjusted effect sizes we report in the other columns in Exhibit 2.

In Appendix 3, we describe our rationale for making these downward adjustments. In particular, we make three types of adjustments that we believe are necessary to better estimate the results that we think each program is likely to actually achieve in real-world settings. We make adjustments for: a) the methodological quality of each of the studies we include in the meta-analyses; b) the relevance or quality of the outcome measure that individual studies use; and c) the degree to which the researcher(s) who conducted a study were invested in the program’s design and implementation.

3a. Methodological Quality. Not all research is of equal quality, and this, we believe, greatly influences the confidence that can be placed in the results from a study. Some studies are well designed and implemented, and the results can be viewed as accurate representations of whether the program itself worked. Other studies are not designed as well and less confidence can be placed in any reported differences. In particular, studies of inferior research design cannot completely control for sample selection bias or other unobserved threats to the validity of reported research results. This does not mean that results from these studies are of no value, but it does mean that less confidence can be placed in any cause-and-effect conclusions drawn from the results.

To account for the differences in the quality of research designs, we use a 5-point scale as a way to adjust the reported results. The scale is based closely on the 5-point scale developed by researchers at the University of Maryland.²⁵ On this 5-point scale, a rating of “5” reflects an evaluation in which the most confidence can be placed. As the evaluation ranking gets lower, less confidence can be placed in any reported differences (or lack of differences) between the program and comparison or control groups.

On the 5-point scale, as interpreted by the Institute, each study is rated with the following numerical ratings.

- A “5” is assigned to an evaluation with well-implemented random assignment of subjects to a treatment group and a control group that does not receive the treatment/program. A good random assignment study should also indicate how well the random assignment actually occurred by reporting

values for pre-existing characteristics for the program and control groups.

- A “4” is assigned to a study that employs a rigorous quasi-experimental research design with a program and matched comparison group, controlling with statistical methods for self-selection bias that might otherwise influence outcomes. These quasi-experimental methods may include estimates made with a convincing instrumental variables modeling approach, or a Heckman approach to modeling self-selection.²⁶ A level 4 study may also be used to “downgrade” an experimental random assignment design that had problems in implementation, perhaps with significant attrition rates.
- A “3” indicates a non-experimental evaluation where the program and comparison groups were reasonably well matched on pre-existing differences in key variables. There must be evidence presented in the evaluation that indicates few, if any, significant differences were observed in these salient pre-existing variables. Alternatively, if an evaluation employs sound multivariate statistical techniques (e.g. logistic regression) to control for pre-existing differences, and if the analysis is successfully completed, then a study with some differences in pre-existing variables can qualify as a level 3.
- A “2” involves a study with a program and matched comparison group where the two groups lack comparability on pre-existing variables and no attempt was made to control for these differences in the study.
- A “1” involves a study where no comparison group is utilized. Instead, the relationship between a program and an outcome, i.e., recidivism, is analyzed before and after the program.

We do not use the results from program evaluations rated as a “1” on this scale, because they do not include a comparison group and we believe that there is no context to judge program effectiveness. We also regard evaluations with a rating of “2” as highly problematic and, as a result, we do not consider their findings in the calculations of effect. In this study, we only consider evaluations that rate at least a 3 on this 5-point scale.

An explicit adjustment factor is assigned to the results of individual effect sizes based on the Institute’s judgment concerning research design quality. We believe this adjustment is critical and is the only practical way to combine the results of a high quality study (i.e., a level 5 study) with those of lesser design quality. The specific adjustments made for these studies depend on the topic area being considered. In some areas, such as criminal justice program evaluations, there is strong evidence that less-than-random assignment studies (i.e., less than level 5 studies) have, on average, smaller effect

²⁵ L. W. Sherman, D. Gottfredson, D. MacKenzie, J. Eck, P. Reuter, and S. Bushway (1998). *Preventing crime: What works, what doesn't, what's promising*. Prepared for the National Institute of Justice. Department of Criminology and Criminal Justice, University of Maryland. Chapter 2.

²⁶ For a discussion of these methods, see W. Rhodes, B. Pelissier, G. Gaes, W. Saylor, S. Camp, and S. Wallace (2001). *Alternative solutions to the problem of selection bias in an analysis of federal residential drug treatment programs*. *Evaluation Review*, 25(3): 331-369.

sizes than weaker-designed studies.²⁷ Thus, for the typical criminal justice evaluation, we use the following “default” adjustments to account for studies of different research design quality:

- A level 5 study carries a factor of 1.0 (that is, there is no discounting of the study’s evaluation outcomes).
- A level 4 study carries a factor of .75 (effect sizes discounted by 25 percent).
- A level 3 study carries a factor of .50 (effect sizes discounted by 50 percent).
- We do not include level 2 and level 1 studies in our analyses.

These factors are subjective to a degree; they are based on the Institute’s general impressions of the confidence that can be placed in the predictive power of criminal justice studies of different quality.

The effect of the adjustment is to multiply the effect size for any study, ES'_m , in equation (A3) by the appropriate research design factor. For example, if a study has an effect size of -.20 and it is deemed a level 4 study, then the -.20 effect size would be multiplied by .75 to produce a -.15 adjusted effect size for use in the benefit-cost analysis.

3b. Adjusting Effect Sizes for Relevance or Quality of the Outcome Measure. As noted in Appendix 1, our focus in this analysis is whether adult corrections programs reduce new criminal activity. We prefer measures such as arrests or convictions and avoid measures such as technical violations of parole or probation, since these may or may not be related to the commission of new crimes. In addition, we require that all studies have at least a six-month follow up period. For those studies that had a follow-up period of under 12 months, but greater than six months, and for those studies that only reported weak measures of new criminal activity, we reduced effects sizes by 25 percent. This adjustment multiplies the effect size for any study with a short follow-up or weak measure by .75.

²⁷ M. W. Lipsey (2003). Those confounded moderators in meta-analysis: Good, bad, and ugly. *The Annals of the American Academy of Political and Social Science*, 587(1): 69-81. Lipsey found that, for juvenile delinquency evaluations, random assignment studies produced effect sizes only 56 percent as large as nonrandom assignment studies.

3c. Adjusting Effect Sizes for Research Involvement in the Program’s Design and Implementation. The purpose of the Institute’s work is to identify and evaluate programs that can make cost-beneficial improvements to Washington’s actual service delivery system. There is some evidence that programs that are closely controlled by researchers or program developers have better results than those that operate in “real world” administrative structures.²⁸ In our own evaluation of a real-world implementation of a research-based juvenile justice program in Washington, we found that the actual results were considerably lower than the results obtained when the intervention was conducted by the originators of the program.²⁹ Therefore, we make an adjustment to effect sizes ES'_m to reflect this distinction. As a parameter for all studies deemed not to be “real world” trials, the Institute discounts ES'_m by .5, although this can be modified on a study-by-study basis.

Appendix 4: Meta-Analytic Results—Estimated Effect Sizes and Citations to Studies Used in the Analyses

Exhibit 2 provides technical meta-analytic results for the effect sizes computed for these groupings of programs, including the results of the adjustments described above. Exhibit 3 lists the citations for all the studies used in the meta-analyses, arranged by program area.

²⁸ Ibid. Lipsey found that, for juvenile delinquency evaluations, programs in routine practice (i.e., “real world” programs) produced effect sizes only 61 percent as large as research/demonstration projects. See also: A. Petrosino, & H. Soydan (2005). The impact of program developers as evaluators on criminal recidivism: Results from meta-analyses of experimental and quasi-experimental research. *Journal of Experimental Criminology*, 1(4): 435-450.

²⁹ R. Barnoski (2004). *Outcome evaluation of Washington State’s research-based programs for juvenile offenders*. Olympia: Washington State Institute for Public Policy, available at <<http://www.wsipp.wa.gov/rptfiles/04-01-1201.pdf>>.

Exhibit 2
Estimated Effect Sizes on Crime Outcomes
(A Negative Effect Size Indicates the Program Achieves Less Crime)

Program listed in <i>italics</i> require, in our judgment, additional research for it can be concluded that they do or do not reduce recidivism.	Number of Studies Included in the Review (total number of subjects in the treatment groups in the studies in parentheses)	Meta-Analytic Results Before Applying Institute Adjustments					Adjusted Effect Size Used in the Benefit-Cost Analysis (estimated effect after downward adjustments for the methodological quality of the evidence, outcome measurement relevance, and researcher involvement)
		Fixed Effects Model			Random Effects Model		
		Weighted Mean Effect Size	p-value	Homo-geneity Test	Weighted Mean Effect Size		
					ES	p-value	
Adult Offenders							
Programs for Drug-Involved Offenders							
Adult drug courts	56 (18957)	-.160	.000	.000	-.183	.000	-.094
In-prison therapeutic communities with community aftercare	6 (1989)	-.152	.000	.735	na	na	-.077
In-prison therapeutic communities without community aftercare	7 (1582)	-.119	.001	.079	na	na	-.059
Cognitive-behavioral therapy in prison	8 (3788)	-.130	.000	.905	na	na	-.077
<i>Case management in the community</i>	12 (2572)	-.046	.114	.000	-.039	.480	.000
Drug treatment in the community	5 (54334)	-.137	.000	.000	-.221	.007	-.109
Drug treatment in jail	9 (1436)	-.110	.008	.025	-.106	.094	-.052
Programs for Mentally Ill and Co-Occurring Offenders							
Jail diversion (pre & post booking programs)	11 (1243)	.060	.141	.682	na	na	.000
<i>Therapeutic community programs</i>	2 (145)	-.361	.004	.542	na	na	-.230
Treatment Programs for General Offenders							
Cognitive-behavioral for the general population	25 (6546)	-.147	.000	.000	-.164	.000	-.081
<i>Faith-based programs</i>	5 (630)	-.015	.767	.043	-.028	.728	.000
Programs for Domestic Violence Offenders							
Education/cognitive-behavioral treatment	9 (1254)	-.025	.523	.120	na	na	.000
<i>Domestic violence courts</i>	2 (327)	-.086	.309	.009	-.013	.956	.000
Programs for Sex Offenders							
Psychotherapy, sex offenders	3 (313)	.134	.179	.038	.027	.892	.000
Cognitive-behavioral treatment in prison	5 (894)	-.144	.005	.173	na	na	-.087
Cognitive-behavioral treatment in the community	6 (359)	-.391	.000	.438	na	na	-.195
Cognitive-behavioral treatment in prison (sex offense outcomes)	4 (705)	-.119	.027	.080	na	na	-.069
Cognitive-behavioral treatment in the community (sex off. outcomes)	5 (262)	-.357	.001	.846	na	na	-.177
<i>Intensive supervision of sex offenders in the community</i>	4 (392)	.207	.003	.000	.202	.359	.000
Behavioral Therapy - Sex Offenders.	2 (130)	-.190	.126	.635	na	na	.000
<i>Mixed Treatment-Sex Offenders in the Community</i>	2 (724)	-.176	.001	.015	-.184	.169	.000
<i>Circles of Support & Accountability (Faith-based supervision of sex offenders)</i>	1 (60)	-.388	.035	na	na	na	-.193
<i>Medical Treatment of Sex Offenders</i>	1 (99)	-.372	.060	na	na	na	-.185
Intermediate Sanctions							
Intensive supervision: surveillance-oriented approaches	24 (2699)	-.033	.244	.146	na	na	.000
Intensive supervision: treatment-oriented approaches	10 (2156)	-.287	.000	.000	-.291	.041	-.190
<i>Regular supervision compared to no supervision</i>	1 (22016)	-.010	.591	na	na	na	.000
<i>Day fines (compared to standard probation)</i>	1 (191)	-.084	.411	na	na	na	.000
Adult boot camps	22 (5910)	-.030	.103	.000	-.017	.632	.000
Electronic monitoring	12 (2175)	.025	.411	.025	.015	.765	.000
Restorative justice programs for lower risk adult offenders	6 (783)	-.077	.130	.013	-.125	.165	.000
Work and Education Programs for General Offenders							
Correctional industries programs in prison	4 (7178)	-.119	.000	.174	na	na	-.077
Basic adult education programs in prison	7 (2399)	-.094	.001	.006	-.114	.034	-.050
Employment training & job assistance programs in the community	16 (9217)	-.047	.003	.017	-.061	.021	-.047
<i>Work release programs from prison</i>	4 (621)	-.122	.045	.285	na	na	-.055
Vocational education in prison	3 (1950)	-.189	.000	.868	na	na	-.124

Notes to the Table:

Appendices 1, 2, and 3 describe the meta-analytic methods and decision criteria used to produce these estimates. Briefly, to be included in this review: 1) a study had to be published in English between 1970 and 2005; 2) the study could be published in any format—peer-reviewed journals, government reports, or other unpublished results; 3) the study had to have a randomly-assigned or demonstrably well-matched comparison group; 4) the study had to have intent-to-treat groups that included both completers and program dropouts, or sufficient information that the combined effects could be tallied; 5) the study had to provide sufficient information to code effect sizes; and 6) the study had to have at least a six-month follow-up period and include a measure of criminal recidivism as an outcome.

Exhibit 3

Citations to the Studies Used in the Meta-Analyses (Some studies contributed independent effect sizes from more than one location)

Program Grouping	Study
Adult Boot Camps	<p>Austin, J., Jones, M., & Bolyard, M. (1993). <i>Assessing the impact of a county operated boot camp: Evaluation of the Los Angeles County regimented inmate diversion program</i>. San Francisco: National Council on Crime and Delinquency.</p> <p>Burns, J. C., & Vito, G. F. (1995). An impact analysis of the Alabama boot camp program. <i>Federal Probation</i>, 59(1): 63-67.</p> <p>Camp, D. A., & Sandhu, H. S. (1995). Evaluation of female offender regimented treatment program (FORT). <i>Journal of the Oklahoma Criminal Justice Research Consortium</i>, 2: 50-77.</p> <p>Colorado Department of Corrections. (1993). <i>Colorado regimented inmate training program: A legislative report</i>.</p> <p>Farrington, D. P., Ditchfield, J., Hancock, G., Howard, P., Jolliffe, D., Livingston, M. S., & Painter, K. (2002). <i>Evaluation of two intensive regimes for young offenders</i>. Home Office Research Study 239. London, UK: Home Office</p> <p>Gransky, L. A. & Jones, R. J. (1995). <i>Evaluation of the post-release status of substance abuse program participants: The impact incarceration program at Dixon Springs and the Gateway substance abuse program at Dwight Correctional Center</i>. Chicago: Illinois Criminal Justice Authority Report.</p> <p>Harer, M. D., & Klein-Saffran, J. (1996). <i>Lewisburg ICC evaluation</i>. Washington DC: Bureau of Prisons, Office of Research and Evaluation, memo.</p> <p>Jones, M., & Ross, D. L. (1997). Is less better? Boot camp, regular probation and rearrest in North Carolina. <i>American Journal of Criminal Justice</i>, 21(2): 147-161.</p> <p>Kempinen, C. A., & Kurlychek, M. C. (2003). An outcome evaluation of Pennsylvania's boot camp: Does rehabilitative programming within a disciplinary setting reduce recidivism? <i>Crime and Delinquency</i>, 49(4): 581-602.</p> <p>MacKenzie, D. L. & Souryal, C. (1994). <i>Multisite evaluation of shock incarceration: Executive summary</i>. Washington, DC: U.S. Department of Justice/NIJ.</p> <p>Smith, R. P. (1998). Evaluation of the work ethic camp. Olympia: Washington State Department of Corrections.</p> <p>Stinchcomb, J. B., & Terry, W. C. (2001). Predicting the likelihood of rearrest among shock incarceration graduates: Moving beyond another nail in the boot camp coffin. <i>Crime and Delinquency</i>, 47(2): 221-242.</p> <p>Wright, D. T., & Mays, G. L. (1998). Correctional boot camps, attitudes, and recidivism: The Oklahoma experience. <i>Journal of Offender Rehabilitation</i>, 28(1/2): 71-87.</p>
Adult Drug Courts	<p>Barnoski, R., & Aos, S., (2003). <i>Washington State's drug courts for adult defendants: Outcome evaluation and cost-benefit analysis</i> (Document No. 03-03-1201). Olympia: Washington State Institute for Public Policy.</p> <p>Bavon, A. (2001). The effect of the Tarrant County drug court project on recidivism. <i>Evaluation and Program Planning</i>, 24: 13-24.</p> <p>Bell, M. M. (1998). <i>King County drug court evaluation: Final report</i>. Seattle, WA: M. M. Bell, Inc.</p> <p>Breckenridge, J. F., Winfree, Jr., L. T., Maupin, J. R., & Clason, D. L. (2000). Drunk drivers, DWI 'drug court' treatment, and recidivism: Who fails? <i>Justice Research and Policy</i>, 2(1): 87-105.</p> <p>Brewster, M. P. (2001). An evaluation of the Chester County (PA) drug court program. <i>Journal of Drug Issues</i>, 31(1): 177-206.</p> <p>Carey, S. M., & Finigan, M. W. (2004). A detailed cost-analysis in a mature drug court setting: A cost-benefit evaluation of the Multnomah County drug court. <i>Journal of Contemporary Criminal Justice</i>, 20(3): 315-338.</p> <p>Craddock, A. (2002). <i>North Carolina drug treatment court evaluation: Final report</i>. Raleigh: North Carolina Court System.</p> <p>Crumpton, D., Brekhus, J., Weller, J., & Finigan, M. (2003). <i>Cost analysis of Baltimore City, Maryland drug treatment court</i>. Portland, OR: NPC Research, Inc.</p> <p>Deschenes, E. P., Cresswell, L., Emami, V., Moreno, K., Klein, Z., & Condon, C. (2001). <i>Success of drug courts: Process and outcome evaluations in Orange County, California, final report</i>. Submitted to the Superior Court of Orange County, CA.</p> <p>Ericson, R., Welter, S., & Johnson, T. L. (1999). <i>Evaluation of the Hennepin County drug court</i>. Minneapolis: Minnesota Citizens Council on Crime and Justice.</p> <p>Spokane County Drug Court. (1999). <i>Evaluation: Spokane County drug court program</i>. Spokane, WA: Spokane County Drug Court.</p> <p>Fielding, J. E., Tye, G., Ogawa, P. L., Imam, I. J., & Long, A. M. (2002). Los Angeles County drug court programs: Initial results. <i>Journal of Substance Abuse Treatment</i>, 23(3): 217-224.</p> <p>Finigan, M. W. (1998). <i>An outcome program evaluation of the Multnomah County S.T.O.P. drug diversion program</i>. Portland, OR: NPC Research, Inc.</p> <p>Godley, M. D., Dennis, M. L., Funk, R., Siekmann, M., & Weisheit, R. (1998). <i>An evaluation of the Madison County assessment and treatment alternative court</i>. Chicago: Illinois Criminal Justice Information Authority.</p> <p>Goldkamp, J. S. & Weiland, D. (1993). <i>Assessing the impact of Dade County's felony drug court. Final report</i>. Philadelphia: Crime and Justice Research Institute.</p> <p>Goldkamp, J. S., Weiland, D., & Moore, J. 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Document No. 06-01-1201



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