**The Effect of Prison Sentence Length on Recidivism: Evidence from Random**

**Judicial Assignment**

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**Abstract**

Whether punishment promotes or deters future criminal activity by the convicted offender is a key public policy concern. Longer prison sentences further isolate offenders from the legitimate labor force and may promote the formation of criminal networks in prison. On the other hand, greater initial punishment may have a deterrence effect on the individual being punished, sometimes called “specific deterrence,” through learning or the rehabilitative effect of prison. We test the effect of prison sentence length on recidivism by exploiting a unique quasi-experimental design from adult sentences within a courthouse in Seattle, Washington. Offenders who plead guilty are randomly assigned to a sentencing judge, which leads to random differences in prison sentence length depending on the sentencing judge’s proclivities. We find that one-month extra prison sentence reduces the rate of recidivism by about one percentage point, with possibly larger effects for those with limited criminal histories. However, the reduction in recidivism comes almost entirely in the first year of release, which we interpret as consistent with prison’s rehabilitative role.

**I. Introduction**

Whether and at what point additional punishment promotes or deters future criminal activity by the convicted offender is critical to criminal justice policy, especially under the present American system of mass incarceration. It is not difficult to explain how longer prison sentences can increase the likelihood of new criminal conduct after release. Longer prison sentences further isolate offenders from the legitimate labor force while at the same time promoting the formation of criminal networks in prison. These criminogenic effects of imprisonment alter the relative financial benefits of criminal versus non-criminal activities. Additionally, the psychic costs of prison may be diminished by exposure to it. If offenders get used to punishment or develop coping strategies, the threat of a new prison sentence will be less deterring after their release from initial incarceration. If these circumstances prevail, the marginal social cost of additional imprisonment begins increasing at some point, thereby offsetting the benefits incarceration provides through deterrence and incapacitation. Thus, additional punishment, while incapacitating and possibly deterring ex-ante, at some point becomes criminogenic, increasing recidivism upon release.

On the other hand, some researchers have suggested that greater initial punishment may have a deterrence effect on the individual serving the punishment beyond the deterrence offered by expected punishment. This additional deterrence effect from a longer prison sentence is sometimes called “specific deterrence.” The notion of offenders reacting to specific deterrence, an ex post effect, instead of expected punishment ex ante is in some tension with canonical economics models of criminal behavior. However, if criminals are boundedly rational, such that they do not anticipate the expected punishment or what prison is like but rather learn from the punishment meted out after a conviction, then specific deterrence is just a form of learning. Additionally, prison may offer some rehabilitative services that facilitate pro-social behavior. If these factors are at play, the marginal benefits of incarcerating an individual could be increasing over some range, even apart from general deterrence.

We test the effect of longer prison sentences on recidivism by exploiting a unique quasi-experimental design from sentences within a courthouse in Seattle, Washington. Offenders who plead guilty prior to trial are randomly assigned to a different judge for sentencing. The combination of random assignment conditional on pleading guilty and large differences in sentencing preferences across judges leads to random variation in prison sentences. We exploit this variation to test the effect of prison sentence length on recidivism. Because 95% of our sample is sentenced to some prison time, we do not test the extensive margin of imprisonment versus no imprisonment, but only the intensive margin of imprisonment length.

Both reduced form and instrumental variable results find that for a one-month increase in prison sentence, the rate of recidivism measured by conviction of a new crime declines by about one percentage point or more. Moreover, the reduction in the rate of recidivism comes entirely in the first year of release. While the reduction persists over two and three year windows of recidivism, the effect of greater punishment does not increase. For reasons we will discuss in greater detail below, we interpret this result as most consistent with a rehabilitative function of prison. In short, we suggest that if offenders get their life back on course post-release, this effect reduces recidivism in the first year of release, and this effect persists over time.

We also test for heterogenous treatment effects by criminal history. Some offenders may be inframarginal in response to punishment, and those are likely offenders who were not deterred by previous encounters with the criminal justice system. Extra prison time does not yield a statistically significant reduction in recidivism for offenders with more significant criminal histories, and the point estimates of the effect of imprisonment on recidivism for this group are at times close to zero. For those with limited criminal histories, extra prison time significantly reduces the probability of recidivism in all reduced-form specifications and in some instrumental variables specifications.

It is important to note that the treatment effects estimated here do not involve lengthy sentences. The average sentence length in the data is nine months, and the median sentence is three months. Thus, the results pertain only to low-level offenders mostly convicted of non-violent property crimes.

**II. Specific Deterrence: Theory and Evidence**

The social benefits to punishment are traditionally thought of in three separate categories: (1) incapacitation, (2) general deterrence, and (3) specific deterrence. Each of these can independently affect crime rates through different pathways. Incapacitating offenders by imprisonment means that those offenders cannot commit crimes against the general public, and this should reduce the crime rate. How much incapacitation reduces the crime rate is dependent upon the elasticity of supply regarding criminal activity, and most studies of incapacitation find some evidence that incapacitation reduces crime rates, but by a smaller percentage than the increase in the prison population (see Abrams 2013 for a comprehensive survey). The threat of punishment for crimes should create “general deterrence” if would-be criminals adjust their criminal activities in response to the penalties they face if caught. The traditional theory holds that stiffer penalties mean a higher expected cost of committing a crime, and therefore crime becomes less attractive to a rational person, and less of it occurs. Investigations into the degree to which general deterrence affects criminal activity are mixed. Levitt and Kessler (1999) find some reduction in crimes to which sentence enhancements were added in the period of time immediately following the enhancements being enacted, a finding consistent with general deterrence. Other studies, such as Marvell and Moody’s (2001) analysis of three-strike laws, find no significant general deterrence effects. There is some evidence that punishment regimes cause substitution between crimes, for example be instances where harsher sentences could conceivably cause more of a particular type of crime through substitution effects (Iyengar, 2008).

In contrast to incapacitation and general deterrence, specific deterrence is the impact of punishment on a punished offender’s future criminal conduct. The idea that offenders react to specific deterrence instead of or in addition to expected punishment is in some tension with canonical economics models of criminal behavior. The experience of punishment may provide additional deterrence because individuals cannot assess how distasteful punishment is until it is actually experienced, and the distaste might increase with the severity of the sentence. Moreover, if criminals are boundedly rational, such that they do not anticipate the expected punishment but rather learn from the punishment meted out after a conviction, then specific deterrence is just a form of learning. Prison policies may also actively try to prevent reoffending. Rehabilitative services may enhance non-criminal opportunities available upon release through mental health, substance abuse, education and job training programs, making reoffending relatively less attractive. MacKenzie (2006) argues that some offenders may benefit from vocational training within prison and GED completion, while Cullen (2002) suggests some types of rehabilitation that increase self-restraint.

On the other hand, there are sound theoretical reasons to suppose that the experience of punishment could have a criminogenic effect as well. For example, offenders in prison may acquire additional criminal capital behind bars through network formation or peer effects, for which there is some evidence (Bayer et al. 2009; Irwin 2005; Wacquant 2001). Moreover, imprisonment removes the offender from the formal labor market, and a conviction and sentence may itself be a barrier to labor force reentry. This criminogenic effect can be more pronounced when offenders return home to the same peers and criminal opportunities they faced before incarceration (Kirk, 2009). In the case of incapacitation or general deterrence, the debate is generally not about whether the punishment incapacitates or deters, but the elasticity between punishment and criminal activity. When studying the effect of punishment on reoffending, however, there are plausible theoretical arguments for why additional punishment would increase or decrease recidivism. Thus, the sign of the relationship between punishment and reoffending is of significant interest because it is theoretically ambiguous. As Nagin et al. (2009) aptly point out, the phrase “effect of punishment on reoffending” is a better descriptor than “specific deterrence.” Abrams (2013) prefers the phrase “net specific deterrence” to capture the ambiguity of the effect. We use the phrase “effect of punishment on recidivism” for the remainder of this article.

Because rising crime rates and the severity of individual offenses lead to harsher sentences, it is difficult to measure the effects of prison sentence length, incarceration, and other punishments on crime rates and recidivism. Studies of incapacitation and specific deterrence have traditionally attempted to get around these issues by exploiting natural experiments such as prison release programs, overcrowding litigation, variation in police intensity, and punishment add-ons, often taking a difference-in-difference approach. Because the effect of punishment on recidivism is particular to the individual sentenced, an approach that approximately randomly assigns prison or different prison sentences to offenders must be used. Several papers have used instrumental variables, regression discontinuity, and natural experiments provided by prison release programs. There are a host of observational studies that attempt to permit inference by conditioning on observables (for a survey see Nagin et. al 2009), but unobserved severity of the crime, measurement error in relevant factors such as criminal history, and other relevant omitted or weakly controlled variables must seriously affect such work. Indeed, in our data we will show that the sign of the effect of imprisonment on reoffending actually flips between OLS and instrumental variable results.

Studies have come to different conclusions about the effect of punishment on the punished. Hjalmarsson (2009) uses a regression discontinuity framework and finds that juveniles punished with incarceration are less likely to commit future crimes than individuals who are not incarcerated. By contrast, Aizer and Doyle (2015) find that the incarceration decision by randomly assigned judges in juvenile proceedings increases the probability of adult incarceration and decreases the probability of high school completion significantly. The Aizer and Doyle (2015) results are stark findings in support of the criminogenic effect of incarceration.

By contrast, studies based on adult offenders have found evidence that longer prison sentences can reduce recidivism. Kuziemko (2013) uses a regression discontinuity approach to find that an extra month in prison reduces recidivism probabilities by 1.3 percentage points. Abrams (2012) in an unpublished paper takes a related approach by using indicator variables for the attorney within the public defender office as an instrumental variable, since the random assignment of an attorney essentially randomizes the quality of a defendant’s criminal defense and, through that mechanism, the sentence length. He finds that an additional month of incarceration reduces recidivism by about one percentage point. Maurin and Ouss (2009) find a negative relationship between sentence length and likelihood of recidivism using a quasi-experimental approach where certain inmates received large reductions in their sentences due to Bastille Day pardons. Drago et al. (2009), on the other hand, find that the deterrent effect of additional punishment decreases the more time one has spent in prison. This evidence is supportive of a criminogenic effect. Berube and Green (2007) and Green and Winik (2008) both use federal data with random assignment of individuals to judges who vary by harshness. Neither finds statistically significant effects of prison sentences, with point estimate taking on both positive and negative values. It is worth noting that the prison sentences at issue in the two federal studies are much longer on average than those in state studies and involve much more serious criminal behavior. Nagin and Snodgrass (2013) use an approach similar to the one employed here by using random assignment to the judge and find that the incarceration decision has little effect on the probability of reoffending, as measured by being rearrested, even though judges vary greatly in their probability of imposing some prison time. Similarly, Abrams (2012) fails to find an impact on the extensive margin, although he found an effect from the intensive margin.

Studies of labor market outcomes are more mixed. For example, Kling (2006) uses random assignment and judge instruments to estimate how longer sentences affect labor market outcomes of adult defendants, and finds that there is actually a positive short-run effect of prison sentence on employment and wages, and no significant relationship in the long run. These results suggest lower recidivism and more pro-social conduct generally after incarceration. Waldfogel (1994) finds that incarceration has a serious negative effect on employment and earnings, in particular for jobs that involve some degree of employer trust, and Lott (1992) likewise finds large market based penalties associated with conviction for white-collar crimes. Cook et. al (2015) report results from an experiment that suggests pre-release interventions may help improve human capital and post-release services can boost labor market outcomes of individuals after incarceration.

**III. Washington Sentencing Guidelines and Data**

Washington has a sentencing guidelines regime in which the severity of the crime combined with criminal history produce a sentencing range from which the judge may choose the sentence. There are 16 levels of severity or “Seriousness” and 10 categories of criminal history, referred to as the “Offender Score.” These two categories create a sentencing grid (see Appendix for Guidelines Grid). A sentencing judge may sentence within the grid box as he or she sees fit. For crimes with low Seriousness Levels, which are the vast majority of our cases, judges have a lot of discretion. For example, taking our sample averages of Seriousness Level 2 and Offender Score of 2, the sentencing range is 3 to 9 months. If the judge finds there are “substantial and compelling reasons justifying an exceptional sentence,” the judge may depart from the guidelines, but must justify the departure in writing (Revised Code Washington 9.94A.020(2)). In addition, under Washington’s guidelines judges have additional discretion for first time offenders. For example, if first-time offenders not convicted of a violent, sex, or drug crime may have their sentences reduced to 3 months and a year of community supervision imposed (Revised Code Washington 9.94A.020(5)). Apart from this alternate sentencing scheme, however, there is almost no supervised release for those convicted of non-violent crimes. Some serious offenders receive two years of supervised release, during which they must submit to various monitoring and restrictions on movement and associations. (Revised Code Washington 9.94A.020(5)). However, the state has a relatively low rate of post-release supervision overall, particularly for those convicted of low-level crimes (Justice Reinvestment Initiative 2015). The population under supervised release in Washington is actually smaller than prison population, and declined greatly after 2003 reforms (Id).

Offender Score is largely based on previous criminal history. There are, however, numerous complicating factors in its calculation, including how to count multiple current convictions in criminal history, how to map out-of-state or federal convictions onto the Washington state’s criminal history calculations, and how to deal with juvenile records (see Washington State Adult Sentencing Manual 2012 pgs. 23-27). There is a large case law in Washington that deals with offender score calculations, especially regarding how to count out-of-state convictions.[[3]](#footnote-3)

The data used in this analysis is from the Washington State Sentencing Guidelines Commission, which compiled case level information on felony cases from Washington State Superior Courts for which a sentence was issued between July 1999 and June 2011. This data set contains information on the charges brought against the defendant, the defendant’s criminal history, the sentencing judge, the case outcome, and sentence. Because criminal cases in King County can be handled at one of two locations (either the Seattle location or the Kent Regional Justice Center), a data set from the Administrative Office of the Courts in King County is used to identify which cases were adjudicated at these respective locations.[[4]](#footnote-4)

If an offender pleads guilty prior to trial, he or she is assigned to a different judge (not the assigned trial judge) to be sentenced at a separate hearing. These sentencing hearings occur on Friday afternoons, and the specific group of judges who are to sentence Friday guilty please are assigned several weeks in advance. The sentencing coordinator, a court employee, assigns judges with the directive that assignments be made “immediately” after the guilty plea is entered and to achieve “balance” among the judges in the number of cases they hear. The King County Superior Court Criminal Department Manual[[5]](#footnote-5) (pg. 41-42) describes the process as follows:

If a defendant pleads guilty in the plea court, at omnibus or at case scheduling, the case shall be assigned a sentencing judge by the Criminal Department Sentencing Coordinator(s)… Sentencing hearings are set by the Sentencing Coordinators for Friday afternoons at three times: 1:00, 1:45, and 2:45 p.m. An average of four sentencing hearings are set in each time slot….The Sentencing Coordinators shall endeavor to assign sentencing hearings equally among all criminal and civil department judges and will assign each judge no more than twelve defendants for mainstream sentencing hearings…. The Sentencing Coordinator assigns a sentencing judge and a sentencing date immediately after the defendant enters a guilty plea or is found guilty.

We limit the sample of observations to guilty pleas that were sentenced on Friday. The assignment process is not perfectly random, in part because there is an attempt to balance the number of sentences across judges. However, “(a)ny change in a sentencing date must first be approved by both parties and by the assigned sentencing court” (Id.). Because domestic violence cases are assigned separately, we remove them from the data. F-tests for random assignment across judges confirm random assignment in the Kent Regional Justice Center only, so we only use data from the Kent courthouse for sentences on Friday.[[6]](#footnote-6) We further exclude observations from judges who sentenced only a few cases,[[7]](#footnote-7) leaving us with 25 judges in the sample.

We use data from 1999 through 2012 and define recidivism as being resentenced for a subsequent felony offense during the periods of one, two, or three years post-release. As a result, we observe between 7,700 and 8,780 individual sentences whom we can follow in the data for one, two, or three years. Table 1 provides summary statistics for variables related to defendant characteristics, case outcomes, and recidivism, conditional on being able to observe the offender post-release for one year or more. The average sentence for an offender in the sample is 9 months. The distribution of sentences is fairly skewed, as the median sentence is 3 months. As Table 1 indicates, 46% of individuals in the sample have an Offender Score below 2, and the average Seriousness Level is also 2. Figure 1 presents kernel density estimates of total sentence in months. There is a long-right tail, but 74% of offenders are sentenced to less than one-year imprisonment, and 90% are sentenced to less than two years.

We identify recidivism directly from the Sentencing Guidelines Commission data based whether a defendant was sentenced for a subsequent crime within the specified time frame (thus it would not include, for example, someone who was rearrested within that time frame but not convicted and sentenced within that time frame). To identify reoffenders, we use a unique individual identifier, also confirmed by subject’s name and date of birth using the entire state sentencing database. The one-year, two-year, and three-year recidivism rates are 12%, 20%, and 25% respectively in our sample. These figures are very much in line with recidivism rates calculated by the Bureau of Justice Statistics in a multi-state sample, which found that the rates individuals returning to prison with a new sentence are 10%, 19%, and 25% respectively for the one-year, two-year, and three-year time horizons (Langan and Levin, 2002).

We do not observe the actual dates of release. However, Washington has a fairly determinate sentencing scheme, allowing a fixed amount of time off for good behavior, followed by very limited supervised release. There is no parole. For serious violent crimes and sex crimes, good behavior can reduce prison sentences by only 15%. For the sample period, offenders convicted of other crimes could receive between 35 and 50% credit for good behavior. Our reported results are based on the assumption that offenders serve their full sentences., the results were little changed by the alternative assumption that an offender’s release date is based on credit for good behavior.

**IV. Analysis and Results**

Our identification strategy relies on random assignment to judge and inter-judge variation in sentence. We report results for instruments using both judge dummies as instruments and a leave-one-out judge average as well. The well-known problem in two-stage least squares regressions is the small-sample bias toward the OLS results when the instruments in the first stage are many and weak. Additionally, when instruments increase as sample size increases, additional concerns arise. We will therefore report results for both judge instruments and a measure of judge harshness for each individual based on a leave-one-out measure. The leave-one-out measure is the jackknife instrumental variables approach or JIVE approach that is favored in these circumstances (Stock, Wright, and Yogo 2002; Kolesar et al. 2011).

a. Identification Strategy

The basic reduced form is as follows:

where *i* indexes individual and *j* indexes judge. *Judge* is a matrix of judge dummies and *X* is a matrix of offender demographic variables which control for race, sex, number and type of prior offenses, and age in months in a cubic form. Because the random assignment occurs within the week, we include a full matrix of weekly fixed effects. Our leave-one-out reduced form takes the following form:

Where:

measures the average relative harshness, determined by sentences meted out in all other weeks, of judge *j* who sentenced offender *i* in week *k*. The variable is the average sentence in week *w*. We say “relative harshness” because the prison sentences are measured relative to the average sentence of each week. We choose to leave out the entire week to break any correlation between the individual error term, which might encompass a weekly component, but the results were quite similar under either approach.

*Recidivate* is measured over one, two, or three years and a person is included in the data only if their recidivism risk can be observed for at least one, two, or three years respectively after his or her estimated release date. The regressions are thus linear probability models, where the dependent variable takes on the value one if recidivism occurs in the relevant time frame and zero otherwise.

The two-stage least squares equation is as follows, where *TotalSentence* is instrumented by either judge dummies or the leave-one-out harshness measure :

Because our data are implicitly grouped by judge, we report results clustering at the judge level for the reduced form and instrumental variables regressions.

The key to the identification strategy is that offenders are randomly assigned to their sentencing judge. Although the assignment process described should generate randomness, and observers believe the process to be basically random, it is not explicitly random. We test random assignment for each courthouse in King County. For the Kent Courthouse, the randomness results are reported in Table 2 as F-tests on the judge dummies. The F-stats do not reject random assignment of offenders by age, race, sex, or for number of previous convictions across judges. Based on the reduced form, these are all key factors in determining recidivism. The Seattle Courthouse, by contrast, failed the random assignment tests for race and sex, and so we do not use that data.

We report results in Table 2 for two factors that we did not believe ex ante to be exogenous to the sentencing judge: (1) offender score, which is primarily determined by criminal history but allows for some judicial determinations how criminal history should be counted toward offender scores and (2) severity level of the crime, which could also be influenced by judicial calculations of severity. In the case of severity level, the F-stat rejects randomness, while in the case of offender score, the F-stat fails to reject randomness at the 5% level, but only marginally so (p-value = 0.0512). It is worth noting that the objective component of offender score, the number of prior criminal convictions in the state of Washington, satisfies the test for randomness.

For each analysis, we also report separate regressions based on the magnitude of criminal history in order to test for heterogeneous effects of prison by criminal history. Those with lengthy criminal histories may be inframarginal to additional prison time, not having been deterred by past punishments and being more likely to be disconnected from labor markets. We roughly split the sample into those with little or no criminal history (offender scores of one or less), and those with some criminal history (offender score of two or more). We choose to use offender score as the criminal history measure because it reflects a broader measure of criminal history, including crimes committed out of state.

First-stage regressions for the judge dummies are represented by Figure 2. Appendix Tables 1 and 2 report first-stage results using the 25 individual judge dummy variables and leave-one-means respectively. Relative to the most lenient judge, the harshest judge sentences around seven months longer on average and the median judge almost two months longer on average. These differences are large relative to an average prison sentence of 9 months. Despite this significant variation, the F-stat for the test of joint significance of the judge dummies is only 2.02, which raises concerns about the bias present in the two-stage least squares estimates with many weak instruments. The coefficient on the leave-one-out mean is roughly 0.7 with or without controls, and with a standard error of about 0.22, which easily encompasses a one-to-one relationship between prison sentence and the judge’s leave-one-out measure of harshness. Ideally, the coefficient should be 1.0 since an increase in one-month of judge harshness in all other weeks the judge sentenced should correspond to an average increase of one-month in the individual “left out” case. However, measurement error should decrease the correspondence.

b. Reduced form regressions

Table 3 presents naïve OLS regression results of Equation 3. In the first three columns, which report results for the entire sample, prison sentence is associated with an increase in the probability of being resentenced for a different crime in one, two, or three years after release. The increase is small. The probability of recidivism increases between 0.13 and 0.20 percentage points for each additional month of prison sentence imposed depending on the measure of recidivism (one, two, or three years). When the data are separated by criminal history, the effect of additional prison on those with little or no criminal history is a precisely estimated zero, while those with more significant criminal histories, additional prison is associated with a statistically significant increase in the probability of recidivism. In short, the OLS estimates suggest a small but positive association between recidivism and additional prison time.

Of course, the OLS estimates are likely to be significantly biased. Offender characteristics that the judge can observe but researchers do not observe (or observe imperfectly) that increase the length of a prison sentence should also increase recidivism. Examples of such unobservables include the underlying severity of the crime or relevant conduct not recorded in the data.

The reduced form regressions are reported in Table 4, where the dependent variables are recidivism in one, two, or three years. The first row reports the coefficient on the judge effect estimated from the first stage regression using judge dummies. Because the judge fixed effect is a fitted value from a regression, we bootstrap the standard errors. The second regression line reports the coefficients on a regression taking the leave-one-out mean for each judge as the independent variable of interest. The first three columns include the full sample, while the remaining six columns repeat the regressions of columns 1 through 3 for those with low and high criminal history respectively.

The coefficients on both fixed effect and leave-one-out means are similar across the specifications, with the coefficients on the leave-one-out mean a little larger in magnitude than the coefficients on the judge fixed effect. In column 1, the coefficients imply that assignment to a judge who imposes a one month greater prison sentence decreases the probability of recidivism by about one percentage point. This coefficient is little changed when the regression is altered to include recidivism over two and three years in columns 2 and 3. When we divide the sample into those with little criminal history (columns 4 through 6) and those with significant criminal history (columns 7 through 9), some differences across the two groups are suggested. For those with low criminal histories, the magnitudes of the effect of imprisonment on recidivism increase and are strongly statistically significant, while the effect of additional imprisonment on those with more significant criminal histories is smaller and not statistically significant. The imprecise standard errors, especially for those with more significant criminal histories, do not permit us to make a statistical inference about differences between these groups. However, the results are suggestive of possible differences in response to imprisonment length across offender types.

c. Instrumental variable results

The reduced-form results strongly support the inference that being assigned to a harsher judge reduces the rate of recidivism. It is natural to infer that this effect takes place solely through the length of the prison sentence itself. However, to rule out the fact that we are not measuring only attributes that also come with judicial tendencies for longer sentences, we employ 2SLS results to get the direct effect of prison sentences on recidivism probabilities.

Table 5 reports 2SLS results for both judge dummy and the leave-out-mean instruments. We report regressions for recidivism over one, two, and three years both with and without demographic controls. The inclusion of demographic controls makes little difference to the point estimate and only slightly increases precision. Using either judge dummy variables or leave-one-out mean instruments, we roughly replicate the results of the reduced-form estimates for recidivism within the first year of release. In this case, a one-month increase in prison sentence is linked to a 1 percentage point decline in recidivism when using judge dummies as an instrument and a 1.5 percentage point decrease in recidivism for the leave-one-out mean instrument. In the case of two-year post release recidivism in columns 3 and 4, the results are quite imprecise when using the leave-one-out instruments increasing to over three with a standard error of more than two. For the three-year recidivism window, when judge dummies are used as instruments, the coefficient on prison sentence implies that a one-month increase in prison sentence reduces recidivism by about 1.5 percentage points. The leave-one-out instrumental variable results for three-year recidivism are also imprecise, with a coefficient of 3.2 and standard errors of over 2.

Table 6 tests the robustness of the 2SLS results to both the LIML and JIVE approach to instrumentation. The leave-one-out instruments already employ a JIVE methodology, and LIML is equivalent to 2SLS with one instrument, so we only report results for the judge dummies. LIML results for recidivism within the first year are a bit larger than the 2SLS results and are statistically significant at the 1% level, suggesting a 1.6 percentage point decline in first-year recidivism for every additional month of prison. Both the coefficients and standard errors increase substantially for two and three-year recidivism rates and do not permit strong inference. JIVE results on judge dummies are also larger in magnitude than the 2SLS results, but are fairly precisely estimated. In short, alternative methods of instrumental variables find results very similar to those in the 2SLS analysis.

Table 7 reports 2SLS results separating the sample into low and high criminal history categories. The judge dummy instruments suggest that additional prison time reduces recidivism for those with low criminal histories, while prison time does not have a measurable effect for those with more significant criminal histories. The leave-one-out instruments are very imprecise.

d. Discussion of results

The reduced form results paint a fairly consistent picture of the effect of imprisonment: an additional month of imprisonment leads to a 1 to 1.5 percentage points decline in recidivism on a 12 percent average rate. The magnitude of the estimated effect in the first year is quite large, suggesting that a one-month increase in imprisonment could reduce recidivism by 8 to 12 percent for the marginal offender. However, the reduced form estimates of the judge effects remain roughly the same even as we expand the window for recidivism from one to two or three years.

The two-stage least squares results are, as expected, less precise than the reduced-form estimates. However, in the case of recidivism in the first year post release, 2SLS, JIVE, and LIML results all show that recidivism declines with total prison sentence. The results for longer recidivism windows are less precise in some cases, but remain negative. Thus, when the source of variation in prison sentence depends on random judicial assignment, the relationship between prison sentence and recidivism is negative. Moreover, when statistically significant, the relationship is quite similar to that estimated in the reduced form.

The overall sample recidivism rate in the first year is only 12%, which means the reduction in recidivism from additional punishment over the first year is proportionately large. However, even as the recidivism rate increases over two and three-year windows after release to over 24%, the effect of additional imprisonment does not increase but stays constant at around one-percentage point per month additional imprisonment. In short, the effect of additional punishment is operative only in the first year of release, but the effect persists over time. We verified this further in unreported regressions in which we re-estimated recidivism over two and three year windows, coding recidivism in the first year as zero. We found that additional imprisonment had a precisely zero impact on recidivism probabilities in the second and third years of release if we disregard recidivism in the first year.

**V. Conclusion**

The point estimates of the impact of one-month additional prison sentence on first year recidivism are large, especially relative to the overall recidivism rate of 12% during the first year of release. However, both Abrams (unpublished 2012) and Kuziemko (2013) find similar point estimates. Abrams uses an instrumental variables strategy relying on defense counsel effectiveness, while Kuziemko uses discontinuities in parole board guidelines. Our results are not directly comparable because Abrams’s and Kuziemko’s measures of recidivism are broader than ours, but they are not out of line.[[8]](#footnote-8)

Though consistent with some prior findings, we believe that the effects measured here seem particularly large unless given a local average treatment effect interpretation and considered in context. First, our average sentences are quite small, with the median sentence being only 4 months. Moving from the most lenient to the harshest judge in the sample could double an offender’s sentence length. The Washington State Sentencing guidelines allow a significant degree of judicial discretion for offenses with low severity levels, which make up the vast majority of our data. Thus, there is a lot of judge-based variation around the average sentence. The local effects are not predicting the effect of moving from a one-year sentence to a multi-year sentence, but rather the variation is in months around a fairly low sentencing level.

Second, and perhaps most important, the effect of months imprisonment is not punishment alone, but also rehabilitation. This includes the potential for job training, substance abuse counseling, and post-release supervision. Washington State has developed a reputation as a leader in rehabilitation programs (Aos et al. 2006) and has limited particularly punitive measures such as solitary confinement. Kling (2006) suggested that the findings of positive employment and wage effects from greater incarceration could be explained by more exposure to rehabilitative services offered in prison. We believe that this interpretation is also consistent with the finding that the effect of imprisonment is concentrated in the first year. In other words, if the offender does not “respond to treatment” in the first year of release, the treatment of additional imprisonment is not likely to be effective in subsequent years. However, this conclusion is fairly speculative given that, as one recent survey (Ouss 2013) concluded, there are few “theoretical, experimental, or quasi-experimental studies on rehabilitation programs.” More work in line with studying the possible effects of various alternative punishment regimes, as in the juvenile context such as in Manski and Nagin (1998), or in the adult context as in Chen and Shapiro (2007), would be key to sorting out the possible role of rehabilitation in reducing recidivism. If our results are traceable to the rehabilitative impacts of additional prison time, then generalizing results from a rehabilitative system to a more punitive system would be in error.

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**Table 1. Descriptive Statistics**

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Notes: Sentencing, conviction, and criminal history data come from Washington's Sentencing Guidelines Commission. Data includes observations involving sentences issued at the Kent Regional Justice Center in King County Washington (as indicated in data from Washington's Administrative Office of the Courts) between July 1999 and January 2011 in which the offender pled guilty and was sentenced during the Friday sentencing block by a Superior Court judge (provided that judge has sentenced at least 150 cases in the data). Only cases where offenders had at least one full year during which they were at risk of recidivism are used.

**Table 2. Tests of Whether Judge Dummy Variables Jointly Predict Case Characteristics**

****

Notes: N=8,780. Sentencing, conviction, and criminal history data come from Washington's Sentencing Guidelines Commission. Data includes observations involving sentences issued at the Kent Regional Justice Center in King County Washington (as indicated in data from Washington's Administrative Office of the Courts) between July 1999 and January 2011 in which the offender pled guilty and was sentenced during the Friday sentencing block by a Superior Court judge (provided that judge has sentenced at least 150 cases in the data). Recidivism is defined as having been resentenced in Washington State within 1, 2, or 3 years of the offender’s estimated release date. Only cases in which offenders had at least one full year during which they were at risk of recidivism are used. Regressions involved 25 individual judge dummy variables.

**Table 3. OLS Regressions of Recidivism Variables on Total Sentence**

****

Notes: Sentencing, conviction, and criminal history data come from Washington's Sentencing Guidelines Commission. Data includes observations involving sentences issued at the Kent Regional Justice Center in King County Washington (as indicated in data from Washington's Administrative Office of the Courts) between July 1999 and January 2011 in which the offender pled guilty and was sentenced during the Friday sentencing block by a Superior Court judge (provided that judge has sentenced at least 150 cases). Only cases where offenders had at least the full one, two, or three year window (depending on the specification) during which they were at risk of recidivism are used. Recidivism is defined as having been resentenced in Washington State within 1, 2, or 3 years of the offender’s estimated release date. Results are from panel regressions using year-week fixed effects, linear, quadratic, and cubic controls for offender age, and dummy variables to control for sex, race, whether the offender had at least one prior nonviolent conviction, whether the offender had at least one prior violent conviction, whether the offender had at least one prior drug conviction, and whether the offender had at least one prior serious violent conviction. Robust standard clustered by judge. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 4. Reduced Form Regressions of Recidivism Variables on Judge Sentencing Fixed Effect and Other Control Variables**

****

Notes: Sentencing, conviction, and criminal history data come from Washington's Sentencing Guidelines Commission. Data includes observations involving sentences issued at the Kent Regional Justice Center in King County Washington (as indicated in data from Washington's Administrative Office of the Courts) between July 1999 and January 2011 in which the offender pled guilty and was sentenced during the Friday sentencing block by a Superior Court judge (provided that judge has sentenced at least 150 cases in the data). Only cases where offenders had at least the full one, two, or three year window (depending on the specification) during which they were at risk of recidivism are used. Results are from panel regressions using year-week fixed effects, linear, quadratic, and cubic controls for offender age, and dummy variables to control for sex, race, whether the offender had at least one prior nonviolent conviction, whether the offender had at least one prior violent conviction, whether the offender had at least one prior drug conviction, and whether the offender had at least one prior serious violent conviction. Recidivism is defined as having been resentenced in Washington State within 1, 2, or 3 years of offender’s estimated release date. In specifications in which one-year recidivism is the dependent variable, there are 25 individual judge effects. In specifications where two-year recidivism is the dependent variable, there are 24 individual judge effects. In specifications where three-year recidivism is the dependent variable, there are 22 individual judge effects. F-stat and p-values come from tests of joint significance of judge dummy variables when those dummy variables are used in the regression model in place of the judge fixed effect variable. Robust standard errors are clustered by judge. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 5. Two-Stage Least Squares Regressions of Recidivism Variables on Total Sentence**

****

Notes: Sentencing, conviction, and criminal history data come from Washington's Sentencing Guidelines Commission. Data includes observations involving sentences issued at the Kent Regional Justice Center in King County Washington (as indicated in data from Washington's Administrative Office of the Courts) between July 1999 and January 2011 in which the offender pled guilty and was sentenced during the Friday sentencing block by a Superior Court judge (provided that judge has sentenced at least 150 cases in the data). Only cases where offenders had at least the full one, two, or three year window (depending on the specification) during which they were at risk of recidivism are used. Results are from panel regressions using year-week fixed effects, linear, quadratic, and cubic controls for offender age, and dummy variables to control for sex, race, whether the offender had at least one prior nonviolent conviction, whether the offender had at least one prior violent conviction, whether the offender had at least one prior drug conviction, and whether the offender had at least one prior serious violent conviction. Recidivism is defined as having been sentenced in Washington State within 1, 2, or 3 years of release. In specifications where one-year recidivism is the dependent variable, there are 25 individual judge dummy variables used as instruments. In specifications where two-year recidivism is the dependent variable, there are 24 individual judge dummy variables used as instruments. In specifications where three-year recidivism is the dependent variable, there are 22 individual judge dummy variables used as instruments. Robust standard errors are clustered by judge. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 6. LIML and JIVE Regressions of Recidivism Variables on Total Sentence**

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Notes: Sentencing, conviction, and criminal history data come from Washington's Sentencing Guidelines Commission. Data includes observations involving sentences issued at the Kent Regional Justice Center in King County Washington (as indicated in data from Washington's Administrative Office of the Courts) between July 1999 and January 2011 in which the offender pled guilty and was sentenced during the Friday sentencing block by a Superior Court judge (provided that judge has sentenced at least 150 cases in the data). Only cases where offenders had at least the full one, two, or three year window (depending on the specification) during which they were at risk of recidivism are used. Results are from panel regressions using year-week fixed effects, linear controls for offender age, and dummy variables to control for sex, race, whether the offender had at least one prior nonviolent conviction, whether the offender had at least one prior violent conviction, whether the offender had at least one prior drug conviction, and whether the offender had at least one prior serious violent conviction. Recidivism is defined as having been sentenced in Washington State within 1, 2, or 3 years of release. In specifications where one-year recidivism is the dependent variable, there are 25 individual judge dummy variables used as instruments. In specifications where two-year recidivism is the dependent variable, there are 24 individual judge dummy variables used as instruments. In specifications where three-year recidivism is the dependent variable, there are 22 individual judge dummy variables used as instruments. JIVE results are calculated using JIVE1 technique as described in Angrist, Imbens, and Krueger (1999). Robust standard errors are used, and are clustered by judge in LIML specifications. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 7. Two-Stage Least Squares Regressions of Recidivism, with Judge Dummies as Instruments, Subsamples Based on Offender Score of Defendant**

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Notes: Sentencing, conviction, and criminal history data come from Washington's Sentencing Guidelines Commission. Data includes observations involving sentences issued at the Kent Regional Justice Center in King County Washington (as indicated in data from Washington's Administrative Office of the Courts) between July 1999 and January 2011 in which the offender pled guilty and was sentenced during the Friday sentencing block by a Superior Court judge (provided that judge has sentenced at least 150 cases in the data). Only cases where offenders had at least the full one, two, or three year window (depending on the specification) during which they were at risk of recidivism are used. Results are from panel regressions using year-week fixed effects, linear, quadratic, and cubic controls for offender age, and dummy variables to control for sex, race, whether the offender had at least one prior nonviolent conviction, whether the offender had at least one prior violent conviction, whether the offender had at least one prior drug conviction, and whether the offender had at least one prior serious violent conviction. Recidivism is defined as having been sentenced in Washington State within 1, 2, or 3 years of release. In specifications where one-year recidivism is the dependent variable, there are 25 individual judge dummy variables used as instruments. In specifications where two-year recidivism is the dependent variable, there are 24 individual judge dummy variables used as instruments. In specifications where three-year recidivism is the dependent variable, there are 22 individual judge dummy variables used as instruments. Robust standard errors are clustered by judge. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Appendix Table 1: Judge Fixed Effect Values for Total Prison Sentence (in Months)**

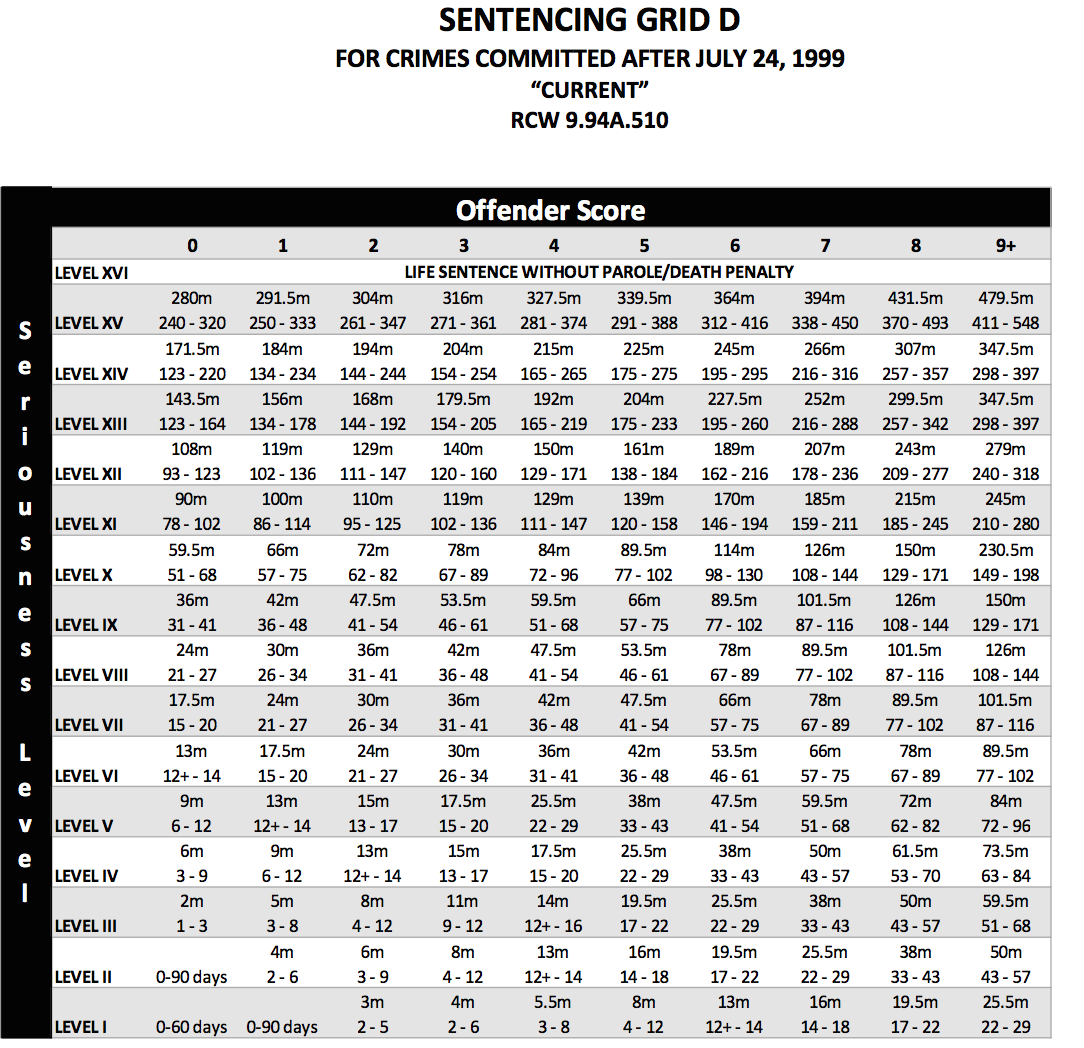


Notes: Dependent variable is total prison sentence in months. Sentencing, conviction, and criminal history data come from Washington's Sentencing Guidelines Commission. Data includes observations involving sentences issued at the Kent Regional Justice Center in King County Washington (as indicated in data from Washington's Administrative Office of the Courts) between July 1999 and January 2011 in which the offender pled guilty and was sentenced during the Friday sentencing block by a Superior Court judge (provided that judge has sentenced at least 150 cases in the data). Only cases where offenders had at least one during which they were at risk of recidivism are used. Reported estimates are from panel regressions of total sentence on individual judge dummy variables using year-week fixed effects, linear, quadratic, and cubic controls for offender age, and dummy variables to control for sex, race, whether the offender had at least one prior nonviolent conviction, whether the offender had at least one prior violent conviction, whether the offender had at least one prior drug conviction, and whether the offender had at least one prior serious violent conviction. Twenty five individual judge dummies are used in the regression. For ease of comparison, the judge with the smallest estimated fixed effect is omitted from the table. Robust standard errors are used.

**Appendix Table 2: Leave-Out Prison Sentence First-Stage**

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Notes: Dependent variable is total prison sentence in months. Sentencing, conviction, and criminal history data come from Washington's Sentencing Guidelines Commission. Data includes observations involving sentences issued at the Kent Regional Justice Center in King County Washington (as indicated in data from Washington's Administrative Office of the Courts) between July 1999 and January 2011 in which the offender pled guilty and was sentenced during the Friday sentencing block by a Superior Court judge (provided that judge has sentenced at least 150 cases in the data). Only cases where offenders had at least one year during which they were at risk of recidivism are used. Reported estimates are from panel regressions of total sentence on individual judge dummy variables using year-week fixed effects, linear, quadratic, and cubic controls for offender age, whether the offender had at least one prior nonviolent conviction, whether the offender had at least one prior violent conviction, whether the offender had at least one prior drug conviction, and whether the offender had at least one prior serious violent conviction. Twenty five individual judge dummies are used in the regression. Robust standard errors are used.

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3. For an impression of the difficulties in calculating criminal history, see In Re Personal Restraint of Lavery, 111 P.3d 837 (Wash. 2005), wherein the Washington Supreme Court struggled how to apply a conviction for robbery under Montana statute to Washington’s sentencing regime and discussing several prior decisions. Washington State Sentencing Guidelines Manual 2011 collects key case law as well (pgs. 15-19). [↑](#footnote-ref-3)
4. Cases in which there is a discrepancy between the Sentencing Guidelines Commission data and the Administrative Office of the Courts data related to the identity of the sentencing judge were dropped from the analysis. [↑](#footnote-ref-4)
5. Version 2.0, Revised April 2013 [↑](#footnote-ref-5)
6. In addition, we only included judges who sentenced more than two people on a Friday to make sure that the judge was actually on the sentencing rotation. [↑](#footnote-ref-6)
7. Among these exclusions are judges who primarily adjudicated cases at the Seattle location or served in a capacity different from that of Superior Court Judge (e.g., Drug Court Judge or Commissioner), judges in years where they sentenced fewer than 25, and judges who sentenced fewer than 150 cases in total. Because of differences in sentencing procedures, domestic violence cases are also excluded from the sample. The roster of Superior Court Judges was taken from Washington State Yearbooks issued between 1998 and 2011. [↑](#footnote-ref-7)
8. Abrams’s measure of recidivism is based on observing whether the offender reappears in the public defender database, which does not imply resentencing. His first-year recidivism rate is 20% compared to ours of 12%. Kuzeimko uses “return to prison within three years” which involves parole violations, but reports a 25% rate which is similar to ours. [↑](#footnote-ref-8)