Title

Building Criminal Capital vs Specific Deterrence: The Effect of Incarceration Length on Recidivism

Author

Abram, David S.

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The Effect of Incarceration Length on Recidivism

David S. Abrams*

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Abstract

In evaluating the efficacy of most modern criminal justice systems, a vital relationship to understand is that between incarceration length (and likelihood) and recidivism. Because most previous attempts to estimate this relationship suffer from omitted variables bias, even the sign is unknown. In this paper, I build on previous work identifying substantial heterogeneity in attorney ability in a public defender office with random case assignment. I make use of this variation to address the omitted variables problem by instrumenting for sentence length and incarceration rate using the randomly assigned public defender. A negative relationship between recidivism and sentence length goes away when instrumenting for sentence. Similarly, a positive and statistically significant relationship between recidivism and incarceration becomes insignificant in the IV regressions. However the regression results do not reveal the full story, as the relationships are rather nonlinear. A graphical examination reveals a negative relationship between recidivism and sentence length and also recidivism and incarceration rate, particularly for shorter sentences and lower incarceration rates. In addition, longer sentences tend to lead to more severe crimes upon offender release. Put together, these findings provide some evidence for a mild specific deterrent effect, but one that rapidly diminishes.

JEL Classifications: J24, K14, K42

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Email: dabrams@law.upenn.edu

I. Introduction

The growth in incarceration rate in the United States in the last several decades of the 20th century and early 21st century is well known. Much has been written about the efficacy, justice, and efficiency of this approach to crime from a myriad of methodological perspectives.[1] In this paper I attempt to understand one piece of the incarceration puzzle: the impact of sentence length and likelihood of incarceration on recidivism, often called specific deterrence.

The relationship between incarceration and deterrence is of interest to economists, as it is one of the components in determining the welfare effects of incarceration.[2] The end goal is to answer the question: In what situations is incarceration welfare-enhancing? This project may be thought of as a large cost-benefit calculus, in which the costs include the capital expenditures on incarceration, the value of freedom, and potential criminogenic effects of imprisonment. Benefits from incarceration include crime reduction due to incapacitation, general deterrence, and specific deterrence.[3]

This paper focuses on the last of these relationships, incarceration and specific deterrence. The questions I explore are "What is the impact of a marginal increase in incarceration length on future recidivism?"[4] and "what is the impact of a marginal increase in an individual's likelihood of incarceration on future recidivism?" While simple to state,

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[1] Cite several background papers here.

[2] I have investigated the costs and benefits of other aspects of the criminal justice system elsewhere. This includes Abrams and Rohlfs (2010) on optimal bail-setting and the value of freedom; Abrams (2010) on general deterrence; Abrams, Bertrand and Mullainathan (2011) on defendant race and sentencing.

[3] One may also include retribution and rehabilitation in the calculus, although these are much harder values to estimate.

[4] I will sometimes refer to the derivative of recidivism with respect to sentence length as the magnitude of specific deterrence or simply specific deterrence.

there is substantial complexity in these questions, and as I show in this paper, in the answers.

Defining recidivism is a hard enough problem that a substantial amount of work has been devoted just to this topic (Maltz, 2001). I engage in some of the complexity of this problem in several ways. I use several alternative definitions of recidivism, including binary indicators with varying time windows, as well as continuous variables that indicate the severity of the recidivating crime. I apply hazard models and additionally examine the effects on recidivism of both the intensive and extensive margin of incarceration.

Perhaps most confounding to much previous research is the fact that there are almost certainly unobservable variables that are correlated with both sentence length and recidivism. It may be the case that defendants who are relatively "bad" in unobservable ways are more likely to both get longer sentences and recommit offenses. Under this model, an inability to control for "badness" will yield upward-biased estimates of the effect of sentence length on recidivism. Alternatively, judges may be influenced by the likelihood that offenders will be apprehended, and thus individuals sentenced to longer durations of incarceration may appear to have lower recidivism rates in the cross section. In previous work (eg Spohn and Holleran, 2002; Gottfredson, 1999) attempts were made to account for these concerns through synthetic control groups, covariate balancing, and other matching techniques based on observables. But there is substantial literature in economics and elsewhere that illustrates the difficulties in using even the most sophisticated matching techniques (Lalonde, 1986; Dehejia and Wahba, 1998; Dehejia, 2000).

Another challenge posed by the research questions is that the relationships investigated are complicated and may not be well-suited to a single coefficient as in an OLS or probit regression. I estimate both OLS and IV regressions in this paper, using different assumptions about the sentencing distribution. Depending on the model for the sentencing distribution, I find either a negative relationship, but more frequently no significant relationship between recidivism and sentence length. The relationship between the variables is not always monotonic and I further explore it through non-parametric techniques.

In the IV regressions, I find a statistically insignificant relationship between recidivism and sentence length. But this masks a negative relationship for relatively low sentence lengths and a flat relationship thereafter. Using the instruments to predict any incarceration also yields insignificant coefficients. Again, though, there appears to be a declining relationship for low probabilities of incarceration which flattens out as probability of incarceration exceeds about 40%.

When taking severity of the recidivating crime into account, increased sentence length predicts more severe recidivism. Taken together, these results tell a story of a moderate deterrent effect of incarceration and sentence length, but one with the perverse effect that the fewer recidivating crimes tend to be more severe. These phenomena are quite consistent if criminals inclined to commit less severe offenses are just those who are more deterred.

The remainder of the paper is organized as follows. In Section II, I review some of the more recent contributions in the long literature on incarceration and recidivism.

Section III provides background information on the Public Defender's office in Clark County and describes the data set. In Section IV, I present the empirical specifications and the main results. Section V concludes.

II. Background on Incarceration and Recidivism

Understanding how punishment affects recidivism is essential to a rational criminal justice policy. So it should not be surprising that a number of attempts have been made to estimate this relationship. A large fraction of these studies potentially suffer from omitted variables bias, so I focus primarily on recent economic studies that attempt to explicitly account for it. They do so with a variety of approaches: matching techniques, regression discontinuity, instrumental variables, natural experiments, and even a field experiment.

Two relatively recent reviews (Nagin, Cullen & Jonson 2009; Bushway & Paternoster 2009) both discuss recent empirical work on recidivism and specific deterrence (the former is a much more extensive survey). They cover a range of studies and both note the lack of consensus on the topic. One important reason for the lack of consensus is that studies often seek to answer slightly different questions. There is variation in the definition of recidivism (re-arrest, reincarceration, etc), whether the intensive or extensive margin is the focus, the size of the potential recidivism window, the initial law enforcement action whose effect is being investigated, and the geographic locale.

A recent study by Nieuwbeerta, Blokland, Nagin (2009) uses a matching technique to attempt to detect the effect of first incarceration on recidivism in the Netherlands. They use propensity score matching within criminal trajectory groups to synthesize control groups in order to estimate the causal effect of incarceration. The authors find that the experience of first incarceration substantially increases the likelihood of future criminality (over twice as high for some individuals). The two primary critiques of the study are the standard critique of matching approaches (the potential that omitted variables still drive results) and the generalizability given the substantially different criminal justice system in the Netherlands.

Much other recent work addresses the OVB problem more squarely, using IV or quasi-experimental techniques. Kuziemko (2007) uses two approaches to estimate the magnitude of specific deterrence using data from Georgia state prisons. A natural experiment that led to the release of 901 prisoners in 1981 provides the potential to compare prisoners with similar sentences and varying lengths of time served, where the variation is determined exogenously by when their sentences began. The identifying assumption hinges sharply on there being no time trend or other direct relationship between date of incarceration and likelihood of recidivism. Using this approach and another making use of prisoner risk assessments she finds a substantial negative effect of sentence length on recidivism: a 7% decline in recidivism for every extra month served.

Bushway and Owens (2010) take a behavioral approach to understanding the impact of sentence lengths on crime. A 2001 law change in Maryland provides them with a natural experiment whereby a natural reference for sentence length is changed, but not the actual sentences. While not directly comparable to other studies examined, the authors find that shorter sentences relative to a reference point lead to higher recidivism rates.

Drago, Galbiati, and Vertova (2009) examine a natural experiment in Italy where all inmates incarcerated in 2006 received up to a 3 year sentence commutation. While the experiment is dramatic and substantial deterrence is detected, the interpretation is difficult. Since prisoners with longer sentence reductions also face higher penalties upon rearrest, the net effect must be a combination of specific and general deterrence. If, as some other studies suggest, specific deterrence is negative (less crime after longer incarceration) then general deterrence must dominate here since defendents who received greater commutations had larger drops in recidivism. An even more recent paper using a European natural experiment (Maurin and Ouss 2010) uses the 1996 Bastille Day pardon in France to estimate the effect of sentence reductions on recidivism. They find that a greater sentence reduction via the pardon leads to an increase in expected future recidivism.

Hjalmarsson (2009) uses a regression discontinuity approach made possible by the Washington State juvenile sentencing guidelines. She finds that juveniles who are incarcerated have a lower probability of recidivism than those that receive a local (non-incarceration) sanction. This is in contrast to the study by Nieuwbeerta, et al discussed above which found a positive effect of first incarceration on recidivism in the Netherlands.

Four recent papers take an instrumental variables approach to the question of specific deterrence, three of which use very similar instruments. Loeffler (2005) uses judge-specific sentencing tendencies and random case assignment to obtain exogenous variation in sentence length. This study, using data from Essex County, NJ, also focuses on the extensive margin and finds that incarceration substantially reduces the likelihood of recidivism.

A virtually identical approach to that of Loeffler is adopted by Berube and Green (2007) and Green and Winik (2010). The Green co-authored papers both make use of data from the DC superior court and both find little relationship between sentence length and recidivism. They find a negative and statistically significant relationship between incarceration length and recidivism when doing ordinary least squares regressions. But the sign becomes positive and the effect statistically insignificant when 2SLS or LIML estimation is performed (although the standard errors are larger).

The closest approach to that used in this paper is found in Turner (2009). Using the Clark County data set used here, Turner instruments for sentence length using random assignment to attorney and heterogeneity in attorney performance. She finds a mostly insignificant relationship between sentence length and recidivism, although one that varies somewhat by the recidivism window.

Perhaps the most significant contribution to the literature on specific deterrence comes from a little-known[5] field experiment performed by the California department in the 1970s (Berecochea and Jaman 1981). The authors randomized a 6-month early release among felons serving time in 1970. They compare one and two year recidivism rates between the treatment and control groups and find no significant difference. Taken together, these studies show that there is not consistent evidence on the effect of imprisonment on future recidivism.

[5] Thanks to Phil Cook for making it known to the author.

III. Criminal Justice in Clark County, Nevada

The data for this paper comes from two sources, both in Nevada.[6] The first data source is the Clark County Public Defender (CCPD)[7] and includes data on defendants represented by the CCPD's office from 2001 – 2008. The initial data set was collected in order to investigate the impact that attorneys have on case outcomes (Abrams & Yoon, 2007; Abrams & Yoon, 2009). The key feature of the CCPD is that the office uses random assignment of cases to public defenders for almost all cases[8]. In this paper, I exploit this random assignment and the substantial variation in public defender skill to instrument for sentence length.

The CCPD is in the minority of public defender offices in that it uses random case assignment and also makes use of vertical representation. This ensures that a single public defender handles almost all cases from beginning to end. The office uses random assignment partly as a recruiting tool, to entice ambitious young attorneys with the prospect of handling interesting cases much more quickly than in other offices. The system is also seen as being more equitable than the hierarchical one used in most offices. For this paper, what is important is its consistent application. In Abrams and Yoon (2007 & 2009) it was empirically verified that observable case characteristics appear to be randomly assigned across public defenders.

The second data set comes from the Nevada Department of Corrections (NDOC) and contains data on prisoners held in Nevada from January, 2006 – October, 2009. The

[6] The Nevada DOC data is still being collected.
[7] Las Vegas contains the vast majority of the population of Clark County.
[8] There are several classes of exceptions, including public defenders in their first year, certain sex crimes, and capital murder cases. For further detail see Abrams and Yoon (2007).

offender information includes demographic information and may include beginning and end dates of the prison term.

Table 1 summarizes the CCPD data set. In addition to the randomly assigned data, a substantial number of additional observations are included in order to calculate recidivism rates. These observations (which bring the initial total to 110,282 observations) often contain missing data, but are taken as indicators of recidivism as long as the defendant is identifiable. Besides the recidivism rates, the data in Table 1 refers only to the randomly assigned cases (n =17,471), which include only the first appearance of an offender in the data set. This implies that the mean sentences used will be lower than the mean of the full data set.

In Nevada, defendants receive a sentence range from the judge, indicating a minimum and maximum. In practice, the vast majority of defendants are incarcerated for a period of time much closer to the minimum sentence length than the mean.[9] For this reason, the principle measure of sentence length I use is the minimum. The mean minimum sentence length is 4.6 months (including zeroes), although about half the sentences are zero. The instrument, when constructed using OLS in the first stage, has an identical mean by construction, but substantially lower standard deviation. When the first stage is a tobit regression the mean of the predicted (minimum) sentence is a bit higher than the underlying mean. These are due to the non-normality of the sentencing distribution and in particular its long right tail (Figure 1). The non-normality is to be expected in part due to truncation at zero. One simple potential fix would be to perform a

[9] In some cases, defendants may be paroled even before serving the minimum sentence length, since a 2007 legislative reform. See Chapter 525, *Statutes of Nevada 2007.*

log transformation, as the distribution of log sentence is much closer to normal. However this necessarily discards all of the zero sentences, which make up the majority of the data. This would invalidate the instrument as data would be selected based on the outcome.[10] For most of the specifications, I use the main instrument based on Public Defender identity, and this distribution may be seen in Figure 2.

Due to the large number of zero sentences, conditioning on the sentence[11] being non-zero yields a mean of 9.3 months, substantially higher than the unconditional mean. By comparison the mean unconditional sentence (the average between the minimum and maximum sentencing range) is 8.5 months. Only 50% of the offenders in the data set receive a sentence with some length of incarceration.[12]

Demographically, the defendants resemble those in many other criminal justice populations: heavily male and minority. 80% of the population is male, with 31% Black and 22% Hispanic (these classifications are not mutually exclusive). The average offender age in the data set is almost 33 years old, but there is substantial variation, as can be seen in Figure 3.

Crime type is broken down into four categories, with property crimes comprising the largest category, containing 40% of the offenders.[13] Drug crimes make up 24% of the

[10] One potential way to use this technique may be to find a subset of data for which all defendants receive some sentence, and thus taking the log does not discard any data based on the dependent variable. However, it is likely that the untransformed data will be close to normally distributed for just such a subset, making the transformation unnecessary.
[11] From here forward I use sentence to indicate the minimum sentence, unless otherwise noted.
[12] Probation (or suspended sentences) is counted as a zero sentence length in this data set.
[13] The property crime category is equivalent to the Embezzlement, Fraud, and Theft (EFT) category used in other crime datasets.

data, and violent crime an additional 16%. Sex crimes account for 2% of the data and 19% do not fall into any of these categories.

Recidivism is the primary dependent variable and is defined here as reappearance in the Clark County Public Defender data set within a fixed amount of time from expected release from incarceration. This measure of recidivism is somewhat intermediate between arrest and imprisonment data. It indicates that prosecution has progressed to the point where a PD has been assigned, but many cases will not result in incarceration or even conviction. The assumption is that this is an unbiased measure of criminal activity. Specifically, the n year recidivism dummy variable is 1 if the offender appears in the data within the n year period immediately following release from incarceration and zero if there is no appearance within this window. The date of release from incarceration is estimated as the case record data plus the minimum sentence length. In order to avoid truncation effects, recidivism dummies are not calculated for offenders whose sentences end within n years of the end of the data set.

The recidivism rates estimated in this population are in line with those found elsewhere, although the populations and method for computing recidivism vary. The one, two, and three year recidivism rates for the sample are 21.2%, 34.9% and 47.5%, respectively. These rates are a bit higher than those found for federal prisoners in (Chen & Shapiro, 2007) of 16.4%, 27.5% and 37.0%. Another point of comparison is the United States Sentencing Commission FY1992 Recidivism Sample for which the two year recidivism rate is 22.1% (USSC 2003). In the next section I investigate to what extent these recidivism rates are affected by sentence length.

IV. The (Complicated) Relationship between Sentence Length and Recidivism

I first investigate the relationship between sentence length and recidivism by running a naïve cross-sectional linear probability model as in (1):

$$(1)\ recid_{dp} = \alpha + \beta sent_d + X_d + mo_t + \epsilon_d$$

Here *recid* is a dummy variable that is 1 if defendant d recidivates within p years of release from incarceration, zero if the defendant does not recidivate in this window, and not observed otherwise. The independent variable of interest is sentence length to which defendant d was initially sentenced. An array of case and defendant-specific controls, including judge, case type, defendant age, defendant race, defendant sex, as well as monthly time dummies are included as regressors. This is also estimated using a probit specification, where Φ is the cumulative normal distribution.

$$(2)\ p(recid_{dp}) = \Phi(\alpha + \beta sent_d + X_d + mo_t + \epsilon_d)$$

The main problem with this approach is that sentences are not randomly assigned to defendants. In fact it is likely that judges determine sentences based in part on characteristics unobservable to the econometrician that are also correlated with likelihood of recidivism. That is, $E[\varepsilon_d|sent_d] \neq 0$ and is likely > 0. The most likely such characteristic is some underlying criminal propensity, which is orthogonal to all observable control variable, but detectable by the judge and correlated with sentence length. Criminal propensity will also certainly be predictive of recidivism, and thus failure to account for it will result in an upward biased coefficient on sentence length.

The random assignment of Public Defenders to cases in the CCPD constitutes a natural experiment in sentence length assignment. Since PD's vary substantially in individual ability, and assignment is random, defendants effectively face a partial lottery over sentence lengths. I use this lottery to obtain an unbiased estimate of the effect of sentence length on recidivism using an IV regression.

An important methodological challenge still remains: how to best model the sentencing distribution. Sentencing can be modeled as a two stage process, composed of the decision of whether to incarcerate and then what magnitude sentence to assign, for those that are non-zero. This type of process leads naturally to considering some sort of censored distribution, like the tobit. Anderson, Kling and Stith (1999) use a zero-inflated negative binomial model to capture the fact that sentences usually are chosen as an integral number of months. In results not reported in the paper, the zero-inflated negative binomial was found to fit the sentencing distribution similarly to the tobit, and so the analysis was run using the latter for simplicity.

For the first stage regression, I instrument for sentence length using a full set of public defender dummy variables (PD_a) as in (3):

(3) $sent_d = \pi_0 + \pi_a PD_a + X_d + mo_t + \nu_d$

The second stage uses the predicted values of sentence length to get exogenous variation in (4), relying on the identifying assumption that $E[\nu_d | PD_a]$ and $E[\varepsilon_d | PD_a] = 0$.

(4) $recid_{dp} = \alpha + \beta \widehat{sent}_d + X_d + mo_t + \epsilon_d$

Table 2 reports results from the OLS regressions as well as the first stage from the IV regression. The first column reports results from the base linear probability model described in equation 1. There is a statistically significant and negative coefficient on sentence length, indicating that in the cross-section an extra month sentence length is associated with a decreased recidivism rate of about 0.7 percentage points. Figure 5a displays the relationship graphically. Off the mean 18% one year recidivism rate found in Table 1, this is a reduction of around 3.3%. Of course, this is likely to suffer from omitted variables bias, as discussed before. Before I attempt to address this concern, I examine the relationship of the control variables and recidivism as well as the other specifications.

The second specification uses a probit model rather than linear probability. The reported marginal effect of -.0079 is statistically indistinguishable from the coefficient in the linear probability model. I run probit versions of all of the other models as well and the results are all consistent with the linear probability model, so they are omitted from the tables.

One of the most difficult empirical challenges with determining recidivism rates is how to properly control for age. It is well known (Bushway and Piehl 2007) that criminality declines over time. In fact one can see trend clearly in this data set, as in Figure 4. This presents a major challenge to the econometrician. Even if a valid instrument is found for sentence length, the treatment will necessarily imply that those defendants who are represented by worse attorneys (and hence get longer sentences) will be older on average at time of release. This then has a direct negative impact on the magnitude of recidivism.

Thus we now have reasons why the naïve estimate may be upward biased (omitted variables) or downward biased (the criminality age profile, which may also simply be an omitted variable). To address the former concern, I use the IV specification described in equations 3 and 4 above. To address the latter, I take three different approaches: First, I control for age at offense. Since I do not know the exact release date from incarceration in the CCPD data set, this is at least likely to be strongly correlated.[14] Second I use the estimated release date, calculated as the initial trial date plus the minimum sentence. Finally, I use the empirical age profile in Figure 4 to normalize the recidivism variable. Thus an observation with recidivism where the offender is 50 will receive substantially more weight than when the offender is 20.

In the cross-section, adding the age controls do increase the coefficient on sentence length, but only very slightly (Table 2, column 3). The point estimates are somewhat larger in absolute magnitude for the 2 and 3 year measures of recidivism, but as a fraction of base recidivism rates they are even smaller, and lose statistical significance for the 3 year rate. For the 3 year measure the cross-sectional regression indicates an approximate 3.5% decline in recidivism for a month increase in minimum sentence (see Figure 6a). Since the sentencing distribution is very skewed, the standard deviation is not necessarily very informative. Still using the value of 11.1 months translates to a substantial 39% decline in recidivism associated with a one standard deviation increase in sentence length. An examination of the control variables indicates little consistency that is statistically significant across most specifications, except that Black and male offenders tend to have higher recidivism rates.

[14] In future work incorporating Nevada DOC data I will know the exact date of release.

Table 3 presents the main regression results from the IV strategy, although as I will discuss shortly, linear regression tells a very incomplete story in this context. The results from the first stage (column 1) are unsurprising: male defendants tend to receive longer sentences. Weak instruments may be a concern in this data set. I test for them by looking at the change in R^2 when the instruments are added to the regression (a relatively small 1.5 percentage points). I also run an F-test on the joint significance of the instruments. The value of around 6 is less than the rule-of-thumb threshold of 10. However, these tests may be inappropriate given the non-normality of the errors.

The main reason that IV is important in this context is to avoid the unobserved variables problem. I argued above that the unobserved criminal propensity is likely to bias the coefficient on sentence upward in this context. In column 2 of Table 3 we find evidence for this argument, as the coefficient on sentence length is almost twice the magnitude as that in the OLS regression, where the first stage is a linear model (see also Figure 5b).[15] Adjusting for the recidivism-age profile does little to change the coefficient. Because of the potential weak instruments concern, I also perform a LIML IV regression which should be consistent even with weak instruments. The point estimate from this specification is negative, but smaller in magnitude and statistically insignificant. When looking at two or three year recidivism (see Figure 6b), the coefficient remains negative and larger in magnitude than OLS, and statistically indistinguishable from zero. As in the OLS regressions, offender age is significantly negatively related to recidivism. Black and male offenders have higher recidivism rates as well, which was also seen in the OLS.

[15] One ramification of using a linear model for the first stage is that predicted sentence can be negative. This leads to the unusual aspect of Figures 5b and 6b having some negative-valued sentences. The slope of the curve is unaffected by axis scale and this is the parameter of interest.

When using a tobit model for the first stage, the results differ somewhat, but are consistent with the OLS first stage results. The coefficient on sentence length is more positive for each of the three time windows examined, but all are still statistically insignificant. An examination of Figures 5c and d and 6c provides the visual analog of these regressions. While the IV approach is key to getting an unbiased measure of specific deterrence, the coefficients are not meaningful if the relationship between deterrence is not linear, as has been assumed thus far. Rather than attempting a more complicated parametric or semi-parametric approach, the figures provide more informative non-parametric evidence.

Figures 5 and 6 show a complicated relationship between recidivism and sentence length, one that is not well-captured in a monotonic regression as reported in Tables 2 and 3. There is a generally negative relationship between recidivism and sentence length, although one that does not appear very linear (which may explain the insignificant regression results). Both figures 5c and 6c seem to indicate that while longer sentences may reduce recidivism for short sentence lengths, the effects rapidly peter out.

I next examine the extensive margin: the effect of a change in likelihood, rather than length, of incarceration on future recidivism. The results are reported in Table 4 and Figures 7a and 7b. As with the intensive margin, the OLS results appear to be upward-biased. This seems like a natural ramification of judges being more likely to sentence the unobservably crime-prone to incarceration. Here the point estimates for all time ranges are positive and statistically significant for OLS, but insignificant for IV. There may still be a substantial positive impact of incarceration on recidivism of up to 10 percentage points, but the large standard errors in the IV specifications make this impossible to distinguish from a null effect.

To this point all recidivating crimes have been treated equally. From a policy perspective, one may want to weight recidivating crimes according to their severity. In Table 5 and Figures 8a and 8b, I perform the same analysis, but replace the binary recidivism dependent variable with the expected sentence length for the recidivating crime. This should be a reasonable measure of the severity of the recidivating crime. The results show that longer sentences lead to an increased severity of recidivating crime. The regression coefficients are not statistically significant, although the standard errors are quite large. The visual evidence in Figures 8a and 8b seem to support the positive relationship.

Since almost all felonies are included in the data set, it is possible that some of the null results are due to heterogenous effects by crime type, crime severity, or other characteristics. Since the random assignment of cases to PD's is essential to the identification strategy, it is not possible to examine subsets of the data by a characteristic like crime severity (as measured by sentence length) that is an outcome of the trial process. But since all types of cases are randomly assigned, it is possible to perform the analysis for a particular type of crime. Unfortunately, the only homogenous crime category for which there is potentially sufficient data is drug crimes. This analysis is reported in Figures 9a, 9b, and 9c. The regression results are omitted, but convey the same information as the figures: namely that there is no significant relationship between sentence length and recidivism for drug crimes. The figures convey the lack of power in this analysis, so even a substantial coefficient might not be detected by this analysis.

V. Conclusion

Seen together, the data tells a complicated story, but one that helps to explain the divergence of previous findings. Loeffler (2005) reports that of the handful of papers that have previously tried to address the omitted variables concern, 6 reported no deterrent effect and 4 reported a positive effect. Depending on which part of the distribution one focuses on and the measure of recidivism, this paper could find a positive, negative, or no effect.

One contribution of this paper is the ability to examine the effect on recidivism over a large range of the sentencing and incarceration distributions. Almost all previous work has focused either on a narrow subset, or assumed a homogeneous effect. But an examination of the figures produced here show that this assumption is incorrect. This paper further shows the importance of the IV approach when there is serious concern about omitted variables.

Running an OLS or probit of recidivism on sentence length yields a negative coefficient for all durations, and one that is significant for 1 year recidivism. An OLS or probit of recidivism on probability of incarceration yields a positive, and statistically significant coefficient. It is striking that the sign of the coefficient is opposite that on sentence length. In the absence of OVB, this would mean that going to prison makes one more likely to recidivate (perhaps evidence for criminal capital formation) but the longer spent in prison makes one less likely to recidivate (perhaps explained by updating of prisoners as to the likelihood of being sentenced to prison and/or the unpleasantness thereof).

By instrumenting for sentence length using randomly assigned public defenders, I am able to obtain a causal estimate of the impact of sentence length and incarceration on recidivism. These analyses show that the OLS point estimates are upward biased, although the IV results are statistically insignificant. An examination of the figures 5-7 shows that this is likely due to the fact that the relationship is nonlinear. While longer sentences may reduce recidivism for short sentence lengths, the effects rapidly diminish.

The other main results make use of the severity of recidivism as the dependent variable. Here I find that longer sentences increase the expected severity of the recidivating crime. This complicated relationship lends at least some support to theories of specific deterrence. The complexity of these findings is important to understand better with an eye toward making better policy decisions.

Appendix A: Data Cleaning Procedure

I begin with a dataset of over 140,000 charge-level observations from the Clark County Public Defender's Office, but only a fraction of them are made use of in the analysis. This Appendix describes the procedure employed to produce the usable dataset.

Approximately 12,500 observations do not contain the name of the lead public defender on the case. Lacking the instrument, these observations are dropped. The initial dataset contains multiple charges per case. For simplicity, only the most severe charge is examined for each case, which eliminates approximately 24,000 observations. First year public defenders do not receive cases through the random assignment process used with other PD's, and dropping their cases eliminates about 20,500 observations.

The key to the identification strategy in this paper is the random assignment of cases to public defenders. Sometimes multiple attorneys may work on a case, making it difficult to know which one to attribute a case to. These cases are eliminated, which drops about 44,500 observations. Certain case types are not handled by random assignment. For example, probation violations are usually handled by the same attorney who handled the underlying case. Eliminating these observations reduces the number by about 13,000. Finally, to ensure that each public defender has a minimum of 50 total cases, I eliminate about 1,500 cases handled by those PD's with fewer than the minimum. This leaves a total of 28,803 observations for which the cases are randomly assigned to public defenders. These cases represent X defendants.

Figure A1a (Alternative Release Dates)

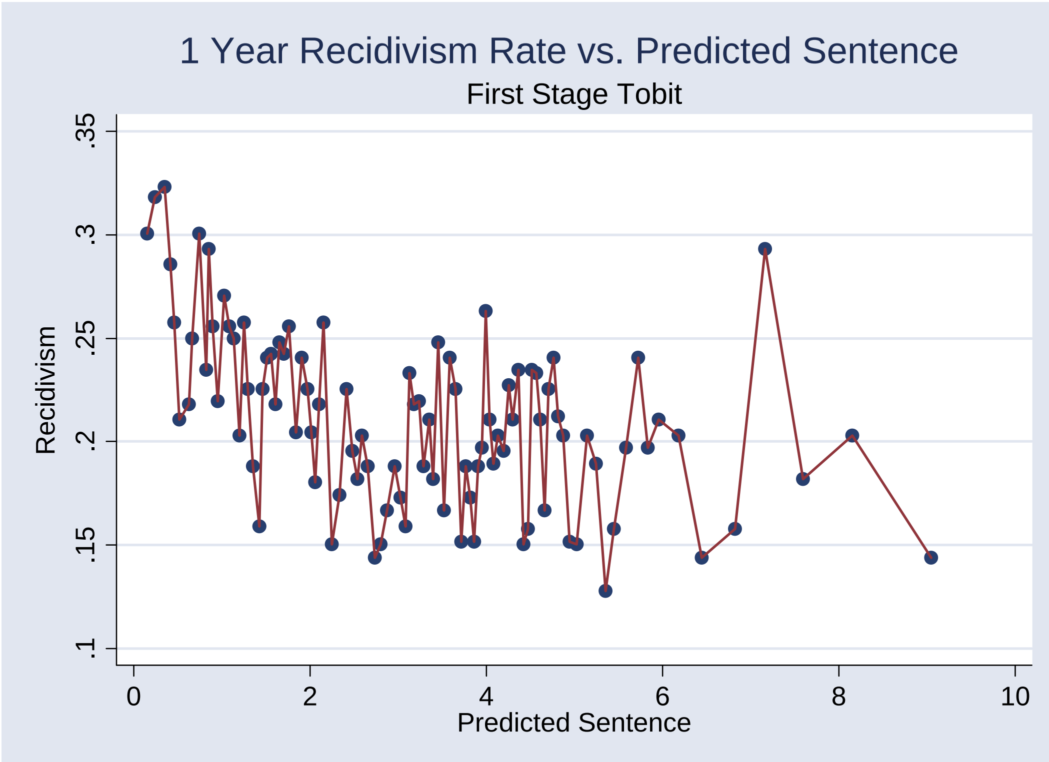

Figure A1b (Alternative Release Dates)

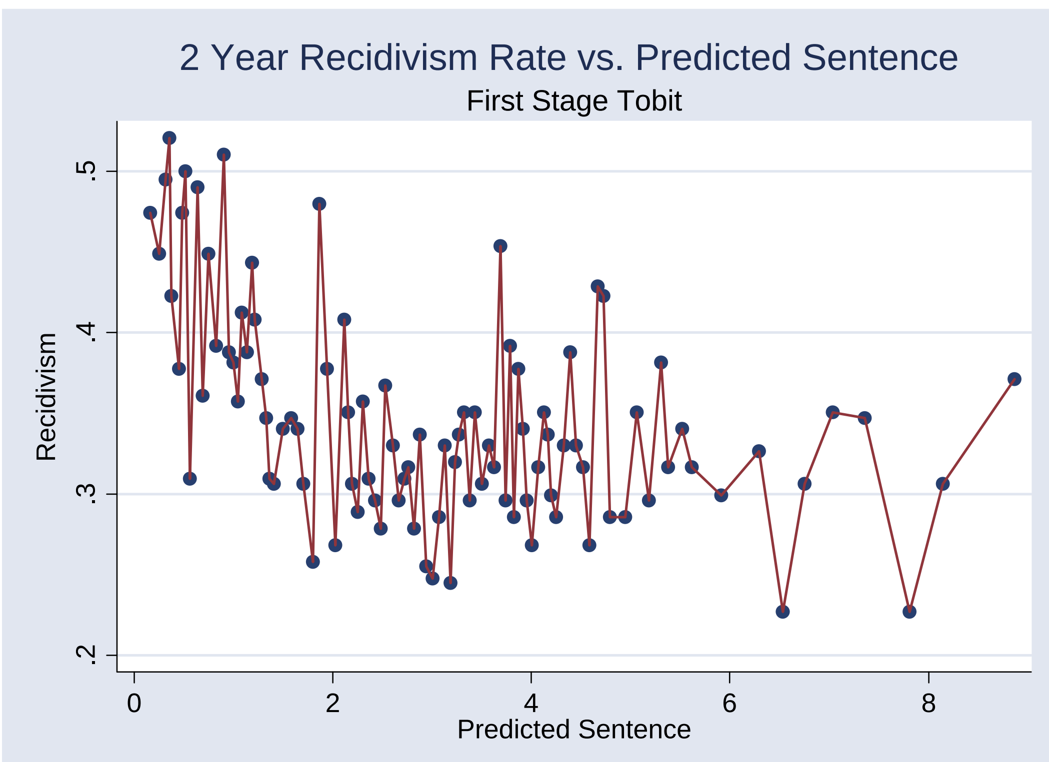

Figure A2a (Alternative Release Dates)

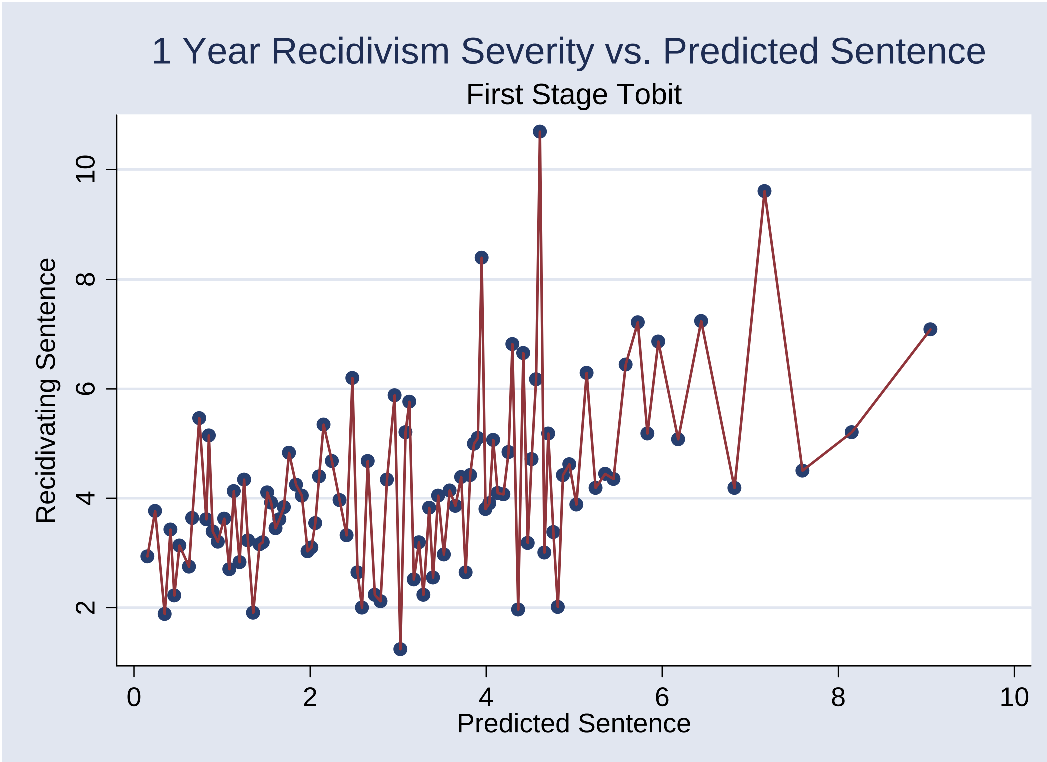

Figure A2b (Alternative Release Dates)

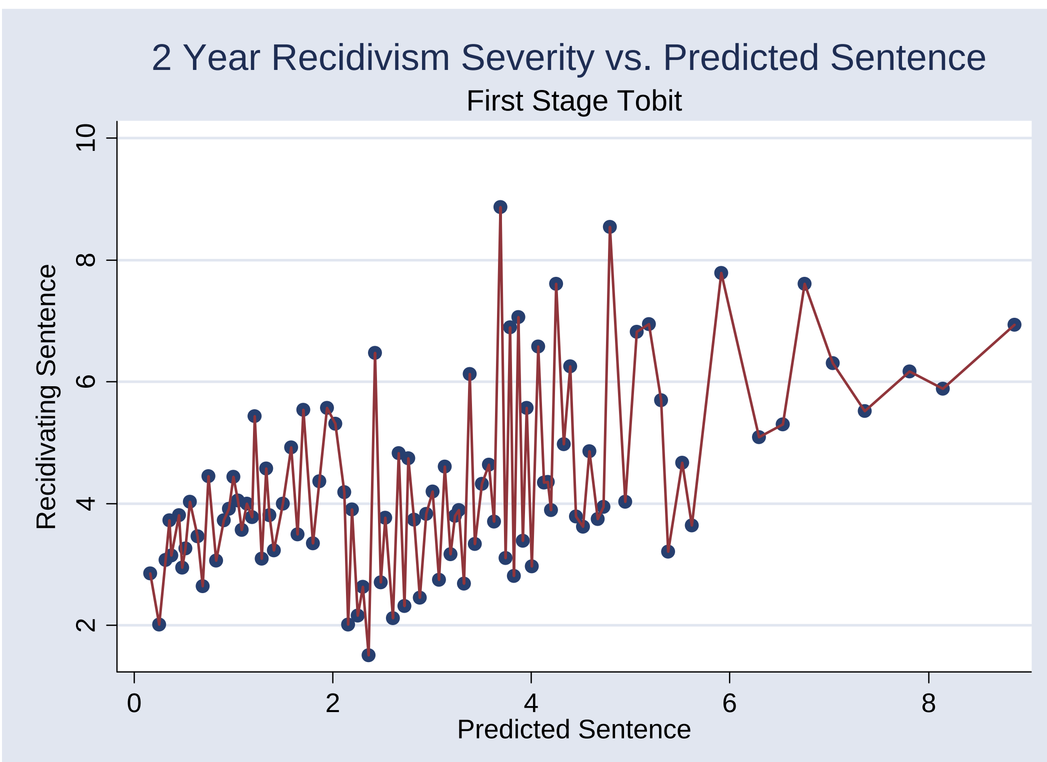

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Figure 1

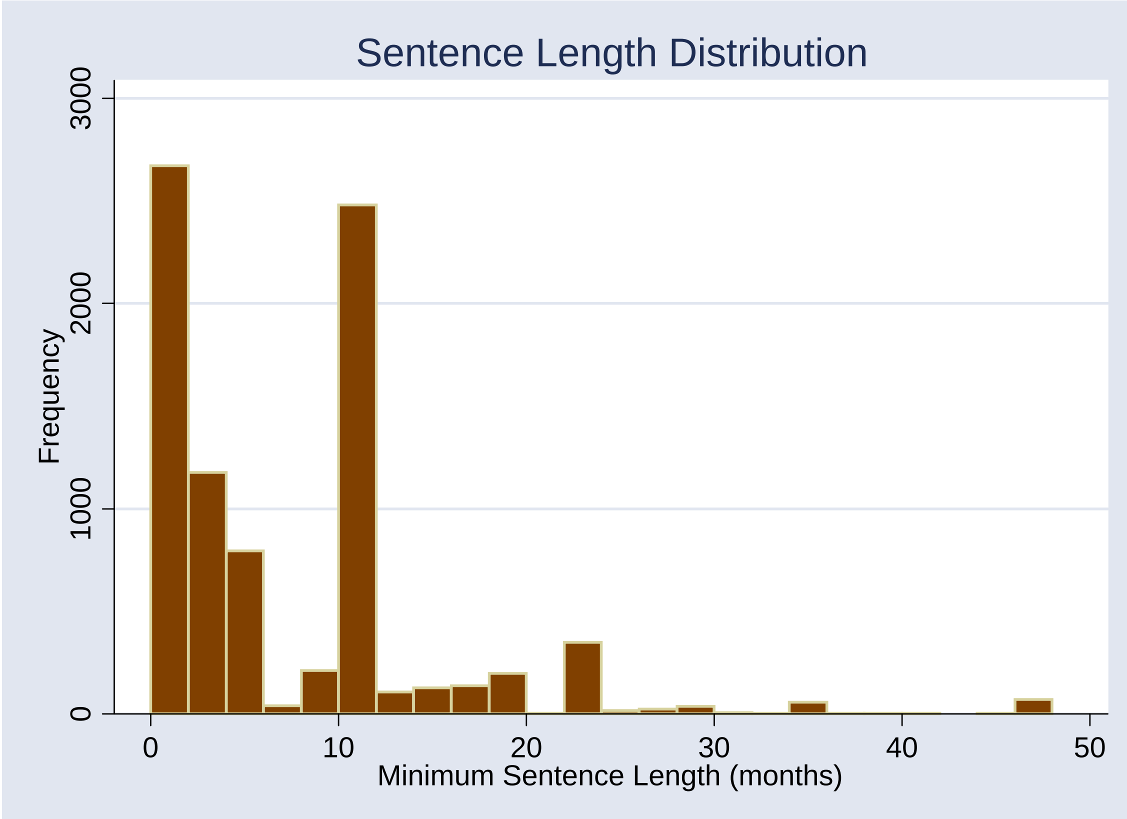


Figure 2

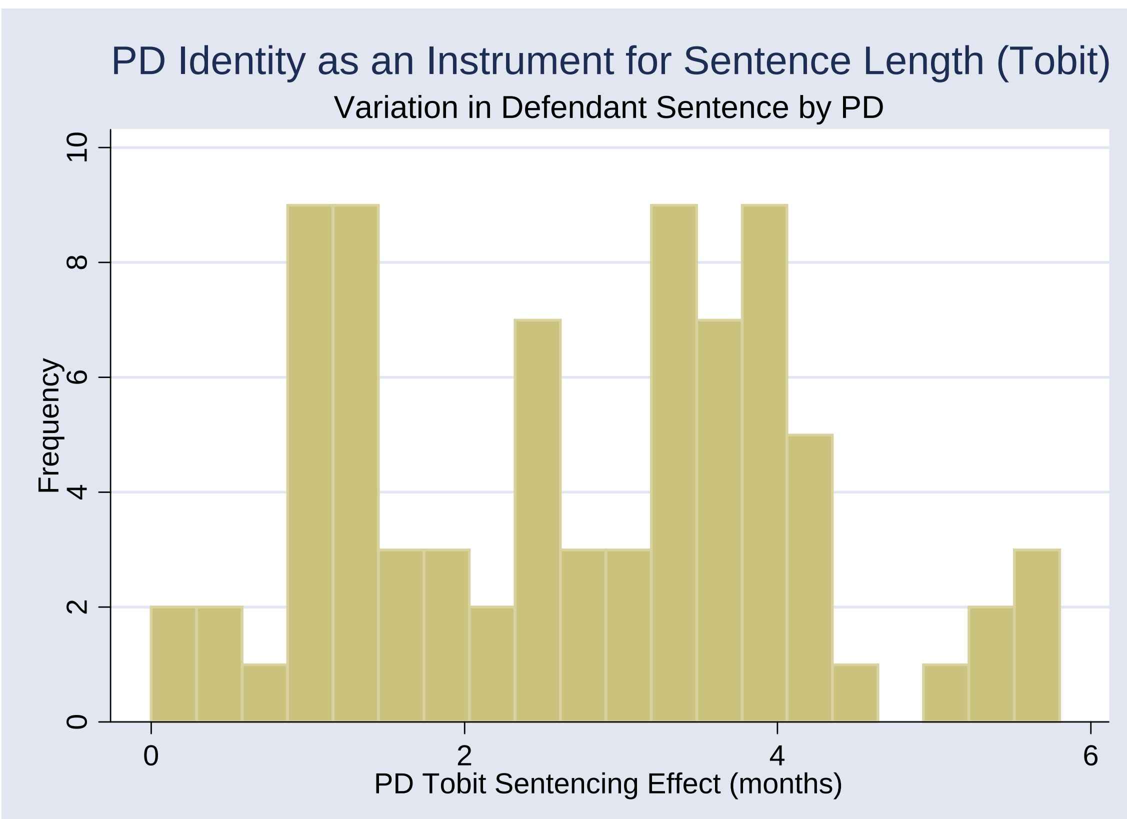


Figure 3

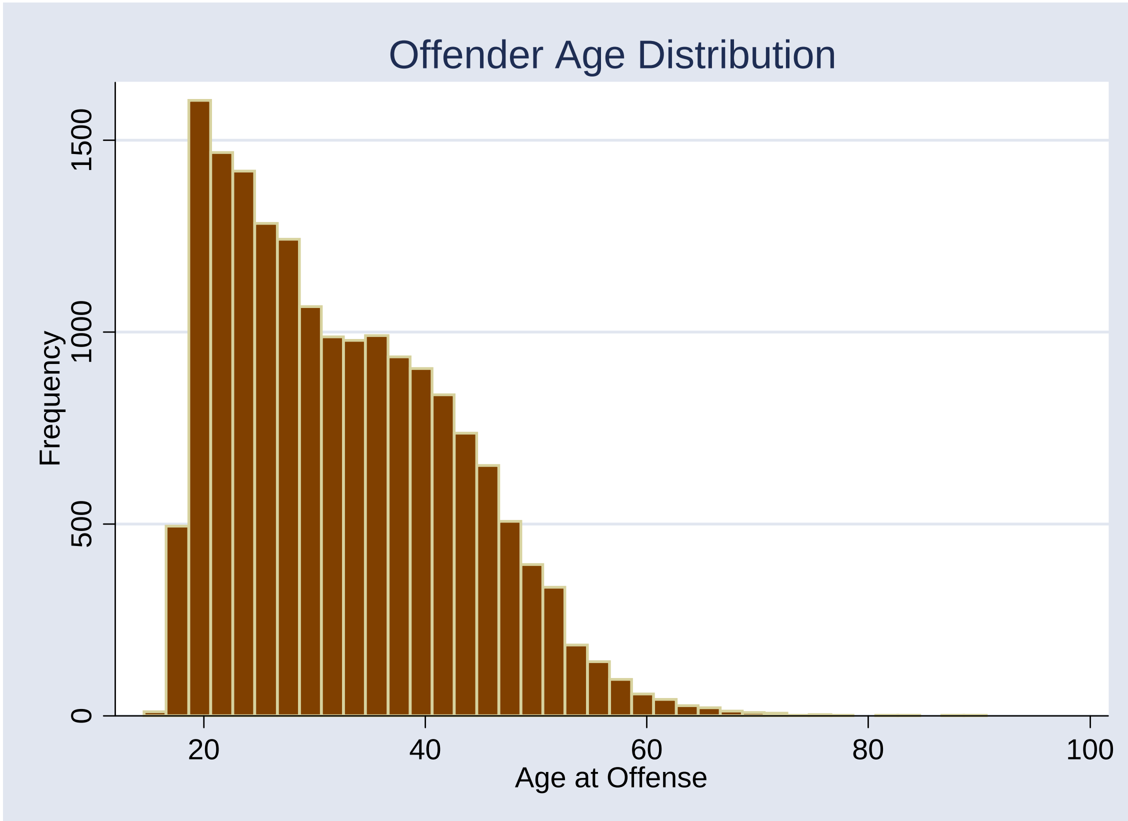


Figure 4 – To Replace

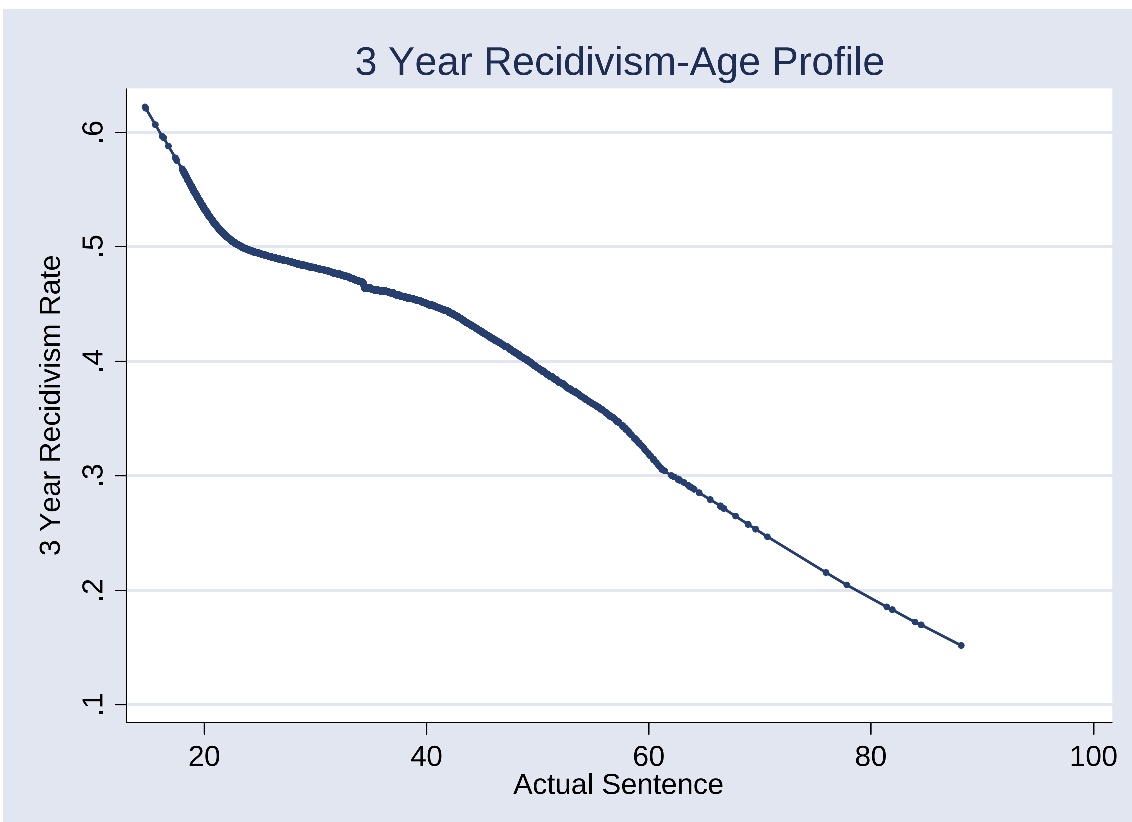


Figure 5a

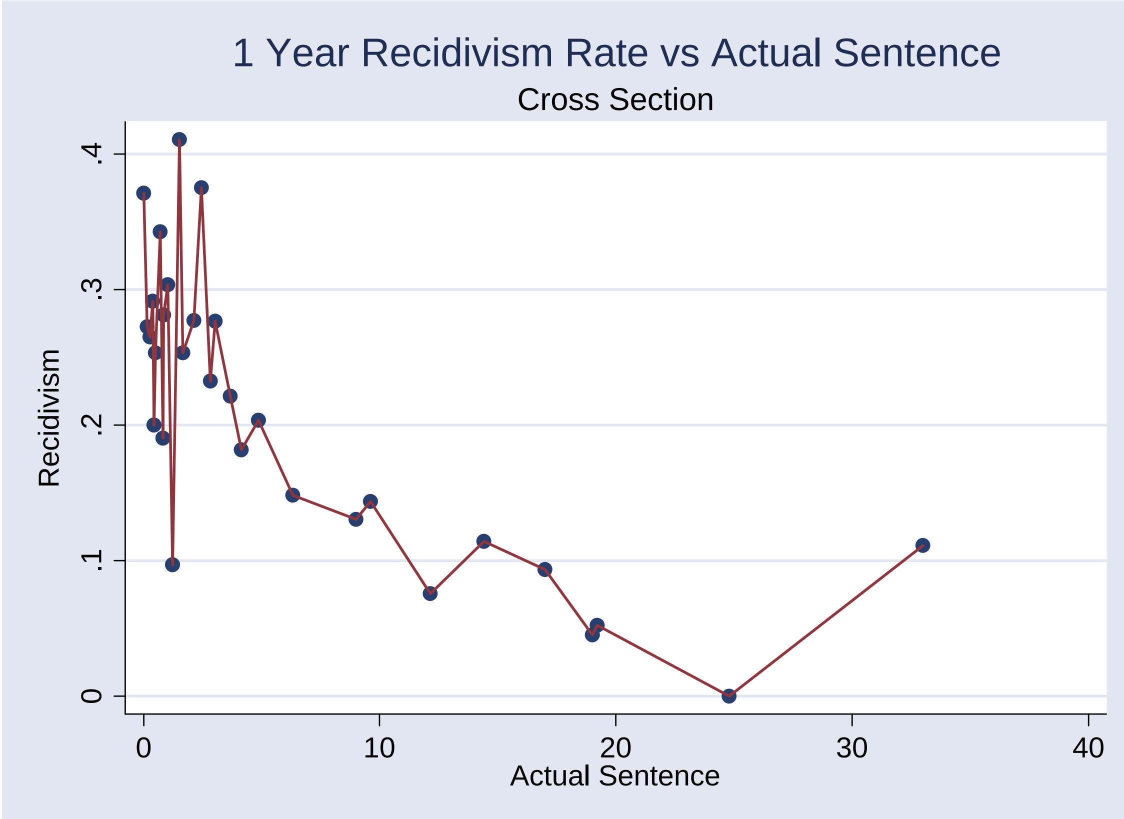


Figure 5b

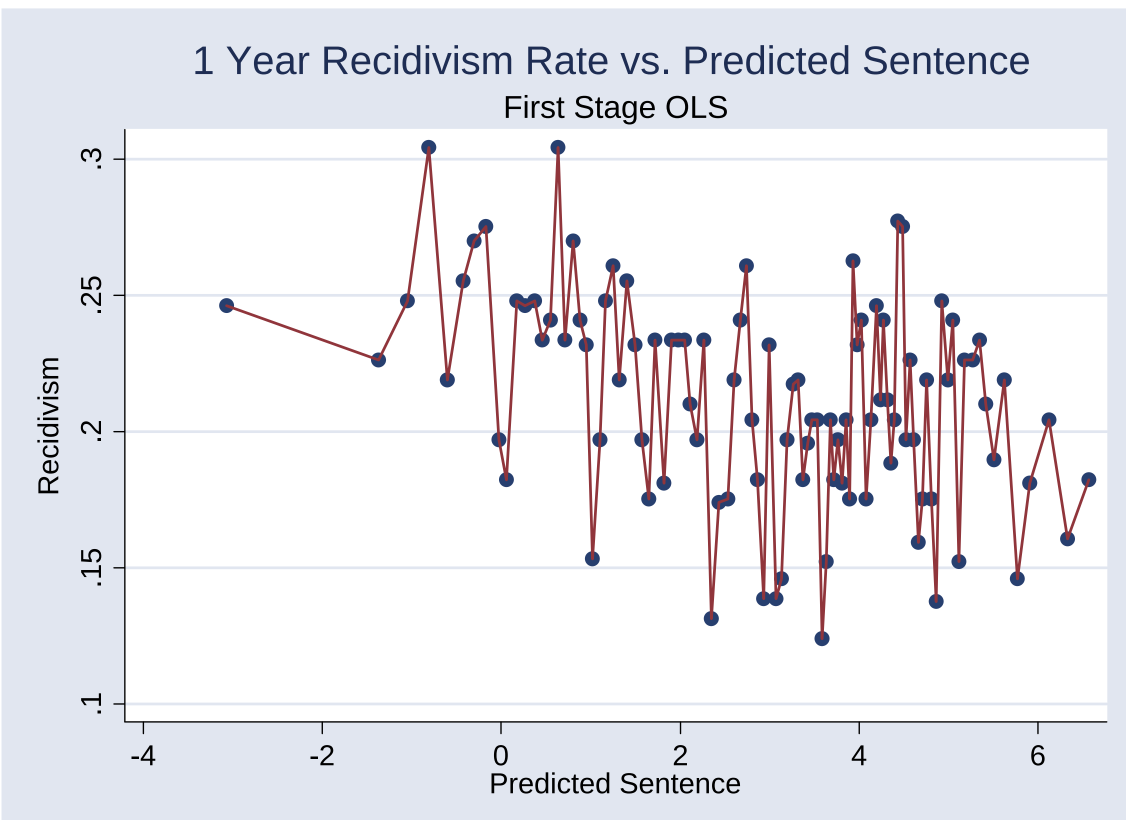


Figure 5c

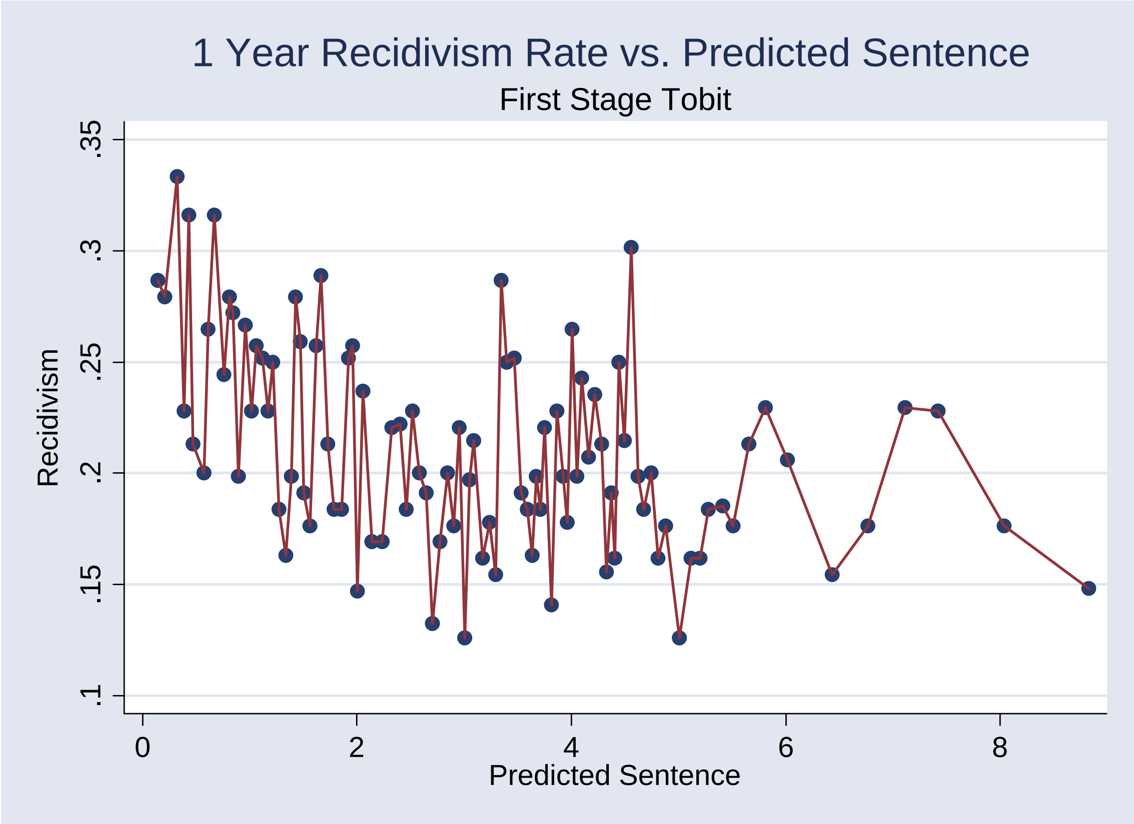


Figure 5d

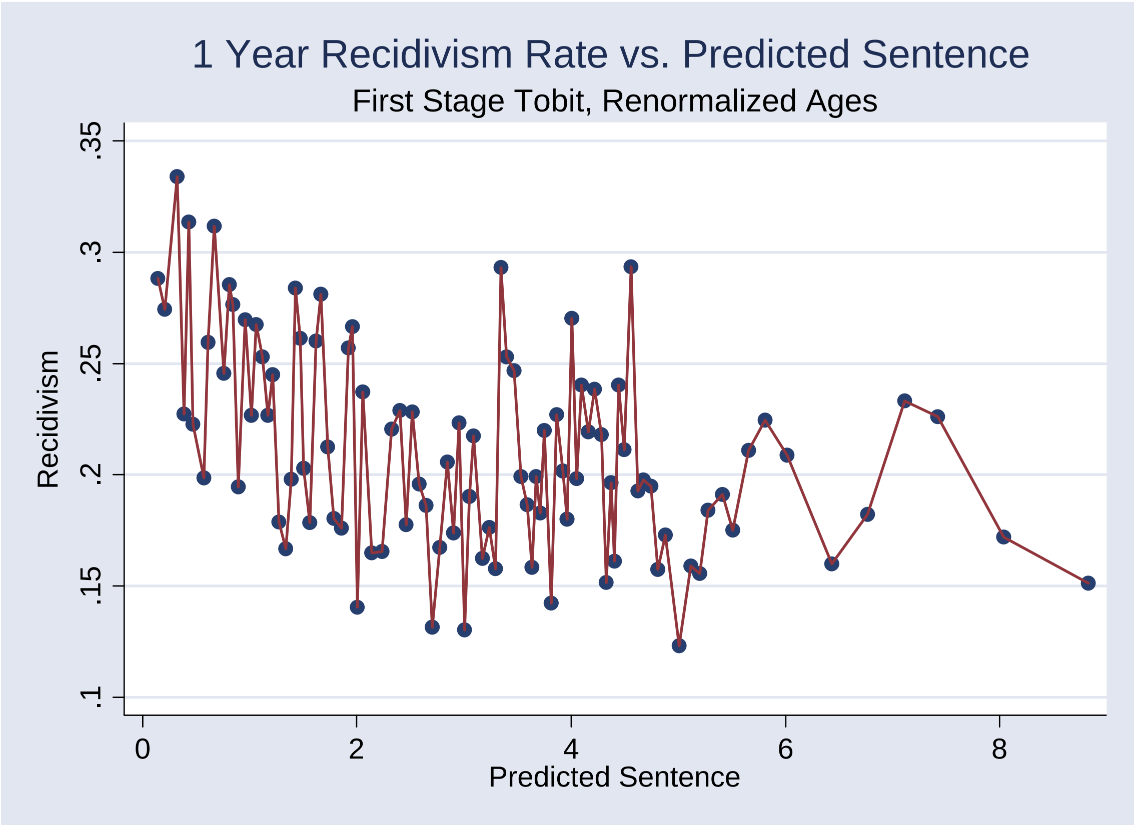
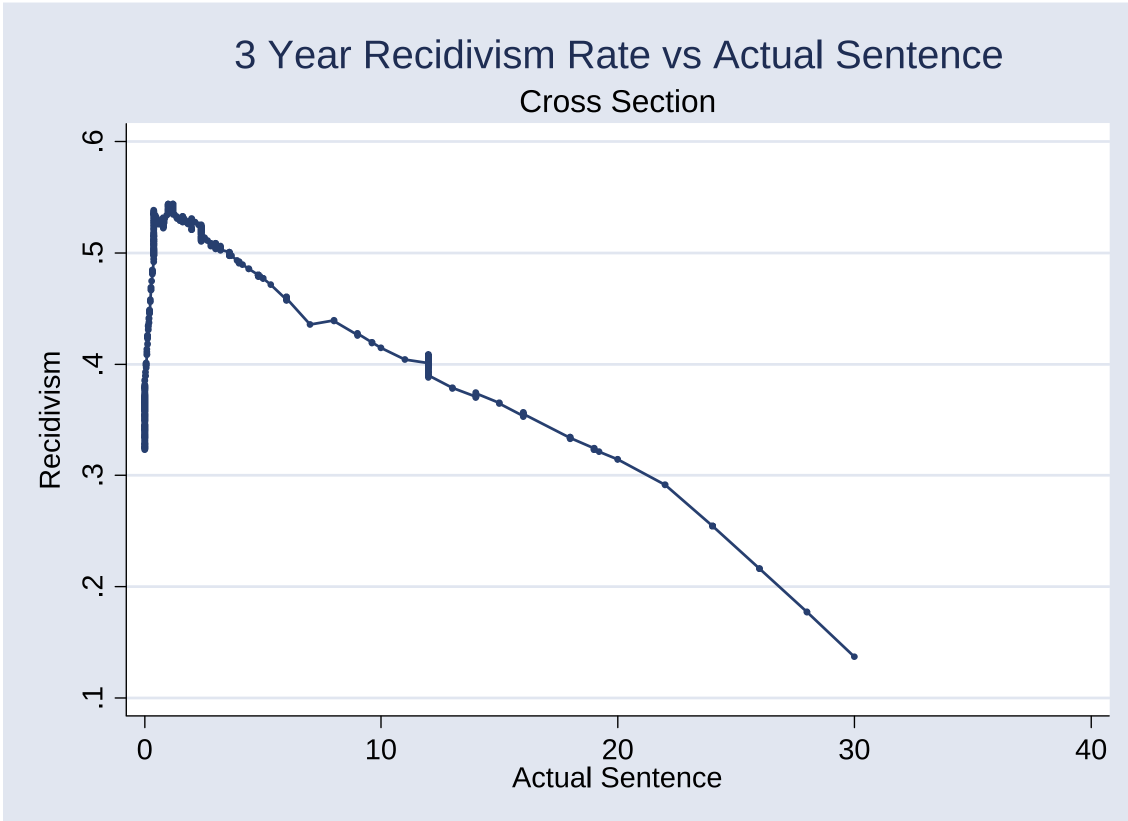


Figure 6a



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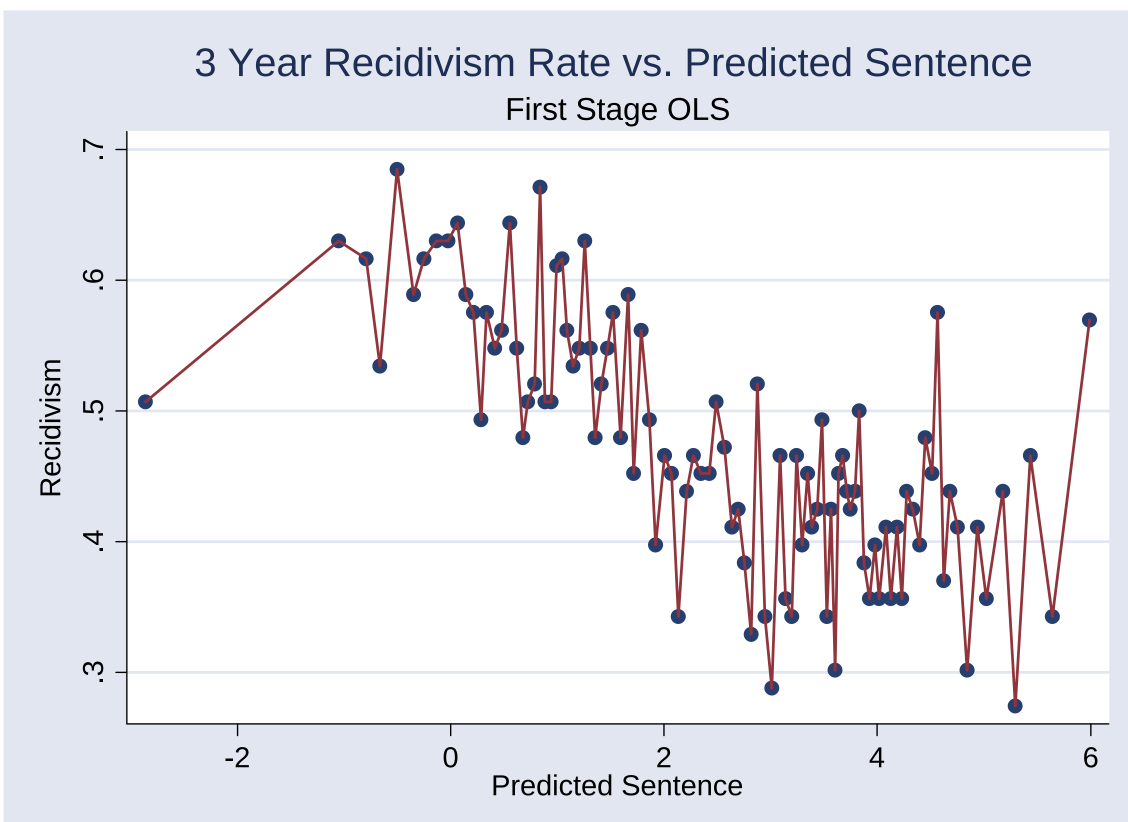

Figure 6c

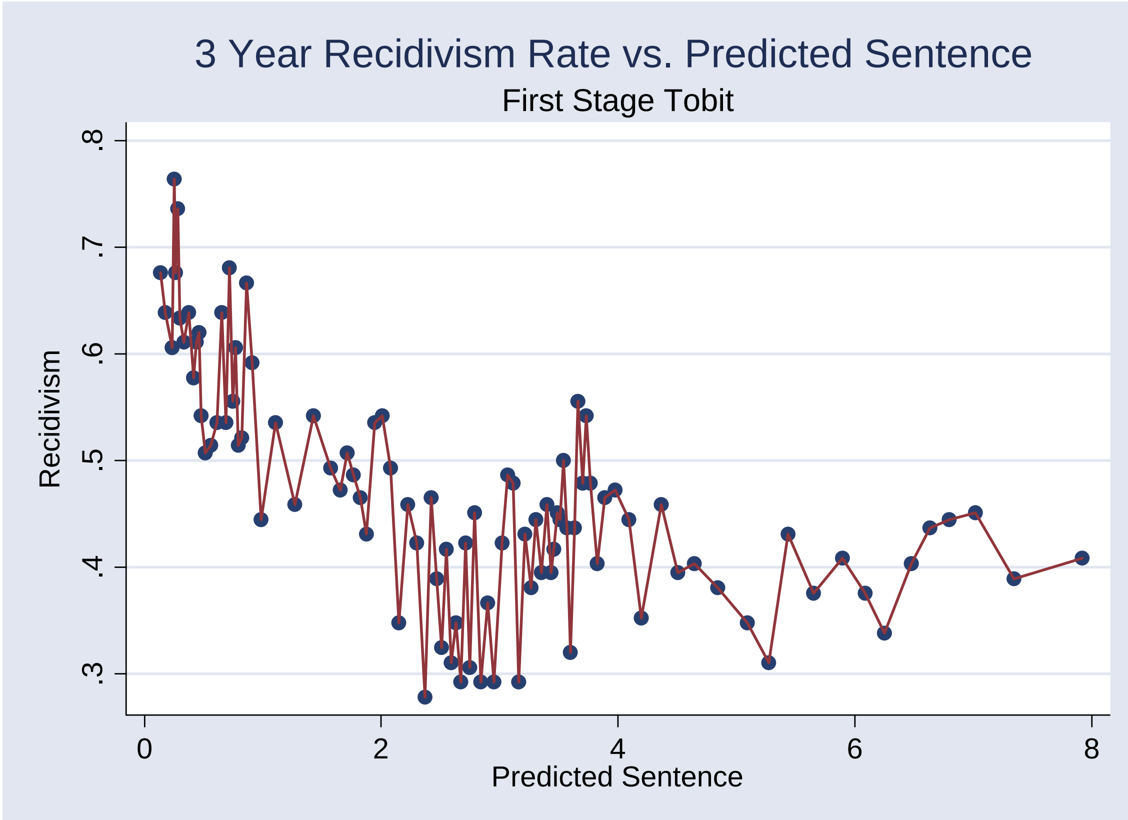
3 Year Recidivism Rate vs. Predicted Sentence
First Stage Tobit
Recidivism
.3
.4
.5
.6
.7
.8
0
2
4
6
8
Predicted Sentence

Figure 7a

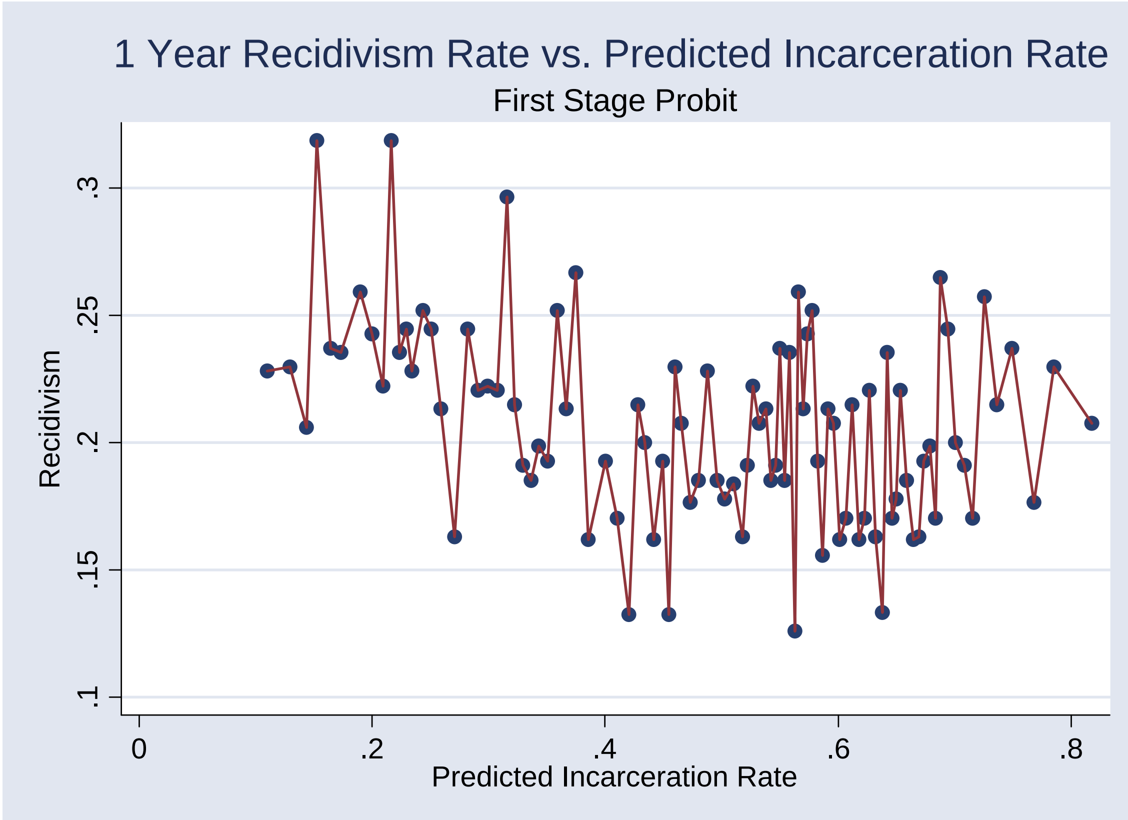

Figure 7b

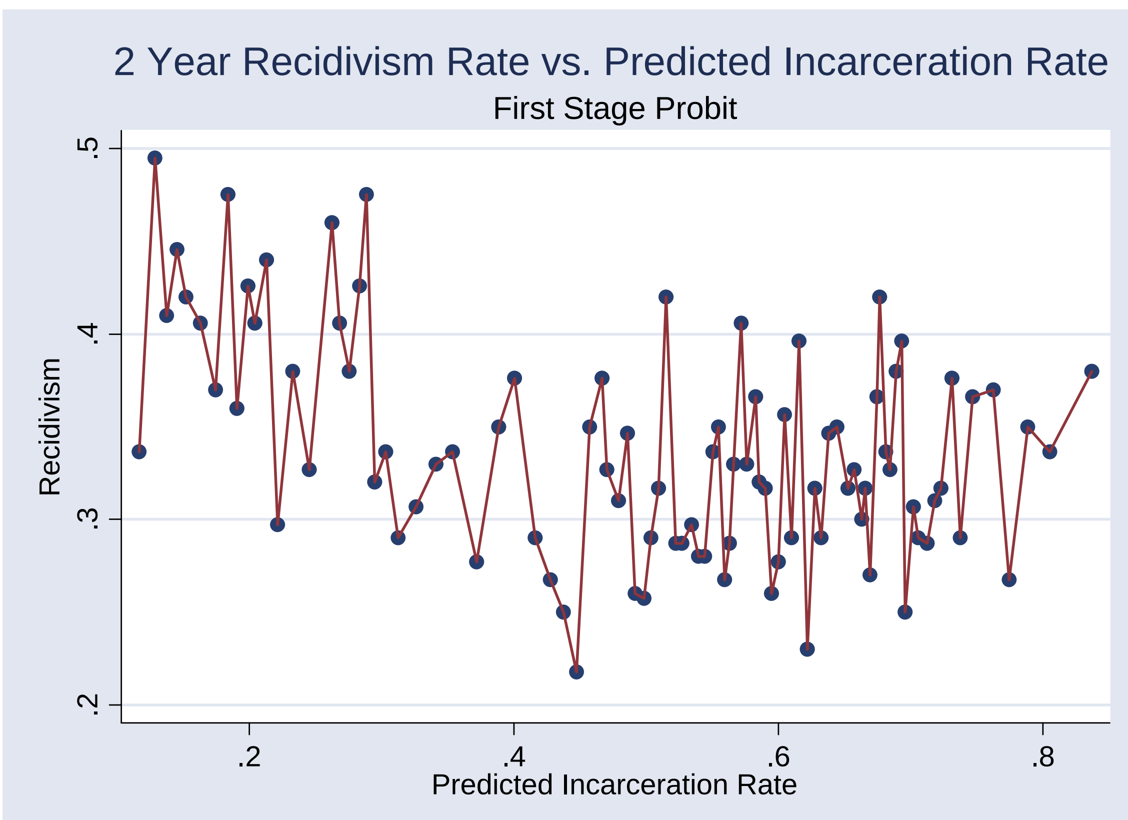

Figure 8a

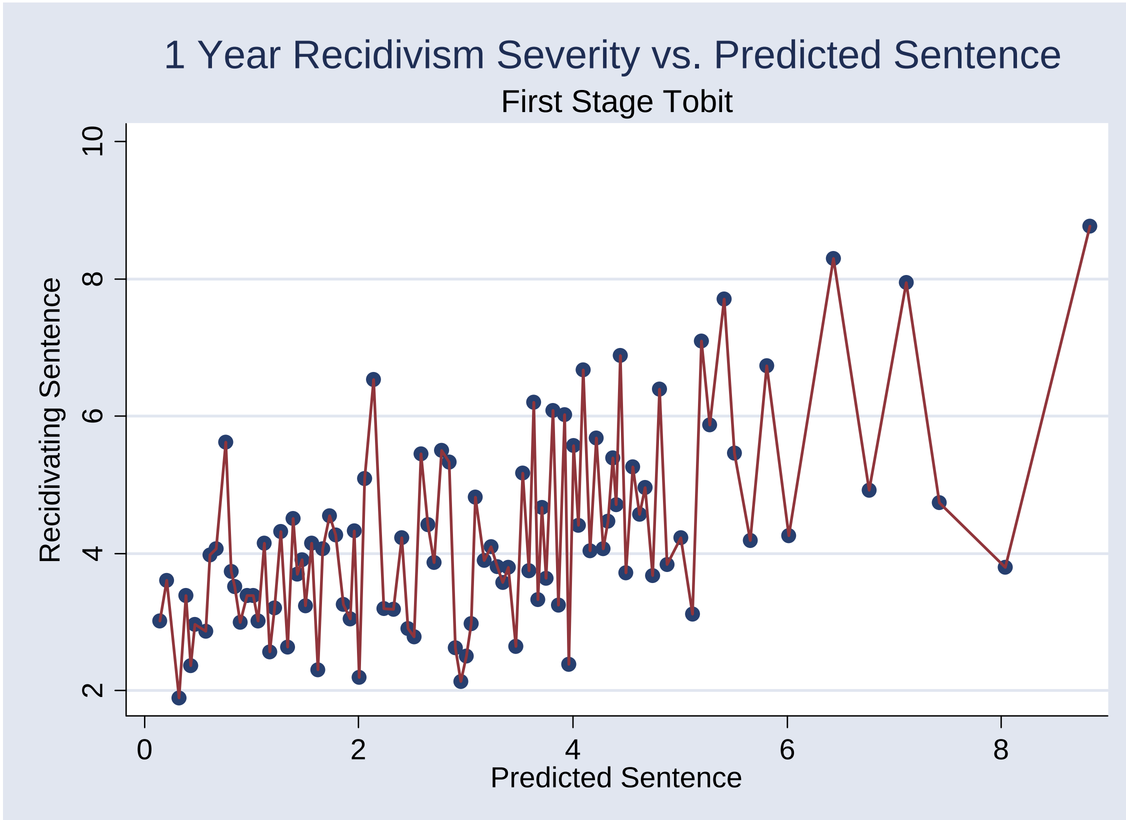

Figure 8b

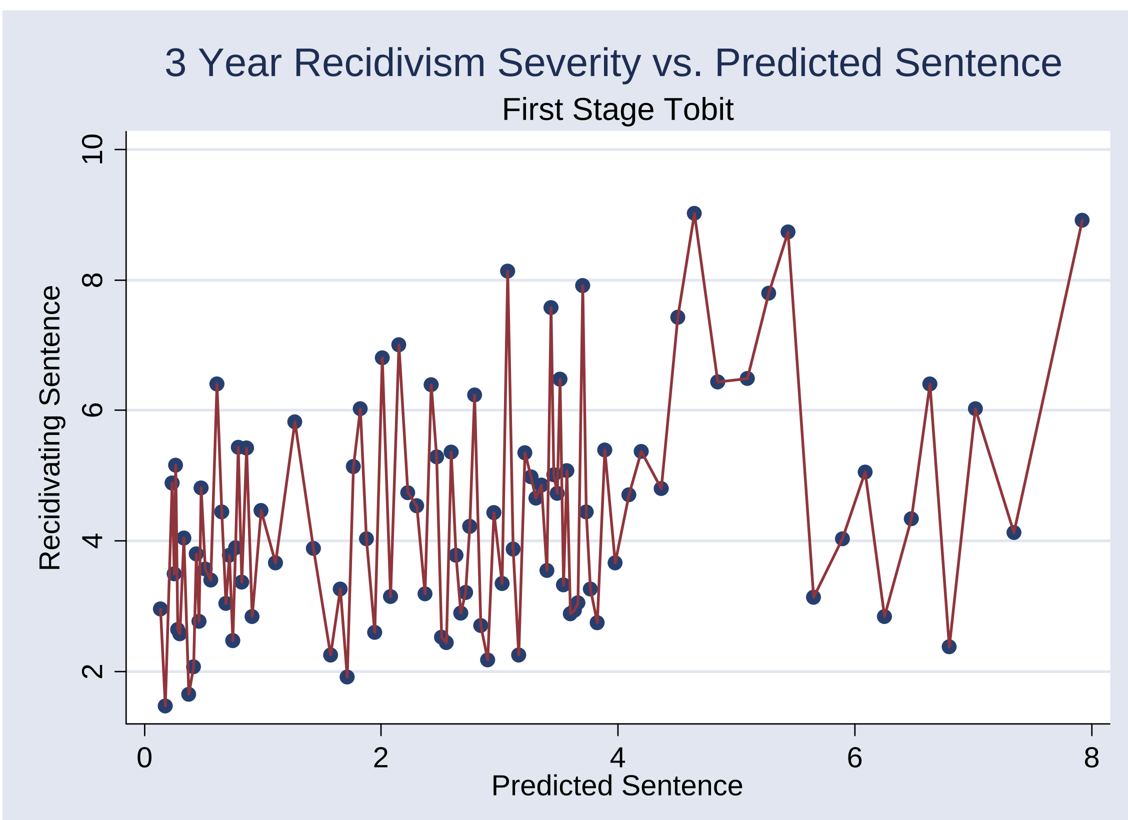

Figure 9a – Drug Possession

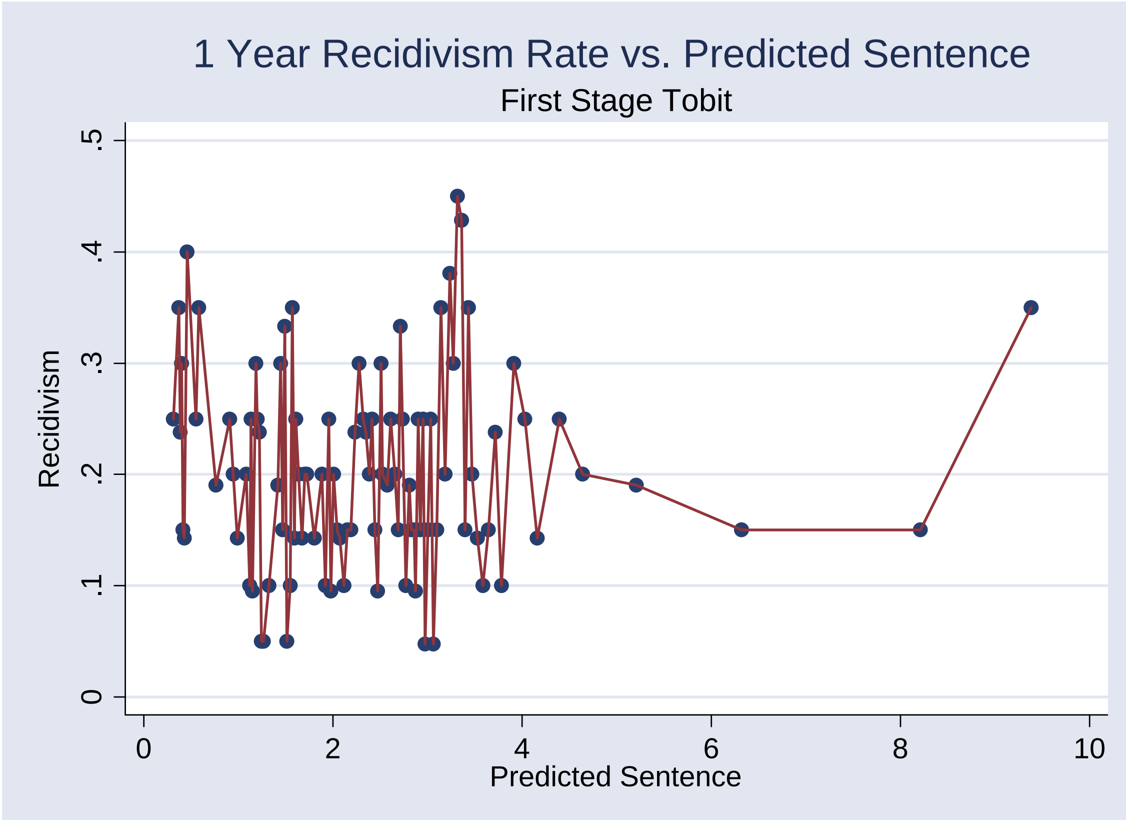

Figure 9b – Drug Possession

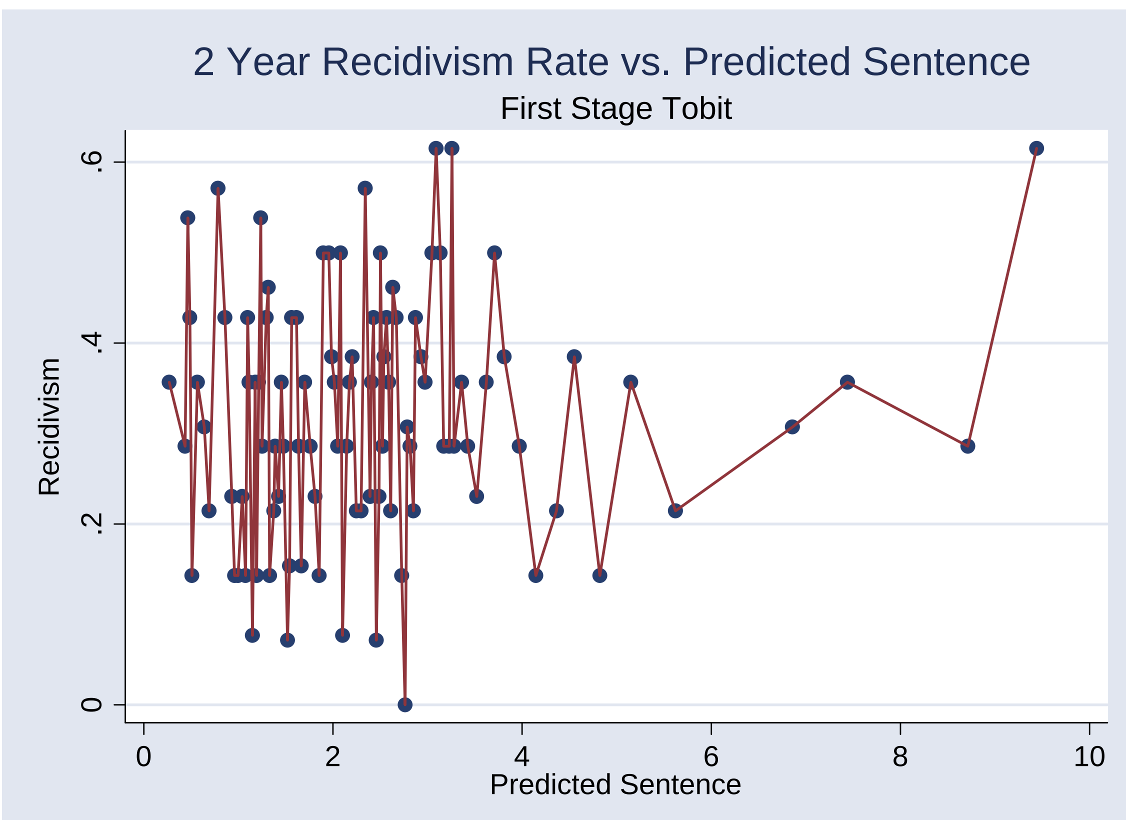

Figure 9c – Drug Possession

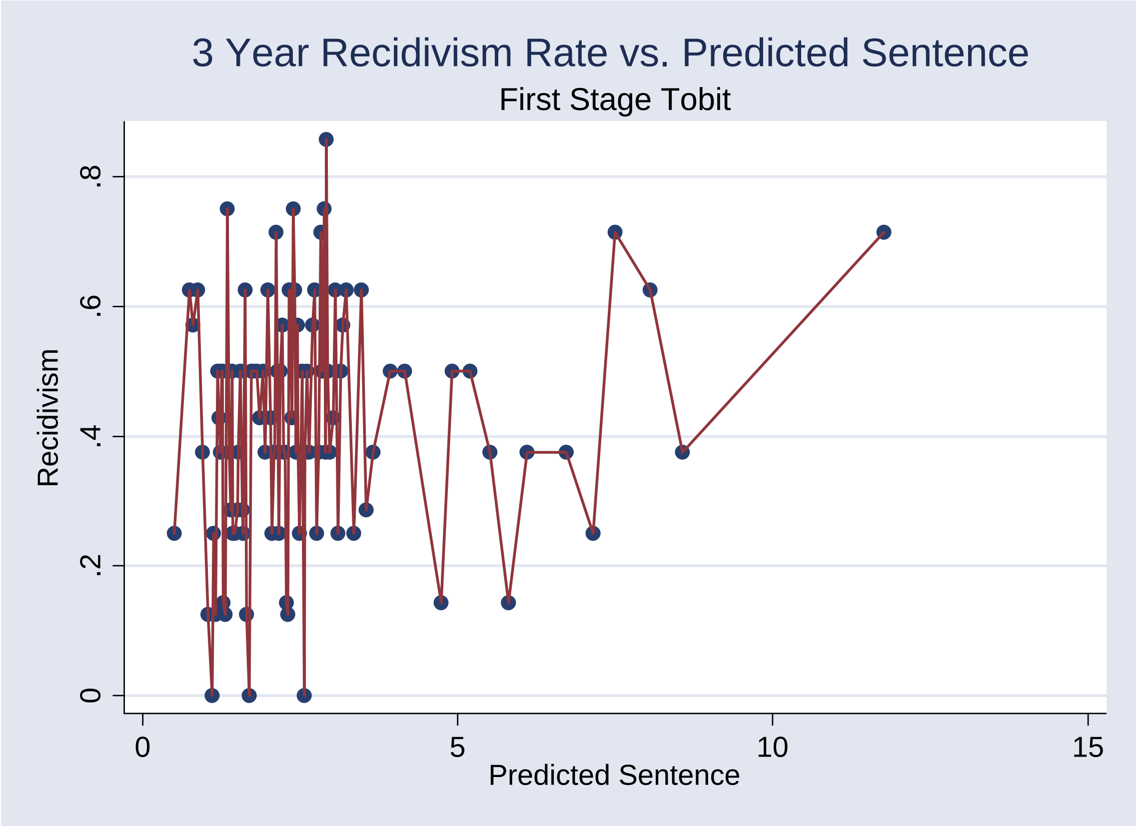

Table 1

Summary Statistics				
Variable	Mean	Median	Std. Dev.	Observations
Sentencing Characteristics				
Minimum Sentence (months)	4.59	0	11.1	17471
Predicted Min Sentence (tobit)	5.7	5.7	2.9	17310
Predicted Min Sentence (ols)	4.59	4.8	2.7	17471
Mean Sentence (months)	8.5	0	22.3	17471
Min Sentence (non-zero)	9.3	6.0	14.3	8647
Incarceration	0.50	0	0.50	17471
Offender Characteristics				
Age at offense	32.6	30.9	10.4	17471
Black	0.31			17471
Hispanic	0.22			17471
Male	0.80			17471
Offense Characteristics				
Drugs	0.24			17471
Violent Crime	0.16			17471
Sex Crime	0.02			17471
Property Crime (EFT)	0.40			17471
Other	0.19			17471
Recidivism Rates				
Within one year	0.212			13725
Within two years	0.349			10243
Within three years	0.475			7295
Recidivating Sentence	4.15	0.00	9.60	6113

Summary Statistics for offender-level data obtained from the Clark County Public Defender for the years 2001 - 2008. Offense characteristics reported for first offense. Data includes 81 Public Defenders, each with a minimum of 50 cases each, and 50 judges. Recidivism is defined as reappearance as a Public Defender client within the indicated time after release from incarceration (if sentenced to incarceration).

Table 2

OLS regression results							
	1 Year Recidivism			2 Year Recidivism		3 Year Recidivism	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Probit					
VARIABLES	No age control	Marginal Effects	Recidivism-Age Profile Adjustment	No age control	Recidivism-Age Profile Adjustment	No age control	Recidivism-Age Profile Adjustment
Sentence	-0.00706	-0.00785	-0.00696	-0.00533	-0.00537	-0.00157	-0.00168
	(0.000580)**	(0.000736)**	(0.000591)**	(0.000899)**	(0.000924)**	(0.00132)	(0.00134)
Sex (1=male)	0.0650	0.0667	0.0648	0.0976	0.0992	0.0935	0.0952
	(0.00837)**	(0.00805)**	(0.00848)**	(0.0116)**	(0.0118)**	(0.0147)**	(0.0149)**
Black Offender	0.0365	0.0360	0.0354	0.0635	0.0622	0.0790	0.0799
	(0.00828)**	(0.00821)**	(0.00841)**	(0.0109)**	(0.0111)**	(0.0132)**	(0.0134)**
Hispanic Offender	-0.0199	-0.0267	-0.0263	-0.0181	-0.0292	-0.0137	-0.0243
	(0.00884)*	(0.00892)**	(0.00882)**	(0.0123)	(0.0123)*	(0.0156)	(0.0156)
Drug Offense	0.0138	-0.0501	0.0171	0.0252	0.0307	0.00630	0.0126
	(0.0140)	(0.00930)**	(0.0142)	(0.0188)	(0.0191)	(0.0238)	(0.0243)
Violent Offense	-0.0283	-0.0869	-0.0299	-0.0253	-0.0281	-0.0582	-0.0632
	(0.0148)	(0.00969)**	(0.0148)*	(0.0201)	(0.0203)	(0.0257)*	(0.0259)*
EFT	0.0227	-0.0380	0.0212	0.0374	0.0342	0.00477	-0.000181
	(0.0134)	(0.00914)**	(0.0135)	(0.0179)*	(0.0180)	(0.0223)	(0.0225)
Sex Offense	-0.0227	-0.0785	-0.0100	-0.0574	-0.0419	-0.0136	0.0142
	(0.0269)	(0.0205)**	(0.0288)	(0.0366)	(0.0393)	(0.0464)	(0.0502)
Constant	0.241		0.223	0.388	0.370	0.533	0.518
	(0.0484)**		(0.0462)**	(0.0524)**	(0.0512)**	(0.0504)**	(0.0500)**
Observations	13573	13725	13557	10101	10087	7156	7146
Adjusted R-squared	0.030		0.028	0.049	0.045	0.079	0.074
** p<0.01, * p<0.05	Robust standard errors in parentheses						

Note: Offender-level data obtained from the Clark County Public Defender Office for the years 2001-2008. Monthly time dummies included to allow for time variation in overall crime rates. Sentence is the minimum sentence measured in months, EFT = embezzlement, fraud, theft. Data includes 81 Public Defenders, each with a minimum of 50 cases and 50 judges. Recidivism is defined as reappearance as a Public Defender client within the indicated time after release from incarceration (if sentenced to incareceration).

Table 3

IV regression results

		1 Year Recidivism				2 Year Recidivism		3 Year Recidivism	
		First Stage OLS		First Stage Tobit	IV LIML	First Stage OLS	First Stage Tobit	First Stage OLS	First Stage Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	First Stage Sentence Length	No age control	Recidivism-Age Profile Adjustment	Recidivism-Age Profile Adjustment	Control for age at offense	Recidivism-Age Profile Adjustment	Recidivism-Age Profile Adjustment	Recidivism-Age Profile Adjustment	Recidivism-Age Profile Adjustment
Sentence		-0.0148	-0.0157	-0.00478	-0.00715	-0.0126	0.00123	-0.00757	0.00514
		(0.00519)**	(0.00525)**	(0.00425)	(0.00434)	(0.00741)	(0.00544)	(0.0103)	(0.00824)
Sex (1=male)	1.081	0.0735	0.0744	0.0624	0.0695	0.107	0.0923	0.1000	0.0901
	(0.123)**	(0.0101)**	(0.0103)**	(0.00965)**	(0.00873)**	(0.0142)**	(0.0128)**	(0.0171)**	(0.0161)**
Black Offender	0.0516	0.0369	0.0359	0.0355	0.0338	0.0622	0.0622	0.0803	0.0791
	(0.110)	(0.00831)**	(0.00844)**	(0.00844)**	(0.00744)**	(0.0111)**	(0.0111)**	(0.0134)**	(0.0134)**
Hispanic Offender	-0.0235	-0.0200	-0.0265	-0.0261	-0.0328	-0.0302	-0.0285	-0.0246	-0.0251
	(0.106)	(0.00888)*	(0.00886)**	(0.00887)**	(0.0105)**	(0.0124)*	(0.0123)*	(0.0156)	(0.0157)
Drug Offense	0.896	0.0199	0.0240	0.0209	-0.0530	0.0392	0.0217	0.0214	-0.00385
	(0.187)**	(0.0145)	(0.0147)	(0.0164)	(0.0149)**	(0.0210)	(0.0228)	(0.0288)	(0.0330)
Violent Offense	1.397	-0.0169	-0.0171	-0.0288	-0.0949	-0.0160	-0.0402	-0.0533	-0.0796
	(0.233)**	(0.0165)	(0.0166)	(0.0177)	(0.0157)**	(0.0237)	(0.0246)	(0.0311)	(0.0337)*
EFT	1.631	0.0357	0.0359	0.0223	-0.0437	0.0468	0.0215	0.0113	-0.0181
	(0.171)**	(0.0158)*	(0.0159)*	(0.0177)	(0.0167)**	(0.0220)*	(0.0235)	(0.0302)	(0.0325)
Sex Offense	1.207	-0.0135	0.000429	-0.00687	-0.0807	-0.0369	-0.0481	0.0202	0.00150
	(0.361)**	(0.0276)	(0.0294)	(0.0307)	(0.0238)**	(0.0397)	(0.0411)	(0.0512)	(0.0532)
Age at Offense	0.000453								
	(0.00425)								
Constant	-0.884	0.233	0.214	0.230	0.271	0.363	0.375	0.514	0.519
	(0.386)*	(0.0485)**	(0.0463)**	(0.0461)**	(0.0174)**	(0.0514)**	(0.0510)**	(0.0504)**	(0.0500)**
Observations	13573	13573	13557	13557	13725	10087	10087	7146	7146
Adjusted R-squared	0.165	0.024	0.022	0.021	0.022	0.043	0.042	0.074	0.074

** p<0.01, * p<0.05 Robust standard errors in parentheses

Note: Offender-level data obtained from the Clark County Public Defender Office for the years 2001-2008. Monthly time dummies included to allow for time variation in overall crime rates. Sentence is the minimum sentence measured in months, EFT = embezzlement, fraud, theft. Data includes 78 Public Defenders, each with a minimum of 50 cases and 51 judges. Recidivism is defined as reappearance as a Public Defender client within the indicated time after release from incarceration (if sentenced to incareceration).

Table 4

Recidivism and Incarceration						
	1 Year Recidivism		2 Year Recidivism		3 Year Recidivism	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	OLS	IV	OLS	IV	OLS	IV
Incarceration	0.0335	0.00396	0.114	0.0148	0.159	0.0889
	(0.00774)**	(0.0437)	(0.0106)**	(0.0521)	(0.0131)**	(0.0804)
Sex (1=male)	0.0543	0.0574	0.0807	0.0910	0.0777	0.0861
	(0.00836)**	(0.00954)**	(0.0115)**	(0.0126)**	(0.0146)**	(0.0162)**
Black Offender	0.0333	0.0345	0.0565	0.0591	0.0706	0.0734
	(0.00832)**	(0.00839)**	(0.0108)**	(0.0109)**	(0.0130)**	(0.0134)**
Hispanic Offender	-0.0272	-0.0257	-0.0325	-0.0289	-0.0311	-0.0288
	(0.00901)**	(0.00907)**	(0.0124)**	(0.0126)*	(0.0156)*	(0.0162)
Drug Offense	0.00782	0.00995	0.00777	0.0161	-0.0257	-0.0358
	(0.0140)	(0.0186)	(0.0189)	(0.0259)	(0.0238)	(0.0436)
Violent Offense	-0.0409	-0.0393	-0.0443	-0.0393	-0.0799	-0.0959
	(0.0147)**	(0.0188)*	(0.0201)*	(0.0261)	(0.0254)**	(0.0403)*
EFT	0.00654	0.00914	0.0115	0.0192	-0.0284	-0.0405
	(0.0134)	(0.0191)	(0.0179)	(0.0257)	(0.0222)	(0.0404)
Sex Offense	-0.0254	-0.0223	-0.0510	-0.0500	-0.0164	-0.0337
	(0.0270)	(0.0309)	(0.0367)	(0.0407)	(0.0466)	(0.0563)
Age at Release	-0.00159	-0.00157	-0.00288	-0.00282	-0.00303	-0.00294
	(0.000340)**	(0.000340)**	(0.000457)**	(0.000460)**	(0.000575)**	(0.000588)**
Constant	0.296	0.290	0.487	0.467	0.633	0.608
	(0.0490)**	(0.0498)**	(0.0535)**	(0.0546)**	(0.0532)**	(0.0545)**
Observations	13573	13540	10103	10059	7156	7110
Adjusted R-squared	0.026	0.025	0.060	0.050	0.100	0.082

** p<0.01, * p<0.05 Robust standard errors in parentheses

Note: This table reports the results of OLS and 2SLS regressions or recidivism on a binary incarceration indicator. Offender-level data obtained from the Clark County Public Defender Office for the years 2001-2008. Monthly time dummies included to allow for time variation in overall crime rates. EFT = embezzlement, fraud, theft. Data includes 81 Public Defenders, each with a minimum of 50 cases and 50 judges. A tobit regression is used for the first stage of the IV regressions reported in this table.

Table 5

Recidivism Severity			
VARIABLES	1 Year Recidivism	2 Year Recidivism	3 Year Recidivism
Sentence	0.195	0.231	0.0726
	(0.159)	(0.175)	(0.218)
Sex (1=male)	1.542	1.538	1.460
	(0.296)**	(0.311)**	(0.357)**
Black Offender	-0.292	-0.168	-0.385
	(0.293)	(0.316)	(0.352)
Hispanic Offender	-0.673	-0.560	-0.526
	(0.347)	(0.376)	(0.420)
Drug Offense	-0.736	-1.008	-1.027
	(0.526)	(0.626)	(0.766)
Violent Offense	0.443	0.363	0.775
	(0.746)	(0.803)	(0.912)
EFT	-0.357	-0.519	-0.0444
	(0.563)	(0.620)	(0.764)
Sex Offense	-1.874	-1.639	-1.299
	(0.914)*	(1.104)	(1.230)
Age at Release	-0.000828	0.000826	0.00533
	(0.0138)	(0.0149)	(0.0168)
Constant	2.461	2.404	2.686
	(1.261)	(1.269)	(1.290)*
Observations	5707	4950	4064
Adjusted R-squared	0.011	0.011	0.082
** p<0.01, * p<0.05	Robust standard errors in parentheses		

Note: This table reports 2SLS regressions where the outcome is the expected sentence of the recidivating crime. Offender-level data obtained from the Clark County Public Defender Office for the years 2001-2008. Monthly time dummies included to allow for time variation in overall crime rates. Sentence is the minimum sentence measured in months, EFT = embezzlement, fraud, theft. Data includes 81 Public Defenders, each with a minimum of 50 cases and 50 judges. A tobit regression is used for the first stage in all of the regressions reported in this table.