



The Effectiveness of Declining Juvenile Court Jurisdiction of Youth

In Washington State, the juvenile courts are a division of the state's superior court system. The juvenile courts have jurisdiction over persons under the age of 18 who are alleged to have committed a crime. In certain circumstances, however, state law requires youth to be "declined jurisdiction" in the juvenile court and the case is then transferred into adult criminal court.

The Washington State Institute for Public Policy (WSIPP) was asked to evaluate the effectiveness of the law that declines youth from the juvenile court.^{1,2} This report contains our findings and is divided into four parts:

- 1) Background on juvenile decline laws,
- 2) Our outcome evaluation on the effectiveness of Washington State's juvenile decline law,
- 3) Review of the national research literature on the effectiveness of transferring juveniles to the adult court system, and
- 4) Our estimates of the benefits and costs associated with this policy.

An appendix is provided for supplemental information and technical detail.

¹ This project was initiated by the Washington State Partnership Council on Juvenile Justice and was approved by WSIPP's Board of Directors on September 17, 2012.

² The preparation of this report was aided by the Office of Juvenile Justice, Juvenile Justice & Rehabilitation Administration, and Department of Social & Health Services through a federal grant from the Office of Juvenile Justice & Delinquency Prevention of the U.S. Department of Justice authorized under the Juvenile Justice, Runaway Youth and Missing Children's Act Amendments of 1992 through a grant approved by the Washington State Partnership Council on Juvenile Justice (WA-PCJJ).

Summary

In Washington State, the juvenile courts have jurisdiction over youth under the age of 18 who are charged with committing a crime. Under certain circumstances, however, the juvenile courts are declined jurisdiction and youth are automatically sentenced as adults.

Since 1994, about 1,300 Washington youth have been processed in the adult system under the automatic decline law. For this report, we examined whether the automatic decline law results in higher or lower offender recidivism for those who were sentenced as adults.

To answer this question, we compared recidivism rates of youth who were automatically declined after the 1994 law with youth who would have been declined had the law existed prior to that time. We employed numerous tests, all of which demonstrate that recidivism is higher for youth who are automatically declined jurisdiction in the juvenile court. These findings are similar to other rigorous evaluations conducted nationally by other researchers.

When possible, WSIPP conducts benefit-cost analysis to understand the long-term financial impacts of programs and policies to society and others. Limitations in the juvenile justice literature, however, prohibit us from empirically investigating the potential benefits (or costs) of avoided crimes due to an increased length of stay in confinement for automatically declined youth.

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EXHIBIT F-3 - JUVJUSTTASKFORCE
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I. Background & Research Approach

In Washington State, adults charged with felony crimes have their cases heard in the superior court system. For adults found guilty of a crime, sentences are prescribed by the ranges in the state's sentencing guidelines.³ Depending on the seriousness of the crime and a person's criminal history, some sentences result in confinement in prison or community supervision.

The juvenile courts are a division of the superior court system. These courts have jurisdiction of youth under the age of 18 charged with criminal offenses. Like the adult system, the juvenile courts follow sentencing guidelines prescribed in statute that are also based on the seriousness of a crime and a youth's criminal history.⁴

Washington State law allows prosecutors to petition to transfer a youth to adult court, at the discretion of the juvenile court.⁵ This type of transfer is known as a *discretionary decline* of jurisdiction.

In addition to discretionary transfer, the 1994 Washington State Legislature passed the Youth Violence Reduction Act establishing an *automatic decline* of jurisdiction to the adult court for certain youth. Youth ages 16 and 17 are automatically "declined" to the adult court when charged with the following violent felonies:⁶

- Serious violent felony (murder 1 and 2, manslaughter 1, assault 1, kidnapping 1, and rape 1)
- Violent felony (with a criminal history of one or more serious violent felonies)
- Violent felony (with a criminal history of two or more violent felonies)
- Violent felony (with a criminal history of three or more class A felonies, class B felonies, vehicular assault, or manslaughter 2 committed after the 13th birthday and prosecuted separately)

The 1997 Legislature revised the automatic transfer criteria and added the following offenses:

- Robbery 1, rape of a child 1, or drive-by shooting
- Burglary 1 (with a criminal history of any prior felony or misdemeanor)
- Violent felony with a deadly weapon

Section II of this report presents our evaluation of the effect of the state's automatic decline law on crime.

³ RCW 9.94A, Sentencing Reform Act of 1981.

⁴ RCW 13.40.0357.

⁵ RCW 13.40.110.

⁶ RCW 13.04.030. In 1999, the Washington State Supreme Court determined that the adult court cannot retain jurisdiction over a juvenile if the charges against the youth are amended so the case no longer meets the automatic transfer criteria (*State v. Mora*, 138 Wn.2d, June 3, 1999).

Confinement of Declined Youth

The Department of Corrections (DOC) has legal authority over declined youth. DOC policy designates a youthful offender as any person under the age of 18 who is convicted and sentenced as an adult.⁷

Federal laws ensure certain protections of youth in the adult criminal justice system.⁸ Youthful offenders under the jurisdiction of DOC are housed separately from adult offenders as required by Washington State law.⁹

Declined youth are managed under the Youthful Offender Program (YOP), which is a coordinated effort between staff at DOC and the Juvenile Rehabilitation Administration (JRA). Under current practice, declined youth less than age 18 are housed at JRA.¹⁰ If the youth is expected to be released from confinement prior to age 21, the youth remains at JRA. If the youth is expected to be released after the age of 21, the case is reviewed at the age of 18 to determine if the youth is able to complete his/her sentence at DOC.¹¹

⁷ Department of Corrections Policy 320.500.

⁸ Such laws include the Juvenile Justice Delinquency and Prevention Act and the Prison Rape Elimination Act.

⁹ RCW 70.01.410

¹⁰ Prior to July 2004, the Youthful Offender Program for male offenders was physically located at DOC's Clallam Bay Corrections Center. Prior to August 2000, females were housed at DOC's Washington Corrections Center for Women. Communication with Arlene Scott-Young at DOC and Jennifer Redman at JRA.

¹¹ Communication with Jennifer Redman at JRA.

II. Outcome Evaluation

When the 1997 Legislature modified juvenile sentencing laws, it directed WSIPP to evaluate the impact of the changes in jurisdiction of juvenile offenders.¹² In 2003, WSIPP published findings on the effectiveness of the juvenile decline of jurisdiction laws.¹³ These findings were inconclusive, however, since the law had not been implemented long enough to sufficiently examine its impact on recidivism.

The current evaluation was initiated by the Washington State Partnership Council on Juvenile Justice (Partnership Council), which asked WSIPP to undertake the study. The Partnership Council serves in an advisory role to the Governor by commenting on the state's juvenile justice and prevention needs.¹⁴

The WSIPP Board of Directors approved this project in 2012. The primary research tasks were to:

- ✓ Conduct an outcome evaluation of the effectiveness of Washington State's juvenile decline law,
- ✓ Review the national research literature on the effectiveness of juvenile decline laws, and
- ✓ Estimate the benefits and costs associated with this policy.

Research Design

The best way to determine the effectiveness of a policy is to compare the outcomes of those who are subject to the policy with a similar group of people who would have been eligible, but the policy did not apply. In an ideal research setting, youth would be randomly assigned to either a treatment or a comparison group and any observed differences in recidivism rates could be attributed to the law.

For the current study, however, since the decline of jurisdiction law was implemented statewide, it was not possible to randomly assign youth to different groups. Instead of random assignment we use statistical controls to compare the recidivism rates of youthful offenders before the 1994 law to rates after the law.

The implementation of the 1994 law requiring automatic decline of juvenile court jurisdiction created a unique situation allowing us to select youth who would have met the exact age and offense criteria prior to the law. Using this research design, we can observe whether youth who were automatically declined had different recidivism rates than youth who would have been eligible, but were not automatically declined, prior to the implementation of the law.

Because this condition does not exist for youth who were declined jurisdiction due to prosecutorial discretion, we were unable to construct a valid comparison group of those youth to test the effects of this law. Therefore, our outcome evaluation focuses on the effect of automatic decline of jurisdiction on recidivism, not on discretionary decline of jurisdiction.

¹² RCW 13.40.0357.

¹³ R. Barnoski (2003). *Changes in Washington State's jurisdiction of juvenile offenders: Examining the impact*. (Doc. No. 03-01-1203). Olympia: Washington State Institute for Public Policy.

¹⁴ Executive Order 10-03. Establishing the Washington State Partnership Council on Juvenile Justice. September 13, 2010. Retrieved from: http://www.digitalarchives.wa.gov/GovernorGregoire/execorders/eo_10-03.pdf

Study Groups

Using WSIPP's criminal history database and the eligibility criteria described in the sidebar on this page, we created study groups to examine the effectiveness of the automatic decline law.¹⁵

We identified the *automatic decline group* by locating youth who had cases filed in adult superior court after July 1, 1994 and met the eligibility criteria for automatic decline.¹⁶

We then constructed a comparison group of youth who would have been automatically declined had the law existed prior to its implementation in 1994.¹⁷ We selected youth whose cases were filed in juvenile court between January 1, 1992 through June 30, 1994.

To examine recidivism, youth in the pre-group and decline group had to be at-risk for recidivism in the community for 36 months. Thus, we included youth who became at-risk in the community through 2009.

Exhibit 1

Differences Between the Study Groups

| Variable | Pre-group | Automatic decline group | p-value |
|-----------------------------|-----------|-------------------------|---------|
| Criminal history score | 13.3 | 10.1 | 0.000 |
| Prior juvenile felony adjs. | 1.7 | 2.0 | 0.048 |
| Age at index offense | 16.4 | 16.6 | 0.000 |
| Age at-risk | 17.9 | 20.1 | 0.000 |
| Black | 24% | 28% | 0.115 |
| White | 69% | 65% | 0.199 |
| Male | 95% | 94% | 0.493 |
| Number | 446 | 770 | |

This selection process resulted in a total of 446 youth in the *pre-group* who would have been eligible for automatic decline had it existed prior to 1994 and 770 youth in the *automatic decline group*.

We compared the decline group with the pre-group to estimate the differences between the groups on key characteristics. [Exhibit 1](#) shows that there are some differences between the groups. Most importantly, youth in the automatic decline group have lower criminal history scores, meaning that these youth have a lower likelihood of recidivating. We addressed the differences using statistical analyses to control for these factors.

¹⁵ WSIPP's criminal history database was developed to conduct criminal justice research at the request of the legislature. The database is a synthesis of data from the Administrative Office of the Courts and the Department of Corrections. We conducted a matching process using the court case number and the primary identification number from the data systems to link criminal history records. The criminal history database is intended for research purposes only.

¹⁶ If a case was filed in adult superior court, but did not meet the eligibility criteria for automatic waiver, we assumed the case was a discretionary decline and was therefore, not included in our study.

¹⁷ More detail on our technical methods and data processing are contained in the technical appendix.

Recidivism Findings

Recidivism is defined as any offense committed after release to the community that results in a Washington State conviction.¹⁸ Three types of recidivism were analyzed for this study:

- Violent felony convictions;
- Felony convictions, including violent felonies; and
- Total recidivism, including misdemeanors, felonies, and violent felony convictions.

Typically, we measure juvenile recidivism within 18-months of becoming at-risk in the community. However, since these youth were processed as adults, we used a 36-month recidivism follow-up that we normally use for adult offenders.

We used logistic regression analyses to adjust for observed differences between the study groups. Controlling for these differences enables us to calculate adjusted recidivism rates within three years of becoming at-risk in the community.

After controlling for observed differences between the two groups, we find that the automatic decline group had higher recidivism rates than the comparison group for all three measures of recidivism as shown in [Exhibit 2](#).¹⁹ None of these differences were statistically significant ($p \leq 0.05$).

One concern with an outcome evaluation using this type of design (with the pre-group selected before the post-group) is that changes in crime rates or recidivism trends over time might influence the outcome and bias the treatment effect. To test for this potential bias, we examined recidivism trends over time. Our analyses demonstrate that juvenile recidivism rates have been decreasing slightly over the 1992 to 2009 timeframe (see appendix). Thus, the direction of any potential bias would result in lower recidivism rates for the decline group.

To account for this bias, we tested many logistic regression model specifications controlling for time trends (see appendix for details). Even when controlling for time trends, however, we found that youth who were automatically declined had higher recidivism rates (not statistically significant). That is, we found that these time trends did not bias our estimate of the impact of automatic declines.

¹⁸ R. Barnoski. (1997). *Standards for improving research effectiveness in adult and juvenile justice*. (Doc. No. 97-12-1201). Olympia: Washington State Institute for Public Policy, pg. E2.

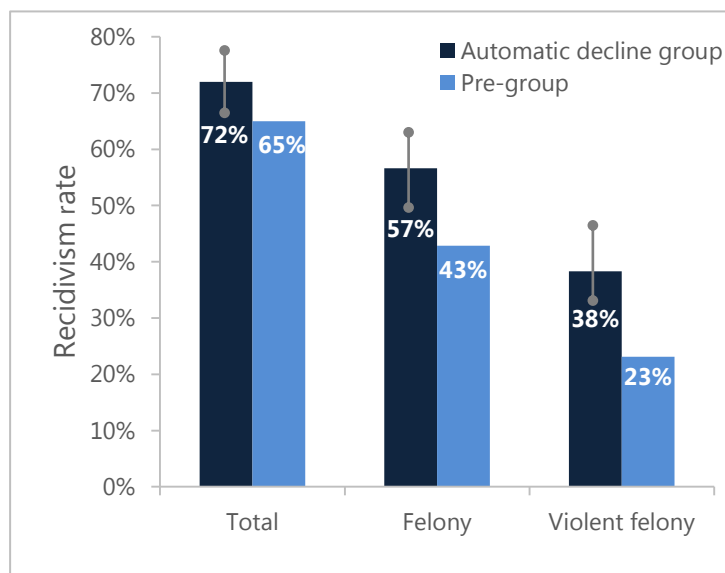
¹⁹ We calculated the automatic decline group recidivism rate using the treatment coefficient from our preferred regression models as shown in the appendix. We use the total recidivism rate for the comparison group, 65%, and the coefficient, 0.354 in the following formula: $(0.65/(1-.65)) * \exp(0.354) / (1 + (0.65/(1-.65)) * \exp(0.354)) = 72\%$. Error bars in Exhibit 2 demonstrate the variation in these adjusted point estimates. These results were not statistically significant in our logistic regression models as shown in the appendix.

One theoretical reason why recidivism rates may have been higher for youth who were automatically declined is that processing youth in the adult system has a criminogenic effect—the tendency to increase crime.²⁰ From this study, however, we are unable to distinguish why declined youth had higher recidivism rates. It is unknown whether processing youth through the adult court or housing youth in the adult system²¹ or some other unknown factor contributed to this effect.

A second reason could be related to increased time in confinement. Youth in the decline group spent an average of 32 months in confinement versus youth in the pre-group who spent 12 months in confinement. We were able to test this theory and found no statistically significant relationship between the increased length of stay and recidivism.²²

Exhibit 2

36-Month Adjusted Recidivism Rates by Type of Recidivism



²⁰ Aizer, A. & Doyle, J. (2013). *Juvenile incarceration, human capital, and future crime: Evidence from randomly-assigned judges*. National Bureau of Economic Research.

²¹ Not all declined youth serve their sentence at DOC (see Section I). Transfer to DOC is dependent on the youth's age as well as other factors such as vulnerability. Unfortunately, due to multicollinearity between location of confinement, time, and the treatment variable (decline), the data do not allow us to test whether the place of confinement has an effect on recidivism.

²² This finding, however, is contrary to our recent meta-analysis of adult research where we found that increasing length of stay of adults in prison leads to a small decrease in recidivism. (see Aos, S. & Drake, E. (2013) *Prison, police, and programs: Evidence-based options that reduce crime and save money*. (Doc. No. 13-10-1901). Olympia: Washington State Institute for Public Policy.)

III. Systematic Review of the Literature

In addition to our analysis of Washington's juvenile decline laws, we were asked to review the national research literature on the effectiveness of transferring juveniles to the adult court system. The sidebar on this page highlights WSIPP's general approach to systematic reviews of the literature and technical detail is provided in the appendix of this report.²³

We systematically reviewed the literature and located all studies that evaluated the impact of juvenile decline laws on crime (or recidivism).

We assessed whether each study met WSIPP's minimum standards of research rigor. For example, to be included in our review, a study must have demonstrated that there was no, or minimal, selection bias, particularly in the comparison group.

Three studies were rigorous enough to be included in the meta-analysis (including the effect from the outcome evaluation in Section II of this report). For each of these studies, we calculated an individual "effect size." An effect size is a metric that measures the degree to which a program has been shown to change an outcome (such as recidivism) for program participants relative to a comparison group.

WSIPP's Approach to Systematic Reviews

At the direction of the Washington State legislature, WSIPP conducts systematic reviews of evaluation research to determine what public policies and programs work and which ones do not work. These evidence-based reviews cover adult and juvenile corrections, child welfare, mental health, substance abuse, prevention, and education.

When WSIPP is asked to conduct a systematic review, we follow a number of steps to ensure a rigorous finding. These criteria include:

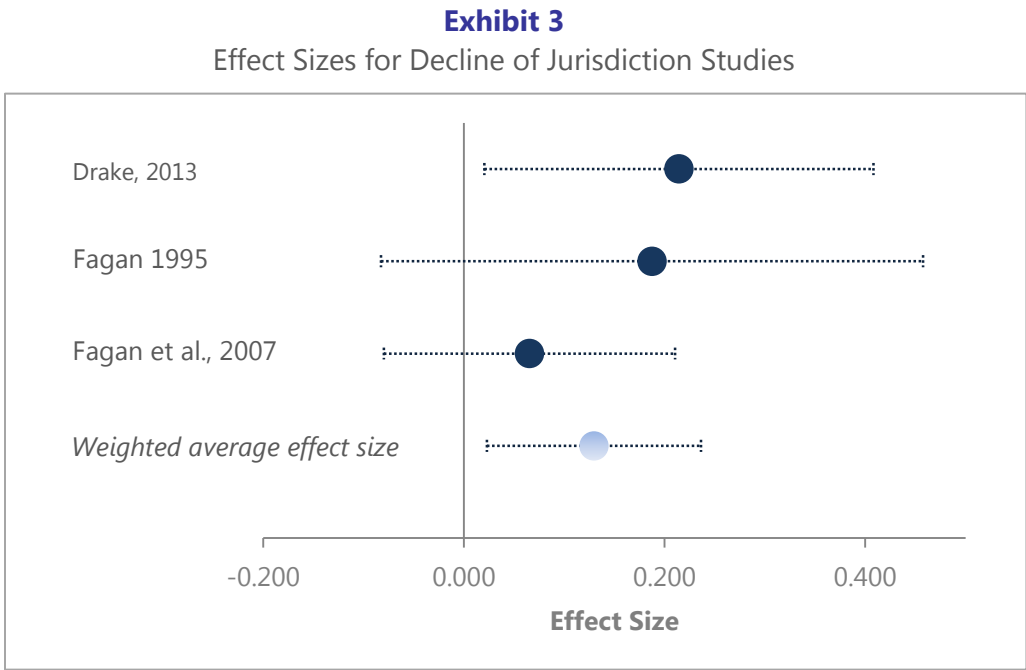
- 1) Considering *all* available studies we can locate on a topic rather than selecting only a few; that is, we do not "cherry pick" studies to include in our reviews.
- 2) Requiring that an evaluation's research design include treatment and comparison groups from intent-to-treat samples. Random assignment studies are preferred, but we include quasi-experimental studies when the study uses appropriate statistical techniques to control for selection bias.

We then use a formal statistical procedure, called meta-analysis, to calculate an average "effect size," which indicates the expected magnitude of the relationship between the treatment and the outcome of interest.

²³ For additional detail, see technical manual www.wsipp.wa.gov/rptfiles/BCTechnicalManual.pdf

All three studies in the meta-analysis found that declining youth to adult court is associated with an increase in recidivism. The weighted average effect size is 0.130 (SE = 0.054). This effect is statistically significant at $p = 0.017$). The effect sizes are reported in [Exhibit 3](#).

We also found several evaluations examining the impact of juvenile decline laws on general deterrence.²⁴ Unfortunately, however, none of these studies met our minimum standards of rigor to be included in our meta-analysis.



²⁴ General deterrence refers to the effect that punishment has on the general population. For example, an individual may make the choice to remain crime-free because the threat of punishment prevents him or her from committing a crime.

IV. Benefit-Cost Analysis

When possible, we use WSIPP's benefit-cost model to generate return-on-investment statistics for all evaluations for the legislature. The results provide a consistent comparison with the benefit-cost results of other programs and policies.²⁵

In benefit-cost analyses of criminal justice programs, the valuation of benefits in monetary terms often takes the form of savings when crime is avoided. Crime can produce many costs, including those associated with the criminal justice system as well as those incurred by crime victims. When crime is avoided, these reductions lead to monetary savings or benefits. WSIPP's benefit-cost analysis estimates the number and types of crimes avoided due to the effects of a policy and determines the monetary value associated with reduced or incurred crimes.

The result of our study of the effect of automatic decline laws provides an estimate of how declining juvenile court jurisdiction of youth affects recidivism compared with similar youth sentenced prior to the law. We found an increase in recidivism for those who were automatically declined (though not statistically significant at conventional levels). In addition to this specific deterrent effect, however, the juvenile decline law can also affect crime rates in Washington by what criminologists call "incapacitation" which accounts for crimes averted during a period of confinement.

Empirical research indicates that statewide crime rates are affected by statewide incarceration rates.²⁶ For example, if everyone in Washington were

incarcerated then the crime rate would drop to zero. On the other extreme, if no one were incarcerated, then the crime rate would be higher than it is today.

The result of the automatic decline law has been to increase the statewide incarceration rate by imposing, on average, a longer length of stay in confinement. From our evaluation in Section II, we found that youth in the decline group were confined an average of 33 months compared with youth in the pre-group who were confined an average of 13 months. Thus, the law could affect crime in Washington through an incapacitation effect. This difference between the groups translates to an increase of 1.66 in prison average daily population.

The empirical task is to calculate this incapacitation effect by estimating how changes in the incarceration rate affect the crime rate. Recently, WSIPP produced an incapacitation model to estimate the number of crimes avoided or incurred when incarceration rates change.²⁷

The body of research that drives this model was derived, however, from the effects of incarcerating adult offenders. At this time, unfortunately, no body of research estimates the effect of incapacitation of juvenile offenders on crime.²⁸ Therefore, we are unable to reliably estimate how many crimes society avoids when incarcerating youth.

Given our findings from the adult incapacitation literature, we can presume that the number of crimes avoided through incapacitating youth will be

²⁵ Lee, S., Aos, S., Drake, E., Pennucci, A., Klima, T., Miller, M., Anderson, L., Mayfield, J., & Burley, M. (2012). *Return on investment: Evidence-based options to improve statewide outcomes* (Doc. No. 12-04-1201). Olympia: Washington State Institute for Public Policy.

²⁶ Nagin, D. (2013). *Deterrence in the twenty-first century: A review of the evidence. Crime and Justice: A review of research*. Chicago, IL: University of Chicago Press. Marvell, T. B. (2010). *Prison population and crime*. Handbook on the economics of crime, B. L. Benson & P. R. Zimmerman (Eds.). Cheltenham, UK: Edward Elgar Publishing.

²⁷ Aos, S. & Drake, E. (2013). See also: Aos, S. & Drake, E. (2010). *WSIPP's benefit-cost tool for states: Examining policy options in sentencing and corrections*. (Doc. No. 10-08-1201). Olympia: Washington State Institute for Public Policy.

²⁸ Aizer, A. & Doyle, J. (2013). *Juvenile incarceration, human capital, and future crime: Evidence from randomly-assigned judges*. National Bureau of Economic Research.

greater than zero. That is, assumptions from the adult incapacitation literature would lead us to believe that some crime is avoided. We are unable, however, to empirically estimate how many crimes are avoided per juvenile offender incarcerated.²⁹ We provide benefit-cost findings for the recidivism effect in Section II.

Benefit-Cost Findings

Typically, the sum of the estimated benefits, along with the costs, provides a statewide view on whether a program produces benefits that exceed costs. Since we are unable to provide a complete picture of the benefits and costs of the decline of juvenile court jurisdiction law, however, our benefit-cost analysis is incomplete.

The benefit-cost estimates of this policy, thus far, are displayed in [Exhibit 4](#). As demonstrated in Exhibit 4, we cannot estimate the empirical benefits to taxpayers and crime victims of the incapacitation effect.

The cost of the policy is \$72,585 per youth automatically declined from the juvenile court.³⁰ In addition to the cost of incarceration, we also found an increase in recidivism—a cost to taxpayers and crime victims. Results from our benefit-cost model indicate that the increase in recidivism costs is \$2,168 to taxpayers and \$8,071 to crime victims per offender—a total of \$10,239 in costs per offender.

²⁹ We tested this assumption by examining briefly the incarceration-crime relationship of juvenile offenders in Washington State using county panel data as modeled in Aos & Drake (2013). We used county-level UCR crime data from 1982 to 2011 for Washington's 39 counties as the dependent variable and Washington's annual total statewide juvenile incarceration rate for juvenile offenders. Results indicate a negative elasticity. Our model is not rigorous enough to account for simultaneity which biases a coefficient downward (meaning the incapacitation effect would be larger), thus we cannot estimate how many crimes are avoided due to incapacitation, but we can conclude that incapacitation is likely to reduce crime.

³⁰ To estimate the cost per youth declined, we multiplied the cost per youth per year in JRA facilities, \$37,000, plus \$6,726 per offender to operate the Youthful Offender Program (includes DOC and JRA staff) multiplied by 1.66 years (20 months increased length of stay for declined offenders).

Exhibit 4

Monetary Benefits and Costs of Declining Juvenile Court Jurisdiction per Automatically Declined Offender

| Benefits [#] to taxpayers and crime victims | | |
|--|---|--|
| 1 | Incapacitation | An evidence-based finding cannot be estimated at this time for juvenile offenders. |
| 2 | Decreased crime victim cost due to increased incapacitation | |
| 3 | Decreased taxpayer cost due to increased incapacitation | |
| Costs to taxpayers and crime victims | | |
| 4 | Additional cost to confine declined youth | \$72,585 |
| 5 | Recidivism effect: | |
| 6 | Increased crime victim cost due to increased recidivism | \$8,071 |
| 7 | Increased taxpayer cost due to increased recidivism | \$2,168 |
| Bottom Line | | |
| 9 | Total net benefits per participant | An evidence-based finding cannot be estimated at this time for juvenile offenders. |
| 10 | Benefit-to-cost ratio | |
| 11 | Monte Carlo risk analysis ^{##} | |

[#]Benefits and costs are life-cycle present-values per participant, in 2012 dollars.

^{##}We assess the risk in our bottom-line estimates by running our benefit-cost model 5,000 times to determine the odds that the policy will at least break even.

Findings and Limitations

Several limitations to our findings must be considered.

First, we were able to detect that there is an increase in recidivism when juveniles are automatically declined from the juvenile court. While the results from our own evaluation of Washington offenders were not statistically significant, the meta-analysis of all available literature is statistically significant ($p = 0.017$). We were not able to understand *why* we found this effect. We were able to test whether this effect was associated with an increased length of stay and that factor does not appear to play a role. The question still remains as to why the effect was an increase in recidivism.

Second, evaluations that measure recidivism are “retrospective” by design, which means that we did not evaluate the effectiveness of declining juvenile court jurisdiction as it operates today. During the time period of our study, the majority of the youth included in our outcome evaluation were physically transferred and housed at DOC. Today, however, declined youth are housed at JRA facilities. Although state laws have since imposed separate housing restrictions between adult and juvenile offenders, it is unknown how current practice may or may not impact recidivism differently.

Third, although we were able to monetize the costs and benefits of the recidivism effect of declined youth, two important components of this policy have gone unmeasured—incapacitation and general deterrence. At this time, we are unable to quantify the impact of incapacitating youth for an average of 20 additional months in confinement. We are also unable to empirically estimate the general deterrent effects of the decline of juvenile court jurisdiction law on youth at large.



Technical Appendix

The Effectiveness of Declining Juvenile Court Jurisdiction of Youth

Appendix

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I. Outcome Evaluation of Washington State’s Juvenile Decline of Jurisdiction Laws

The Washington State Institute for Public Policy (WSIPP) has a criminal history database that is a synthesis of criminal charge information for individuals. The database was developed using data from the Administrative Office of the Courts (AOC) and the Department of Corrections (DOC) with the intent to conduct legislatively mandated research in a timely fashion. This comprehensive database can be used to determine an offender’s criminal history or to calculate recidivism. We used this database to select the study groups and to calculate criminal history and analyze recidivism for this report.

While every effort is made to accurately identify persons across data sources, 100% accuracy is not possible. For example, multiple birth dates and aliases force us to make processing decisions about the data. Thus, the data should not be used for auditing purposes; however the database does provide a reasonably accurate source of criminal charge data for reporting and analysis at the aggregate level.

A. Data Processing

Study Groups. We included youth who were automatically declined to the adult court according to RCW 13.04.030 and youth who would have met those criteria had the law existed in the pre-period. The pre-group is defined by youth whose offense dates were between July 1, 1991 and June 30, 1994. Juveniles in the pre-group were ages 16 or 17 at the time of the first offense date who met the offense criteria:

- Serious violent felony (murder 1 and 2, manslaughter 1, assault 1, kidnapping 1, and rape 1)
- Violent felony (with a criminal history of one or more serious violent felonies)
- Violent felony (with a criminal history of two or more violent felonies)
- Violent felony (with a criminal history of three or more class A felonies, class B felonies, vehicular assault, or manslaughter 2 committed after the 13th birthday and prosecuted separately)

In the decline group, we identified adult cases filed in superior court that met the offense criteria and ages 16 or 17 at the time of the first offense date.³¹ The decline group is defined by youth whose offense dates were after July 1, 1994.

³¹ To be eligible for a decline of jurisdiction hearing, RCW 13.04.030 states that the youth must be age 16 or 17 at the time of the offense, while RCW 13.40.110 states that the youth must be age 16 or 17 at the time the information is filed. We followed the eligibility criteria in RCW 13.04.030 (age at the time of the offense). There were 63 youth in our decline study group age 18 or older at the file date. We tested the sensitivity of our results (Section B of this appendix) by excluding these 63 youth and it did not impact our overall findings.

If a youth had multiple dates of birth, we used the first date of birth (making the person older). If the offense date was missing, the first file date was used.

Recidivism. Recidivism is defined as any offense committed after release to the community that resulted in a Washington State court conviction.³² The follow-up period is 36-months from the time the offender was “at-risk” in the community—the date an offender was in the community with the potential to re-offend. Typically, we use 18 months follow-up for juvenile offenders, however, since the intention of the policy is to process youth through the adult system, we used a 36-month follow-up period, which is our standard for adult offenders. We have found that this timeframe allows the researcher to capture approximately 80% of re-offense behavior when compared with a longer follow-up.³³ Thus, we have determined that 36-months is a sufficient follow-up period. We limited the study groups to youth who became at-risk in the community through 2009 in order to calculate 36-month recidivism rates.

In addition to the follow-up period, time is needed to allow an offense to be processed in the criminal justice system. The criminal justice process includes the time period between the date recorded for the commission of a subsequent offense and the resulting conviction of that offense. In our previous work, we have found that a 12-month adjudication period is adequate for adult offenders.

When data from DOC or Juvenile Rehabilitation Administration (JRA) indicated the youth was incarcerated, the release date from the respective institution was used as the youth’s at-risk date. We do not have access to detention data for juveniles held at local jurisdictions; therefore, if a youth was not confined in DOC or the JRA, we used the adjudication date as the at-risk date.

B. Sensitivity Testing of Recidivism Findings

We ran alternative model specifications to test the sensitivity of our recidivism findings displayed in [Exhibit A1](#). We used logistic regression analysis with felony recidivism as the dependent variable. The “treatment” variable, *Auto-Declined*, was coded as a 1 for youth who were automatically declined in the post-period (after implementation of the automatic decline law) and 0 for youth who were eligible, but were not declined in the pre-period (prior to the implementation of the automatic decline law).

Results from model (1) show a negative coefficient—a reduction in recidivism—before we added covariates to the model. In model (2), covariates controlling for important characteristics that impact recidivism were added to the model and the treatment coefficient became positive. In model (3), our preferred model, fixed effects were added to capture any unobserved trends over time and the treatment variable remains positive.

³² R. Barnoski (1997). *Standards for improving research effectiveness in adult and juvenile justice*. (Doc. No. 97-12-1201). Olympia: Washington State Institute for Public Policy, pg. 2.

³³ Ibid.

Exhibit A1

Sensitivity of Model Specifications

| Dependent variable = Felony recidivism | | | | | | | | | |
|---|--------------------|----------------|---------|--------------------|----------------|---------|--------------------|----------------|---------|
| | (1) | | | (2) | | | (3) | | |
| | Parameter estimate | Standard error | P-value | Parameter estimate | Standard error | P-value | Parameter estimate | Standard error | P-value |
| Intercept | -0.289 | 0.096 | 0.003 | 0.646 | 0.609 | 0.289 | 0.744 | 0.968 | 0.443 |
| Auto-Declined (1=Y, 0=N) | -0.078 | 0.121 | 0.572 | 0.237 | 0.152 | 0.119 | 0.555 | 0.420 | 0.186 |
| Criminal history | -- | -- | -- | 0.048 | 0.015 | 0.002 | 0.045 | 0.016 | 0.005 |
| Age at risk | -- | -- | -- | -0.146 | 0.029 | 0.000 | -0.177 | 0.034 | 0.000 |
| Black | -- | -- | -- | 0.497 | 0.142 | 0.000 | 0.479 | 0.146 | 0.001 |
| Other race | -- | -- | -- | 0.429 | 0.532 | 0.420 | 0.390 | 0.533 | 0.465 |
| Male | -- | -- | -- | 0.787 | 0.306 | 0.010 | 0.795 | 0.311 | 0.011 |
| Prior juvenile felony adjs. | -- | -- | -- | 0.144 | 0.048 | 0.003 | 0.183 | 0.051 | 0.000 |
| Year 1 | -- | -- | -- | -- | -- | -- | -0.434 | 0.821 | 0.597 |
| Year 2 | -- | -- | -- | -- | -- | -- | 0.495 | 0.596 | 0.406 |
| Year 3 | -- | -- | -- | -- | -- | -- | 0.517 | 0.570 | 0.365 |
| Year 4 | -- | -- | -- | -- | -- | -- | 0.540 | 0.560 | 0.335 |
| Year 5 | -- | -- | -- | -- | -- | -- | 0.767 | 0.553 | 0.165 |
| Year 6 | -- | -- | -- | -- | -- | -- | -0.770 | 0.515 | 0.135 |
| Year 7 | -- | -- | -- | -- | -- | -- | -0.076 | 0.478 | 0.874 |
| Year 8 | -- | -- | -- | -- | -- | -- | -0.408 | 0.460 | 0.375 |
| Year 9 | -- | -- | -- | -- | -- | -- | -0.117 | 0.407 | 0.773 |
| Year 10 | -- | -- | -- | -- | -- | -- | -0.145 | 0.386 | 0.707 |
| Year 11 | -- | -- | -- | -- | -- | -- | 0.094 | 0.370 | 0.799 |
| Year 12 | -- | -- | -- | -- | -- | -- | 0.137 | 0.399 | 0.731 |
| Year 13 | -- | -- | -- | -- | -- | -- | 0.224 | 0.403 | 0.578 |
| Year 14 | -- | -- | -- | -- | -- | -- | 0.194 | 0.375 | 0.606 |
| Year 15 | -- | -- | -- | -- | -- | -- | 0.808 | 0.389 | 0.038 |
| Year 16 | -- | -- | -- | -- | -- | -- | 1.056 | 0.421 | 0.012 |
| Year 17 | -- | -- | -- | -- | -- | -- | 0.139 | 0.422 | 0.741 |
| Number of observations | 1,216 | | | 1,195 | | | 1,195 | | |
| Model fit: | | | | | | | | | |
| Schwartz criterion | 1,667 | | | 1,541 | | | 1,626 | | |
| AUC | 0.508 | | | 0.694 | | | 0.714 | | |
| Pseudo R-squared | 0.0003 | | | 0.1145 | | | 0.1410 | | |

Our preferred model for all three measures of recidivism (felony recidivism, violent felony recidivism, and total recidivism including felony or misdemeanor) is displayed in [Exhibit A2](#). Results from these models indicate a positive relationship between automatic declines and recidivism across all measures. None of the models are statistically significant at conventional levels, but automatic declines are found to be marginally significant for violent felony recidivism ($p = 0.102$).

Exhibit A2

Preferred Model Estimates by Type of Recidivism

| Recidivism Dependent Variable: | Felony (1) | | | Violent (2) | | | Total (3) | | |
|--------------------------------|--------------------|----------------|---------|--------------------|----------------|---------|--------------------|----------------|---------|
| | Parameter estimate | Standard error | P-value | Parameter estimate | Standard error | P-value | Parameter estimate | Standard error | P-value |
| Intercept | 0.744 | 0.968 | 0.443 | -2.011 | 1.150 | 0.080 | 2.019 | 0.970 | 0.037 |
| Auto-Declined (1=Y, 0=N) | 0.555 | 0.420 | 0.186 | 0.727 | 0.444 | 0.102 | 0.354 | 0.429 | 0.409 |
| Criminal history | 0.045 | 0.016 | 0.005 | 0.049 | 0.017 | 0.004 | 0.098 | 0.017 | 0.000 |
| Age at Risk | -0.177 | 0.034 | 0.000 | -0.118 | 0.039 | 0.003 | -0.164 | 0.033 | 0.000 |
| Black | 0.479 | 0.146 | 0.001 | 0.754 | 0.158 | 0.000 | 0.439 | 0.156 | 0.005 |
| Other race | 0.390 | 0.533 | 0.465 | 0.219 | 0.617 | 0.723 | 0.646 | 0.594 | 0.276 |
| Male | 0.795 | 0.311 | 0.011 | 1.381 | 0.486 | 0.005 | 0.641 | 0.278 | 0.021 |
| Prior juvenile felony adjs. | 0.183 | 0.051 | 0.000 | 0.069 | 0.047 | 0.141 | 0.057 | 0.056 | 0.308 |
| Year 1 | -0.434 | 0.821 | 0.597 | -0.807 | 1.210 | 0.505 | -0.539 | 0.806 | 0.504 |
| Year 2 | 0.495 | 0.596 | 0.406 | 0.666 | 0.663 | 0.315 | -0.578 | 0.617 | 0.348 |
| Year 3 | 0.517 | 0.570 | 0.365 | 0.582 | 0.637 | 0.361 | -0.225 | 0.592 | 0.703 |
| Year 4 | 0.540 | 0.560 | 0.335 | 0.802 | 0.619 | 0.195 | -0.357 | 0.581 | 0.538 |
| Year 5 | 0.767 | 0.553 | 0.165 | 0.912 | 0.597 | 0.127 | -0.380 | 0.573 | 0.507 |
| Year 6 | -0.770 | 0.515 | 0.135 | -0.142 | 0.568 | 0.803 | -0.787 | 0.553 | 0.154 |
| Year 7 | -0.076 | 0.478 | 0.874 | 0.177 | 0.535 | 0.741 | -0.871 | 0.498 | 0.080 |
| Year 8 | -0.408 | 0.460 | 0.375 | -0.644 | 0.581 | 0.268 | -0.708 | 0.456 | 0.121 |
| Year 9 | -0.117 | 0.407 | 0.773 | -0.085 | 0.475 | 0.859 | -0.656 | 0.408 | 0.108 |
| Year 10 | -0.145 | 0.386 | 0.707 | -0.284 | 0.469 | 0.545 | -0.805 | 0.383 | 0.036 |
| Year 11 | 0.094 | 0.370 | 0.799 | 0.057 | 0.438 | 0.896 | -0.306 | 0.374 | 0.414 |
| Year 12 | 0.137 | 0.399 | 0.731 | -0.374 | 0.508 | 0.462 | -0.773 | 0.397 | 0.052 |
| Year 13 | 0.224 | 0.403 | 0.578 | 0.328 | 0.471 | 0.486 | -0.221 | 0.392 | 0.572 |
| Year 14 | 0.194 | 0.375 | 0.606 | 0.282 | 0.437 | 0.519 | -0.674 | 0.370 | 0.069 |
| Year 15 | 0.808 | 0.389 | 0.038 | 0.554 | 0.447 | 0.216 | 0.009 | 0.392 | 0.982 |
| Year 16 | 1.056 | 0.421 | 0.012 | 0.556 | 0.485 | 0.252 | 0.260 | 0.430 | 0.546 |
| Year 17 | 0.139 | 0.422 | 0.741 | -0.094 | 0.512 | 0.855 | -0.283 | 0.412 | 0.492 |
| Number of observations | 1,195 | | | 1,195 | | | 1,195 | | |
| Model fit: | | | | | | | | | |
| Schwartz criterion | 1,626 | | | 1,346 | | | 1,574 | | |
| AUC | 0.714 | | | 0.697 | | | 0.722 | | |
| Pseudo R-squared | 0.141 | | | 0.090 | | | 0.142 | | |

Time trends. As demonstrated in Section III later in this appendix, it was necessary to control for long-term trends because recidivism rates and juvenile arrest rates, in general, have declined slightly over time. This finding is consistent with adult recidivism rates in Washington State.³⁴ Due to the nature of our research design (pre- and post-law study groups), not controlling for these time trends could potentially bias a treatment effect in favor of reducing recidivism. That is, without accounting for observed and unobserved time trends, if the treatment coefficient were negative, that estimate could simply be capturing already declining crime trends.

We captured time trends using year fixed effects based on the youth's at-risk date for recidivism. Due to the nature of the research design, the treatment variable was defined by time (pre- and post-law changes); therefore inherently creating multicollinearity among the treatment variable and the year fixed effects. We used the variance inflation factor from an ordinary least squares regression to test for multicollinearity (felony recidivism as the dependent variable and the covariates from our preferred model). The estimates were below 2 for all year dummies and below 8 for the treatment variable.

We also conducted a likelihood ratio test to compare the fit of models (2) and (3) from Exhibit A1 (the nested and full models, respectively) and determine whether the difference between the two models is statistically significantly different. Findings show that model (3), with fixed year effects, is a statistically significantly better fit than the nested model ($p = 0.01$). Although multicollinearity will bias the standard errors, it will not affect the precision of the coefficients and it is important to capture trends over time due to the nature of the research design.

As a final test of time trends, we restricted the sample to youth only released through 2004 (as compared with the full sample who were released through 2009) to test if there was a treatment effect for a more contemporaneous sample ($n = 876$). These results closely mirrored the findings from our preferred models in Exhibit A2. All treatment variable coefficients were positive and not statistically significant at the traditional level. The treatment variable was positive and marginally significant for violent felony recidivism ($p = 0.087$).

Location. Not all declined youth serve their sentence at DOC. Transfer to DOC is dependent on the youth's age as well as other factors such as vulnerability. Unfortunately, due to multicollinearity between location of confinement of the youth, time, and the treatment variable (*Auto-Declined*), which is defined by time, the data do not allow us to test whether the place of confinement had an effect on recidivism. The variance inflation factors for these variables exceeded 10 indicating multicollinearity.

Length of stay. We ran several models testing the effect of length of stay in confinement. First, we ran an ordinary least squares regression model with length of stay as the dependent variable with the covariates from our preferred model in Exhibit A2. Results indicated that the treatment variable, whether a youth was declined from juvenile court or not, did not have a statistically significant effect on length of stay when controlling for all other factors. We further tested the impact of length of stay on felony recidivism by interacting the treatment variable with length of stay in confinement. The interaction term itself was also not statistically significant ($p = 0.2903$). We tested the joint significance of the treatment variable and the interaction term and it was also not statistically significant ($p = 0.1959$).

Race. In order to determine whether the automatic decline law was more or less effective for black or Hispanic youth, we tested the effects by interacting the dummy variables for race with the treatment variable (similar to our analysis for length of stay in confinement); thus, creating two interaction terms. The interaction terms were not statistically significant for black youth ($p = 0.7669$) or for Hispanic youth ($p = 0.1550$). We tested the joint significance of the treatment variable and the interaction terms which were also not statistically significant for

³⁴ E. Drake, (2011). *Washington State recidivism trends: Adult offenders released from prison (1990-2006)*. (Doc. No. 11-01-1201). Olympia: Washington State Institute for Public Policy.

black youth ($p = 0.3985$) or for Hispanic youth ($p = 0.1908$). As a final test, we restricted the data in our regression model to include only black youth ($n = 313$) and then only Hispanic youth ($n = 176$). Using our preferred model covariates for felony recidivism, we found that the treatment variable was positive, but not statistically significant for black youth ($p = 0.2618$) or for Hispanic youth ($p = 0.5022$). Thus, we found that the treatment, automatic declines, is not different for black youth or for Hispanic youth compared to all youth declined.

II. Meta-Analysis and Benefit-Cost Analysis

The Washington State legislature often directs WSIPP to update its review of the benefits and costs of programs and policies to improve public outcomes. For example, the legislature or the WSIPP Board of Directors has asked WSIPP to identify public policies that have been shown to improve the following broad outcomes of public interest: crime, education, child maltreatment, substance abuse, mental health, employment and workforce development, health care, and general prevention.

WSIPP built its first model in 1997 to estimate the economic value of programs that reduce crime. As WSIPP received additional assignments from the Washington legislature, the benefit-cost model was revised and expanded to cover other public policy outcomes. Our ongoing goal is to provide Washington policy makers with better “bottom-line” estimates each successive legislative session.

There are three basic steps to WSIPP’s analysis:

1. **What Works?** First, we conduct a systematic review of the research literature to identify policies and programs that have demonstrated an ability to improve the outcomes. The objective is to draw statistical conclusions about what works—and what does not—to achieve improvements in the outcomes, along with an estimate of the statistical error involved.
2. **What Makes Economic Sense?** The second basic step involves applying economic calculations to put a monetary value on the improved outcomes (from the first step). Using WSIPP’s benefit-cost model, the estimated benefits are then compared to the costs of programs to arrive at a set of economic bottom lines for the investments.
3. **How Risky are the Estimates?** Part of the process of estimating a return on investment involves assessing the riskiness of the estimates. Any rigorous modeling process, such as the one described here, involves many individual estimates and assumptions. Our analytical goal is to deliver two benefit-cost bottom-line measures: an expected return on investment and, given the uncertainty, the odds that the investment will at least break even.

In this section of the appendix, we provide technical detail specifically relevant to the current assignment on estimating the effectiveness of the decline of jurisdiction of youth in the juvenile court. For a comprehensive review of WSIPP’s approach to identifying evidence-based public policies, see our technical manual: Washington State Institute for Public Policy, (2013). Benefit-Cost Technical Manual: Methods and User Guide. (Document No. 13-09-1201b). Olympia, WA: Author.

A. Meta-Analysis

The first step in our approach produces estimates of policies and programs that have been shown to improve particular outcomes. We carefully analyze all high-quality studies from the United States and elsewhere to identify well-researched interventions that have achieved outcomes (as well as those that have not). We look for research studies with strong, credible evaluation designs, and we ignore studies with weak research methods.

Our empirical approach follows a meta-analytic framework to assess systematically all relevant evaluations we can locate on a given topic. By including all rigorous studies in a meta-analysis, we are making a statement about the average effectiveness of a policy as measured in all relevant studies. For example, in determining whether declining a youth’s jurisdiction in juvenile court impacts crime, we do not rely on just one evaluation. Rather, we compute a meta-analytic average effect from all the rigorous studies.

Mean-difference effect size. To estimate the effects of programs and policies on outcomes, we employ statistical procedures researchers have developed to facilitate systematic reviews of evaluation evidence. This set of procedures is called “meta-analysis.”³⁵ For this study, we coded mean-difference effect sizes following the procedures in Lipsey and Wilson.³⁶ For dichotomous measures, we used the D-cox transformation to approximate the mean difference effect size, as described in Sánchez-Meca, Marín-Martínez, and Chacón-Moscoso.³⁷ We chose to use the mean-difference effect size rather than the odds ratio effect size because we code both dichotomous and continuous outcomes (odds ratio effect sizes could also have been used with appropriate transformations).

Outcome measures of interest. The primary outcome of interest is crime. Our preference was to code convictions; however, if primary researchers did not report convictions, we coded other available measures of crime. Some studies reported multiple measures of the same outcome (e.g., arrest and incarceration). In such cases, we meta-analyzed the similar measures and used the combined effect size in the meta-analysis. As a result, each study sample coded in this analysis is associated with a single effect size for a given outcome.

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

To account for the differences in the quality of research designs, we use a 6-point scale (with values ranging from zero to five) as a way to adjust the reported results. On this scale, a rating of “5” reflects an evaluation in which the most confidence can be placed: a well-implemented random assignment study. Generally, as the evaluation ranking gets lower, less confidence can be placed in any reported differences (or lack of differences) between the program and comparison or control groups.³⁸ A rating of “0” reflects an evaluation that does not have a comparison group or has a comparison group that is not equivalent to the treatment group (for example, because individuals in the comparison group opted to forgo treatment).

On the 0-to-5 scale as interpreted by WSIPP, each study is rated as follows.

- A “5” is assigned to an evaluation with well-implemented random assignment of subjects to a treatment group and a control group that does not receive the treatment/program. A good random assignment study should also indicate how well the random assignment actually occurred by reporting values for pre-existing characteristics for the treatment and control groups.
- A “4” rating is used to designate an experimental random assignment design that had problems in implementation. For example, there could be some crossover between the treatment and control groups or differential attrition rates (such as 10 % study dropouts among participants versus 25% among non-participants).
- A “3” is assigned to an observational study that employs a rigorous quasi-experimental research design with a program and matched comparison group, controlling with statistical methods for self-selection bias that

³⁵ In general, we follow the meta-analytic methods described in: Lipsey, M. W., & Wilson, D. (2001). *Practical meta-analysis*. Thousand Oaks, CA: Sage Publications.

³⁶ Ibid.

³⁷ Sánchez-Meca, J., Marín-Martínez, F., & Chacón-Moscoso, S. (2003). Effect-size indices for dichotomized outcomes in meta-analysis. *Psychological Methods*, 8(4), 448-467.

³⁸ In a meta-analysis of juvenile delinquency evaluations, random assignment studies produced effect sizes only 56% as large as nonrandom assignment studies. Lipsey, M. W. (2003). Those confounded moderators in meta-analysis: Good, bad, and ugly. *The Annals of the American Academy of Political and Social Science*, 587(1), 69-81.

might otherwise influence outcomes. These quasi-experimental methods may include estimates made with a convincing instrumental variables modeling approach, or a Heckman approach to modeling self-selection.³⁹

- A “2” indicates a non-experimental evaluation where the program and comparison groups were reasonably well matched on pre-existing differences in key variables. There must be evidence presented in the evaluation that indicates few, if any, significant differences were observed in these salient pre-existing variables. Alternatively, if an evaluation employs sound multivariate statistical techniques (e.g., logistic regression) to control for pre-existing differences, then a level “2” study with some differences in pre-existing variables can qualify as a level 3.
- A “1” is used when a level “3” or a “2” study design was less well implemented or didn’t use many statistical controls.
- A “0” involves a study with program and comparison groups that lack comparability on pre-existing variables and no attempt was made to control for these differences in the study. A zero rating also is used in studies where no comparison group is utilized. Instead, the relationship between a program and an outcome, i.e., drug use, is analyzed before and after the program.

We do not use the results from evaluations rated as a “0” on this scale, because they do not include a comparison group and, thus, no context to judge program effectiveness. In this study, we only considered evaluations that were rated at least a 1 on this scale.

Systematic review findings. Some studies examined specific deterrence while others addressed general deterrence. Specific deterrence is the notion that individual offenders are less likely to commit future crime because of experiencing punishment. General deterrence is the notion that others, or society at-large, will be deterred from committing crime for fear of punishment.

Exhibit A3 lists the studies that met our minimum standard of rigor—rated as a 1 or higher on the rigor scale—to be included in our meta-analysis. In addition to two rigorous studies that we found, both of which were natural experiments, we also included the effect of our study in this report. We coded the coefficient from our preferred multiple regression model using felony recidivism as the outcome.

Juveniles who were declined to the adult court were coded as the treatment group and youth who remained in the juvenile justice system were coded as the comparison group. Thus, a positive effect size indicates an increase in recidivism for youth who were declined and a negative effect size indicates a decrease in recidivism. The weighted mean effect size for this group of studies was 0.190 (SE = 0.098, p-value = 0.052).

³⁹ For a discussion of these methods, see Rhodes, W., Pelissier, B., Gaes, G., Saylor, W., Camp, S., & Wallace, S. (2001). Alternative solutions to the problem of selection bias in an analysis of federal residential drug treatment programs. *Evaluation Review*, 25(3), 331-369.

Exhibit A3

Rigorous Studies Used in the Meta-Analysis

| Author and year of publication | Description and methods | Effect size | Full citation |
|--------------------------------|---|-------------|--|
| Drake, 2013 | This study uses a natural experiment comparing recidivism rates of youth who were automatically declined after the law came into effect with youth who would have been eligible had the law existed prior to that time. Multiple regression mode analysis was used to control for relevant observed characteristics. Multiple sensitivity tests demonstrated that recidivism is not lowered for youth who are automatically declined jurisdiction in the juvenile court. | 0.214 | Drake, E. (2013). <i>The effectiveness of declining juvenile court jurisdiction of youthful offenders</i> (Doc. No. 13-12-1901). Olympia: Washington State Institute for Public Policy. |
| Fagan et al., 2007 | This study uses a natural experiment comparing adolescent felony offenders prosecuted in criminal court in New York City to those charged in juvenile court in New Jersey. The authors use criminal court cases from three counties in New York and juvenile court cases from three matched counties in New Jersey. The authors use many multiple regression models and control for relevant case characteristics. They examine arrests and incarcerations. | 0.065 | Fagan, J., Kupchick, A., & Liberman, A. (2007). <i>Be careful what you wish for: Legal sanctions and public safety among adolescent offenders in juvenile and criminal court</i> . Public Law Research Paper no. 03-61. Columbia Law School, New York. |
| Fagan, 1995 | This study uses a natural experiment examining youth adjudicated in 1981 and 1982 in four counties within New York and New Jersey. These cases were randomly sampled from the population. Since this is a metropolitan area that shares similar demographic, social and cultural commonalities, the author can compare youth automatically transferred to adult court in New York to equivalent youth who were not transferred in New Jersey. The author specifically looks at adolescents age 15-16 charged with robbery 1 & 2 and burglary 1. He compares the recidivism rates in the four counties. The author uses a proportional hazard model for time to first re-arrest controlling for sentence length. | 0.188 | Fagan, J. (1995). Separating the men from the boys: The comparative advantage of juvenile versus criminal court sanctions on recidivism among adolescent felony offenders (NCJ No. 165071). In J. C. Howell, B. Krisberg, et. al., (Eds.), <i>Sourcebook on serious, violent, and chronic juvenile offenders</i> (pp. 238-260). Washington, DC: US Dept of Justice, National Institute of Justice. |

We reviewed six other studies that were commonly cited throughout the literature; however those studies did not meet our minimum standard of rigor to be included in our meta-analysis. [Exhibit A4](#) displays those studies and the reason for exclusion. Typically, these studies had selection bias issues that would not allow us to confidently attribute the causal effect of declining juveniles to the adult system on recidivism.

Exhibit A4

Citations and Summary of Studies Reviewed but not Included in the Analysis due to Methodological Rigor

| Author and Year of Publication | Description and Methods | Reason for Exclusion | Full Citation |
|--------------------------------|--|--|---|
| Bishop et al., 1996 | The authors compare recidivism rates of juvenile offenders in Florida and contemporaneously match these offenders to delinquents retained in the juvenile system. | The transfer process is not sufficiently described to determine why some offenders are transferred and others are not. Thus, selection bias is a threat to causality even after observed variables are controlled. | Bishop, D. M., Frazier, C. E., Lanza-Kaduce, L., & Winner, L. (1996). The transfer of juveniles to criminal court: Does it make a difference? <i>Crime & Delinquency</i> , 42(2), 171-191. |
| Jordan, 2011 | Youth in Pennsylvania are automatically waived to adult court based on age and offense criteria. Youth may be decertified by a judge (reverse waived) back to juvenile court. Out of 308 youth, 173 were retained in adult court and 135 were decertified to juvenile court. | The authors use propensity score matching on observed characteristics to match the contemporaneous groups (waived and reverse waived youth). This technique does not fully account for the unobserved selection bias of the youth who were reverse waived back to juvenile court. | Jordan, K. L. (2012). Juvenile transfer and recidivism: A propensity score matching approach. <i>Journal of Crime and Justice</i> , 35(1), 53-67. |
| Lanza-Kaduce et al., 2005 | The authors examine adult felony recidivism for 475 matched pairs in Florida, comparing juveniles transferred to adult court and those retained in the juvenile justice system. | The authors do not explain why some juveniles were not transferred to criminal courts. Although the authors control for various case characteristics, they do not control for unobservable variables, such as the reasons that prompt prosecutors to apply for transfer. There may be inherent differences between the treatment and control group that are not accounted for. | Lanza-Kaduce, L., Lane, J., Bishop, D. M., & Frazier, C. E. (2005). Juvenile offenders and adult felony recidivism: The impact of transfer. <i>Journal of Crime & Justice</i> , 28(1), 59-77. |

Exhibit A4, cont.

| Author and Year of Publication | Description and Methods | Reason for Exclusion | Full Citation |
|--------------------------------|---|---|--|
| Loughran et al., 2010 | The authors examine 654 youths between the ages of 14 and 17 in Maricopa County, Arizona. Authors use propensity score matching for transferred and non-transferred youths. The transfer process for youths can be judicial, prosecutorial, or statutory. | Authors use propensity score matching on contemporaneous study groups. This technique does not fully account for selection bias that threatens causality. | Loughran, T. A., Mulvey, E. P., Schubert, C. A., Chassin, L. A., Losoya, S., Steinberg, L., . . . Cauffman, E. (2010). Differential effects of adult court transfer on juvenile offender recidivism. <i>Law and Human Behavior</i> , 34(6), 476-488. |
| Myers, 2003 | The authors evaluate the discretionary waiver of youth processed in 1994 in Pennsylvania, prior to the implementation of a new law on statutory waivers. The authors perform a control function approach where the residuals from first stage equation are used as a control variable in the second stage equation. | The control function requires the use of an instrumental variable, which is not included in the authors' regression model. | Myers, D.L. (2003). The recidivism of violent youths in juvenile and adult court: A consideration of selection bias. <i>Youth Violence and Juvenile Justice</i> 1(1), 79-101. |
| Podkopacz & Feld, 1996 | The authors analyze 330 transfer motions from 1986 to 1992, examining the recidivism of transferred youth in Hennepin County, Minnesota. | This study is not an outcome evaluation with a valid comparison group. The aim of the article is to determine the characteristics that influence the judicial waiver decision with a brief analysis on recidivism without any statistical controls. | Podkopacz, M. R., & Feld, B. C. (1996). The end of the line: An empirical study of judicial waiver. <i>The Journal of Criminal Law and Criminology</i> , 86(2), 449-492. |

We also examined studies that measure the effect of juvenile decline laws on general deterrence. Unfortunately, however, there were only three such studies and none were sufficiently rigorous or provided enough information to code these studies and conduct a meta-analysis.

Exhibit A5

General Deterrence Studies Reviewed but not Included in the Analysis due to Methodological Rigor

| Author and year of publication | Description and methods | Reason for exclusion | Full citation |
|--------------------------------|---|---|---|
| Jensen & Metsger, 1994 | Authors do time-series analysis in Idaho to test general deterrence of juvenile waiver laws in Montana and Wyoming as a comparison state. | The authors include three control variables; however the relevance of those variables is questionable (infant mortality as a measure of economic deprivation, population under age 18, and the number of agencies reporting UCR crime). | Jensen, E. L., & Metsger, L. K. (1994). A test of the deterrent effect of legislative waiver on violent juvenile crime. <i>Crime & Delinquency</i> , 40(1), 96-104. |
| Singer & McDowall, 1988 | The authors use a time series analysis for before and after New York's Juvenile Offender Law of 1978 was implemented. They examine the impact on crime rates, specifically looking at juvenile arrest rates. | The authors disaggregate the results, but do not provide the number of juveniles. Thus, there is not enough information to code an effect size. | Singer, S. I., & McDowall, D. (1988). Criminalizing delinquency: The deterrent effects of the New York Juvenile Offender Law. <i>Law and Society Review</i> , 22(3), 521-535. |
| Steiner & Wright, 2006 | The authors use a multiple interrupted time series model using 14 states' monthly juvenile arrest rates (violent index crimes). They examine the data five years before and five years after laws of automatic decline of jurisdiction are implemented. The authors use an ARIMA model, which controls for serial dependence. | The authors do not include control variables in their analysis. Further, they do not provide enough information to code an effect size. | Steiner, B., & Wright, E. (2006). Assessing the relative effects of state direct file waiver laws on violent juvenile crime: Deterrence or irrelevance. <i>The Journal of Criminal Law & Criminology</i> , 96(4). |
| Risler et al., 1998 | The authors examine mean arrest rates before and after the Georgia Legislative Waiver was implemented. | The authors do not use any control variables. | Risler, E. A., Sweatman, T., & Nackerud, L. (1998). Evaluating the Georgia legislative waiver's effectiveness in deterring juvenile crime. <i>Research on Social Work Practice</i> , 8(6), 657-667. |

B. Benefit-Cost

We include estimates of the long-term benefits and costs of programs and policies. In most cases, this involves WSIPP projections well into the future. Projections are necessary, because most evidence about programs comes from evaluations with relatively short follow-up periods. It is rare to find longitudinal program evaluations. This problem, of course, is not unique to public programs. Most private investment decisions are based on past performance, and future results are projected by entrepreneurs or investment advisors based on certain assumptions. We adopt that private-sector investment approach in this model. We forecast, using a consistent and empirically based framework, the long-term benefits and costs of programs and policies. We then assess the riskiness of the projections.

At this time, we are unable to estimate the full benefits and costs of the law to automatically decline youth from the juvenile court. Our estimates only include the meta-analytic findings from our specific deterrent effect. We are not able to estimate the benefits and costs of incapacitation and general deterrence.

Three Perspectives on Benefits and Costs. We present these monetary estimates from three distinct perspectives: the benefits that accrue solely to program participants, those received by taxpayers, and any other measurable (non-participant and non-taxpayer) monetary benefits. The sum of these three perspectives provides a “total Washington” view on whether a program produces benefits that exceed costs. Restricting the focus solely to the taxpayer perspective can also be useful for certain fiscal analysis and state budget preparation.

Criminal Justice System Resources. Calculating the monetary value of benefits from a reduction in crime requires the estimation of several essential elements. The four essential elements necessary for WSIPP to conduct its benefit-cost analysis of criminal justice programs include the estimation of:

1. *Risk of reconviction.* We estimate the risk of being reconvicted of a crime for program participants relative to a base population of offenders who do not participate in the evidence-based program. These avoided crimes are estimated using criminal recidivism data from a base population of offenders who did not participate in the evidence-based program. Combining the effect size with criminal recidivism information from the untreated offenders allows us to estimate and compare the cumulative recidivism rates of offenders who participated in the evidence-based program with offenders who did not participate.
2. *Criminal justice system response.* We estimate the criminal justice system’s response to crime and the resources used when crime occurs. We estimate the volume of crime that comes to the attention of the criminal justice system. Then, in conjunction with the program effect size, we estimate how much crime is avoided and the monetary benefits to taxpayers that result from this avoidance. For criminal justice system resources, such as police, courts, and prison, we estimate the frequency and duration of utilization for each resource affected. For example, if a conviction occurs, we estimate the probability that a certain type of offense (e.g., rape) results in a certain type of sanction (e.g., prison or probation) and the average length of time the sanction will be used.
3. *Crimes in Washington.* We estimate the total crime that occurs in Washington State including both crimes reported and not reported to the police to estimate the true impact of evidence-based programs on crime. To do this, we estimate the total number of crimes that occur statewide in Washington. We scale-up statewide reported crimes to include crimes that do not necessarily result in a conviction, which includes crimes that were not reported to the police. From this, we estimate the total number of crimes that occur per conviction. This number is used in conjunction with recidivism data from the offender base population described previously to estimate the total number of crimes per conviction.
4. *Costs.* Costs for each criminal justice system resource, victimization costs, and evidence-based program costs are estimated. The costs paid by taxpayers for each significant part of the local and state criminal justice system, such as police and sheriffs, superior courts and county prosecutors, local juvenile

detention facilities, local adult jails, state juvenile rehabilitation, and state adult corrections agencies, were estimated. Marginal operating costs were estimated for these components as well as annualized capital costs, when applicable.

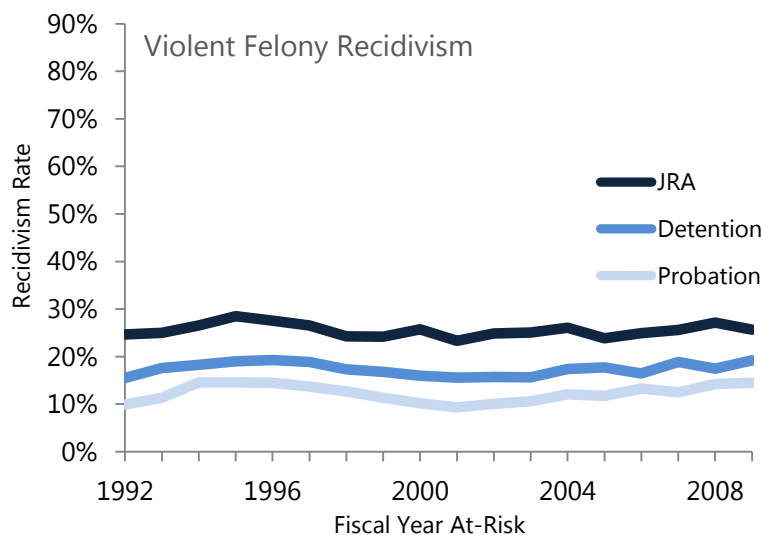
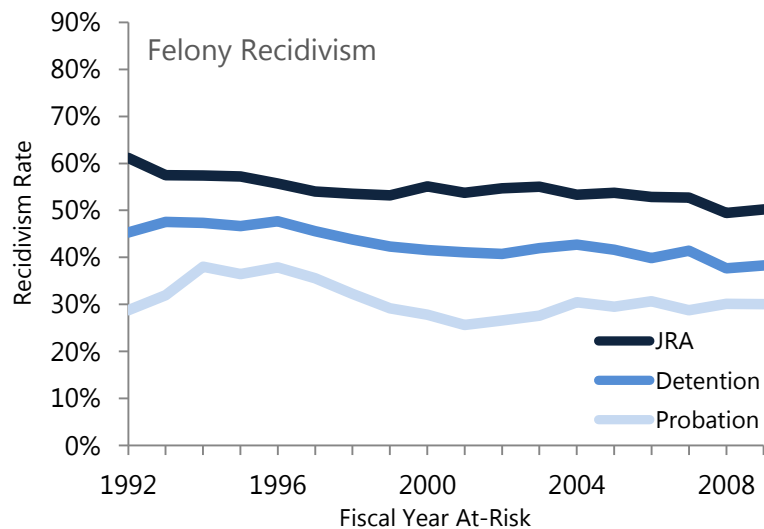
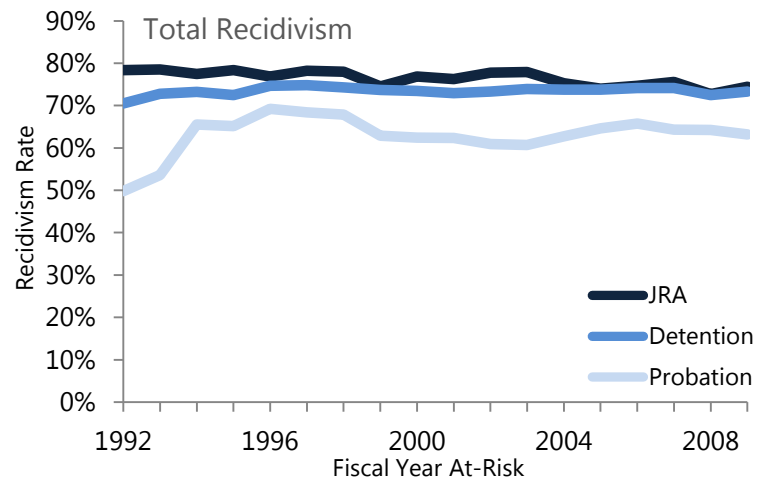
Cost Inputs. To conduct a benefit-cost analysis of declining jurisdiction of the juvenile court, we needed to estimate the cost of incarcerating a youth—the equivalent to a program cost. Under current policy, youth who are declined are under the jurisdiction of DOC, but housed at JRA until the age of 18 or, in some instances, the age of 21. We communicated with DOC, JRA, and legislative staff to estimate the cost of the program. JRA receives funding for declined youth as part of their budget. These youth are included in the forecasted JRA population. Thus, the cost for these youth is \$37,000 per offender per year.⁴⁰ In addition to the base cost to house declined youth, DOC reimburses JRA for the cost of any special or extraordinary medical services, legal services and three full-time equivalent JRA staff dedicated to the Youthful Offender Program.⁴¹ DOC also has 1.65 full-time equivalent staff dedicated to managing the Youthful Offender Program. These additional costs, divided by the average daily population in Fiscal Year 2013 equal \$6,726 per youth. Thus, the total cost per youth per year is \$43,726. Because declined youth are incarcerated for 1.66 years longer than youth who are not declined (20 months), this translates to \$72,585 per declined youth.

⁴⁰ Correspondence with Mary Mulholland from the Office of Program Research, House of Representatives.

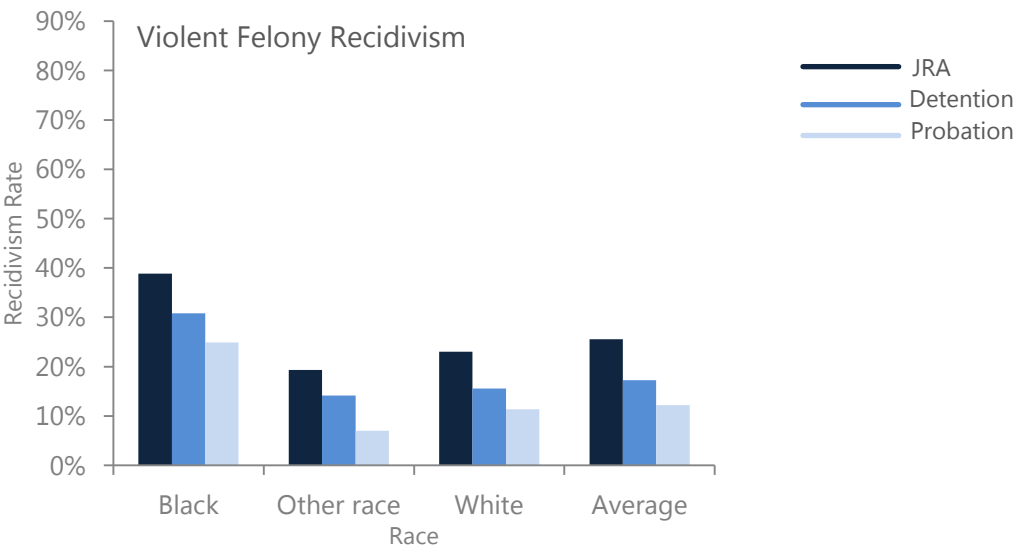
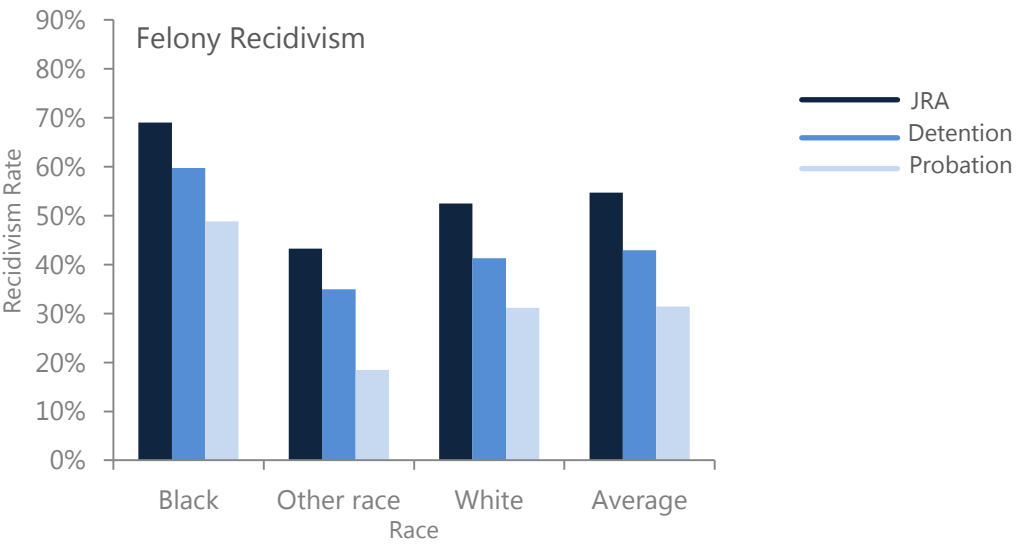
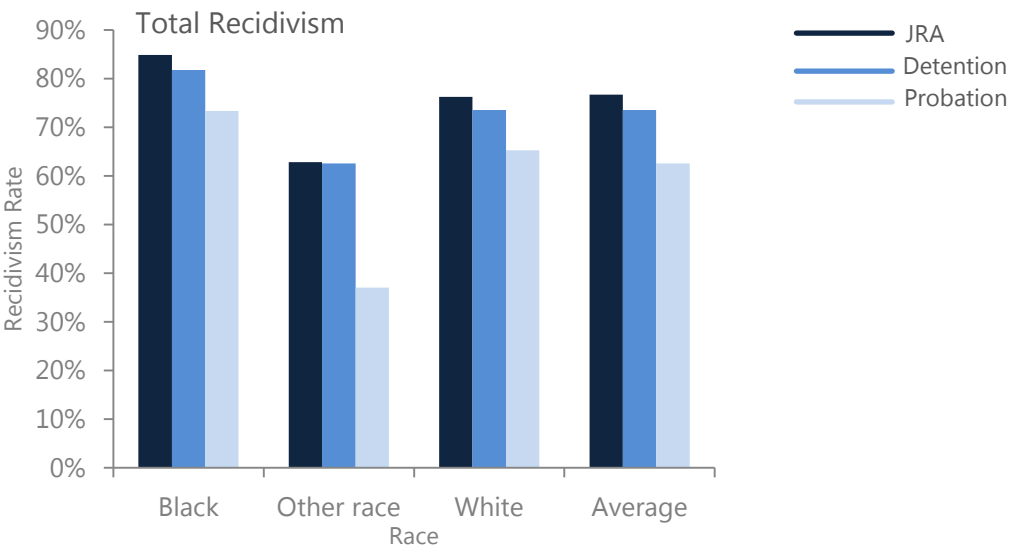
⁴¹ Correspondence with Jennifer Redman, Youthful Offender Program, Juvenile Rehabilitation Administration.

III. Recidivism Trends in Washington State

To provide context to this report, we analyzed historical recidivism trends for youth in the juvenile justice system. We analyzed data for three populations: youth releasing from JRA, youth sentenced to detention, and youth sentenced to probation. Youth were at-risk for 36-months after release from JRA or upon adjudication if not confined. We analyzed total recidivism (including felony and misdemeanor), felony recidivism, and violent felony recidivism.



Recidivism results were disaggregated by race as indicated by the work requirements for this contract.



IV. Treatment

As part of the work requirements for this contract, we examined whether juveniles who were declined jurisdiction received treatment programs at the Department of Corrections (DOC).⁴² We received data through a special request from DOC.

Using data from the outcome evaluation reported in Section II of this report, we were able to determine how many youth who were physically located in DOC facilities and participated in treatment programs. Of the 770 automatically declined youth, 750 youth were found in DOC's database. Ninety percent of those youth were in prison and the remaining 10% were supervised in the community.

Of the 750 automatically declined youth found in DOC's database, 78% were determined to have participated in some programming while in custody of DOC. It should be noted that 40% of the youth who were determined not to have participated in any programming were released from DOC custody prior to 1999 which is when DOC had the ability to electronically capture programming data in their data system.

Of the youth who participated in a program, 92% participated in more than one program (up to five programs). Displayed below are the percentages of youth participating in programs by the type of program.

| Program | Percentage Participating |
|---------------------------|--------------------------|
| Chemical dependency | 5% |
| Education | 85% |
| Offender change | 76% |
| Vocational | 53% |
| Work | 88% |
| Total youth participating | 585 |

Description of Programs

- Chemical dependency includes inpatient, outpatient, and therapeutic communities.
- Education includes the Youthful Offender Program high school diploma program, basic skills programs, job readiness, and English as a second language.
- Offender change includes stress and anger management, victim awareness, job hunter, and parenting skills.
- Vocation includes Youthful Offender Program vocational grant as well as a variety of vocational training including information technology, electronic systems, math for the trades, and building maintenance.
- Work includes institutional support jobs such as food service, custodian, forestry, and work crews.

⁴² Prior to 2004, JRA did not have an automated tracking system; thus, treatment/program data were not available for youth in our study. Although these data were not available, it should be noted, however, that Washington state law requires that education be provided to common school age children who are confined (see, for example, RCW 28A.190, RCW 28A.193, and 28A.194).

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