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Evidence-Based Public Policy Options to Reduce Crime and Criminal Justice Costs: Implications in Washington State

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Abstract: In 2006, long-term forecasts indicated that Washington faced the need to construct several new prisons in the following two decades. Since new prisons are costly, the Washington legislature directed the Washington State Institute for Public Policy to project whether there are "evidence-based" options that can reduce the future need for prison beds, save money for state and local taxpayers, and contribute to lower crime rates. The institute conducted a systematic review of all research evidence that could be located to determine what works, if anything, to reduce crime. We found and analyzed 545 comparison-group evaluations of adult corrections, juvenile corrections, and prevention programs. We then estimated the benefits and costs of many of these evidence-based options and found that some evidence-based programs produce favorable returns on investment. This paper presents our findings and describes our meta-analytic and economic methods.

Keywords: cost effectiveness, correctional intervention, evidence-based policy

During the mid-1990s, the Washington legislature began to enact statutes to promote an "evidence-based" approach to several public policies. While the phrase "evidence-based" has not always been precisely defined in legislation, it has generally been constructed to describe a program or policy supported by outcome evaluations clearly demonstrating effectiveness. Additionally, to determine if taxpayers receive an adequate return on investment, the legislature began to require cost-benefit analyses of certain state-funded programs and practices.

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Washington's initial experiments with evidence-based and cost-beneficial public policies began in the state's juvenile justice system. The legislature funded several nationally known and well-researched programs designed to reduce the reoffending rates of juveniles. At the same time, the legislature eliminated the funding of a juvenile justice program when a careful evaluation revealed it was failing to reduce juvenile crime. Following this initial successful venture into evidence-based public policy, Washington began to introduce the approach to other areas including child welfare, mental health, substance abuse, K–12 education, and adult corrections.

In 2005, long-term forecasts indicated that Washington would need two new prisons by 2020 and possibly another by 2030. That year's legislature directed the institute to determine if evidence-based options existed that could reduce the need for prison construction, save money for state and local taxpayers, and contribute to lower crime rates (Capital Budget, 2005). We conducted a systematic review of all the research evidence we could locate in adult corrections, juvenile corrections, and prevention programs and found that some evidence-based programs reduce crime while others do not; we also conducted an economic analysis of many of the programs (Aos, Miller, & Drake, 2006).

Based on the findings, the 2007 legislature made significant investments by allotting $48 million in the biennial budget for the expanded use of evidence-based programs. Investments were made in many adult and juvenile justice programs, as well as in prevention programs—including drug treatment, education, vocational training, correctional industries, functional family therapy, multisystemic therapy, aggression replacement training, and early childhood education. The state's prison forecast was subsequently adjusted downward to reflect the resource decisions made by the 2007 legislature.

In this paper, we present the findings from our 2006 study, including some revisions since its publication. This research is part of an ongoing effort to improve Washington's criminal justice system; the narrative presented here is a snapshot of the current analytical process. Due to space limitations, we focus on our statistical review of the evaluation literature and on our per-program economic analysis. We do not include our estimates of the aggregate impacts of evidence-based programs on forecasted prison populations or statewide crime rates.

We proceed in two steps. The first step addresses the question: What works? Specifically, do rigorous evaluations indicate that some adult corrections programs, juvenile corrections programs, or prevention programs lower crime rates? To answer this fundamental question, we employ a systematic review of the research and use meta-analytic procedures to evaluate the evidence.

While the purpose of the first step is to determine if anything works to lower crime outcomes, in the second step we ask a follow-up question: Per dollar spent on a program, do the benefits of the program's crime reduction exceed its costs? Since all programs cost money, this additional economic test seeks to determine whether the amount of crime reduction justifies the program's expenditures. A program may have demonstrated an ability to reduce crime but, if the program costs too much, it may not be a good investment—especially when compared with alternatives including incarceration. We describe the economic model we have developed to predict how much money is spent or saved in Washington when crime goes up or down.

META-ANALYTICAL PROCEDURES

To estimate the benefits and costs of different approaches to reduce and prevent crime, we conducted separate meta-analyses of the relationship between evaluated programs and crime. In this section, we describe our procedures for searching for, including, and coding studies—along with the statistical methods we used to estimate the weighted average effects of a program.

Search Strategy

We searched for all adult and juvenile corrections and prevention evaluation studies conducted since 1970 that are written in English. We used three primary means to identify and locate these studies: (a) we consult the study lists of other systematic and narrative reviews of the adult and juvenile corrections and prevention research literature; (b) we examine the citations in the individual evaluations; and (c) we conduct independent literature searches of research databases using search engines such as Google, Proquest, Ebsco, ERIC, and SAGE. We obtained and examined copies of all individual program evaluation studies we could locate using these search procedures.

Many of these studies were published in peer-reviewed academic journals, while others were from government reports obtained from the agencies themselves. It was important to include non–peer reviewed studies, because it has been suggested that peer-reviewed publications may be biased to show positive program effects (Lipsey & Wilson, 2001). Therefore, our meta-analysis includes all available studies we could locate regardless of published source.

Criteria for Inclusion and Exclusion of Studies

Comparison group. The most important inclusion criterion in our systematic review of the literature was that an evaluation must have a control or comparison group. We did not include studies with a single-group, pre-post research design in order to avoid false inference on causality (Coalition for Evidence-Based Policy, 2003). Random assignment studies were preferred for inclusion in our review, but we also included nonrandomly assigned control groups. We only included quasiexperimental studies if sufficient information was provided to demonstrate reasonable comparability between the treatment and comparison groups on important pre-existing conditions such as age, gender, and prior criminal history. Of the 545 individual studies in our review, about 4% involved effects estimated from well-implemented random assignment studies.

Participant sampling procedures. We did not include a study in our meta-analytic review if the treatment group was made up solely of program completers. We adopted this rule to avoid unobserved self-selection factors that distinguish a program completer from a program dropout; these unobserved factors are likely to significantly bias estimated treatment effects (Lipsey, 2003). Some comparison group studies of program completers, however, contained 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 noncompleters, or if the demonstrated rate of program noncompletion was very small (e.g., under 10%). In these cases, the study still needed to meet the other inclusion requirements listed here.

Outcomes. A crime-related outcome had to be reported in the study to be included in our review. 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 are total arrests and total convictions. When a study reported both total arrests and total convictions, we calculated an effect size for each measure and then took a simple average of the two effect sizes.

Some studies included two types of measures for the same outcome: a dichotomous outcome and a continuous (mean number) measure. In these situations, we coded an effect size for the dichotomous measure. Our rationale for this choice was 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 reduce this problem (Farrington & Loeber, 2000). Of course, if a study only presented a continuous measure, we coded the continuous measure.

When a study presented outcomes with varying follow-up periods, we generally coded the effect size for the longest follow-up period. This allowed us to gain the most insight into the long-run benefits and costs of various treatments. 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.

Miscellaneous coding criteria. Our unit of analysis was an independent test of a treatment at a particular site. Some studies reported outcomes for multiple sites; we included each site as an independent observation if a unique and independent comparison group was also used at each site.

Some studies presented two types of analyses: raw outcomes that were not adjusted for covariates such as age, gender, or criminal history; and those that had been adjusted with multivariate statistical methods. In these situations, we coded the multivariate outcomes.

Procedures for Calculating Effect Sizes

Calculations for dichotomous and continuous outcomes. Effect sizes measure the degree to which a program has been shown to change an outcome for program participants relative to a comparison group. In order to be included in our review, a study had to provide the necessary information to calculate an effect size. Several methods can be used by meta-analysts to calculate effect sizes. We used the standardized mean difference effect size for continuous measures and the D-cox transformation as described in Sánchez-Meca, Chacón-Moscoso, and Marín-Martínez (2003, Equation 18) to approximate the mean difference effect size for dichotomous outcome variables.

$$d_{Cox} = \ln\left(\frac{P_e(1-p_c)}{P_c(1-p_e)}\right)/1.65 \tag{1}$$

In Equation 1, d_{cox} is the estimated effect size, which is derived by dividing the log odds ratio by the constant 1.65. Pe represents the percentage outcome for the experimental or treatment group and Pc is the percentage outcome for the control group.

For continuous outcome measures, we used the standardized mean difference effect size statistic (Lipsey & Wilson, 2001, table B10, Equation 1).

$$ES_m = \frac{M_e - M_c}{\sqrt{\frac{SD_e^2 + SD_c^2}{2}}} \tag{2}$$

In the second equation, ES_m is the estimated standardized mean effect size where M_e is the mean outcome for the experimental group, M_c is the mean outcome for the control group, SD_e is the standard deviation of the mean outcome for the experimental group, and SD_c is the standard deviation of the mean outcome for the control group.

Sometimes research studies reported the mean values needed to compute ES_m in Equation 2, but they failed to report the standard deviations. Often, however, the research reported information about statistical tests or confidence intervals that could then allow the pooled standard deviation to be estimated. These procedures are further described in Lipsey and Wilson (2001).

Some studies had very small sample sizes, which have been shown to upwardly bias effect sizes—especially when samples are less than 20. Therefore, we followed Hedges (1981) and Lipsey and Wilson (2001, Equation 3.22) and report the "Hedges correction factor," which we used to adjust all mean difference effect sizes (N is the total sample size of the combined treatment and comparison groups).

$$ES'_m = \left[1 - \frac{3}{4N-9}\right] \times [ES_m, or, d_{cox}] \tag{3}$$

Techniques Used to Combine the Evidence

Once effect sizes were calculated for each program effect, the individual measures were summed to produce a weighted average effect size for a program area. We calculated the inverse variance weight for each program effect and these weights were used to compute the average. These calculations involved three steps. First, we calculated the standard error of each mean effect size. For continuous outcomes, the standard error, SE_m, was computed with (Lipsey & Wilson, 2001, Equation 3.23)

$$SE_m = \sqrt{\frac{n_e + n_c}{n_e n_c} + \frac{(ES'_m)^2}{2(n_e + n_c)}} \tag{4}$$

In Equation 4, n_e and n_c are the number of participants in the experimental and control groups and ES'_m is from Equation 3.

For dichotomous outcomes, the standard error, SEd_{cox}, was computed with (Sánchez-Meca et al., 2003, Equation 19)

$$SE_{d_{Cox}} = \sqrt{0.367\left[\frac{1}{O_{1E}} + \frac{1}{O_{2E}} + \frac{1}{O_{1C}} + \frac{1}{O_{2C}}\right]} \tag{5}$$

In Equation 5, O_{1E} and O_{1C} represent the success frequencies of the experimental and control groups. O_{2E} and O_{2C} represent the failure frequencies of the experimental and control groups.

The second step in calculating the average effect size for a program area was to compute the inverse variance weight, w_m, for each mean effect size with (Lipsey & Wilson, 2001, Equation 3.24)

$$w_m = \frac{1}{SE_m^2} \tag{6}$$

The weighted mean effect size for a group of studies was then computed with (Lipsey & Wilson, 2001, p. 114)

$$\overline{ES} = \frac{\sum (w_m ES'_m)}{\sum w_m} \tag{7}$$

Finally, confidence intervals around this mean were computed by first calculating the standard error of the mean with (Lipsey & Wilson, 2001, p. 114)

$$SE_{\overline{ES}} = \sqrt{\frac{1}{\sum w_m}} \tag{8}$$

The lower, ES_L, and upper, ES_U, limits of the confidence interval were computed with (Lipsey & Wilson, 2001, p. 114)

$$\overline{ES_L} = \overline{ES} - z_{(1-\alpha)}(SE_{\overline{ES}}) \tag{9}$$

$$\overline{ES_U} = \overline{ES} + z_{(1-\alpha)}(SE_{\overline{ES}}) \tag{10}$$

In Equations 9 and 10, $z_{(1\text{-}\alpha)}$ is the critical value for the z-distribution.

Techniques Used to Assess Heterogeneity

Computing random effects weighted average effect sizes and confidence intervals. Once the weighted mean effect size was calculated, we tested for homogeneity. This provides a measure of the dispersion of the effect sizes around their mean and is given by (Lipsey & Wilson, 2001, p. 116)

$$Q = (\sum w\ ES^2) - \frac{(\sum wES)^2}{\sum w} \tag{11}$$

The Q-test is distributed as a chi-square with k-1 degrees of freedom (where k is the number of effect sizes). When the p-value on the Q-test indicates significance at values of p less than or equal to .05, a random effects model was performed to calculate the weighted average effect size. This was accomplished by first calculating the random effects variance component, v (Lipsey & Wilson, 2001, p. 134).

$$v = \frac{Q - (k-1)}{\sum w - (\sum wsq / \sum w)} \tag{12}$$

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

Adjustments to Effect Sizes

Methodological quality. Not all research is of equal quality and this greatly influences the confidence that can be placed in interpreting the policy-relevant results of a study. Some studies are well-designed and implemented and the results can be reasonably viewed as causal effects. Other studies are not designed as well and less confidence can be placed in the causal interpretation of any reported differences. Studies with inferior research designs 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 used a 5-point scale as a way to adjust the raw effect sizes. The scale is based closely on the 5-point scale developed by researchers at the University of Maryland (Sherman et al., 1998, chap. 2). On the 5-point scale as interpreted by our institute, each study was rated with the following numerical ratings.

A “5” was 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 report how well the random assignment actually occurred by reporting values for pre-existing characteristics for the treatment and control groups.

A “4” was assigned to a study that employed a rigorous quasiexperimental research design with a program and matched comparison group, controlling with statistical methods for self-selection bias that might otherwise influence outcomes. These quasiexperimental methods might have included estimates made with a convincing instrumental variables or regression discontinuity modeling approach or other techniques such as a Heckman self-selection model (Rhodes et al., 2001). A value of 4 might also be assigned to an experimental random assignment design that reported problems in implementation, perhaps because of significant attrition rates.

A “3” indicated a nonexperimental 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 indicated few, if any, significant differences were observed in these salient pre-existing variables. Alternatively, if an evaluation employed sound multivariate statistical techniques to control for pre-existing differences, and if the analysis was successfully completed and reported, then a study with some differences in pre-existing variables could qualify as a level 3.

A “2” involved a study with a program and matched comparison group where the two groups lacked comparability on pre-existing variables and no attempt was made to control for these differences in the study. A “1” involved an evaluation study where no comparison group was utilized.

In our meta-analytic review, we only considered evaluations that rate at least a 3 on this 5-point scale. We did not use the results from program evaluations rated as a “1” on this scale, because they did not include a comparison group and thus provided no context to judge program effectiveness. We also regarded evaluations with a rating of "2" as highly problematic and, as a result, did not consider their findings in our analyses.

An explicit adjustment factor was assigned to the results of individual effect sizes based on the institute's judgment concerning research design quality. The specific adjustments made for these studies were based on our knowledge of research in particular fields. For example, in criminal justice program evaluations, there is strong evidence that random assignment studies (i.e., level 5 studies) have, on average, smaller absolute effect sizes than studies with weaker designs (Lipsey, 2003). We used the following default adjustments to account for studies of different research design quality. The effect size of a level 3 study was discounted by 50 percent and the effect size of a level 4 study was discounted by 25 percent, while the effect size of a level 5 study was not discounted. While these factors were subjective, we believed not making some adjustments for studies with varying research design quality would severely overestimate the true causal effect of the average program.

Researcher 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 closely controlled by researchers or program developers have better results than those that operate in "real world" administrative structures (Lipsey, 2003; Petrosino & Soydan, 2005). For example, in our 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 (Barnoski, 2004). Therefore, we made an adjustment to effect sizes to reflect this distinction. As a general parameter, the institute discounted effect sizes by 50 percent for all studies deemed not to be "real world" trials.

COST-BENEFIT PROCEDURES

Once we conducted the meta-analyses to determine if a program reduces crime at a statistically significant level, we then monetized the benefits to taxpayers and crime victims of future crimes avoided, and estimated the costs of a program versus the costs of not participating in the program. We then compared the benefits to the costs in order to determine the bottom-line economics of a program.

Criminal Justice System and Crime Victim Costs

In the institute's cost-benefit model, we estimated the costs of criminal justice system resources that are paid by taxpayers for each significant part of the publicly financed system in Washington. The costs of police and sheriffs, superior courts and county prosecutors, local juvenile detention services, local adult jails, state juvenile rehabilitation, and state adult corrections were estimated separately in the analysis. Operating costs were estimated for each of these criminal justice system components, and annualized capital costs were estimated for the capital-intensive sectors.

The model used estimates of marginal operating and capital costs of the criminal justice system. In a few cases average cost figures were used when marginal cost estimates could not be reasonably estimated. Marginal criminal justice costs were defined as those costs that change over the period of several years as a result of changes in workload measures. For example, when one prisoner is added to the state adult corrections system, certain variable food and service costs increase immediately, but new corrections staff are not hired the next day. Over the course of a governmental budget cycle, however, new corrections staff are likely to be hired to handle the larger average daily population of the prison. In the institute's analysis, these "longer-run" marginal costs have been estimated—rather than immediate, short-run marginal costs. Costs and the equations used to estimate per-unit marginal operating costs can be found in Aos, Lieb, Mayfield, Miller, and Pennucci (2004).

In addition to costs paid by taxpayers, many of the costs of crime are borne by victims. Some victims lose their lives; others suffer direct, out-of-pocket personal or property losses. Psychological consequences also occur to crime victims, including feeling less secure in society. The magnitude of victim costs is very difficult—and in some cases impossible—to quantify.

National studies, however, have taken significant steps in estimating crime victim costs. One U.S. Department of Justice study by Miller, Cohen, and Wiersema (1996) divides crime victim costs into two types: (a) monetary costs, which include medical and mental health care expenses, property damage and losses, and the reduction in future earnings incurred by crime victims; and (b) quality of life cost estimates, which place a dollar value on the pain and suffering of crime victims. In that study, the quality of life victim costs were computed from jury awards for pain, suffering, and lost quality of life; for murders, the victim quality of life value was estimated from the amount people spend to reduce risks of death. In the institute's analysis, victim costs from the Miller et al. study were used as estimates of per-unit victim costs in Washington.

Crime Distributions for Offender Populations

In order to estimate the long-run effectiveness of programs, we combined the effect sizes discussed earlier with other information on offender populations in Washington. We computed recidivism parameters for various offender populations using the institute's criminal records database. Recidivism was defined as any offense committed after release to the community, or after initial placement in the community, that results in a conviction in Washington. This included convictions in juvenile and adult court.

We collected recidivism data on five general populations of offenders who became at-risk in the community during calendar year 1990. We selected 1990 because that year allowed a 13-year follow-up period to observe subsequent convictions. A one-year adjudication period was included in the follow-up to allow for court processing of any offenses toward the end of the 13-year follow-up. These recidivism data included the probability of any reoffense, the timing of reoffenses over the 13-year period, the volume of reoffenses, and the type of reoffenses.

For adult offenders, we observed the 13-year recidivism patterns for those offenders released from Washington Department of Corrections (DOC) facilities in 1990, and those offenders sentenced directly to DOC community supervision in 1990. For juvenile offenders, we observed the 13-year recidivism patterns for those offenders released from Washington State Juvenile Rehabilitation Administration (JRA) facilities in 1990, those offenders sentenced to diversion through local-sanctioning courts in 1990, and those offenders sentenced to detention/probation through local-sanctioning courts in 1990.

These five populations were further broken down by the offender's most serious current offense category. That is, we computed recidivism information for populations based on the most serious offense for which they were convicted prior to the 13-year follow-up period. These categories included drug, property, sex, violent (nonsex), drug and property, violent (sex), misdemeanors, and total felony and misdemeanor offenses. Thus, we calculated separate crime distributions for 40 populations (five offender populations multiplied by eight offense categories).

Next, we calculated probability density distributions for each of the 40 populations using lognormal, gamma, or weibull distributions, which indicated when convictions were likely to happen over the 13-year follow-up period.

From the recidivism data, we also calculated the total number of adjudications and offenses a person had during the follow-up period. Recidivism adjudications and offenses were broken down into the following offense categories: murder, sex, robbery, assault, property, drug, and misdemeanor. Using this information, we then determined the average number of adjudications a person had through the criminal justice system. In addition, we calculated the average number of offenses per adjudication. Finally, we computed the average time between sentences over the follow-up period.

For prevention programs, we similarly estimated long-run crime distributions for nonoffender populations by calculating the probability of obtaining a conviction over the life-course. We selected the 1973 birth cohort because this gave us the longest follow-up period (32 years) possible with Washington criminal records data.

Criminal Justice System Effects

Relative risk. In order to calculate the benefits of evidence-based programs, first we calculated the degree to which a program was estimated to affect crime, notated as $relativerisk_y$. This variable indicated the change in the relative risk of being convicted for a crime in year y as a function of the estimated effect size for a program and the base crime probability for the offender population. $Relativerisk_y$ is computed as

$$relativeRisk_y = \left(\frac{\dfrac{e(\mathrm{ES} * 1.65) * Crimeprob}{1 - Crimeprob + Crimeprob * e(\mathrm{ES} * 1.65)}}{Crimeprob} - 1 \right) * (1 + decayrate)^y \tag{13}$$

In Equation 13, using the D-cox transformation we computed the estimated change in outcome of the treatment group as a function of the effect size, *ES*, and the long-run recidivism rate for the relevant population, *Crimeprob*. *ES* represents the institute-adjusted effect size for each evidence-based option, as computed from the meta-analyses described in the previous section. The variable *decayrate* is a parameter that allowed us to model exponential rates of decay (or growth) in the effect size over time. We put this feature in the model because most of the evaluations included in our review analyzed crime outcomes with relatively short follow-up periods, often one or two years. In our model, however, we estimated long-run crime curves using a 13-year follow-up period. Since we applied short-term effect sizes to make long-term estimates of the effect a program had on crime outcomes, we made the assumption that the effectiveness of a program decays over time. In the model, we estimated this decay rate as 2.5% per follow-up year, a rate which lowered the effect size by about 25% at the end of the 13-year follow-up period.

Crimes committed. After calculating the degree to which a program affects the relative risk for being convicted of a crime, we estimated the impact of this crime change on the criminal justice system. We estimated the effect that an evidence-based option is expected to have on the number of crimes a person commits in each year of the follow-up period, $Crime_y$ in Equation 14. We computed this by summing for each subsequent adjudication, A, the product of the probability of being convicted for a crime, *Crimeprob*; the relative risk of being convicted after applying the effect of the program in year y, $RelativeRisk_y$; the probability density distribution that indicated when recidivism was likely to occur over the 13-year follow-up period, *Crimedist*; the average number of offenses per adjudication over the course of the follow-up period, *Offperadj*; and the estimate of the average number of victimizations per adjudication, *Vicperadj*. In the model, we attributed only 20% of the estimated total crimes committed to those who are convicted of a crime. Each of the adjudications, a, is distributed over the 13-year follow-up period with a spacing parameter, s.

$$Crime_y = \sum_{a=1}^{A} (Crimeprob * relativeRisk_y * Crimedist_{(a-1)*s+1} * Offperadj * Vicperadj) \tag{14}$$

Adjudications in the system. In Equation 15, we estimated the impact that evidence-based options have on the number of times a person enters the criminal justice system for a conviction, *Adj*, in the long-term follow-up. Not all of these adjudications, of course, occur immediately. We estimated the number of adjudications in year y by summing the product of the probability of being convicted for a crime, *Crimeprob*, for each subsequent adjudication, A; by the relative risk of being convicted after applying the effect of the program in year y, $RelativeRisk_y$; and by the probability density distribution that indicates when recidivism is likely to occur over the 13-year follow-up period, *Crimedist*. Each of the adjudications, a, is distributed over the 13-year follow-up period with a spacing parameter, s.

$$Adj_y = \sum_{a=1}^{A} \left(Crimeprob * relativeRisk_y * Crimedist_{(a-1)*s+1}\right) \tag{15}$$

Average daily prison population. In Equation 16, we estimated the effect that evidence-based programs have on the prison population, ADP_y, in year y of the long-term follow-up. We followed similar procedures as outlined in the previous two equations. We summed the number of adjudications after multiplying the probability of a crime occurring, *Crimeprob*; the relative risk after applying the effect size of the program, *RelativeRisk*; and the probability distribution of when the crime is likely to occur, *Crimedist*. Each of the adjudications, a, is distributed over the 13-year follow-up period with a spacing parameter, s.

$$ADP_y = \sum_{a=1}^{A} \left(Crimeprob * relativeRisk_y * Crimedist_{(a-1)*s+1} * PLOS_a\right) \tag{16}$$

The ADP equation also contains a term indicating the average length of stay in prison per adjudication, $PLOS_a$. In Washington, a sentencing grid is used to determine the length of a sentence, which is based upon the severity of the crime and the offender's criminal history (Sentencing Reform Act, 1981). For each type of offender population, we calculated *PLOS* by multiplying the probability that a certain type of offense would occur by the probability that a conviction would result in a prison sentence for each offense type and by the average length of stay for that offense in Washington. In order to estimate the effect of subsequent adjudications on sentence length, we accounted for increasing sentence length by computing the average extra sentence length for each subsequent adjudication.

Avoided costs. We computed the expected cash flows of avoided costs for each option with the following equations:

$$Taxben_y = ADP_y * Prison\$ + Adj_y * (Stateadj\$ + Localadj\$) \tag{17}$$

$$Totalben_y = Taxben_y + Crime_y * Victim\$ \tag{18}$$

The $Taxben_y$ equation calculated the expected streams of annual benefits that accrued to taxpayers as a result of reduced crimimal justice costs. The $Totalben_y$ equation added the benefits that accrued to crime victims (who are not victimized when crime does not happen) to the taxpayer benefits to produce an annual stream of total benefits.

In these two equations, there are four marginal cost terms: *Prison$*, *Stateadj$*, *Localadj$*, and *Victim$*. These terms described how mariginal operating and capital costs change when the average daily prison population goes up or down by one unit; how other state and local criminal justice operating and capital costs change when convictions go up or down by one unit; and how victim costs of crime change when crime goes up or down by one unit. The procedures to calculate these four marginal cost terms can be found in Aos et al. (2004).

The net present value of the annual stream of benefits and costs was computed with Equation 19. For each year y in the 13-year follow-up period, annual benefits and costs were discounted to present value with an overall real discount rate, *DiscountRate*.

$$NPV = \sum_{y=1}^{Y} \frac{(TotalBen_y - Cost_y)}{(1 + DiscountRate)^y} \tag{19}$$

FINDINGS

What Reduces Crime?

Table 1 summarizes the findings from our current systematic review of the evaluation research literature. We find that a number of adult and juvenile justice and prevention programs demonstrate statistically significant reductions in crime outcomes. We also find that some programs do not achieve a statistically significant reduction in recidivism. Thus, the overall lesson from our evidence-based review is that public policy makers need to be smart investors: some programs work, some programs do not, and careful analysis is needed to inform policy decisions.

Table 1: Reducing crime with evidence-based options: What works and analysis of benefits and costs.

	Effect on Crime Outcomes: Percent Change in Crime Outcomes and the Number of Evidence-Based Studies on Which the Estimate is Based (in Parentheses)	Benefits and Costs (Per Participant, Net Present Value, 2007 Dollars)			
		Benefits to Crime Victims (of the Reduction in Crime)	Benefits to Taxpayers (of the Reduction in Crime)	Costs (Marginal Program Cost, Compared to the Cost of Alternative)	Benefits (Total) Minus Costs (per Participant)
	(1)	(2)	(3)	(4)	(5)
Programs for People in the Adult Offender System					
Vocational education in prison	–9.8% (4)	$14,504	$7,419	$1,210	$20,714
Intensive supervision: treatment-oriented programs	–17.9% (11)	$16,239	$10,235	$7,356	$19,118
Washington's Dangerously Mentally Ill Offender program	–20.7% (1)	$30,732	$15,720	$27,617	$18,836
General education in prison (basic education or postsecondary)	–8.3% (17)	$12,319	$6,302	$985	$17,636
Cognitive-behavioral therapy in prison or community	–6.9% (25)	$10,234	$5,235	$107	$15,361
Correctional industries in prison	–6.4% (4)	$9,518	$4,869	$427	$13,961
Drug treatment in prison (therapeutic communities or outpatient)	–6.4% (21)	$9,498	$4,859	$1,642	$12,715

Drug treatment in community	−8.3% (6)	$7,471	$4,972	$588	$11,856
Adult drug courts	−8.7% (57)	$7,798	$5,190	$4,474	$8,514
Employment and job training in the community	−4.6% (16)	$4,147	$2,614	$409	$6,351
Sex offender treatment in prison with aftercare	−9.6% (6)	$12,901	$4,044	$12,881	$4,064
Washington's Work Release from prison	−1.3% (1)	$1,835	$1,069	$615	$2,288
Electronic monitoring to offset jail time	0% (12)	$0	$0	−$926	$926
Intensive supervision: surveillance-oriented programs	0% (23)	$0	$0	$3,869	−$3,869
Adult boot camps	0% (22)	$0	$0	n/e	n/e
Domestic violence education/cognitive-behavioral treatment	0% (9)	$0	$0	n/e	n/e
Drug treatment in jail	0% (8)	$0	$0	n/e	n/e
Jail diversion for mentally ill offenders	+5.3% (11)	−$4,831	−$3,045	n/e	n/e
Life skills education programs for adults	0% (4)	$0	$0	n/e	n/e
Restorative justice programs for lower risk adult offenders	0% (6)	$0	$0	n/e	n/e

(*Continued*)

Table 1: (*Continued*)

	Effect on Crime Outcomes: Percent Change in Crime Outcomes and the Number of Evidence-Based Studies on Which the Estimate is Based (in Parentheses)	Benefits and Costs (Per Participant, Net Present Value, 2007 Dollars)			
		Benefits to Crime Victims (of the Reduction in Crime)	Benefits to Taxpayers (of the Reduction in Crime)	Costs (Marginal Program Cost, Compared to the Cost of Alternative)	Benefits (Total) Minus Costs (per Participant)
	(1)	(2)	(3)	(4)	(5)
Programs for Youth in the Juvenile Offender System					
Multidimensional treatment foster care (versus regular group care)	–17.9% (3)	$69,519	$26,360	$6,926	$88,953
Functional family therapy on probation	–18.1% (7)	$35,470	$16,686	$2,380	$49,776
Adolescent diversion project (for lower risk offenders)	–17.6% (6)	$34,318	$16,145	$1,975	$48,488
Family integrated transitions	–10.2% (1)	$39,678	$15,045	$9,970	$44,753
Sex offender treatment	–9.7% (5)	$49,443	$8,061	$33,842	$23,662
Aggression replacement training	–8.3% (4)	$16,276	$7,657	$918	$23,015
Multisystemic therapy	–7.7% (10)	$15,001	$7,057	$4,364	$17,694
Teen courts	–14.0% (5)	$11,401	$5,507	$937	$15,971
Restorative justice for low risk offenders	–8.0% (21)	$6,479	$3,130	$907	$8,702
Boot camp to offset institution time	0% (14)	$0	$0	-$8,325	$8,325
Interagency coordination programs	–1.9% (14)	$3,726	$1,753	$210	$5,269
Regular surveillance-oriented parole (versus no parole supervision)	0% (2)	$0	$0	$1,237	–$1,237

Intensive probation supervision programs	0% (3)	$0	$0	$1,650	–$1,650
Wilderness challenge	0% (9)	$0	$0	$3,185	–$3,185
Intensive parole supervision	0% (10)	$0	$0	$6,670	–$6,670
Scared straight	+6.1% (10)	–$11,840	–$5,570	$60	–$17,470
Behavior modification	–7.6% (4)	$29,443	$11,164	n/e	n/e
Cognitive-behavioral treatment	–2.6% (8)	$5,007	$2,356	n/e	n/e
Counseling/psychotherapy	–16.6% (6)	$32,528	$15,303	n/e	n/e
Court supervision versus simple release without services	0% (8)	$0	$0	n/e	n/e
Diversion programs with services (versus simple release)	0% (7)	$0	$0	n/e	n/e
Diversion programs with services (versus regular juvenile court)	–3.1% (20)	$2,553	$1,233	n/e	n/e
Education programs	–19.4% (3)	$75,722	$28,713	n/e	n/e
Guided group interaction	0% (4)	$0	$0	n/e	n/e
Intensive probation (as alternative to incarceration)	0% (5)	$0	$0	n/e	n/e
Life skills education programs	–2.5% (3)	$9,585	$3,635	n/e	n/e
Other family-based therapy programs	–13.3% (12)	$26,047	$12,254	n/e	n/e
Team child	–9.7% (2)	$7,860	$3,797	n/e	n/e
Prevention Programs (Crime Reduction Effects Only)					
Pre-K education for low-income 3- and 4-year-olds	–16.6% (8)	$9,882	$5,579	$612	$14,848
Nurse family partnership: children	–15.7% (1)	$8,515	$4,808	$756	$12,567
Nurse family partnership: mothers	–38.2% (1)	$8,093	$5,676	$5,580	$8,189
Guiding good choices	–7.2% (1)	$959	$1,627	n/e	n/e
High school graduation	–21.1% (1)	$3,647	$5,915	n/e	n/e

(*Continued*)

Table 1: (*Continued*)

	Effect on Crime Outcomes: Percent Change in Crime Outcomes and the Number of Evidence-Based Studies on Which the Estimate is Based (in Parentheses)	Benefits and Costs (Per Participant, Net Present Value, 2007 Dollars)			
		Benefits to Crime Victims (of the Reduction in Crime)	Benefits to Taxpayers (of the Reduction in Crime)	Costs (Marginal Program Cost, Compared to the Cost of Alternative)	Benefits (Total) Minus Costs (per Participant)
	(1)	(2)	(3)	(4)	(5)
Parent-child interaction therapy	–5.1% (1)	$1,793	$994	n/e	n/e
Seattle social development project	–15.7% (1)	$2,270	$3,652	n/e	n/e
Program Types in Need of Additional Research and Development Before We Can Conclude They Do or Do Not Reduce Crime Outcomes:					
Programs Needing More Research for People in the Adult Offender System					
		Comment			
Case management in the community for drug offenders	0% (13)	Findings are mixed for this broad grouping of programs.			
COSA (faith-based supervision of sex offenders)	–35.3% (1)	Too few evaluations to date.			
Day fines (compared to standard probation)	0% (1)	Too few evaluations to date.			
Domestic violence courts	0% (2)	Too few evaluations to date.			
Faith-based programs	0% (5)	Too few evaluations to date.			
Intensive supervision of sex offenders in the community	0% (4)	Findings are mixed for this broad grouping of programs.			
Medical treatment of sex offenders	–22.0% (1)	Too few evaluations to date.			

Mixed treatment of sex offenders in the community	0% (2)	Too few evaluations to date.
Regular parole supervision versus no parole supervision	0% (1)	Too few evaluations to date.
Therapeutic community programs for mentally ill offenders	–21.8% (2)	Too few evaluations to date.
Work release programs, other	–4.4% (4)	Too few recent evaluations.
Programs Needing More Research for Youth in the Juvenile Offender System		
Curfews	0% (1)	Too few evaluations to date.
Day reporting	0% (2)	Too few evaluations to date.
Dialectical behavior therapy	0% (1)	Too few evaluations to date.
Drug courts	0% (15)	Findings are mixed for this broad grouping of programs.
Increased drug testing (on parole) versus minimal drug testing	0% (1)	Too few evaluations to date.
Jobs programs	0% (3)	Too few recent evaluations.
Mentoring	0% (1)	Too few evaluations to date.
Therapeutic communities	0% (1)	Too few evaluations to date.

"n/e" means not estimated at this time; prevention program costs are partial program costs, prorated to match crime outcomes.

We reviewed and meta-analyzed the findings of 545 comparison-group evaluations of adult corrections, juvenile corrections, and prevention programs. Each of these evaluations included at least one relevant crime outcome that we were able to analyze. It is important to note that evaluations of prevention programs typically measure several other outcomes in addition to crime, such as measures of education, substance abuse, and child abuse. In Table 1, however, we show only the results of crime effects for studies that measured crime outcomes. In an earlier institute report, we analyzed the degree to which a wide array of evidence-based prevention programs affected noncrime outcomes (Aos et al., 2004).

To make this information useful for policy making in Washington, we categorized each of these 545 evaluations into relevant subject areas. For example, we found 57 evaluations of adult drug courts, and we analyzed these studies as a group. This categorization process illustrates a key characteristic of our study. For each category of programs we analyze, our results reflect the evidence-based effect we expect for the "average" program. As shown in Table 1, our results indicate that the average adult drug court reduces the recidivism rate of participants by 8.7%. Some drug courts, of course, achieve better results than this, some worse. On average, however, we find that the typical drug court can be expected to achieve this result.

At the bottom of Table 1, we also list a number of programs for which the research evidence, in our judgment, is inconclusive at this time. Some of these programs have only one or two rigorous (often small sample) evaluations that do not allow us to draw general conclusions. Other programs have more evaluations but the program category is too diverse or too general to allow meaningful conclusions to be drawn at this time. Subsequent research on these types of programs is warranted.

In column 1 of Table 1, we show the expected percentage change in crime outcomes for the program categories we review. This figure indicates the average amount of change in crime outcomes—compared to no treatment or treatment as usual—that can be achieved by a typical program in each category of programs. A negative value indicates the magnitude of a statistically significant reduction in crime. A zero percent change means that, based on our review of the evidence, a typical program does not achieve a statistically significant change in crime outcomes. A few well-researched programs have a positive sign indicating that crime is increased with the program, not decreased. In addition to reporting the effect of the programs on crime outcomes, column 1 also reports the number of studies on which the estimate is based.

As an example of the information provided in Table 1, we analyzed the findings from 25 well-researched studies of cognitive-behavioral programs for adult offenders in prison and community settings. We find that, on average, these programs can be expected to reduce recidivism rates by 6.9%. These findings are consistent with Landenberger and Lipsey (2005). To put this in perspective, our analysis indicates that, without a cognitive-behavioral program, about 63% of offenders will recidivate with a new felony or misdemeanor conviction after a 13-year follow-up. If these same offenders had participated in the evidence-based cognitive-behavioral treatment program, then we expect their recidivism probability would drop four points to 59%—a 6.9% reduction in recidivism rates.

Table 1 shows there are many programs in the adult criminal justice system, in addition to cognitive-behavioral programs, that are effective at reducing crime, such as vocational education in prison, drug treatment, correctional industries, and employment and work programs. Other programs such as intensive supervision, however, are only found to be effective when coupled with treatment-oriented programs—intensive supervision alone is not found to be effective.

As noted, most of the categories we report in Table 1 are for general types of policy-relevant programs, such as drug treatment in prison or adult basic education in prison. We also categorize and report, however, the results of several very specific programs, such as a program for juvenile offenders named "functional family therapy."

The functional family therapy (FFT) program follows a specific training manual and approach. These types of programs are more capable of being reproduced in the field when appropriate quality control is assured. Several of these programs have been listed as "Blueprint" programs by the Center for the Study and Prevention of Violence at the University of Colorado (Center for the Study and Prevention of Violence, n.d.).

The FFT program, which has been implemented in Washington, involves an FFT-trained therapist working for about three months with a youth in the juvenile justice system and his or her family. The goal is to increase the likelihood that the youth will stay out of future trouble. We located and meta-analyzed seven rigorous evaluations of this program—one conducted in Washington—and found that the average FFT program with quality control can be expected to reduce a juvenile's recidivism rate by 18.1%. Our analysis indicates that, without the program, a youth has a 70% chance of recidivating for another felony or misdemeanor conviction after a 13-year follow-up. If the youth participates in FFT, then we would expect the recidivism rate to drop to 57%—an 18.1% reduction.

In addition to FFT, many other juvenile justice programs are also shown to be effective at reducing crime and often produce a greater reduction in recidivism than adult programs. Such programs include the adolescent diversion project for lower risk offenders, multidimensional treatment foster care, and multisystemic therapy.

A third example is a prevention program called nurse family partnership (NFP), another program that has been implemented in Washington. This program provides intensive visitation by nurses to low-income, at-risk women bearing their first child; the nurses continue to visit the home for two years after birth. Thus far, there is evidence that NFP reduces the crime outcomes of the mothers and, many years later, the children born to the mothers. Both of these effects are included in our analysis of the program. Our analysis of the NFP studies indicates that the program has a large effect on the future criminality of the mothers who participate in the program, reducing crime outcomes by 38.2%. NFP also reduces the future crime levels of the youth by 15.7% compared with similar youth who did not participate in the NFP program.

What Are the Costs and Benefits?

While our first research question deals with what works, our second question concerns economics. We find that there are economically attractive evidence-based options in three areas: adult corrections programs, juvenile corrections programs, and prevention. Per dollar of spending, several of the successful programs produce favorable returns on investment. Public policies incorporating these options can yield positive outcomes for Washington.

Table 1 also contains our estimates of the benefits and costs of many of the program categories we analyze. Within three broad groupings—programs for adult offenders, programs for juvenile offenders, and prevention programs—we rank many of the options by our assessment of each program's "bottom line" economics for reducing crime.

For programs that have an evidence-based ability to affect crime, we estimate benefits from two perspectives: taxpayers' and crime victims'. For example, if a program is able to achieve statistically significant reductions in recidivism rates, then taxpayers will spend less money on the criminal justice system. Similarly, if a program produces less crime, then there will be fewer crime victims. The estimates shown in columns 2 and 3 of Table 1 display our estimates of victim and taxpayer benefits, respectively. Of course, a program category that does not achieve a statistically significant reduction in crime outcomes will not produce any benefits associated with reduced crime.

In column 4 we show our estimates of the costs per participant for many of the programs. At this time, we have not estimated the costs for every program category listed on Table 1; thus, we do not produce full cost-benefit results for all programs in the table.

Finally, in column 5 of Table 1, we show our "bottom line" estimate of the net gain (or loss). These figures are the net present values of the long-run benefits of crime reduction minus the net up-front costs of the program. This provides our overall measure of what each type of program can be expected to achieve per program participant.

An examination of column 5 provides an important finding from our analysis. While there are many adult corrections programs that provide a favorable return to taxpayers, there are some programs for juvenile offenders that produce especially attractive long-run economic returns.

To continue the three examples already discussed, we find that the average cognitive-behavioral program costs about $107 per offender to administer. These programs are typically run in groups of 10 to 15 offenders and involve 40 to 60 hours of therapeutic time. We estimate that the 6.9% reduction in recidivism rates generates about $15,469 in life-cycle benefits (a present-valued sum) associated with the crime reduction. Thus, the net value of the average evidence-based cognitive-behavioral program for adult offenders is $15,361 per offender.

For the functional family therapy example, we find that the program costs, on average, $2,380 per juvenile participant. The costs are higher because it is a one-on-one program between a FFT therapist and the youth and his or her family. The 18.1% reduction in recidivism rates that we expect FFT to achieve generates about $52,156 in life-cycle benefits, measured in terms of the taxpayer and crime victim costs that are avoided because of the reduced long-run level of criminal activity of the youth. Thus, the net present value of this juvenile justice program is expected to be $49,776 per youth.

For the NFP program, we find that the crime reduction associated with the mothers produces $8,189 in benefits while the crime reduction linked to the children produces $12,567 in benefits. Together, the benefits total $20,756 per participant in NFP. We estimate the total cost of the NFP program to be $6,336 per family for crime-related outcomes. For our current study of crime outcomes, we prorate the NFP total program cost per participant by the ratio of crime benefits to total benefits estimated from our earlier study of prevention programs. In addition to crime outcomes, the NFP program has been shown to reduce child abuse and neglect and increase educational test scores (Aos et al., 2004).

As mentioned, we find that some programs show no evidence that they reduce crime outcomes. This does not mean, however, that these programs are not economically viable options. An example of this type of program is electronic monitoring for adult offenders. As indicated in Table 1, we located twelve studies of electronic monitoring and find that the average electronic monitoring program does not have a statistically significant effect on recidivism rates. As future evaluations are completed, this result may change; but currently we report no crime reduction benefits in columns 2 and 3. We do expect, however, that the average electronic monitoring program is typically used in Washington to offset the costs of more expensive resources to process the sanctions of the current offense. That is, we find that an average electronic monitoring program costs about $1,301 per offender. The alternative to electronic monitoring, however, is most often increased use of jail time, and we estimate this to cost $2,227 per offender. The cost shown on column 4 (-$926) is our estimate of the difference in these up-front costs. The bottom line is reported in column 5 and provides evidence that electronic monitoring can be a cost-beneficial resource. Thus, although there is no current evidence that electronic monitoring reduces recidivism rates, it can be a cost-effective resource when it is used to offset the costs of a more expensive criminal justice system resource such as jail time.

CONCLUSIONS

Public policy makers in Washington have learned a number of lessons from their experience with evidence-based initiatives. First, it is possible to undertake a careful review of evidence-based options and then use the results to inform public policy decisions in a "real-world" setting. This initiative began in Washington in the mid-1990s when legislation was enacted in the juvenile justice system with the goal of reducing crime through research-based programs. After a review of the national literature, the institute (in conjunction with the state's juvenile courts) identified particular research-based programs to be implemented in Washington, which were subsequently funded by the legislature. Evaluations of these programs indicated they reduce crime and are cost-beneficial to Washington when implemented with fidelity. In the years after this initial successful endeavor, the Washington legislature introduced the evidence-based approach to other policy areas including child welfare, mental health, substance abuse, K–12 education, and adult corrections. In 2006, the institute conducted a meta-analytic review of the criminal justice and prevention literature and found that evidence-based options exist to reduce the need for future prison construction while saving taxpayers' money and contributing to lower crime rates. Based upon these findings, significant investments were made in Washington by allocating $48 million in the biennial budget for expanded use of evidence-based programs.

Policy makers in Washington have also learned that estimating the economics of an evidence-based program is as important as determining whether or not a program works. A program may be found to reduce crime, but its costs may exceed benefits. Legislators routinely face budget constraints and, as a result, an economic analysis of options can improve decisions on how to allocate scarce resources. Thus, while determining whether a program reduces crime remains the necessary first condition for rational public policy making, an economic analysis constitutes the necessary additional condition for identifying viable and fiscally prudent options.

Our analysis of evidence-based and economically sound options for corrections indicates that there are ways to provide more cost-effective use of taxpayers' monies. Serious crime is costly to victims and taxpayers; our economic analysis for Washington indicates that evidence-based—and reasonably priced—programs that achieve even relatively small reductions in crime can produce attractive returns on investment.

NOTES

1. The Washington State Institute for Public Policy conducts nonpartisan research at the direction of the state's legislature, and findings are used to help inform public policy makers.

2. The institute maintains a criminal records database, which is a synthesis of criminal charge information, for all individuals in Washington.

3. Citations used in the meta-analysis are found at http://www.wsipp.wa.gov/Files/CrimeCitations

4. Additional information on the programs shown in Table 1 can be obtained from the institute.

5. We use a two-tailed test in order to determine statistical significance and we set the p value at less than or equal to .10 to designate statistical and policy-relevant significance.

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