RESEARCH REPORT

Impact and Cost-Benefit Analysis of the Maryland Reentry Partnership Initiative

John Roman
Lisa Brooks
Erica Lagerson
Aaron Chalfin
Bogdan Tereshchenko

January 2007

research for safer communities

URBAN INSTITUTE
Justice Policy Center

This document was prepared under grants from the Maryland Governor's Office of Crime Control and Prevention and Catholic Charities.

URBAN INSTITUTE
Justice Policy Center
2100 M STREET, NW
WASHINGTON, DC 20037
www.urban.org

ACKNOWLEDGMENTS

The authors would like to thank the many individuals who assisted our efforts in collecting, preparing and analyzing the data used in this study. Rada Moss, Executive Director of the Maryland Reentry Partnership Initiative within Catholic Charities, answered countless questions about the program, as did her predecessor at REP, Tomi Hiers. We benefited greatly from our collaboration with Jeffrey Gersh at the Maryland Governor's Office of Crime Control and Prevention, whose patience and guidance were critical to the completion of the study. Tom Stough and Bob Gibson at the Maryland Department of Public Safety and Correctional Services provided invaluable assistance deciphering the data.

We are also grateful to staff from the Justice Policy Center at the Urban Institute who contributed to this report. Michael Kane ably managed the project through its early stages. Courtney Schaeffer conducted site visits for the cost-benefit portion of the study and co-authored the initial cost-benefit report. Caterina Gouvis Roman, Christy Visher and Shelli Rossman offered critical guidance in shaping the final project report. Avi Bhati and Bill Adams supported the data management process. Doug Wissoker and Alex Cowell at RTI, International, provided thoughtful insights to the cost-benefit analysis.

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OVERVIEW

This study evaluates the impact of the Maryland Reentry Partnership Initiative (REP) on crime using a cohort of prisoners released from the Maryland Transition Center in Baltimore, Maryland, between March 2001 and January 2005. We use retrospective administrative data to test the hypotheses that participation in REP reduced re-arrest and re-conviction, and increased time to re-arrest, and also to test whether changes in those outcomes were cost-beneficial. We compare 229 REP clients to a contemporaneous cohort of 370 prisoners released from the Maryland Transition Center to neighborhoods in Baltimore City that were not in the REP catchment area. The quasi-experimental design tests whether REP reduced the prevalence and incidence of criminal justice contact during their post-release period, which averaged 38 months.

We find that REP was successful in reducing criminal offending. Fewer REP clients (72% compared to 77.6%) committed at least one new crime in the study period, which averaged 38 months. Overall REP participants committed 68 fewer crimes during the study period than ex-prisoners in the comparison group. There were no significant differences in time to re-arrest, likelihood of a new conviction, number of new convictions, or time to a new conviction.

We find that the REP program was cost-beneficial, returning about $3 in benefits for every dollar in new costs. The total net benefit, total benefits minus total costs, to the citizens of Baltimore from the REP program is about $7.2 million, or about $21,500 per REP participant. While there was a small and non-significant benefit to public agencies from REP, most of the program's benefit accrued to the citizens of Baltimore, whose risk of victimization was reduced. Much of the difference in cost-effectiveness is due to a difference in the incidence of serious crimes, as we observed 11 attempted murder charges and two murder charges among the comparison group and no murder or attempted murder charges within the treatment group.

Some caution is warranted in interpreting these results. Using conventional standards for statistical significance only the number of new re-arrests was statistically significant in the final analysis. However, the finding that REP reduced the incidence and prevalence of new arrest and that REP produced a marginal benefit was significant at $p<0.15$. The lack of statistically significant results may well be due to the relatively small sample size that limited our ability to detect real effects. The consistency of the results across all model specifications supports this hypothesis.

MAIN FINDINGS REPORT

INTRODUCTION

Background

In 2001, the City of Baltimore was home to 59 percent of returning prisoners released from Maryland state prisons. As is the case in many urban areas, returning prisoners are not evenly distributed throughout the city, but are concentrated in a few neighborhoods. Thirty percent of prisoners returning to the City of Baltimore returned to six of the 55 neighborhoods in the city.[1] These communities are not randomly distributed across the city, but rather are clustered according to socio-demographic factors, and are distinguished by substantial resource deprivation, social isolation, and limited community capacity. Generally, poverty rates and crime rates in these neighborhoods exceed the mean for Baltimore City, and are considerably higher than state-wide averages.[2] Exacerbating the problem, in Maryland, as in the United States generally, the number of persons incarcerated and released from prison has increased drastically in recent decades. As the number of returning prisoners increases, these most vulnerable communities are disproportionately impacted by the challenges of prisoner reentry.

Established in 1999, the Maryland Reentry Partnership Initiative (REP) is a coalition of service providers that coordinate efforts to provide prisoners returning to select Baltimore neighborhoods with comprehensive reentry services including housing assistance, substance abuse treatment, mental health counseling, education, vocational training and other services. Offered to inmates preparing for release from the Metropolitan Transition Center (MTC) located in East Baltimore, the program was designed to provide pre-release preparation, as well as support and services in the community. The stated goals of the program are:

- Enhancing public safety by reducing recidivism among the ex-offender population;
- Increasing offender accountability and community reparation; and
- Increasing community and correctional capacity to adequately assess offender needs and identify community resources to match needs.

REP was designed as a community-justice partnership in which public agencies and community based organizations work together to provide continuous case management as prisoners transition into the community. The REP model addresses prisoner reentry needs at three levels: *individual*, *community,* and *systems*. At the individual level, returning prisoners are matched to social and medical services tailored to their needs and designed to help them successfully reintegrate into the

[1] La Vigne, N., V. Kachnowski, J. Travis, R. Naser and C. Visher. A Portrait of Prisoner Reentry in Maryland. 2003. Washington, DC: The Urban Institute. p 3
[2] ibid. 53

community. Services are delivered by community-based organizations, which also seek to strengthen returning prisoners' support networks, enhance informal social controls within the target neighborhoods, improve community service availability and accessibility, and increase offender accountability. At the systems level, REP brings together corrections agencies and community service providers to coordinate services, share information, and ensure continuous case management during the transition to the community. State agencies involved in the REP program include the Maryland Division of Correction, the Maryland Division of Probation and Parole, the Maryland Parole Commission, the Mayor's Office on Criminal Justice, the Mayor's Office of Employment Development, and the Baltimore Police Department. The REP program, itself, is managed by an independent non-profit: until mid-year 2005, REP was housed within The Enterprise Foundation, and subsequently moved to Catholic Charities.

This study evaluates the impact of REP on crime using a cohort of prisoners released from the Maryland Transition Center in Baltimore, Maryland, between March 2001 and December 2004. The analysis uses retrospective data to test the hypotheses that participation in REP reduced re-arrest, re-conviction, and time to re-arrest. REP clients are compared to a contemporaneous cohort of prisoners released from MTC to neighborhoods in Baltimore City that were not in the REP catchment area.

The evaluation has two components. In the first part, we evaluate REP's effectiveness at reducing (1) any new arrest for any offense, (2) the number of new arrests, (3) any new conviction for any offense, (4) the number of new convictions, and (5) the number of days until first re-arrest. The second stage estimates the costs of REP as compared to the benefits, which are measured as savings from reduced costs of victimization, and reduced costs of investigating, arresting, processing, and incarcerating convicted offenders. We test the following hypotheses:

- REP participants are less likely than non-participants to be arrested or convicted of a new crime following their release from prison;
- REP participants avoid arrest and conviction longer than non-participants;
- Given the number of crimes averted in the REP service area, the benefits of REP outweigh the costs.

REP Operation

The REP partnership began operating in the spring of 2001, and served 337 former prisoners through the beginning of 2005. During the study period, the program served former prisoners returning to three neighborhoods in Baltimore City—Druid Heights, Greater East Baltimore, and Sandtown-Winchester, although it has since expanded to five zip codes covering seven Baltimore communities. The REP program is offered to prisoners returning to these neighborhoods from the Metropolitan Transition Center in Baltimore City. MTC serves as a step-down facility for many prisoners returning to Baltimore City who have been transferred from facilities across the state in the final months of their sentences. Potential REP participants are identified by the Division of Correction—typically within five or six months of inmates' expected release—based on the residence address of record in the Division of Correction's Offender-Based State Correctional Information System I (OBSCIS I) database.

Initial screening for program eligibility is based on residence in one of the three target zip codes. All inmates returning to a target community[3] are eligible for REP, except those with an outstanding warrant, a sex offense conviction, or a conviction for an offense against a child. Eligible prisoners attend a mandatory orientation session where they are given an overview of REP services (subsequent enrollment into REP is voluntary). REP also accepts recently released prisoners on a walk-in basis. Any ex-offender residing in the REP service area may request enrollment, subject to the same eligibility criteria as other clients, and about 15 percent of individuals served by REP have been walk-in clients.

REP Case Management

The partnership is staffed primarily by community case managers and advocates employed by various non-governmental partnering organizations. During the first years of the REP program, pre-release case management was offered concurrently with other in-prison programming. As the program evolved, pre-release programming has been significantly reduced, but most participants have at least one session with a case manager prior to their release. At the initial interview, case managers are responsible for conducting a needs assessment and developing a case plan for each participant, generally within a few weeks of expected release. Community advocates support both case managers and clients by providing case management assistance, monitoring progress, and helping to ensure compliance. Community advocates often have the most contact with clients, providing transportation and assisting clients in accessing services and treatment.

The REP treatment model is highly individualized and tailored to the assessed needs of the client. Upon release, the case manager or advocate meet the client at the prison gate, review the case plan, and assist in immediate post-release logistics (such as securing identification, medication, or transitional housing). The case manager revises the plan as necessary, and provides the client with treatment and service referrals. The services provided include education, substance abuse treatment, transitional housing, employment services, and vocational training. Some clients also participate in an ex-offender support group.

Due to the differentiated case planning, the length of REP participation varies according to client need, but may last up to two years. After two years, clients are considered to have completed the program and are deemed graduates; however, clients may opt out at any time if they no longer need or desire assistance. Clients may also be disqualified for a number of reasons, including arrest, technical violation, imprisonment, non-compliance, and relocation out of the service area. A client who is made inactive for any of these reasons may subsequently be reinstated.

[3] The residence information maintained in OBSCIS I and used to identify participants is entered at intake. Since many prisoners will not return to the neighborhood in which they resided before their current sentence, prisoners are asked at orientation to confirm their intention to reside in the target zip code upon release.

THE RESEARCH DESIGN

The evaluation is a retrospective quasi-experimental comparison of prisoners released from MTC to REP-eligible and non-REP zip codes in Baltimore City using administrative data. The treatment group is a census of all REP clients entering the program between March 11, 2001 to January 10th, 2005 who could be matched to official criminal justice data (for a detailed description of the sample construction, see **Appendix A**). The comparison group was generated from a cohort of all prisoners (3,876) released to non-REP Baltimore zip codes in the same period. Since all but one of the REP clients were African-American and male, the comparison sample was restricted to African-American men. The final sample cohort was created using a propensity score matching technique. The propensity score match selects those who would have been most likely to accept admission to the REP program had it been offered to them, using a combination of personal data including socio-economic information, prior criminal histories, and recent prison experiences. A detailed description of the propensity matching approach used here can be found in **Appendix B.** The nearest neighbor propensity score match yielded a final sample of 229 offenders in the treatment group (REP) and 370 in the comparison group for a total pooled matched sample of 599 offenders

DATA

Data for the study were provided primarily by the REP program office and the Maryland Department of Public Safety and Correctional Services (DPSCS). The REP program office provided identifying information for clients, as well as data on client status and type. For all offenders in the sample, demographic characteristics and institutional data related to the current incarceration were collected from the DPSCS Offender-Based State Correctional Information System I (OBSCIS I). Criminal history (arrests and convictions) and recidivism data were gathered for all members of the sample from the Criminal History Records Information (CHRI) maintained by the DPSCS Criminal Justice Information System (CJIS). Additional data on community-level demographics and economic indicators for Baltimore zip codes were retrieved from published results of the US Census of 2000.

This analysis used individual offender characteristics available in OBSCIS I and CHRI. The characteristics gathered from OBSCIS I included age (measured in years), current offense, number of prior offenses, and release status (under community supervision or mandatory release). In addition, the records included institutional data indicating program participation, sentence type, and administrative alerts (e.g., suicide or escape risk). Offender characteristics gathered from the CHRI data include number of prior arrests and number of prior convictions.

Dependent Variables–Defining Recidivism

Recidivism is broadly defined as contact with the criminal justice system that results in an arrest or conviction for a new crime. Recidivism is measured by arrests and convictions in the post-release period using data from the CHRI maintained by the Maryland DPSCS' Criminal Justice Information System (CJIS). CJIS is the main repository for Maryland criminal justice data, and collects automated history data (local and national) collected at arrest, incarceration history from the Offender Based State Correctional Information System (OBSCIS I), and probation and parole histories from OBSCIS II. These data include detailed information on key criminal justice history indicators, including prior arrests and charges, dispositions and time served, and post-release supervision. Data on arrests and convictions prior to release—used in calculating the propensity score—were also obtained from the CHRI.

SAMPLE

The sample includes 599 prisoners released from MTC to the City of Baltimore between March 11, 2001 and January 10th, 2005. The sample includes 229 REP clients and 370 former prisoners returning to Baltimore to neighborhoods that were not REP-eligible. **Table 1** describes the characteristics of the sample used in the analysis. On average, a sample member was almost 37 years old at the time of his release (we include persons as young as 18 and as old as 57 in the sample). The average person enrolled in the study had substantial prior experience with the criminal justice system with an average of more than 12 prior arrests and almost 6 prior convictions. Only a small proportion (6 percent) had a prior parole violation. With respect to the current offense (i.e.,. the period of incarceration and subsequent release that made the person eligible for enrollment in this study), 60 percent were serving a sentence for a felony conviction and 20 percent were released without subsequent supervision (mandatory release).

Table 1. Comparison of Characteristics of Sample Members

Offender Characteristics	Sample	REP	Non-REP
Percent Black	100.0%	100.0%	100.0%
Release Age	36.6	37.0	36.3
Prior Arrests	12.09	12.24	12.00
Prior Arrests - Property Offense	3.08	3.17	3.03
Prior Arrests - Person Offense	2.19	2.17	2.20
Prior Arrests - Drug Offense	4.71	4.79	4.65
Prior Convictions	5.87	5.82	5.90
Prior Convictions - Property Offense	1.46	1.50	1.44
Prior Convictions - Person Offense	0.82	0.77	0.85
Prior Convictions - Drug Offense	2.72	2.73	2.71
Parole Violation	6.3%	6.5%	6.2%
Mandatory Release	20.5%	21.8%	19.7%
Current Offense is a Felony	60.1%	61.5%	59.1%
Poor Performance Record in Prison	52.7%	50.2%	54.3%
Escape Risk	12.6%	13.5%	12.1%
N	599	229	370

Note: Significance: *** = p < 0.01; ** = p<0.05; * = p<0.1; a = p<0.15

Source: Maryland Department of Public Safety and Correctional Services Offender-Based State Correctional Information System I (OBSCIS I); Criminal Justice Information System (CJIS) Central Repository

There were no significant differences between the matched treatment and the comparison group on characteristics associated with differential outcomes. The data in **Table 1** suggest a well-balanced sample. Where there are small, non-significant differences, those differences create a conservative bias in the analysis, as they create a modest bias toward a finding of no program effect. Most importantly, treatment group members were about 10 percent more likely to have a mandatory release and no subsequent community supervision. Our analysis suggests that a mandatory release is the best predictor of future offending, and those with a mandatory release were four times more likely to commit a new offense. Thus, it is reasonable to expect that all else being equal, the treatment group would have more new arrests based on this difference alone.

IMPACT RESULTS

The impact analysis tests five hypotheses about the impact of the REP program:

- REP participants are less likely than non-participants to be arrested for a new crime following their release from prison;
- REP participants are arrested fewer times following their release from prison;
- REP participants are less likely than non-participants to be convicted of a new crime following their release from prison;
- REP participants have fewer convictions following their release from prison; and
- REP participants had longer time to re-arrest.

The study includes prisoners who were released during the four-year study period (2001-2004). Data on client outcomes, including recidivism, are available through March, 2006. Clients have varying follow-up periods ranging from one to five years, and therefore their individual opportunities to re-offend vary as well. In order to account for this variation, each of the first four models described above uses four follow-up periods: (1) (arrest/conviction) in the first year following release, (2) in the first two years following release, (3) in the first three years following release, and (4) at any point following release. The full sample is observed in (1) and (4), and sub-samples are tested in (2) and (3) as some sample members have not had a long enough follow-up period to be included in those models. While this approach creates a more complicated analysis—and a more complicated interpretation of results—we believe that it is important to test different time periods to determine whether time affects program outcomes. That is, is the program effect initially large, but with attenuation over time; is the initial effect small, but increasing over time; or is the effect constant? Determining the effect of time on outcomes has important policy ramifications. For instance, if the effect is large and then attenuates, the policy challenge is to identify programmatic improvements that would assist participants as they move away from their release date. Using a single time period would mask these differences.

In addition, we test a fifth hypothesis that REP participants have a longer time to re-arrest, if re-arrested. Since time variation is accounted for in this model, we use a single test of this hypothesis. In total, we test 17 hypotheses about REP impact. The results from the impact model are then linked with data collected separately about program costs. The value of outcomes are monetized and these monetized benefits of REP are compared to costs to determine whether the program was cost-effective.

Bivariate Analysis

Table 2 describes outcomes for the entire sample (both treatment and comparison groups). More than 75 percent of the sample were re-arrested during the follow-up period, which average slightly more than three years (about 38 months). Sample members averaged 2.5 new arrests and more than one new conviction. The first new arrest occurred about 10 months after release, on average. The new crimes committed by sample members were costly, averaging more than $63,000 in harms during the follow-up period.

Those in the REP sample were significantly less likely to experience any new arrest, with a 5.6 percentage point reduction in re-arrest, which equates to a 7.3 percent reduction in arrests. REP clients also were arrested fewer times overall, though the difference is not significant. There was no difference in likelihood of a re-conviction, and REP clients had a small non-significant reduction in the number of re-convictions. The treatment cohort had a slightly longer time to first re-arrest, and a slightly shorter time to first re-conviction, although neither difference was significant at the bivariate level. The costs of incarceration and probation following a subsequent conviction were similar, and there was a large, but non-significant reduction in costs of offending to victims. Overall, the bivariate comparison suggests that REP clients were significantly less likely to commit any crime, committed fewer crimes (non-significant), and committed crimes that caused less harm to victims (non-significant).

Table 2. Comparison of Outcomes of Sample Members			
Offender Characteristics	**Sample**	**REP**	**Non-REP**
Any Re-arrest	75.4%	72%[a]	77.6%
Number of Re-arrests	2.54	2.36	2.66
Number of Re-arrests - Property Offense	0.40	0.39	0.41
Number of Re-arrests - Person Offense	0.39	0.34	0.42
Number of Re-arrests - Drug Offense	1.44	1.33	1.51
Any Re-conviction	57.7%	58.0%	57.5%
Number of Re-convictions	1.19	1.14	1.22
Offense	0.16	0.17	0.15
Number of Re-convictions - Person Offense	0.00	0.00	0.00
Number of Re-convictions- Drug Offense	0.83	0.77	0.86
Number of Days in Study	1219.9	1212.9	1224.3
Number of Days at Risk	1171.9	1171.9	1171.9
Number of Days until First Re-arrest	316.3	321.9	313.1
Number of Days until First Re-conviction	526.8	505.9	540.0
Monetized Cost of Offending to Victims	$20,787	$9,658	$27,675
Monetized Cost of Offending to Prisons	$37,715	$38,147	$37,448
Monetized Cost of Offending to Probation	$2,195	$2,354	$2,097
Agencies	$42,459	$42,864	$42,210
Monetized Cost of Offending to Society	$63,247	$52,522	$69,884
N	599	229	370

Note: Significance: * = p < 0.01; ** = p<0.05; *** = p<0.1; a = p<0.15

Source: Maryland Department of Public Safety and Correctional Services Offender-Based State Correctional Information System I (OBSCIS I); Criminal Justice Information System (CJIS) Central Repository

Predictive Analysis

The data in **Table 1** and **Table 2** suggest that while REP clients are slightly less likely to recidivate, the small differences in baseline risk of future offending—particularly in the rates of mandatory release—may be mediating larger real differences in recidivism. That is, since REP clients were slightly more likely to have baseline characteristics associated with greater future offending, they would be expected to commit more new offenses, all else being equal. When there are factors such as the rates of mandatory releases that tend to hide real differences (known as moderating variables) multivariate analysis is used to observe program effects while controlling for these baseline differences.

Three different multivariate model specifications were used to test recidivism hypotheses. To test whether there was any difference in arrest and conviction prevalence—e.g., the proportion of the sample that experienced at least one arrest or at least one conviction—logistic regression models were used. To test whether there was any difference in arrest and conviction incidence—e.g., the number of new arrests or new convictions—negative binomial regression models were used. To test whether the two groups differed with respect to how fast a recidivism event occurred (i.e.., whether the impact of treatment on recidivism varied by group over time), Cox proportional hazard models were used. The following general model framework was used to estimate these models:

$$Y_i = \alpha_i + \beta_1 REP_i + \beta_2 TIME\ AT\ RISK_i + K\lambda + \varepsilon_i \qquad (1)$$

where Y_i is an indicator of recidivism (measured alternatively as re-arrest and re-conviction), REP_i is a dummy variable indicating whether the person participated in REP, TIME AT $RISK_i$ is the number of days after initial release from the Division of Correction that an offender is not incarcerated, and K is a matrix of offender-level demographic variables.

The control variables used in all models include:

- **Offender characteristics:** age;
- **Prior Criminality:** total prior arrests for person crimes, property crimes, and drug crimes; prior parole violations;
- **Current Offense:** felony (or not); mandatory release; poor performance while incarcerated; escape risk;
- **Exposure**: number of days on the street following the initial release.

Regression Models Testing the Impact of REP Participation

Tables 3 and **4** provide estimates of program impact on any re-arrest, any re-conviction, and the time to re-arrest and re-conviction. **Table 3** analyzes re-arrests and **Table 4** analyses re-convictions. In **Table 3**, columns (1)–(3) report results for a logistic regression on a binary re-arrest variable, columns (4)–(6) report results for a negative binomial regression on the number of re-arrests, and columns (7)–(8) report Cox proportional hazard estimates on time until re-arrest.

Table 3. Outcome Analysis (Re-arrest)

	Any Re-arrest			Number of Re-arrests			Time Until Re-arrest	
Independent Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
MREP	0.745[a] (.143)	0.724[a] (0.157)	0.722[a] (0.157)	-0.120 (0.087)	-0.139* (0.081)	-0.152* (0.079)	0.867[a] (0.084)	0.863[a] (0.086)
Time at Risk			1.000 (0.000)			0.000*** (0.000)		
Age at Release		0.923*** (0.011)	0.924*** (0.011)		-0.038*** (0.004)	-0.037*** (0.004)		0.961*** (0.005)
Prior Arrests for Property Crimes		1.110*** (0.380)	1.108*** (0.037)		0.035*** (0.009)	0.031*** (0.008)		1.028*** (0.008)
Prior Arrests for Person Crimes		1.078[a] (0.526)	1.082[a] (0.053)		0.056*** (0.017)	0.066*** (0.016)		1.041** (0.020)
Prior Arrests for Drug Crimes		1.237*** (0.446)	1.238*** (0.044)		0.054*** (0.010)	0.058*** (0.009)		1.072*** (0.012)
Parole Violator		1.299 (0.635)	1.303 (0.640)		0.170 (0.162)	0.165 (0.156)		0.924 (0.179)
Mandatory Release		5.265*** (1.942)	5.395*** (1.991)		0.169[a] (0.105)	0.265*** (0.102)		1.956*** (0.241)
Poor In-Prison Performance Record		1.154 (0.264)	1.128 (0.260)		0.101 (0.085)	0.079 (0.082)		1.099 (0.116)
Escape Risk Alert		1.286 (0.466)	1.284 (0.468)		0.060 (0.121)	0.073 (0.117)		0.994 (0.142)
Instant Offense is a Felony		0.773 (0.181)	0.802 (0.190)		-0.111 (0.088)	-0.003 (0.087)		0.972 (0.106)
Intercept				0.980*** (0.053)	1.776*** (0.190)	0.789*** (0.234)		
N	599	599	599	599	599	599	595	595
Pseudo R^2	0.0034	0.1787	0.1811	0.0008	0.0384	0.0555		
Likelihood Ratio							2.14	115.35

Note: Significance: *** = p < 0.01; ** = p<0.05; * = p<0.1; [a] = p<0.15
All tests are two-tailed.

In **Table 3**, the first three columns show the results of regression models testing whether participation in REP reduced the likelihood that participants were re-arrested at least once during the follow-up period. In column (1), outcomes for REP participants and non-participants are compared without control variables. REP participants are 25 percent less likely to be re-arrested, with a p-value less than 0.15. In column (2), outcomes for REP participants and non-participants are compared using a vector of covariates that controls for competing effects on outcomes. REP participants are 27 percent less likely to be re-arrested, with a p-value less than 0.15. In column (3), outcomes for REP participants and non-participants are compared using control variables, plus a variable that controls for time at risk. In that model, the time at risk variable measures the number of days during the follow-up period when individuals were not incarcerated and at risk of arrest. Again, REP participants are 27 percent less likely to be re-arrested, with a p-value less than 0.15.

Columns (4)–(6) compare the number of re-arrests. In column (4), the number of re-arrests for REP participants and non-participants are compared without control variables. In this model, REP participants are re-arrested 0.12 fewer times, and the result is non-significant. In column (5), the number of re-arrests for REP participants and non-participants are compared using a vector of covariates that controls for competing effects on outcomes. REP participants are re-arrested 0.14 fewer times, with a p-value less than 0.05. In column (6), outcomes for REP participants and non-participants are compared using control variables, plus a variable that controls for time at risk. Again, REP participants are re-arrested 0.14 fewer times, with a p-value less than 0.05.

Columns (7)–(8) compare the time to re-arrest. In column (7), REP participants and non-participants are compared with no control variables. In this model, REP clients have a longer time to re-arrest, although the difference is only marginally significant ($p<0.15$). The result holds with the introduction of covariates in (8).

Additional analyses (see **Appendix D**) test the impact of REP on any re-arrest and the number of re-arrests in varying periods post-release (the first year, the first two years, the first three years, and the first four years). All of the tests find that REP clients have better outcomes, although the only significant outcome is that REP clients have fewer re-arrests in the first year after release. This is consistent with the empirical literature that suggests that programs such as REP tend to have an effect that attenuates over time.

In **Table 4**, the first three columns show the results of regression models testing whether participation in REP reduced the likelihood that participants were re-convicted at least once during the follow-up period. In column (1), outcomes for REP participants and non-participants are compared without control variables. REP participants are slightly more likely to be re-convicted, although the result is not significant. This result is repeated in column (2) with the addition of a vector of covariates and in column (3), which adds a variable that controls for time at risk.

Columns (4)–(6) compare the number of re-convictions. In column (4), REP participation is associated with slightly fewer re-convictions, with no control variables, although the result is not significant. Again, this result persists across model specifications, as adding a vector of covariates in column (5) and covariates and a measure of time at risk in column (6) produces the same results: a finding that REP participation yields a non-significant reduction in the number of re-convictions.

Columns (7)–(8) compare the time to re-conviction. In column (7), REP participants and non-participants are compared without control variables. In this model, REP clients have a slightly longer time to re-conviction, although the difference is not significant. The result holds with the introduction of covariates in (8).

Table 4. Outcome Analysis (Re-conviction)

	Any Re-conviction			Number of Re-convictions			Time Until Re-conviction	
Independent Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
MREP	1.025 (0.186)	1.025 (0.186)	1.024 (0.188)	-0.061 (0.104)	-0.075 (0.101)	-0.082 (0.099)	1.019 (0.113)	1.008 (0.113)
Time at Risk			1.000*** (0.000)			0.000*** (0.000)		
Age at Release		0.953*** (0.009)	0.953*** (0.009)		-0.026*** (0.005)	-0.025*** (0.005)		0.971*** (0.006)
Prior Arrests for Property Crimes		1.061*** (0.023)	1.059*** (0.023)		0.033*** (0.010)	0.030*** (0.010)		1.024*** (0.009)
Prior Arrests for Person Crimes		1.013 (0.039)	1.020 (0.040)		0.007 (0.021)	0.018 (0.021)		1.013 (0.023)
Prior Arrests for Drug Crimes		1.133*** (0.028)	1.140*** (0.029)		0.055*** (0.012)	0.060*** (0.012)		1.073*** (0.013)
Parole Violator		1.617 (0.628)	1.169 (0.639)		0.050 (0.206)	0.039 (0.202)		1.235 (0261)
Mandatory Release		3.107*** (0.785)	3.462*** (0.890)		0.308** (0.131)	0.421*** (0.129)		1.930*** (0.264)
Poor In-Prison Performance Record		1.323[a] (0.251)	1.294 (0.248)		0.209** (0.106)	0.200* (0.103)		1.192[a] (0.143)
Escape Risk Alert		1.229 (0.349)	1.222 (0.352)		-0.020 (0.153)	-0.004 (0.150)		1.043 (0.167)
Instant Offense is a Felony		0.689* (0.135)	0.754 (0.151)		-0.218** (0.109)	-0.105 (0.108)		0.870 (0.109)
Intercept				0.200*** (0.064)	0.679*** (0.236)	-0.430 (0.302)		
N	599	599	599	599	599	599	598	598
Pseudo R^2	0.0000	0.0925	0.1066	0.0002	0.0290	0.0474		
Likelihood Ratio							0.03	73.25

Note: Significance: *** = p < 0.01; ** = p<0.05; * = p<0.1; [a] = p<0.15
All tests are two-tailed.

Additional analyses (see **Appendix D**) test the impact of REP on any re-conviction and the number of re-convictions in varying periods post-release (the first year, the first two years, the first three years, and the first four years). The results are somewhat inconsistent with the findings from the re-arrest analysis. In these models, there is very little difference between REP and comparisons (REP participants are slightly more likely to be re-convicted, but have slightly fewer re-convictions). However, by the end of the follow-up period, REP clients are significantly less likely to be re-convicted.

COST-BENEFIT ANALYSIS

Cost-benefit analysis (CBA) is an empirical approach designed to measure the economic impact of government intervention into private markets. It is a "broad general approach" (Rothenberg 1975, 55) used to quantify "in monetary terms the value of all policy consequences to all members of society" (Boardman et al. 2001, 2). A CBA compares program costs to program benefits in order to estimate the program's impact on net social welfare. This CBA of REP defines costs as all costs associated with the provision of services to REP clients. These costs include in-house program costs, as well as costs accruing to community partners. Program benefits span two domains: (1) averted costs to crime victims and (2) averted costs to public agencies.

COST DATA

Cost Collection Protocol

Cost data were collected via semi-structured interviews with REP staff and those staff employed by REP contractors. Cost data were collected using a "bottom up" approach whereby the cost of each program input was estimated by multiplying the price of the input (typically the wage of the service provider) by the quantity of the input received by the offender (hours of treatment).

In general, the purpose of the cost collection interviews was to:

- Validate the data contained in the reconciled project budgets;
- Identify any partners, subcontractors, REP services, or other REP-related expenditures that were associated with the REP program, but were not included in the REP budget (such as cost-sharing arrangements or in-kind contributions;
- Identify unit prices of services and quantity of services delivered, where available, or in the absence of those data, identify average cost.

The cost collection process began with a request to REP for all their budget information for FY2004. Once research staff was oriented to the budget, REP staff was asked to describe the project in general terms in order to determine to what extent budgeted project activities replaced existing activities conducted either by REP or by another entity. In the REP case, much of this information was already available from prior research. Staff reported that the only outside activity directly replaced by REP would be some portion of the community supervision of REP clients by the Division of Parole and Probation. All other REP services were new. We conducted additional interviews with Maryland DPSCS staff to determine the value of these services.

Findings

Overall, we found that the REP program cost about $1.2M to administer in FY2004 (**Table 5**). This reflects about $190,000 in costs associated with the Enterprise portion of the REP program, $560,000 for subcontractors, and $460,000 for project partners. Of the $1.2M, more than $450,000 are costs borne by subcontractors that are not covered by REP subcontracts. This finding supports the use of opportunity costs in this type of evaluation. That is, it is common for programs to leverage resources or receive in-kind contributions. Therefore, cost analyses of a program that examine accounting costs (actual expenditures) as opposed to opportunity costs (market value of resources consumed) will tend to underestimate costs in this type of program.

Much of the difference in costs between what was budgeted and what was spent to serve REP clients was due to costs associated with the provision of transitional housing. In these cases, subcontractors providing transitional housing bore substantial costs in terms of rent, overhead, and supplies that were only fractionally covered by REP subcontracts. If REP clients displaced other clients of these transitional housing slots, or if these services would have been provided to REP clients whether or not REP funding was received, then these should not be counted as new costs. Respondents reported that neither situation occurred in this case, but this can not be independently confirmed.

Table 5. REP Costs

Cost Category	Annual Costs	
Total REP In-House Costs		**$188,136**
Personnel	$163,051	
Other program costs	$25,085	
Total Partner Costs		**$563,597**
Budgeted	$352,316	
Not Budgeted	$211,281	
Total Subcontract Costs		**$458,568**
Budgeted	$217,482	
Non-Budgeted	$241,086	
Total REP Costs		**$1,210,301**

The total annual cost of REP was $1,210,301. REP program records show that 176 clients were active in FY2004. The average annual per participant cost of REP is about$6,900[4].

Benefits Data

Calculating the benefits of a public/private collaborative like REP is less straightforward than cost estimation. Unlike private sector ventures, the goal of REP is not to yield a return on investment by increasing revenues as a result of expenditures. Rather, the goal is to improve the functioning of former prisoners and as a result to reduce the burden on the public from offending. Therefore, the appropriate benefits to consider are those associated with reduced offending, even though those benefits do not accrue to the REP program. There are two groups of beneficiaries of reduced offending: (1) private citizens whose harms are reduced as the number and/or severity of crimes are reduced, and (2) public agencies who spend less to investigate, arrest, and supervise participants who desist from expected offending.

[4] No information about the amount of services nor when those services were delivered was available from the program.

Estimation of the benefits to public agencies is relatively straightforward. First, we estimate the average unit costs to two public agencies— the Baltimore Police Department and the Maryland Division of Correction—of investigating, arresting, and incarcerating offenders. Data on the cost of arrest and processing were drawn from prior research in the same geographic area.[5] Data on the daily cost of incarceration were obtained from the Maryland Division of Correction. Daily corrections costs were multiplied by total jail sentences served for recidivism offenses in order to obtain a total incarceration cost for each offender. Next, we use data from the impact analysis to estimate the number of arrests and convictions that were prevented by the REP program.

We repeat the same process for the estimation of benefits to private citizens. Benefits to private citizens occur when the number and/or severity of crimes are reduced. To estimate these benefits, we first estimate the unit cost of being victimized. The unit cost of victimization has two components: tangible and intangible costs. Tangible costs of crime include direct costs of victimization such as medical bills, rehabilitation costs, and lost wages from being unable to work. Intangible costs include psychological harm associated with victimization, including fear, pain, and suffering. We use extant estimates of the costs of victimization to estimate REP benefits. A detailed description of the benefits calculations can be found in **Appendix C**. Second, we again use the data from the impact analysis to estimate the number and severity of crimes prevented by the REP program.

We estimate the marginal benefits to society from reduced offending using multivariate statistical methods that control for the impact of a number of determinants of re-offending. Since the dependent variable for those offenders who do not recidivate is censored at zero, it is inappropriate to use OLS regression to directly estimate marginal benefits. Instead, Tobit regression is employed where the dependent variable is the total monetized cost to society (the sum of cost to victims and cost to public agencies) associated with each offender. The following model is employed to isolate the impact of REP:

$$\text{SOCIAL COST}_i = \alpha_i + c_1\text{REP}_i + K\gamma + \varepsilon_i \qquad (2)$$

where REP_i is a binary treatment indicator equal to one if the offender is enrolled in REP and zero otherwise. K is a matrix of covariates that predicts re-offending. c_1, the coefficient on REP, allows us to directly estimate the marginal benefit of treatment – the amount of money that society saves as a result of REP.

In **Table 6**, the first four columns show the results of regression models testing whether participation in REP reduced the costs to (1) victims, (2) prisons, (3) probation, and (4) total supervision. The final column (5) shows the results of regression models testing whether REP reduced costs overall (costs to society), a reduction which can also be interpreted as the net benefit of the program. All of the models include covariates and a control for time at risk.

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[5] Roman, J. and A. V. Harrell. "Assessing the costs and benefits accruing to the public from the Washington, DC Superior Drug Intervention Program." Journal of Law and Policy. April, 2001. Vol. 23, No. 2. 237-268.

In column (1), the results show that REP participation reduced the cost to victims by more than $23,000 per REP participant, although the results are not significant. Columns (2–4) show no significant change in costs for public agencies resulting from the REP program.

Column (5) reports the marginal benefit of REP. Compared to non-participants, REP reduced all costs to society (public and private) by more than $31,000, which is the marginal benefit of adding a participant to REP. The results are significant at p<0.15.

Table 6. Marginal Benefits (Tobit Regression)

	All Years				
	(1)	(2)	(3)	(4)	(5)
Independent Variable	**Total Victim Cost**	**Prison Cost**	**Probation Cost**	**Total Supervision Cost**	**Total Cost to Society**
MREP	-$23,813 ($18,513)	-$3 ($8,345)	$486 ($754)	$95 ($8,664)	-$31,824[a] ($19,122)
Time at Risk	$37[a] ($24)	$51*** ($11)	$5*** ($1)	$54*** ($12)	$61** ($25)
Age at Release	-$5,971*** ($1,078)	-$2,633*** ($486)	-$195*** ($45)	-$2,764*** ($505)	-$6,712*** ($1,106)
Prior Arrests for Property Crimes	$5,943*** ($1,874)	$2,195*** ($833)	$19 ($77)	$2,225*** ($865)	$6,604*** ($1,936)
Prior Arrests for Person Crimes	$4,442 ($3,939)	$1,877 ($1,778)	$190 ($163)	$1,965 ($1,847)	$6,371[a] ($4,048)
Prior Arrests for Drug Crimes	$6,507*** ($2,343)	$5,585*** ($1,051)	$512*** ($95)	$5,849*** ($1,090)	$8,803*** ($2,412)
Parole Violator	$9,425 ($37,433)	$18,142 ($16,526)	$229 ($1,531)	$18,074 ($17,171)	$26,297 ($38,310)
Mandatory Release	$40,182* ($23,632)	$46,116*** ($10,785)	$2,273*** ($994)	$47,859* ($11,201)	$54,577** ($24,253)
Poor In-Prison Performance Record	$7,748 ($19,322)	$10,823 ($8,730)	$990 ($792)	$11,760 ($9,062)	$5,962 ($19,876)
Escape Risk Alert	-$6,191 ($27,663)	$23,782* ($12,224)	$914 ($1,120)	$24,555 ($12,699)	$13,761 ($28,455)
Instant Offense is a Felony	$19,540 ($20,452)	-$1,058 ($9,219)	-$1,147* ($829)	-$1,184 ($9,566)	$19,702 ($21,060)
Intercept	$59,829 ($53,143)	-$18,552 ($24,603)	-$5,332** ($2,293)	-$18,686 ($25,546)	$97,986* ($54,991)
N	599	599	599	599	599
Likelihood Ratio	47.17	92.82	78.95	93.97	64.67

Note: Significance: *** = p < 0.01; ** = p<0.05; * = p<0.1; [a] = p<0.15
All tests are two-tailed.

Additional analyses (see **Appendix D**) test the impact of REP on marginal benefits in varying periods post-release (the first year, the first two years, the first three years, and the first four years) independently for costs to victims, costs to prison and probation, and costs to society. None of the results are statistically significant. However, we note that all of the models, except one, show a marginal benefit of REP participation. The one exception is the coefficient on prison and probation in the first year post-release. There is no consistent pattern to the magnitude of the marginal benefit across models, suggesting that the positive effects of REP occur relatively consistently across time.

Table 7 reports the total impact of REP on costs and benefits. Overall, REP returns a benefit of about $3 for every dollar in cost. The total cost estimate assumes that REP cost $6,213 per participant per year in the program, and that program participation averaged one and one-half years. The benefits, however, do not accrue equally to all beneficiaries. Most of the benefit to REP is received by private citizens who benefit from both a decrease in the number of victimizations and a decrease in the severity of victimizations. The latter impact is important. We observe 11 attempted murders and two completed homicides in the comparison and none in the treatment group. This suggests that REP impact is driven at least in part by a decrease in the number of crimes with the greatest harm to the public.[6]

Table 7. Total Marginal Benefit of REP

	Overall		
	(1)	**(2)**	**(3)**
	Victims	**Public Agencies**	**Society**
Total Benefits	$7,763,038	$2,961,650	$10,724,688
Total Costs	N/A	N/A	$3,476,324
Net-Benefits	$7,763,038	$2,961,650	$7,248,364
Benefit-Cost Ratio	N/A	N/A	3.09
Proportion of Savings Accrued	75%	25%	100%

LIMITATIONS

There are three important limitations that should be considered in interpreting these findings. First, many of the results reported here are not significant at conventional levels, suggesting a problem with statistical power. We are confident, however, that the finding that REP reduced the incidence and prevalence of new arrest and that REP produced a marginal benefit was less then $p<0.15$ is robust. The consistency of the results across all model specifications supports this hypothesis. The lack of statistically significant results may well be due to the relatively small sample size, which limited our ability to detect real effects. A post-hoc power analysis suggests that if we

[6] To test the robustness of this finding, we reviewed recidivism data from the full sample of ex-prisoners released from MTC and returning to Baltimore during this period. We hypothesize that if there are a disproportionately large number of attempted murders and murders in the sample we compare to REP, then it would be prudent to exclude these events from the CBA as outliers. However, we find that there were 135 homicides or attempted homicides in the full sample of 4,105. Therefore, if those events are evenly distributed across all MTC releasees, we would expect to see one homicide (or attempt) for every 30 ex-prisoners. In the REP comparison sample, we observe one homicide (or attempt) in every 28 comparisons. We find this reasonable justification to include the attempted murder and murders in the cost-benefit analysis.

had observed 100 additional treatment group participants with the same distribution of outcomes, those outcomes significant at $p<0.15$ would have been significant at $p<0.05$. However, we cannot rule out the possibility that there is no difference between the groups.

Second, there is the potential for identification problems. That is, although we carefully matched the treatment and comparison group using a sophisticated matching algorithm, we cannot rule out the possibility that some unobserved factor was related to both treatment enrollment and outcomes. If true, then results here may be biased. However, all available model diagnostics suggest a well-balanced sample that minimizes this threat to internal validity.

Finally, an important portion of the marginal benefit from REP is due to the difference in incidence rates of crimes that cause exceptional harm to victims (e.g., attempted murder and completed murder). The impact of rare events such as homicide on cost-benefit analysis of criminal justice programs is an important issue and researchers have suggested that ignoring the consequences of rare events may lead to spurious findings[7]. However, while again we can not rule out the possibility that these rare events support the inclusion of these benefits.

SUMMARY OF FINDINGS

We find that REP was generally successful in reducing criminal offending. During the study period, which averaged 38 months, fewer REP clients (i.e., 72 percent compared to 77.6 percent for non-REP subjects) committed new crimes, and REP clients were arrested on 68 fewer charges. The REP program appears to be cost-beneficial, returning about $3 in benefits for every dollar in new costs. The program yielded $31,800 in benefits, per participant, yielded total benefit to society of about $7.2 million, and a total benefit to criminal justice agencies of about $3.5 million. The total annual cost of REP was $1.2 million, and the total cost of REP over this period was almost $3.5 million.

The total net benefit, total benefits minus total costs, to the citizens of Baltimore from the REP program is about $7.2 million, or about $21,500 per REP participant.

The findings from this study reinforce a result from other cost-benefit analyses in the field that is worth the attention of policymakers. That is, when community-justice partnerships work—whether they are reentry programs, drug courts, or some other intervention—the benefits tend to disproportionately accrue to private citizens, rather than public agencies. That is, public agencies looking to programs such as REP as a means of creating revenue streams that more than offset the cost of the program are likely to be disappointed. We believe, however, that this should not be a reason to turn away from programs like REP. If it is the goal of agencies in the criminal justice system to improve public safety (i.e., to serve and protect the public at large), then findings such as these should be taken as evidence that they have done so effectively.

[7] See Roman, J. "Can cost-benefit analysis answer criminal justice policy questions, and if so, how?" Journal of Contemporary Criminal Justice. 20(3), 257-275. 2004.

APPENDIX A. SAMPLE CONSTRUCTION

Data used in the construction of the study sample were provided by the REP program office at Catholic Charities and the Maryland Department of Public Safety and Correctional Services (DPSCS). The REP program provided data on 337 participants which is a census of participants who received services from March 2001 through December 2004[8]. Data provided by REP included identifying information, type (pre-release or walk-in), and status (active, inactive, opt-out, or completed). Corrections and criminal history data on REP participants and members of the comparison groups were obtained from the Division of Correction's Offender-Based State Correctional Information System I (OBSCIS I) database and from Criminal History Records Information (CHRI) maintained within the DPSCS Criminal Justice Information System (CJIS).

The sample was constructed as follows:

- Data were collected on REP participants in the study period;
- Data were collected on prisoners released from MTC before, during and after the study period;
- REP clients were matched to official corrections data.

Treatment Sample

Data were available for participants who started the REP program between March 2001 and December 2004. An individual who meets with a case manager, has a case plan completed and has a record in the REP client tracking database is considered a REP participant. The REP program office does not maintain electronic data on contacts between clients and case managers, nor does it track service utilization. Therefore there are no data available to measure treatment dosage for participants, and no means to distinguish between those who received large or small treatment doses. All participants receive some level of case planning services at intake; therefore, all are eligible for inclusion in the treatment group. Clients were only excluded from the analysis in cases where correctional or criminal history records were not available from DPSCS.

Administrative data from OBSCIS I were obtained for all male sentenced inmates who were (1) released from Maryland DOC custody between January 1, 2001 and March 31, 2006;[9] and (2) had been housed at the Metropolitan Transition Center (MTC) at any time during their period of confinement. This data set included 18,012 inmate records. As each record corresponded to a DOC commitment, inmates with multiple incarcerations during this period had more than one record.

In order to obtain administrative corrections and criminal history data on REP participants, client records were matched to OBSCIS I records using a combination of social security numbers, date of birth, and name. Of clients in the REP program database, 275 (85%) were located in the

[8] Twelve records were removed from the client tracking database after they were discovered to be duplicates, leaving 325 unique participants in the client tracking database. However, these records were included in the denominator for the total cost calculations since the program allowed multiple program enrollments.

[9] Sample members were enrolled through December 2004, but follow-up records were obtained through March 2006.

OBSCIS I data. REP Clients who were not identified in the OBSCIS I data were omitted from the analysis. Of those who were not identified in OBSCIS I, 60 percent were walk-in clients. These clients may have been released prior to January 1, 2001, released from the custody of an authority other than Maryland DOC, or may not have been housed at MTC during their period of incarceration.

Next, we needed to identify a single release date for each client for the purposes of calculating recidivism rates. The client tracking database contained a release date, but we were concerned that the date might be self-reported. We determined that the OBSCIS I data would provide a more reliable release date for our purposes. However, each record in the OBSCIS I data may have multiple releases, and a single inmate may appear on multiple rows. Confronted with these challenges, we calculated a release date for each row in the OBSCIS I data that met the following criteria:

1. The release date must be associated with either a permanent release or a release to one of several halfway houses.[10]
2. The release from prison must immediately follow a stay at MTC.
3. The release date must roughly fall within the program's operational dates (January 1, 2001 to December 31, 2004).

After identifying an appropriate release date for each row in the OBSCIS I data, we then compared the release date identified with the release date supplied in the client tracking database. For 209 (87%) of MREP participants the difference between the two release dates was 30 days or less. The remaining MREP participants where the difference between the two dates was greater than 30 days were manually examined. In all but 9 cases, the date identified through OBSCIS I was retained as the release date for this study.

We then requested arrest and conviction records from the CHRI data system to develop a full criminal history file to be used in the propensity score match. Arrest and conviction records were available for 235 of the 275 REP clients, which constituted the final REP sample[11].

Comparison Sample

The 18,012 records in the OBSCIS I data were used as a starting point for comparison group sample identification. From these records, we identified those who 1) were released from MTC to the community or to a transitional halfway house operated by Maryland DOC, 2) were released during the REP period of operation (i.e., after January 1, 2001) and 3) were released to a Baltimore zip code. In instances where more than one release date met the criteria set for this study, we chose the earliest release date. Additionally, we eliminated from the analysis all inmates who would have been ineligible for services based on an outstanding warrant, a sex offense conviction or a

[10] DPSCS provided a list of five halfway houses that are used for inmates released from MTC. They include the Baltimore Pre-Release Unit (BAPRU), the Central Home Detention Unit (CHDU), Dismas House East (DHE), Dismas House West (DHW), and Threshold (THRESH).

[11] Six clients had missing data for at least one variable included in the final models, leaving 229 clients with data for all analyses.

conviction for an offense against a child. We combined data from OBSCIS I and OBSCIS II to identify likely zip code of return, and excluded all inmates who were not REP clients but returned to a REP zip code. Criminal history records (CHRI) were requested for the resulting cohort of 7,320 potential comparison cases.

Arrest and conviction records were retrieved for potential comparison groups based on a unique state identification number (SID) that is included in both the OBSCIS I and CRHI systems. Arrest and conviction records were available for 4,105 of the 7,320 comparison cases. Complete incarceration histories were also requested for this sample. The final comparison group was constructed using a propensity score model described in **Appendix B**.

APPENDIX B. PROPENSITY SCORE MATCHING

A common problem in non-random experiments is that individuals in the treatment group may differ systematically from individuals in the comparison group. If those systematic differences are related to outcomes (re-arrest, re-conviction) and not observable in the covariates of a multivariate model, then the observed program impact is biased. For instance, if one group systematically differs on a characteristic that makes them less likely to recidivate (such as motivation to get a job or desist from crime), that group would be expected to have better outcomes whether or not they received the intervention.

This phenomenon, known as selection bias, might arise for any number of reasons – for example, if individuals possessing certain characteristics are more likely to seek a treatment such as REP, or, alternatively, if individuals possessing certain characteristics are more likely to be selected for treatment. Research by Heckman[12] and others has shown that including covariates in the outcome model is not sufficient to control for the selection bias. One of the most promising techniques used to reduce bias from selection on observables is the propensity score approach described in Rosenbaum and Rubin.[13]

Propensity score matching is a statistical algorithm that enables researchers to match participants with non-participants in the presence of multiple determinants of program participation (Heckman, Ichimura, and Todd [HIT]).[14] The premise of matching is that out of a universe of possible comparison individuals, only those who are actually comparable to the treatment group on observable factors associated with program participation are retained in the sample. The challenge, however, is that there may be relatively little overlap between the universe of potential comparisons and the treatment group (HIT). Thus, the matching process requires identification of a large number of potential comparison cases to be matched with a relatively small number of comparison cases. Once matches are made on a matrix of observable characteristics, those potential comparisons that do not overlap with a treatment case can be excluded.

The matching procedure has two steps. First, a logistic regression is run where a binary treatment variable (treatment or not) is regressed on a vector of predictors of program participation using the complete pooled sample. For each observation in the pooled sample, the predicted dependent variable—the propensity score—indicates that offender's probability of receiving the treatment. The propensity score generated from this model—the likelihood of treatment—is then used to identify a sample from the comparison pool that has a probability of receiving treatment similar to the treatment group that has been previously identified.

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[12] Heckman, James J. "Sample Selection Bias as a Specification Error," Econometrica 47(1): 153-161, 1979.

[13] Rosenbaum, Paul R., and Donald B. Rubin. "The Central Role of the Propensity Score in Observational Studies for Causal Effects." Biometrika, Vol. 70, No. 1 (April 1983), pp. 41-55.

[14] Heckman, James J., Hidehiko Ichimura, and Petra E. Todd. "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme." *The Review of Economic Studies*, Vol. 64, No. 4 (October 1997), pp. 605-654.

The Maryland Division of Correction database representing our total pooled sample initially contained data on 4,105 offenders. Prior to running the propensity score matching algorithm, we stratified the sample by dropping all non-black offenders and offenders below the age of 17 or over the age of 58 since no offenders with these characteristics were included in the REP (treatment) sample. We specified the following basic treatment model:

$$REP_i = \alpha_i + D\lambda + \varepsilon_i \qquad (1)$$

where REP_i is a binary indicator of REP participation and D is a matrix of variables that predict participation in REP. **Table B.1** provides the results of the logistic regression of treatment. Odds ratios are reported. Standard errors are in parentheses. The Pseudo R-squared value indicates the proportion of selection into REP that is explained by the model.[15] The p-value on the likelihood ratio is 0.00 indicating that the model is a significant predictor of treatment receipt.

[15] The Pseudo R^2 measure is Aldrich and Nelson's coefficient that serves as an analog to the ratio of the explained to the total sum of squares in ordinary least squares regression.

Table B.1. Treatment Model of Selection Bias

Independent Variable	Odds Ratio	Independent Variable	Odds Ratio	Independent Variable	Odds Ratio
Release Age	1.034*** (0.009)	prgnum7	1.000 (0.000)	alert96	2.626[a] (1.664)
parviol	1.015 (0.328)	prgnum8	1.000 (0.000)	sentlength	0.999 (0.000)
mrviol	1.163** (0.390)	religion	0.999[a] (0.000)	simzip	3.815*** (0.638)
total_prarrests	1.001 (0.041)	totsmths	1.002 (0.002)	sntcncur	1.126 (0.206)
escape	1.095 (0.612)	prog1	0.820 (0.183)	npgseg	1.043 (0.151)
felony	1.297[a] (0.224)	prog4	0.678** (0.131)	total_prdrug	1.015 (0.047)
finsecpre	1.881*** (0.303)	alert1	1.538* (0.397)	total_prproperty	1.021 (0.048)
prgnum1	1.000*** (0.000)	alert7	1.382[a] (0.300)	total_prsex	0.549 (0.331)
prgnum2	1.000** (0.000)	alert13	1.051 (0.318)	total_prperson	1.021 (0.061)
prgnum3	1.000** (0.000)	alert16	1.668 (0.788)	total_pr_other	0.878 (0.088)
prgnum4	1.000 (0.000)	alert43	2.562[a] (1.615)	total_prtraffic	0.986 (0.586)
prgnum5	0.999 (0.000)	alert51	0.835 (0.697)		
prgnum6	1.000 (0.000)	alert86	1.220 (0.279)		
N	3318				
Pseudo R^2	0.1582				
Likelihood Ratio	263.54				

Note: Significance: *** = $p < 0.01$; ** = $p<0.05$; * = $p<0.1$; [a] = $p<0.15$

The matrix of predictors of REP participation contains 37 independent variables that explain program receipt. As the only purpose of the model is to predict program participation, the model is atheoretic and, as such, individual point estimates may be biased by the presence of collinearity among the predictors. The mean propensity score for the treatment group was 0.16. The mean propensity score among matched comparison group offenders is 0.14.[16] The p-value on a group mean comparison test between the propensity scores for REP and comparison participants indicates that the two groups do not differ significantly in their probability of receiving treatment.[17]

[16] One ad hoc diagnostic of the fit of the propensity model is a comparison of the ratio of the number in the treatment and comparison cohorts to the mean propensity score, which should be relatively similar. In this case, that ratio of treatment to comparison samples is about 1:10 and the mean propensity score about 0.14, suggesting a good model fit.

[17] The difference between group means was not significantly different at the 5 percent level of significance.

In order to match treatment offenders with comparison offenders, we selected the nearest neighbor matching algorithm with replacement. Using this technique, some specified number of potential comparison group members (in this case two) with the closest propensity scores to a given treatment individual are included in the final sample. In practice, this technique yields less than a two-to-one match, since a single comparison member can be the nearest neighbor to more than one treatment member. Dehejia and Wahba[18] find that nearest neighbor matching largely reproduces experimental results. In order to match comparison offenders with treatment offenders, we used the user-written module for Stata 9 called 'psmatch2.' Unmatched comparison offenders were excluded from the final dataset, yielding a final sample of 229 offenders in the treatment group (REP) and 370 in the comparison group for a total pooled matched sample of 599 offenders.

[18] Dehejia, Rajeev H., and Sadek Wahba. "Propensity Score-Matching Methods for Nonexperimental Causal Studies." The Review of Economics and Statistics, Vol. 84, No. 1 (February 2002), pp. 151-161.

APPENDIX C. COMPUTATION OF BENEFITS OF THE MARYLAND RE-ENTRY PROGRAM

Two types of benefit domains are considered—averted costs to crime victims and averted costs to public agencies. All costs are converted to 2004 dollars using the consumer price index as a deflator. Averted costs to crime victims are drawn from extant estimates of the tangible and intangible costs of crime. Tangible costs of crime include direct costs of victimization, such as medical bills, rehabilitation costs, and foregone productivity. Intangible costs include psychological harm associated with victimization. While tangible costs are estimated in a straightforward manner (for example, by observing hospital bills associated with each offense type), intangible costs are more difficult to monetize. Extant estimates generally rely on jury awards in civil actions in which the plaintiff has suffered a similar type of harm. For murder, we consider only tangible costs. For all other crimes, we consider the total estimates of costs to victims, both tangible and intangible. Tangible costs include property damage and loss, medical care, victim legal expenses, mental health care, police and fire services, victim services, and productivity losses.[19] The jury-compensation method proposed by Cohen[20] was used to estimate intangible costs of offenses to victims. Intangible costs are high in relation to the total costs for violent crimes such as aggravated assault, and low for property crimes such as larceny/theft. This pattern reflects the fact that violent offenses incur greater psychological distress and physical health harm to victims and, consequently, result in higher compensatory jury awards. Because of the difficulty of assigning such costs to individual offenses, we did not include estimates of costs to society such as expenses from protection services.

We draw on three extant sources of data on monetized costs of crime. Given the fact that there are several estimates of crime costs in the literature which use similar approaches, we chose to use the most recent estimate for each crime. Thus, we used estimates by McCollister for rape, aggravated assault, robbery, arson, larceny/theft, burglary, motor vehicle theft, murder, stolen property offenses, vandalism, forgery and counterfeiting, embezzlement and fraud. We used Miller et al. to obtain averted victim cost estimates for child abuse (non-sexual and sexual), simple assault,[21] fatal assault (which we equated to manslaughter), and drunk driving.[22] Finally, we used estimates from Rajkumar and French[23] for gambling, prostitution, and drug law violations.

Since McCollister and Rajkumar and French's estimates include criminal justice costs, which we estimated separately, a deflator was used to adjust their numbers downward. Since Rajkumar and French report criminal justice system costs for aggravated assault, robbery, motor vehicle theft,

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[19] Miller, Ted R., Mark A. Cohen and Brian Wiersema, "Victim Costs and Consequences: A New Look," Final Summary Report to the National Institute of Justice, January 1996.

[20] Cohen, Mark A. "Pain, Suffering, And Jury Awards: A Study Of The Cost Of Crime To Victims," Law & Society Review, Volume 22, Number 3, 1988.

[21] They report separate estimates for assault with and without injury, from which we calculate a weighted average for simple assault. Weights are 0.7 for assault without injury and 0.3 for assault with injury, based on their findings of incidence of each type of arrest.

[22] We used the estimate for drunk driving without injury on the assumption that in the event of injury, it would involve another offense.

[23] Rajkumar, Andrew S. and Michael T. French, "Drug Abuse, Crime Costs, and the Economic Benefits of Treatment," *Journal of Quantitative Criminology*, Vol. 13, No. 3, 1997, pp. 291-323.

household burglary, larceny, forgery, gambling, prostitution, and drug law violations, a criminal justice multiplier was constructed for each offense costed by McCollister.

Table D.1 reports monetized estimates of victim costs for each crime we consider in 2004 dollars. Offense data from Maryland DPSCS were coded to most closely fit offenses to the crime categories listed in **Table D.1**. To the extent that the available data allowed us to ascertain that an offense was minor (such as really petty theft) such records were not assigned a cost. Attempted offenses and conspiracies to commit offenses were treated as if they were offenses themselves, except for attempted homicides, which were treated as three times aggravated assault. Certain offenses, however, were not matched, and, as a result, no cost estimates for them were calculated.

Table D.1**. Victimization Cost Estimates of Crimes ($2004)**

Crime Name	Crime Codes	Estimate Net of CJS Costs
Murder/Homicide	11	$1,139,922
Attempted Murder		$388,257
Rape/Sexual Assault	12	$196,601
Sexual Abuse of Minor	63	$129,419
Aggravated Assault	13	$109,881
Child Abuse	26	$78,436
Robbery	14	$41,292
(Simple) Assault	24	$19,478
Arson	15	$8,260
Motor Vehicle Theft	16	$3,577
DUI/Drunk Driving	27	$3,530
Burglary	17	$1,239
Larceny/Theft	18	$292
Stolen Property Offenses	19	$107
Vandalism	20	$97
Forgery and Counterfeiting	21	$92
Embezzlement	22	$92
Fraud	23	$89
Drug Offenses	2	$4
Gambling	57	$0
Prostitution	58	$0

In addition, costs accruing to three public agencies—the Police Department and the Division of Correction and the Division of Probation—were estimated. For the cost of arrest and processing, we used the estimate of $1,000 per arrest.[24] To obtain cost savings from averted incarceration and probation that can be attributed to REP, we rely on recidivism offenses documented in the DPSCS data. We consider a set of all arrest records for which the arrest date falls after the 'releasedate' other than those for which the verdict is Not Guilty, Nolle Prosequi or Removed, to constitute recidivism. 'Releasedate' was set equal to the release date from MTC, that is closest to the entry date into REP (as reported by program officials), for the treatment group, and the first release date from MTC in the period of program operation (2001–2004) for the comparison group. For each individual, we identified the longest prison sentence and the longest probation sentence on file. The longest sentences are used as the total prison sentence and total probation sentence for all recidivism offenses. Although offenders are sometimes incarcerated for more than one charge, and may be arrested multiple times, sources at DPSCS advised us to use the maximum sentence as the best available estimate of total statutory sentence due to the high prevalence of concurrent sentencing. To estimate total incarceration costs for each offender, each confinement sentence was multiplied by 55 percent, an estimate of the percentage of sentence likely to be actually served (based on findings using DPSCS data on Maryland inmates sentenced in 1993, reported in Wellford and Souryal).[25] The imputed sentence length is multiplied by a daily prison cost of $77.19 per day to obtain a monetized value of the cost of incarcerating a given recidivating offender. The present value of the cost of imputed prison sentences is discounted at a rate of five percent per annum. In a similar fashion, we multiply the maximum probation sentence by the cost of probation, $7 a day as reported by Alemi et al.[26] Finally, the cost of incarceration and the cost of probation are added together to obtain the total cost of sanctions to public agencies.

In order to estimate marginal benefits, tobit regression is employed where the dependent variable is the total monetized cost to society (the sum of cost to victims and cost to public agencies) associated with each offender. The following model is employed to isolate the impact of REP:

$$\text{SOCIAL COSTi} = \alpha_i + c_1 \text{REP}_i + K\gamma + e_i \qquad (3)$$

c_1, the coefficient on REP, allows us to directly estimate the marginal benefit of treatment – that amount of money the society saves as a result of REP.

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[24] Some existing estimates put the cost of arrest (including investigation and pre-trial detention) higher. See Harrell, Cavanagh, and Roman, 1998. However, some of the victimization cost estimates (McCollister, 2002, and Rajkumar and French, 1997) already include criminal justice system costs, so a lower number is justified.

[25] Wellford, Charles F. and Claire Souryal, "An Examination of Time-to-Serve in the Maryland State Correctional System," Maryland State Commission on Criminal Sentencing Policy, 1998.

[26] Alemi, Farrokh et al. "Activity Based Costing of Probation with and without Substance Abuse Treatment: A Case Study," *Journal of Mental Health Policy and Economics*, Vol. 7, No. 2, 2004, pp. 51-58.

APPENDIX D. ADDITIONAL ANALYSES

Table D.1.. Re-arrest by Number of Years Since Release

	Any Re-arrest				Number of Re-arrests			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Independent Variable	**Year 1**	**Years 1-2**	**Years 1-3**	**Years 1-4**	**Year 1**	**Years 1-2**	**Years 1-3**	**Years 1-4**
MREP	0.836 (0.151)	0.761 (0.167)	0.857 (0.241)	0.604 (0.241)	-0.202* (0.106)	-0.129 (0.092)	-0.073 (0.098)	-0.056 (0.140)
Time at Risk	1.000[a] (0.0002)	0.999** (0.0003)	0.999** (0.001)	0.998* (0.001)	0.00008 (0.0001)	-0.00007 (0.0001)	-0.0002 (0.0002)	-0.001* (0.0003)
Age at Release	0.937*** (0.010)	0.931*** (0.012)	0.924*** (0.016)	0.944** (0.021)	-0.039*** (0.006)	-0.037*** (0.005)	-0.034*** (0.006)	-0.036*** (0.008)
Prior Arrests for Property Crimes	1.068*** (0.023)	1.072** (0.029)	1.086** (0.043)	1.327*** (0.119)	0.032*** (0.009)	0.027*** (0.009)	0.029*** (0.010)	0.042*** (0.015)
Prior Arrests for Person Crimes	1.097** (0.043)	1.057 (0.050)	1.104[a] (0.070)	1.146 (0.117)	0.061*** (0.021)	0.050*** (0.019)	0.055*** (0.021)	0.048[a] 0.033
Prior Arrests for Drug Crimes	1.113*** (0.027)	1.245*** (0.045)	1.300*** (0.064)	1.281*** (0.091)	0.061*** (0.013)	0.072*** (0.011)	0.064*** (0.013)	0.047** (0.019)
Parole Violator	0.737 (0.271)	0.706 (0.313)	0.873 (0.527)	0.956 (0.719)	0.014 (0.212)	-0.087 (0.191)	-0.068 (0.227)	-0.083 (0.277)
Mandatory Release	2.299*** (0.555)	3.348*** (1.102)	3.147*** (1.382)	2.401 (1.474)	0.222* (0.133)	0.117 (0.122)	0.059 (0.137)	0.136 (0.200)
Poor In-Prison Performance Record	1.190 (0.225)	1.297 (0.298)	1.957** (0.583)	1.745 (0.703)	0.193* (0.108)	0.123 (0.095)	0.142 (0.102)	0.235[a] (0.147)
Escape Risk Alert	0.898 (0.251)	1.188 (0.429)	1.242 (0.548)	0.650 (0.409)	-0.041 (0.155)	-0.013 (0.137)	0.002 (0.145)	-0.220 (0.235)
Instant Offense is a Felony	0.847 (0.168)	0.857 (0.202)	0.890 (0.269)	0.780 (0.322)	0.064 (0.114)	0.017 (1.000)	-0.124 (0.106)	-0.274* (0.148)
Intercept					0.494* (0.297)	1.270*** (0.290)	1.761*** (0.393)	2.935*** (0.652)
N	599	509	369	201	599	509	369	201
Pseudo R^2	0.100	0.154	0.186	0.220	0.048	0.047	0.043	0.041

Note: Significance: *** = p < 0.01; ** = p<0.05; * = p<0.1; [a] = p<0.15
All tests are two-tailed.

Table D.2. Reconviction by Number of Years Since Release

	Any Reconviction				Number of Reconvictions			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Independent Variable	**Year 1**	**Years 1-2**	**Years 1-3**	**Years 1-4**	**Year 1**	**Years 1-2**	**Years 1-3**	**Years 1-4**
MREP	1.001 (0.189)	1.061 (0.209)	0.936 (0.223)	0.620[a] (0.206)	-0.026 (0.147)	-0.053 (0.118)	-0.084 (0.123)	-0.175 (0.171)
Time at Risk	1.000 (0.0002)	1.000 (0.0003)	0.999* (0.0005)	0.999* (0.001)	0.0004** (0.0002)	0.00003 (0.0002)	-0.0002 (0.0002)	-0.0004 (0.0004)
Age at Release	0.962*** (0.011)	0.955*** (0.011)	0.950*** (0.013)	0.949*** (0.018)	-0.024*** (0.009)	-0.025*** (0.007)	-0.025*** (0.007)	-0.025** (0.010)
Prior Arrests for Property Crimes	1.048** (0.020)	1.042** (0.021)	1.093*** (0.036)	1.153*** (0.062)	0.029** (0.013)	0.024** (0.011)	0.029** (0.012)	0.031* (0.017)
Prior Arrests for Person Crimes	1.020 0.041	1.003 (0.042)	0.996 (0.051)	0.996 (0.077)	-0.0005 (0.032)	-0.004 (0.026)	-0.009 (0.027)	-0.018 (0.041)
Prior Arrests for Drug Crimes	1.096*** (0.026)	1.161*** (0.032)	1.164*** (0.040)	1.108** (0.053)	0.071*** (0.018)	0.077*** (0.014)	0.071*** (0.015)	0.037[a] (0.023)
Parole Violator	0.970 (0.377)	0.984 (0.395)	1.449 (0.773)	1.272 (0.806)	-0.083 (0.304)	-0.151 (0.249)	-0.176 (0.290)	-0.281 (0.350)
Mandatory Release	1.707** (0.414)	2.403*** (0.637)	2.421*** (0.829)	1.504 (0.704)	0.366* (0.189)	0.285* (0.155)	0.241 (0.172)	0.312 (0.247)
Poor In Prison Performance Record	1.275 (0.253)	1.194 (0.243)	1.545* (0.385)	1.154 (0.393)	0.284* (0.154)	0.177[a] (0.122)	0.157 (0.129)	0.193 (0.178)
Escape Risk Alert	0.897 (0.259)	1.097 (0.333)	0.882 (0.317)	0.587 (0.307)	-0.232 (0.233)	-0.219 (0.185)	-0.120 (0.187)	-0.513* (0.312)
Instant Offense is a Felony	0.763 (0.159)	0.649** (0.138)	0.649* (0.167)	0.609 (0.211)	-0.027 (0.159)	-0.127 (0.127)	-0.207[a] (0.134)	-0.397** (0.180)
Intercept					-1.183*** (0.436)	0.139 (0.375)	0.864* (0.489)	1.848** (0.781)
N	599	509	369	201	599	509	369	201
Pseudo R^2	0.050	0.087	0.108	0.109	0.034	0.037	0.036	0.035

Note: Significance: *** = p < 0.01; ** = p<0.05; * = p<0.1; [a] = p<0.15
All tests are two-tailed.

Table D.3. Marginal Benefits by Number of Years Since Release

	Crime Victims				Prison & Probation				Society			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Independent	**Year 1**	**Year 2**	**Year 3**	**Year 4**	**Year 1**	**Year 2**	**Year 3**	**Year 4**	**Year 1**	**Year 2**	**Year 3**	**Year 4**
MREP	-$5,556 ($7,051)	-$17,724 ($20,870)	-$7,059 ($8,073)	-$8,284 ($14,098)	$5,877 ($10,782)	-$3,016 ($10,314)	-$3940 ($10,905)	-$7,243 ($14,222)	-$4,743 ($9,965)	-$27,933 ($20,328)	-$11,089 ($13,345)	-$13,984 ($21,571)
Time at Risk	$1 ($9)	-$43 ($33)	-$20 ($17)	-$43 ($33)	$9 ($14)	-$6 ($16)	-$30 ($23)	-$60* ($33)	$1 ($13)	-$37 ($32)	-$33 ($28)	-$79 ($51)
Age at Release	-$2,363*** ($422)	-$5,946*** ($1,226)	-$2,498*** ($483)	-$3,109*** ($821)	-$2,359*** ($634)	-$2,878*** ($604)	-$2,750*** ($649)	-$2,371*** ($817)	-$3,506*** ($587)	-$6,127*** ($1,183)	-$4,392*** ($793)	-$4,824 ($1,243)
Prior Arrests for Property Crimes	$3,066*** ($678)	$5,717*** ($1,967)	$3,983*** ($784)	$6,719*** ($1,385)	$2,722*** ($1,037)	$2,002** ($964)	$2,634** ($1,052)	$2,369* ($1,388)	$4,425*** ($973)	$5,986*** ($1,925)	$5,538*** ($1,308)	$8,118 ($2,138)
Prior Arrests for Person Crimes	$1,608 ($1,478)	$2,792 ($4,461)	$1,952 ($1,744)	$787 ($3,347)	$726 ($2,301)	-$88 ($2,226)	$1,235 ($2,370)	$899 ($3,344)	$2,464 ($2,088)	$4,962 ($4,304)	$3,584 ($2,882)	$2,515 ($5,087)
Prior Arrests for Drug Crimes	$1,941*** ($894)	$8,940*** ($2,637)	$1,433 ($1,034)	$1,736 ($1,944)	$5,118*** ($1,362)	$6,904*** ($1,296)	$6,055*** ($1,396)	$3,019[a] ($1,964)	$4,167*** ($1,263)	$10,177*** ($2,561)	$5,486*** ($1,714)	$4,194 ($2,974)
Parole Violation	-$10,708 ($15,030)	-$33,675 ($44,100)	$881 ($17,938)	-$7,933 ($27,543)	$10,043 ($21,773)	$2,902 ($20,898)	$27,839 ($23,867)	$35,361 ($26,742)	-$4,268 ($20,674)	-$18,115 ($41,638)	$19,536 ($29,601)	$17,815 ($41,770)
On Supervision	$9,245** ($9,044)	$19,166 ($27,166)	$9,208 ($11,132)	$18,456 ($19,865)	$28,675** ($13,992)	$40,384*** ($13,508)	$21,152* ($15,104)	$5,417 ($19,860)	$27,585** ($12,706)	$32,375 ($26,343)	$24,724 ($18,450)	$13,922 ($30,393)
Poor In Prison Performance Record	$12,759 ($7,315)	-$3,199 ($21,586)	$18,642** ($8,472)	$34,665** ($14,678)	$16,946[a] ($11,251)	$12,216 ($10,652)	$20,176* ($11,387)	$17,278 ($14,577)	$17,683 ($10,332)	$2,993 ($20,895)	$34,028** ($14,003)	$49,010 ($22,242)
Escape Risk Alert	-$3,707 ($10,726)	-$9,080 ($31,415)	$8,764 ($11,878)	-$5,264 ($23,258)	-$6,128 ($16,356)	$5,490 ($15,443)	$16,349 ($16,060)	-$5,457 ($23,096)	-$2,282 ($14,956)	$1,413 ($30,350)	$24,987 ($19,638)	-$8,487 ($35,263)
Instant Offense is a Felony	$7,704 ($7,717)	$12,860 ($22,670)	$9,273 ($8,755)	$7,202 ($14,741)	-$2,530 ($11,811)	-$5,421 ($11,187)	-$9,585 ($11,860)	$11,887 ($14,845)	$11,409 ($10,935)	$20,259 ($21,978)	$7,081 ($14,447)	$7,194 ($22,425)
Intercept	$11,643 ($20,110)	$134,377** ($65,571)	$73,094** ($31,861)	$115,318* ($64,079)	-$37,453 ($31,780)	$45,294 ($32,326)	$100,214** ($43,076)	$169,244*** ($64,223)	$40,801 ($28,478)	$174,873*** ($63,585)	$159,844** ($52,430)	$249,911 ($98,385)
N	599	509	369	201	599	509	369	201	599	509	369	201
Likelihood Ratio	52.82	40.24	53.02	36.97	35.67	56.72	46.40	17.48	63.75	50.45	58.71	30.80

Note: Significance: *** = p < 0.01; ** = p<0.05; * = p<0.1; [a] = p<0.15
All tests are two-tailed.