THE CRIMINAL AND LABOR MARKET IMPACTS OF INCARCERATION

MICHAEL MUELLER-SMITH

AUGUST 18, 2015

ABSTRACT. This paper investigates the impacts of incarceration on criminal behavior and labor market activity using new data from Harris County, Texas. The research design exploits exogenous variation in incarceration due to defendants' random courtroom assignment. I show that two factors, multidimensional and non-monotonic sentencing, generate bias and propose a new estimation procedure to address these features. The empirical results indicate that incarceration generates net increases in the frequency and severity of recidivism, worsens labor market outcomes, and strengthens dependence on public assistance. A cost-benefit exercise finds that substantial general deterrence effects are necessary to justify incarceration in the marginal population.

Keywords: incarceration, recidivism, labor market outcomes
JEL: J24, K42

Department of Economics, University of Michigan, 426 Thompson Street, Rm. 2044, Ann Arbor, Michigan 48106-1248, mgms@umich.edu. I would like to thank Cristian Pop-Eleches, Bernard Salanié and Miguel Urquiola for their advice and support, and the participants in the NBER Summer Institute, University of Wisconsin Institute for Research on Poverty Summer Workshop and Columbia Applied Microeconomics Workshop for their comments. I am particularly indebted to the staff at the Ray Marshall Center who generously hosted my research in Texas. This project would not have been possible without the approval of the Harris County District Clerk, the Harris County Sheriff's Office the Texas Department of Criminal Justice, the Texas Department of Public Safety, the Texas Health and Human Services Commission, and the Texas Workforce Commission. IRB approval was received from Columbia University (IRB-AAAL0614) and the University of Texas at Austin (2012-12-0079). Funding for this project was provided by the National Science Foundation (SES-1260892).

After three decades of rapid growth in the prison population, the United States now stands as the global leader in the use of incarceration (Walmsley (2009), Carson (2013)). In 2012, the annual U.S. correctional population included roughly 7 million adults (Glaze and Herberman (2013)), and combined federal, state and local justice-related expenditures topped $260 billion per year (Kyckelhahn (2013)). Theoretical models generate ambiguous predictions for incarceration's effects on long-run behavior and social externalities raising the need for empirical research, yet credible causal evidence remains scarce (Donohue III (2009)).

In this paper, I investigate the impacts of incarceration on criminal and economic activity using original data from Harris County, Texas. I linked over 2.6 million criminal court records accounting for 1.1 million unique defendants to state prison and county jail administrative data, unemployment insurance wage records, public assistance benefits as well as future criminal behavior.

My research design leverages the random assignment of criminal defendants to courtrooms as a source of exogenous variation. The courts are staffed by judges and prosecutors who differ in their propensity to incarcerate. As a result, which courtroom a defendant is assigned to influences whether he will be incarcerated and for how long. This increasingly popular identification strategy has been used in numerous applications where judges, case workers, or other types of program administrators are given discretion on how to respond to a randomly assigned caseload.[1]

This empirical strategy is contaminated by two sources of bias in my setting. First, sentencing takes on multiple dimensions (e.g. incarceration, fines, etc.) and second judges display non-monotonic tendencies (e.g. hard on drug offenders but easy on property offenders). Failure to account for these features leads to violations of the *exclusion restriction* and *monotonicity assumption*. I propose a new estimation procedure that addresses both biases through estimating a model that simultaneously instruments for all observed sentencing dimensions and that allows the instruments' effect on sentencing outcomes to be heterogeneous in defendant traits and crime characteristics.

[1]For studies specifically related to incarceration, see Kling (2006), Di Tella and Schargrodsky (2004), or Aizer and Doyle (2015). For research in other fields, see Doyle [2007, 2008], Autor and Houseman (2010), Belloni et al. (2012), Doyle et al. (ming), French and Song (2014) Maestas et al. (2013), Autor et al. (2015), Dahl et al. (2014), and Dobbie and Song (2015).

My empirical results indicate that incarceration may be less attractive compared to prior work. While I find evidence of modest incapacitation effects while defendants are held in jail or prison, I show that these short-run gains are more than offset by long-term increases in post-release criminal behavior. My results also suggest that incarceration encourages more serious offenses and promotes new types of criminal behavior, especially property and drug-related offenses, post-release.

I also show clear evidence of lasting negative effects on economic self-sufficiency. Each additional year behind bars reduces post-release employment by 3.6 percentage points. Among felony defendants with stable pre-charge earnings incarcerated for one or more years, post-release employment drops by at least 24 percentage points. These results are paralleled by an increased take-up of Food Stamps and cash welfare. Whether through reduced tax revenue or increased public assistance spending, the findings imply that public finance is affected in ways that extend beyond the direct administrative "bed" costs.

With these new estimates, I conduct a partial cost-benefit exercise that accounts for the administrative expenses, criminological effects and economic impacts. What is absent are the general deterrence effects which cannot be measured in my study. As such, the findings need to be evaluated relative to the number of crimes that would need to be prevented in the general population to achieve welfare neutrality. Using the most conservative estimates, I find that a one-year prison term generates $56,200 to $66,800 in costs. In order for this sentence to be welfare neutral, it would need to deter at least 0.4 rapes, 2.2 assaults, 2.5 robberies, 62 larcenies or 4.8 habitual drug users in the general population. Unless the general deterrence effects are at the literature's upper bound or other sizable intangible benefits exist, it is unlikely that incarcerating marginal defendants in this context is welfare improving.

1. RELATED LITERATURE

Empirical research on the incarceration has primarily focused on questions relating to criminal behavior. Incapacitation, in particular, has received significant focus. Credible estimates range from 2.8 to 15 crimes prevented per year of incarceration (Levitt (1996), Owens (2009), Johnson and Raphael (2012), Buonanno and Raphael (2013), Kuziemko (2013)). Lower estimates generally rely on individually linked records, while larger estimates allow for incapacitation effects to also measure potential multiplier effects in the population. Diminishing returns to incarceration has also been put forth as an explanation for the wide range (Liedka et al. (2006), Johnson and Raphael (2012)). Few studies consider the ramifications or measure the magnitude of post-release behavior.

How general and specific deterrence inform criminal decision making remains an open question. Poor prison conditions and three strikes laws appear to discourage criminal behavior (Katz et al. (2003), Helland and Tabarrok (2007)), yet sharp changes in the severity of sentencing at age of maturity and own experiences of incarceration seem to have zero or positive effects on recidivism (Lee and McCrary (2009), McCrary and Sanga (2012), Chen and Shapiro (2007), Di Tella and Schargrodsky (2004), Green and Winik (2010), Nagin and Snodgrass (2013)). Salience of future penalties may play a role. Drago et al. (2009)'s analysis of a collective pardon in Italy finds that each additional month carried over to future potential sentencing decreases a newly released inmate's criminal activity by 0.16 percentage points. Conversely, getting off easy through early release (without sentence carry over) or retroactive sentencing guidelines modifications encourages recidivisim (Maurin and Ouss (2009), Bushway and Owens (2013), Kuziemko (2013), Barbarino and Mastrobuoni (2014)).

An emerging agenda has begun to show that peer effects can play an important role in criminality. Bayer et al. (2009) and Ouss (2013) find that inmate interactions influence their post-release criminal activity. Drago and Galbiati (2012) relatedly find that inmates stimulate the criminal behavior of their non-incarcerated peers after being released. This stands in contrast to Ludwig and Kling (2007) which found no measured correlation between criminal behavior and neighborhood crime levels in the Moving to Opportunity experiment.

Data constraints have limited the ability of researchers to study outcomes beyond criminal activity. Several studies consider whether incarceration and criminal history generate stigma in the labor market (Pager (2003), Bushway (2004) and Finlay (2009)). Another group of studies use panel data with individual fixed effects to evaluate how income changes after being released from incarceration (Grogger (1996), Western (2006), and Raphael (2007)). Inconsistent findings and concerns over omitted variables bias raise the need for further research.

Two recent papers that also rely on judge random assignment are most closely related my study. Kling (2006) studies the impact of incarceration length on labor market outcomes using state and federal prison records from Florida and California, respectively. He finds no evidence that longer prison sentences adversely affected labor market outcomes. Aizer and Doyle (2015) examine the impact of incarceration among juvenile offenders in Chicago and find that being sentenced to a juvenile delinquency facility reduces the likelihood of high school graduation and increases the likelihood of adult incarceration. Whether these studies are also contaminated by multidimensional or non-monotonic sentencing is not addressed. The population differences (e.g adult versus juvenile offenders) may explain their disparate findings, but the stark divergence raises the need for further investigation.

2. The Harris County Criminal Justice System

The setting for this study is Harris County, Texas. It includes the city of Houston as well as several surrounding municipalities. The Houston MSA has the fifth largest population in the United States and encompasses a geographical area slightly larger than the state of New Jersey. Its residents are economically and demographically diverse, which is reflected in the study population.

Texas is known for being tough on crime. Figure 1 plots the imprisonment rate in the United States and Texas. For the majority of the 20th century, the national rate hovered close to 100 prisoners per 100,000 residents. In the late 1970s, when the earliest state-level data is available, Texas stood as one of the leaders among states in its use of incarceration. While a binding capacity constraint in the prison system kept Texas close to the national levels throughout the late 1980s, a massive prison expansion in the early 1990s quickly elevated the imprisonment rate.[2]

Two court systems operate in Harris County: the Criminal Courts at Law (CCL) and the State District Courts (SDC). The fifteen CCLs have jurisdiction over cases involving misdemeanor charges and serve slightly more than 4,500 cases per court

[2]The widespread use of incarceration in Texas implies that defendants on the margin of being incarcerated may be less dangerous than marginal defendants in other settings. This will tend to tip the scale in favor of finding welfare losses in this context, and the results should be interpreted with caution when applying them to other settings. But, as Texas expanded its prison system, so too did the nation as a whole suggesting the marginal defendant in many locals has become less risky. And, given that Texas accounts for roughly 12 percent of the non-federal institutional population, this population is important to study in and of itself.

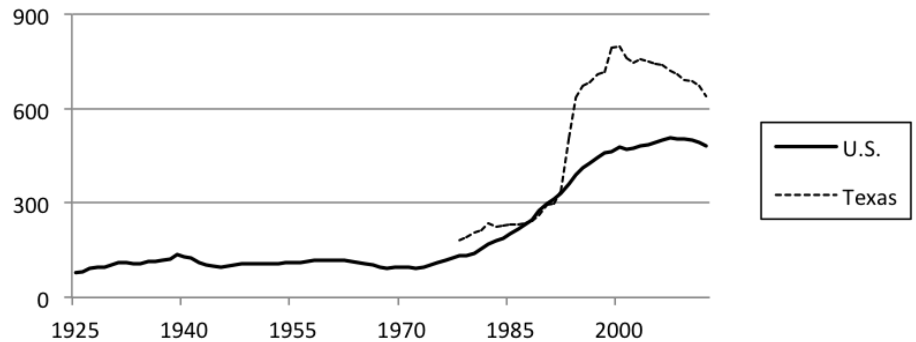


FIGURE 1. U.S. and Texas imprisonment rates per 100,000 residents

Sources: SUNY Albany, Sourcebook of Criminal Justice Statistics; BJS, Corrections Statistical Analysis Tool.

per year. Typical cases include traffic violations, non-habitual driving while intoxicated offenses, minor possession of marijuana, larceny of items worth less than $1,500, and non-aggravated assault. The twenty-two SDCs adjudicate cases involving felony charges and serve roughly 1,800 cases per court per year. The felony and misdemeanor courts are administratively segregated yet physically co-located at the Harris County Criminal Justice Center (1201 Franklin St., Houston, TX 77002).[3,4]

Both caseloads are predominantly male with mean age around 30 years old (see Table 1). As expected, misdemeanor defendants have less serious criminal histories compared to felony defendants. The most common crime types for misdemeanor cases are driving while intoxicated (DWI), other traffic related offenses, larceny involving less than $1,500 worth of property and minor possession of marijuana. For felony cases, the most common crimes are more serious drug possession (in terms of quantity or types), more costly property crimes, and aggravated assault.[5]

[3]In 1980, 10 CCLs and 18 SDCs were active. Additional CCLs were added in 1983, 1985 and 1995 and additional SDCs were added in 1982 and 1984.

[4]In addition to the Criminal Courts at Law and the State District Courts, there are also Justices of the Peace who rule on misdemeanor level C charges, and Federal District Courts for the Southern District of Texas which address federal crimes. In addition, minor offenders are generally prosecuted through the Family District Court system. None of these institutions are considered in the analysis and so they are not addressed at length.

[5]Crime types reflect the first charge a defendant faced in the event subsequent charges were added at a later date in the court proceedings, and the most severe crime if multiple charges were made at the same time.

TABLE 1. Characteristics of Harris County's Criminal Courts at Law and State District Courts' caseloads, 1980-2009

Defendant Characteristics	Criminal Court at Law (Misdemeanor Offenses)	State District Court (Felony Offenses)
Male	0.78	0.81
Age	29.84	30.26
First time offender	0.61	0.45
Total prior felony charges	0.44	0.93
Total prior misdemeanor charges	0.80	1.20
Type of criminal charge		
Driving while intoxicated	0.25	0.04
Traffic	0.11	0.01
Drug possession	0.11	0.26
Drug manufacture or distribution	0.00	0.09
Property	0.23	0.31
Violent	0.09	0.13
Median duration of trial (months)	1.35	2.14
Race/Ethnicity		
Caucasian	0.39	0.30
African American	0.31	0.46
Hispanic	0.29	0.23
Other	0.01	0.01
Total cases	1,449,453	775,576

Source: Author's calculations using Harris County District Clerk's court records.
Notes: Calculations do not include sealed court records, juvenile offenders, parole or probation violations or defendants charged with capital murder.

Roughly equal shares of Caucasian, African American and Hispanic defendants are represented in both caseloads. Misdemeanor cases have a larger proportion of Caucasians, whereas felony defendants are more likely to be African Americans. A number of other physical descriptors not shown are also available in the data including skin tone, height, weight, body type, eye color and hair color.

When criminal charges are filed against a defendant in Harris County, the case is randomly assigned to a courtroom.[6] Randomization is viewed as an impartial

[6]Two types of cases do not undergo random assignment. If a defendant is already on probation from a specific court, his new charges will automatically be assigned to that same courtroom. In addition, charges at the Capital Felony level are not randomly assigned because they generally require significant resources to adjudicate. Because neither of these types of charges are randomly assigned, they are dropped from the analysis.

assignment mechanism for defendants and an equitable division of labor between courtrooms. Up to the late 1990s, assignment was carried out using a bingo ball roller; this was later transitioned to a computerized system for automatic random case assignment. In order to ensure the case allocation mechanism is not corrupted, the Harris County District Clerk handles all assignments.

If a defendant violates the terms of their probation, they will generally return to their original court for new sentencing. Depending on the severity of the violation, this may or may not generate a new court charge.[7] Parole violators are not served by the court system and instead return directly to the Texas Department of Criminal Justice. Again, violations may generate new charges but this depends on the crime severity and time left on the inmate's original sentence.

Judges are elected to serve a specific bench and are responsible for presiding over all cases assigned to their courtroom. Elections occur every two years, and the vast majority of judges are reelected. As a result, a defendant's initial court assignment will likely determine the judge who presides over the entirety of his proceedings.

The Harris County District Attorney's office stations a team of three assistant district attorneys (one chief ADA and two subordinate ADAs) to each CCL and SDC. This team prosecutes all cases assigned to their courtroom with discretion over how to divide the workload within the team and the desired sentencing outcome.[8] Generally, ADAs serve close to a year or more in the same courtroom until staffing needs or promotions require reassignment. Courtroom assignment, therefore, also dictates a defendant's prosecution team as well.

In total, there were 111 elected judges and 1,262 ADAs operating in the Harris County criminal court system between 1980 and 2009. All but one judge served exclusively in either the misdemeanor or felony system. The majority of ADAs, however, worked in both caseloads with 73 percent serving in the felony courts and 91 percent working in the misdemeanor courts. The multitude of court actors creates many effective instruments for identification.

[7]When probation violations result in new charges, they are are dropped from this analysis since they are not randomly assigned.

[8]Interviews with the District Attorney's office revealed that prosecutors' conviction rates or trial outcomes are not routinely monitored for performance evaluation. Instead, their ability to consistently "clear" cases from the docket in a timely manner determines their standing in the department.

Criminal punishment depends of the combined discretion of the judge and prosecutor through plea bargaining and judicial sentencing. Sentencing guidelines established by the Texas Penal Code (see Online Appendix A) provide broad recommendations on maximum and minimum sentencing for defendants based on the degree of criminal charges. For instance, a second degree felony can receive anywhere between two and twenty years incarceration in state prison, while a class A misdemeanor can receive up to a year in county jail. The court can also choose to suspend most sentences of 10 years or less in favor of probation allowing defendants to forgo incarceration altogether under terms similar to parole.[9,10]

Texas subscribes to a combination of *determinate* and *indeterminate* sentencing systems depending on the degree of the criminal charge. Crimes that fall under determinate sentencing result in incarceration sentences that must be served in full regardless of behavioral considerations.[11] Indeterminate sentences represent a court-ordered maximum sentence and the Texas Board of Pardons and Parole (TBPP) decides if the inmate deserves early release. Sentence adjustments come in the form of granting "good time" credits to inmates and permitting supervised early release through discretionary parole. Texas's establishment of mandatory supervised release and truth in sentencing laws, however, which respectively set percentage floors and ceilings on time served narrow the influence of indeterminacy and TBPP.

This study relies on the fact that defendants are randomly assigned to courtrooms. This can be tested by estimating the following equation:

$$x_{i,t} = \alpha + \tau_t + \beta Court_i \otimes \tau_t + \epsilon_{i,t}$$

In the model, $x_{i,t}$ is a defendant trait, $Court_{i,t}$ is a vector of dummy variables for court assignment and τ_t are charge-year fixed effects. Because Harris County introduced several new courtrooms in response to growing caseloads over time, it is

[9] The flexibility given to judges in Texas stands in contrast to *presumptive* or *structured* sentencing. Such schemes provide a predetermined formula for assigning punishment typically based on crime severity and criminal history. While the formulaic recommendation is not always binding, Bushway et al. (2012) show it can act as a psychological anchor.

[10] Several additional features give judges and prosecutors influence over court outcomes. These include determining the admissibility of evidence, the indigent defense system, prosecution strategy, sentencing enhancements and other plea bargain terms.

[11] The only method of modifying these sentences is by court order.

TABLE 2. Testing for differences between courts

	F-Test			F-Test	
	Fel.	Misd.		Fel.	Misd.
Panel A: Defendant Characteristics			*Panel B: Sentencing Outcomes*		
Female	0.9	1.1	Verdict = Guilty	14.0	15.0
Race/Ethnicity = Caucasian	1.1	1.2	Verdict = Def. Adj. of Guilt	19.3	22.8
Age	2.3	1.0	Sentenced to Incarceration	14.8	19.7
Height	1.0	1.0	Incarceration Length	3.4	11.0
Weight	1.0	1.1	Given Fine	24.5	8.1
First Time Offender	1.3	1.2	Fine Amount	10.7	242.5
Crime = Drug Possession	1.3	1.1	Sentenced to Probation	18.8	26.3
Crime = Property Crime	1.4	1.2	Probation Length	18.7	24.3
Crime = Violent Crime	1.2	1.0	Misdemeanor Conviction	7.4	-

necessary to include τ_t to absorb compositional changes. In addition, since the identities of courtroom actors (i.e. judge, chief prosecutor, etc) are constantly evolving, I fully interact courtroom and year fixed effects so that courtroom deviations are not arbitrarily constrained over time. To evaluate if the observed caseloads are statistically equivalent, I conduct an F-test of the joint significance of the coefficients in β. I repeat this procedure with sentencing outcomes to establish a baseline of the instrument relevance based on average courtroom differences.

Table 2 shows the results of this exercise. The first panel shows the F-tests for differences in the balance of defendant covariates. The second panel shows the F-tests for differences in the balance of various sentencing outcomes. For dosage variables like fine amount or incarceration length, zeros are included for individuals who do not receive that sentencing outcome. The test statistics for defendant characteristics generally range between 1 and 1.4. These indicate a technical rejection of the null hypothesis, but capture very minor differences in court balance. In contrast, the test statistics for sentencing outcomes generally are all greater than 10, indicating a much stronger rejection of the null. Together these results indicate that assigned caseloads look very similar ex-ante but quite different ex-post.

3. SOURCES OF DATA AND MATCHING METHODS

This project uses multiple sources of administrative data. Information on court assignment, defendant and crime characteristics as well as sentencing outcomes were acquired from the Harris County District Clerk.[12] Initial filings of felony and misdemeanor charges between 1980 and 2009 are included in the data regardless of final verdict. Cases sealed to the public by order of the court, which account for less than half of a percentage point of the overall caseload, and criminal appeals were not included in the data.

For the purpose of the analysis, defendants charged with multiple criminal offenses or recharged for the same crime after a mistrial were collapsed to a single observation. In this scenario, I retained only the earliest filing date, charge characteristics and original sentencing outcomes. When a defendant faced multiple charges, I used the most severe charge in coding the defendant's crime type.

Administrative identifiers link defendants to their criminal histories in Harris County. This is supplemented by two additional sources to measure illegal activity. Booking data was acquired from the Harris County Sheriff's Department from 1978 to 2013, providing an opportunity to observe arrests that did not progress to court charges. Additionally, records from the Computerized Criminal History Database, provided by the Texas Department of Public State, track statewide convictions in Texas from the mid-1970s up to the present.[13]

Due to concerns regarding indeterminate sentencing, I acquired data on actual incarceration spans between 1978 and 2013 from the Texas Department of Criminal Justice for state prisons and from the Harris County Sheriff's Office for the local county jail. The data was matched using the defendant's full name and date of birth.

Quarterly unemployment insurance wage records for Texas between 1994 and 2012 were accessed through a data sharing agreement with the Texas Workforce Commission. Monthly Food Stamps and Temporary Assistance for Needy Families benefits between 1994/1992 and 2011 were accessed through a data sharing agreement with the Texas Health and Human Services Commission. Matching between the various data sources was based on full name, sex, exact date of birth and social security number depending on variable availability in each dataset.

[12]Archival research gathered judge tenure and assistant district attorney staffing documents from the courts and transcribed the information into an electronic database. Judges and assistant district attorneys were then mapped to criminal court cases using the defendant's filing date and assigned court number.

[13]Parole data was not available, and so parole violations are not tracked in this analysis.

4. Multidimensional and Non-monotonic Sentencing: Challenges and Solutions

To evaluate the impact of incarceration, this study relies on exogenous variation in sentencing outcomes attributable to random assignment of defendants to criminal courts. Prior work using this research design has generally been formalized using the following standard instrumental variable equations:

$$Y_i = \beta_0 + \beta_1(X_i)D_i + \beta_2 X_i + \epsilon_i \ , \tag{1}$$

$$D_i = \gamma_0 + \gamma_1 J_i + \gamma_2 X_i + \nu_i \ , \tag{2}$$

where,

$$E[\epsilon_i, \nu_i | X_i] \neq 0 \ , \ E[\epsilon_i, J_i | X_i] = 0 \text{ and } \gamma_1 \neq 0 \ .$$

In this notation, Y_i is the outcome variable, D_i is a criminal sentence (such as an indicator variable for being incarcerated or a continuous measure of the duration of incarceration), X_i is the observed defendant characteristics, and J_i is a vector of dummy variables for the defendant's randomly assigned judge.[14] The program effect can potentially be heterogeneous in defendant characteristics so $\beta_1(X_i)$ is allowed to depend on these traits. Non-zero coefficients in γ_1 indicate differences in average sentencing outcomes between judges who serve statistically equivalent populations. Such differences are often motivated on the basis that some judges are thought to be "tough" while others are "easy" on defendants.

Two additional assumptions are required in order to achieved unbiased results (Imbens and Angrist (1994), Angrist et al. (1996)). First, the exclusion restriction requires that $E[Y_i|D_i, X_i, J_i] = E[Y_i|D_i, X_i, J_i']$ meaning that judge assignment can only impact the final outcome through its influence on the criminal sentence. The second requirement is that the data must satisfy a monotonicity assumption: $\{E[D_i|X_i, J_i = j] \geq E[D_i|X_i, J_i = k] \ \forall i$ or $E[D_i|X_i, J_i = j] \leq E[D_i|X_i, J_i = k] \ \forall i\} \ \forall j, k$ meaning that defendants assigned to judges with higher incarceration rates must be at weakly higher risk for incarceration.

[14] In the specific context of this study, random court assignment results in both a random judge as well as a random team of assistant district attorneys. For the ease of notation and to remain consistent with the existing literature, however, I proceed using only judges in the model but knowing that they are a placeholder for all influential actors who are attached to a specific courtroom.

The parsimony of this model makes it quite appealing. The source of identification is intuitive, and the estimation is generally straightforward. The fact that my data exhibits multidimensional and non-monotonic sentencing patterns, however, limits the plausibility of satisfying these assumptions. Instead, two distinct biases arise which I call *omitted treatment bias* and *non-monotonic instruments bias*. This section describes the challenges associated with these features of the data and my strategies to mitigate the biases. Readers are referred to Online Appendix B for detailed empirical evidence that documents the biases and illustrates how addressing them changes the statistical and economic interpretation of the results.

Omitted treatment bias. Judges and other decision makers may have influence over several aspects of court outcomes (e.g. guilt or innocence, incarceration versus probation, duration of punishment, etc.). The researcher may, however, only be interested in a subset of sentencing outcomes. I refer to this subset of the endogenous variables as the focal sentencing outcomes (D_i^f) while the remaining ones are the non-focal set (D_i^n).

When judicial tendencies on focal and non-focal sentencing outcomes are correlated yet the latter is excluded from the estimation altogether, there is a violation of the exclusion restriction. This would happen, for instance, if judges who incarcerate more often also impose more fines. In this case the estimated impact of incarceration (when ignoring fines in the estimation) will capture a weighted sum of the combined effect of incarceration and fines. It is unrealistic to think that researchers ever observe the full set of treatments a defendant receives. For instance, the manner in which the judge speaks to the defendant is almost surely not documented in the data. But, to the extent that unobserved treatments play a minor role in producing long-term outcomes, are correlated with observed non-focal tendencies, or are uncorrelated with focal tendencies, the resulting bias would be minimal.[15]

[15]Compared to other settings, like research on the impact of going to a better school where treatments may include complex interactions between various school inputs and peer interactions, the criminal justice context relatively straightforward with respect to what the major components of D_i should include. These are: incarceration status and length, fine status and amount, probation status and length, and less common enrollment in alternative sentencing programs like electronic monitoring, drug treatment, boot camps, or driver's education. Since there is little to no interaction among defendants in the court room setting, there is minimal concern for peer influence at this stage.

Omitted treatment bias is avoided by estimating the full model, inclusive of both D_i^f and D_i^n. The model will have multiple endogenous sentencing variables, which are simultaneously instrumented, ensuring point estimates for the focal variables are identified off of residual variation after accounting for judicial tendencies on non-focal sentencing.

Computational implementation of this strategy can be challenging. To improve processing time, I split the estimation into two steps. Predicted values are first constructed for each non-focal variable using fitted first stage equations. The predicted values are then added to the set of controls in the focal first stage and outcome equations. The coefficients and standard errors on the focal variables from this alternative approach are statistically and numerically equivalent to simultaneously instrumenting for all endogenous variables jointly.

Non-monotonic instruments bias. Judges may vary in their relative treatment of different types of defendants.[16] One could be relatively tough on drug cases while easy on other crimes. This non-uniformity creates the opportunity for a given assignment to increase or decrease the probability of incarceration depending on a given defendant's traits. The resulting violation of the monotonicity assumption creates a bias that may lead to over or underestimates of the true effect; however, if non-uniformities in judicial behavior respond to observed defendant characteristics the bias can be avoided.

An alternative first stage equation to Equation 2 is:

$$D_i = \Gamma_0 + \Gamma_1(X_i)J_i + \Gamma_2 X_i + \nu_i \ . \tag{3}$$

In contrast to the standard approach, I propose allowing judicial preference to adjust according to defendant characteristics. The implication is that the monotonicity of judge assignment no longer needs to hold across all defendants but instead only among a group of peers with similar observable characteristics (e.g. Caucasian male

[16]Whether or not judges, case workers or other program administrators exhibit non-uniform preferences depends on the specific research context. Empirical work provides several examples of situations including medical care, criminal law and professional sports in which decision makers demonstrate non-uniform within-caseload preferences (see Korn and Baumrind (1998), Waldfogel (1998), Abrams et al. (2010) and Price and Wolfers (2010)).

drug offenders).[17] While the modified approach adds complexity to the model, it relaxes the assumptions necessary for unbiased results.

The structure of Equation 3 suggests non-parametric estimation, but when many covariates are observed this approach suffers from the curse of dimensionality. The problem could be simplified if the combination of traits included as interactions with judge assignment were pre-specified, which could be motivated by detailed institutional knowledge of the research setting. However, putting this choice in the hands of the researcher unfortunately opens the door to undisciplined specification searching which limits the reliability of the produced estimates.

A semi-parametric approach where $\Gamma_1(X_i)$ is approximated in a linear model using a series of basis functions provides a feasible compromise. In this framework,

$$\Gamma_1(X_i)J_i = \sum_{k=1}^{K} \omega_k b_k(X_i, J_i) + \eta_i, \tag{4}$$

where $b_k(\cdot)$ is a basis function using information on defendant traits (X_i) and judge assignment (J_i) that measures relative judicial preferences, the parameters ω_k provide weights to each $b_k(\cdot)$ and η_i is an approximation error.

Any number of basis functions could be utilized. I use a series of functions measuring judge-specific deviations from caseload-wide trends after conditioning on various combinations of defendant traits. The defendant traits I consider are: crime type, degree of charge, race, skin tone, sex, body type (i.e. thin, medium or heavy), height, weight, whether the defendant has a visible scar, whether the defendant has a visible tattoo, eye color, age, time since last criminal charge, time since last criminal conviction, total prior felony charges, total prior felony convictions, total prior misdemeanor charges, and total prior misdemeanor convictions.[18] The exact definitions of these basis functions is provided in Online Appendix C.

The basis functions themselves could be used jointly as separate instrumental variables, but the set of constructed preference measures is very large and could lead to many instruments bias (Hansen et al. (2008)). This problem is easily addressed

[17]A more general model could adopt a random effects framework to account for unobserved variation as well (see Heckman and Vytlacil (1998) and Wooldridge (1997), but is beyond the scope of this study.

[18]For continuous characteristics, I winsorize the top and bottom 5 percent of the distribution to improve boundary performance of the basis functions.

by invoking a cross validation sample splitting technique (Angrist and Krueger (1995)) wherein the overall sample is randomly divided into two halves, ω_k for one half of the data is estimated using the other half of the data, and vice versa. Through using "out-of-sample" observations to construct the final weighting of the $b_k(\cdot)$ to estimate $\hat{\Gamma}_1(X_i)$, overfitting the first stage is avoided and test statistics will not need to be adjusted.

The difficulty with cross validation is that the full set of candidate instruments likely contains many variables that contribute little to no additional information on judicial preferences. These variables add noise to the estimation and decrease prediction accuracy. A variety of shrinkage procedures can be employed to reduce dimensionality and isolate the key sources of variation in a model (see Hastie et al. (2009)). While it is acknowledged that these procedures introduce bias into the estimation of model parameters (first stage coefficients in my context), the potential variance reduction has generally been shown to result in improved prediction accuracy (Leeb and Pötscher (2008a, 2008b)), which is precisely my goal. I adopt the *least absolute shrinkage and selection operator* (Lasso) and its related cousin, *Post-Lasso*, originally proposed in Tibshirani (1996), which has received growing interest in recent years (Belloni et al. (2014)), and follow Belloni et al. (2012)'s specific implementation in my empirical analysis. See Online Appendix C for details.

To document which traits have the most influence on relative court tendencies, Table 3 reports the five strongest predictors of sentenced incarceration status selected by Lasso among the full set of candidate interactions between defendant characteristics and judge/prosecutor assignment. Because certain defendant-court interactions exhibit greater variance than others, each candidate instrument is normalized to mean zero and standard deviation one. As such, the largest coefficients will identify the characteristics over which courts exhibit the largest differences, which will have greatest influence over the final constructed instrument.

Members of the prosecution team are featured prominently in each set of selected variables reflecting their role in the courtroom and establishing plea bargains. In the felony caseload, judge and chief prosecutor preferences are difficult to disentangle due to the colinearity in their court tenures and so either individual measure should be thought to reflect their joint tendencies. The defendant characteristics that most heavily influence relative court opinion are interactions between the defendant's

TABLE 3. Five strongest selected instruments of incarceration

Court Agent		Defendant/Crime Characteristics			$\hat{\omega}$
Panel A: Felony caseload					
2nd Asst. Pros.	×	Crime type	×	Defendant age	0.008
Judge	×	Crime type	×	Total prior felony convictions	0.008
2nd Asst. Pros.	×	Crime type	×	Charge degree	0.008
2nd Asst. Pros.	×	Crime type	×	Defendant race	0.006
1st Asst. Pros.	×	Crime type	×	Charge degree	0.005
Panel B: Misdemeanor caseload					
2nd Asst. Pros.	×	Time since last charge	×	Total prior misd. charges	0.025
Judge	×	Crime type	×	First-time/Repeat offender	0.007
Chief Pros.	×	Crime type	×	Charge degree	0.004
2nd Asst. Pros.	×	Crime type	×	Defendant age	0.004
1st Asst. Pros.	×	Crime type	×	Charge degree	0.004

Notes: Asst. Pros. (Chief Pros.) stands for the assistant prosecutor (chief prosecutor).

type of crime, degree of charge, and criminal history. Among demographic characteristics, defendants' age and race generate the largest amount of between-court variation. Interestingly, although the algorithm is given the opportunity to select first- and second-order terms, Lasso only selects third-order interactions indicating a predictive gain when using a more flexible albeit noisier specification.

5. REDUCED FORM AND GRAPHICAL EVIDENCE

I now turn to a basic exercise that previews the main results and illustrates the challenges faced with multidimensional and non-monotonic sentencing. Figure 2 plots histograms for the court-specific deviations from the caseload-wide incarceration rate per quarter. The exercise pools both the felony and misdemeanor caseloads together, but calculates the deviations separately by court system.[19] These are shown relative to kernel-weighted local polynomial regression lines illustrating how average defendant baseline traits and five year followup outcomes covary with court incarceration rates. The first column uses variation at the court level while the second column relies on variation at the disaggregated court × crime level to account for possible non-monotonicities.[20] The final row residualizes all of the variables

[19]Disaggregating this exercise by court system yields the same conclusions in both subsamples. Figures are available upon request.

[20]Exercises respectively have quarter-of-charge or quarter-of-charge × crime type fixed effects.

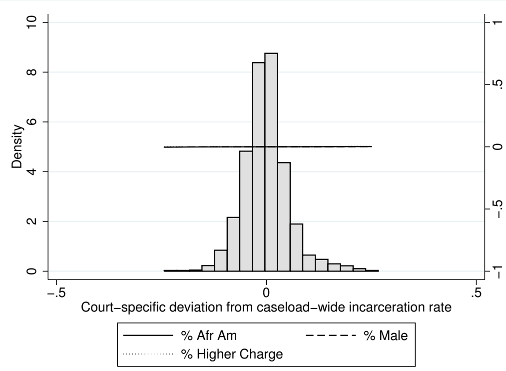
based on the court (or court $\times$ crime) tendencies on probation length, fine amount and use of deferred adjudication of guilt to account for omitted treatment bias.

Significantly more between court variation appears when disaggregating the analysis by crime type as the histograms in the second columns are notably more dispersed. This suggests substantial heterogeneity between courts in their treatment of caseload subgroups. Whether relying on differences in overall court deviations or crime-specific court deviations from caseload-wide trends, there does not appear to be a strong correlation between defendant baseline characteristics (race, sex, and severity of charge) and incarceration tendencies (see subplots A and D). This supports the assumptions of the research design, mainly that (1) defendants are randomly assigned, and (2) courts exert discretion over sentencing outcomes.

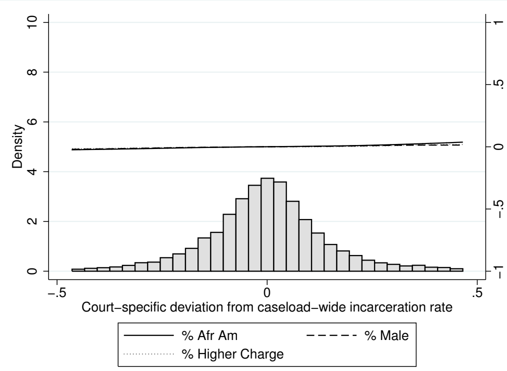
In comparing subplots B and E, there is a notable difference in the estimated relationship between the incarceration rates and future outcomes for both the misdemeanor and felony caseloads. When relying on court level variation, it appears as if there is minimal relationship between the court's incarceration rate and its defendants' future criminal and labor market activity. But, when allowing the analysis to vary at the finer grained level of court $\times$ crime, there is a clear positive relationship between incarceration and recidivism and negative relationship between incarceration and future employment. The divergence in these results suggests the non-monotonic instruments bias is non-trivial in the full sample in addition to the subsamples used in Online Appendix B.

Finally, subplots C and F residualize the variation in the plots according to the court (or court $\times$ crime) tendencies on probation length, fine amount and use of deferred adjudication of guilt to address potential omitted treatment bias. Comparing subplots B and C as well as E and F show that the residual variation in incarceration deviations is relatively more compressed compared to the non-residualized deviations and that the estimated relationships are somewhat weaker than initially estimated. This indicates that there is systematic correlation in judicial tendencies on a variety of sentencing outcomes that affect future outcomes. Relying simply on point estimates from subplot E then would tend to overstate the impact of incarceration on future outcomes.

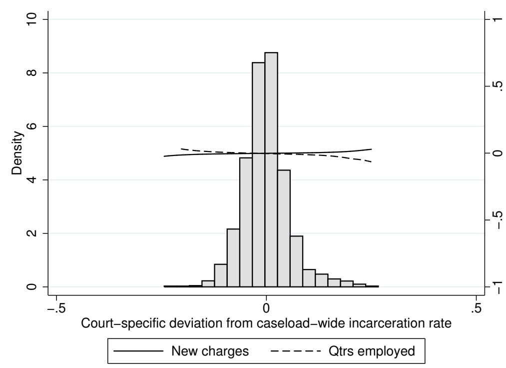
The results documented in subplot F, which is my preferred specification, show that judges and prosecutors who rely more heavily on incarceration tend also to



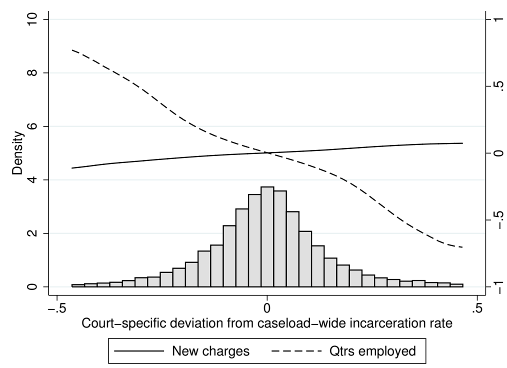
(A) Defendant traits relative to court incarceration rate



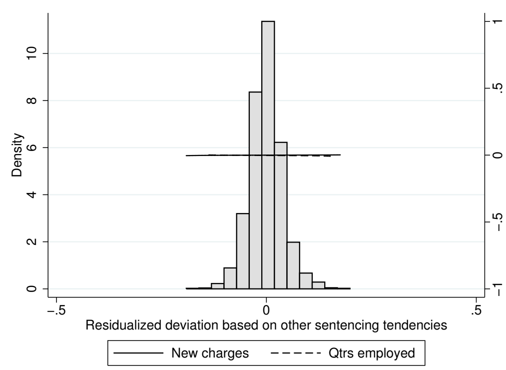
(D) Defendant traits relative to court incarceration rate, disaggregated by crime type



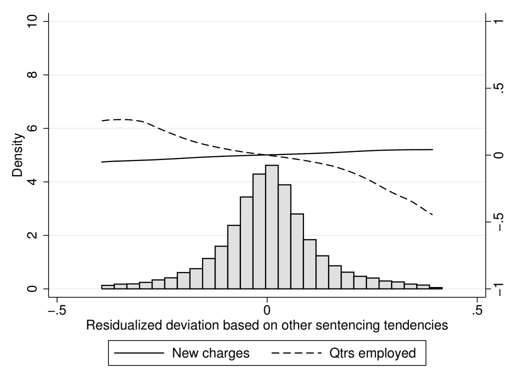
(B) Future outcomes relative to court incarceration rate



(E) Future outcomes relative to court incarceration rate, disaggregated by crime type



(C) Future outcomes relative to residualized court incarceration rate



(F) Future outcomes relative to residualized court incarceration rate, disaggregated by crime type

FIGURE 2. Defendant traits, future charges and future employment by incarceration rate among felony courts

have caseloads that exhibit higher rates of recidivism and worse labor market outcomes. Incarceration results in a net gain of 0.3 additional criminal charges and -1.1 quarters worked per marginal defendant. The impact on future criminal behavior is concerning as it suggests that the post-release criminogenic effect of incarceration is sufficiently strong to overcome potential incapacitation effects. Given the implications of this finding, corroborative evidence that can disentangle the incapacitation versus post-release effects would strengthen this result. The modest average relationship between incarceration and future employment is not surprising given that only one-third to two-fifths of defendants are employed prior to being charged; however, the concentrated effect for those with stable pre-charge employment is likely much higher. Whether the result is purely attributable to the mechanical separation from the labor market during incapacitation or the lasting effects of incarceration post-release cannot be distinguished. To make these finer distinctions, a panel model is necessary and is the focus of the next section and remaining empirical analysis.

6. THE PRE- AND POST-RELEASE EFFECTS OF INCARCERATION

The main results use a panel framework developed to estimate both the contemporaneous and post-release effects of incarceration. Outcome Y for individual i, q quarters after being charged at time t is modeled as a linear function of his incarceration status and history, estimated court tendencies for non-focal sentencing outcomes ($\hat{D}_i^n$), and individual characteristics (X_i). Incarceration status and history are formalized as three variables: (1) the percent of days in a quarter that a defendant was incarcerated, (2) whether the defendant has been released from incarceration, and (3) the total amount of time the defendant has spent incarcerated.[21]

Using a quarterly (as opposed to monthly, weekly or daily) unit of observation may introduce measurement error into the analysis. Sixty to seventy percent of defendants are booked in county jail the week charges are filed. This generates a positive, mechanical correlation between incarceration status and criminal charges in any given quarter during my followup period. To address this, I ignore days incarcerated until a new quarter has started. This breaks the mechanical relationship

[21] Incarceration spans in county jail that were less than 48 hours were omitted from any of these variables to avoid conflating incarceration with arrest and booking activity.

between new charges and imprisonment and eliminates the simultaneity bias. This modification has minimal impacts on estimates for the felony caseload since incarceration spells generally span several quarters if not years, but is important for the misdemeanor caseload where the median incarceration spell is 10 days.

To account for any unobserved differences based on the timing of defendant's original charge or the amount of follow-up time since the charge was filed, fixed effects μ_t and μ_q are also included. This model is presented below:

$$Y_{i,t+q} = \delta_1 Incar_{i,t+q} + \delta_2 Rel_{i,t+q} + \delta_3 \left(Rel_{i,t+q} \times Exp_{i,t+q}\right) + \delta_{4,q} \hat{D}_i^n + \delta_{5,q} X_i + \mu_t + \mu_q + \xi_{i,t+q} \ , \tag{5}$$

where the primary variables of interest are defined as:

$$\begin{aligned} Incar_{i,t+q} &= \frac{\text{Days Incarcerated}_{i,t+q}}{\text{Days in Quarter}_{t+q}} \ , \\ Rel_{i,t+q} &= 1\left[\sum_{\tau=1}^{T} Incar_{i,t+q-\tau} > 0\right] \times 1\left[Incar_{i,t+q} < 1\right] \ , \\ Exp_{i,t+q} &= \min\left[\sum_{\rho=1978q1}^{t+q} \frac{Incar_{i,\rho}}{4} \quad , \quad 5 \quad\right] . \end{aligned}$$

To compute $Rel_{i,t+q}$, a maximum retrospective window (denoted by T) is necessary. A narrower window is used will capture primarily short-run effects, while a longer window will average short-run impacts with long-term impacts. To strike a balance between short and long-run outcomes, I set T equal to 5 years.

Total incarceration exposure is measured as the cumulative time spent incarcerated since the first quarter of 1978 when the prison data begins. It is measured in years of incarceration and is capped at 5 years to reflect the likely diminishing returns to incarceration length and to improve the precision in the construction of the instruments. I allow total exposure to impact outcomes only once an inmate has been released to avoid confounding duration with incapacitation effects. The model is estimated on five years of quarterly post-charge data. Because defendants are tracked starting at their charge date, pre-trial detention will equally contribute to my incarceration measures as post-conviction sentencing. To account for repeated observations for the same defendant over time as well as over for multiple charges, the standard errors are clustered at the defendant level.

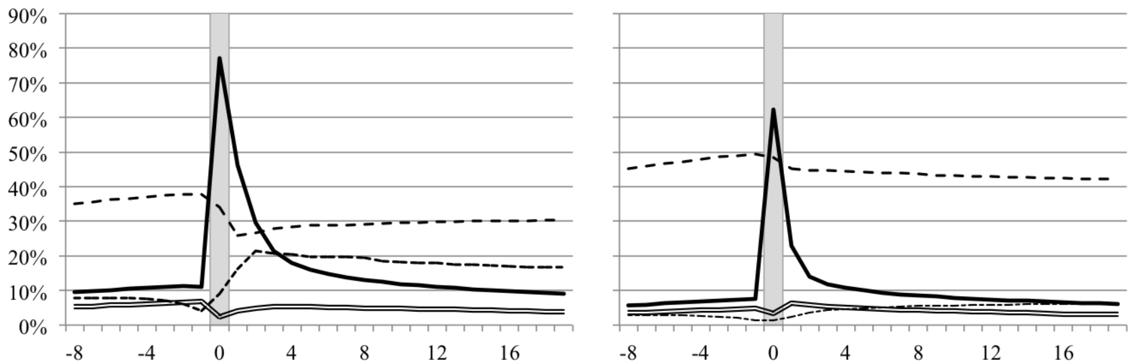
Instruments for the primary variables of interest are constructed using the methodology discussed in Section 4. Because the data collected for this study spans 30 years of court filings, a time during which some judges remain in office for over 20 years, the full set of instruments are recalculated every 2 years over the range of t. This allows the estimated preferences of judges and assistant district attorneys who remain in the court system for many years to change with time. Their relative preferences will correspondingly adjust according to the court composition at the time charges were filed (e.g. a "tough" judge becomes relatively less tough when all of the "easy" judges are replaced with other "tough" judges).

Unlike fixed court outcomes such as guilt or fines, incarceration status and history evolve with time. As a result, instruments for these variables must be recalculated each followup quarter. This amounts to comparing the relative portion of each court's caseload that is incarcerated one quarter after charges were filed, two quarters after and so on. A benefit of recalculating the instruments is the ability to leverage non-linear differences in the sentencing length distributions between courtrooms. As an example, one court may rely on a bimodal distribution of primarily short-term and long-term incarceration, whereas another may utilize a uniform distribution of sentences. While the courts' average sentence lengths might be equal, the realization of these sentences over time will vary substantially.

The misdemeanor caseload does not have a wide distribution in the length of incarceration; the median incarceration length in this caseload is only 10 days. This limits the feasibility of estimating the full model proposed. Instead, I will exclude the $[Released_{i,t+q} \times Exposure_{i,t+q}]$ variable leaving the misdemeanor analysis to only focus on incapacitation effects and extensive margin post-release effects .

Restructuring the data into a panel format yields several observations. Figure 3 shows the incarceration, criminal charge and employment rates of felony and misdemeanor defendants relative to the timing of their criminal charges. Incarceration status is separated out into being in county jail versus state prison, and in order to preserve the scale of the figures the focal court charge in quarter 0 is excluded.

In the run up to being charged, there is a relative decline in the incarceration rate of both felony and misdemeanor defendants which is mirrored by an increase in criminal activity. Once charges are filed, there is an immediate uptick in being jailed which is later transitions to prison for felony defendants. Incarceration coincides



(A) Felony defendants

(B) Misdemeanor defendants

FIGURE 3. Incarceration, criminal charges, and employment by relative quarter

with a distinct drop in criminal activity and employment. However, as inmates are released (months for felony defendants, weeks for misdemeanor defendants), there appears to be a modest short-run increase in criminal activity. In the 5 years of post-charge data, employment does not return to pre-charge levels.

To evaluate how long court assignment affects incarceration status, Figure 4 plots the first stage's R^2 separately by followup quarter. Evaluating instrument strength by quarter also provides an opportunity to further validate the estimation procedure using pre-charge data as a falsification test. The constructed pre-charge instruments have zero explanatory power in both caseloads as expected.[22] There is a sharp break, however, once charges are filed indicating that court assignment has a clear albeit modest impact on incarceration status. The influence of random assignment is most pronounced during the first year after being charged. At its peak, the R^2 is 0.01 in the first quarter after charges were filed for the felony caseload and 0.0025 for the misdemeanor caseload. Despite the decline, the predictive power remains non-zero in the post period for the felony caseload.

The panel model is first estimated using criminal activity as the dependent variable. This is captured using three different measures: county jail bookings, Harris County criminal court charges, and statewide criminal convictions in Texas. Each of these measures comes from a different data source, and they are not perfectly

[22]Contrary evidence would indicate unbalanced caseloads based on prior incarceration status.

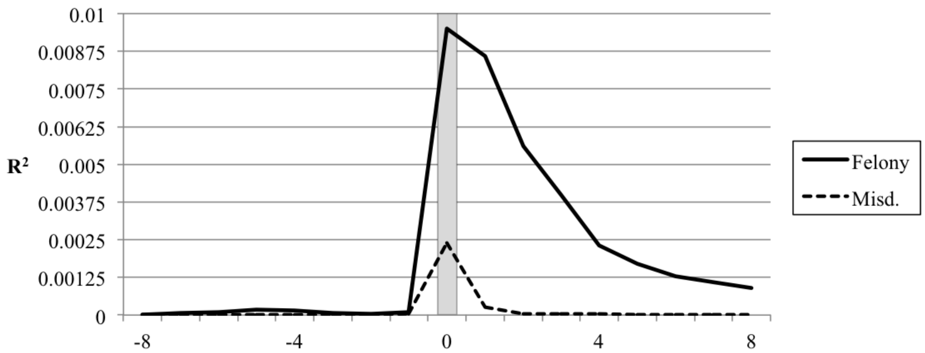


FIGURE 4. R^2 of incarceration first stage regression, by quarter relative to charges

nested as a result. Table 4 shows the coefficient estimates separately for the felony and misdemeanor caseloads using OLS in the first and third columns and IV in the second and fourth columns. Each panel shows the coefficients for a different outcome (bookings, charges, and statewide convictions).

The OLS estimates show a negative impact of incarceration on criminal activity while defendants are in jail or prison. The estimates indicate that about 2 to 4 percent of defendants would be arrested, charged or convicted in relation to a new criminal offense per quarter in the absence of incarceration. Once defendants are released from incarceration, however, they are more likely to be involved in criminal activity especially those returning after longer incarceration sentences. The incapacitation effects measured here, however, likely underestimate the true effect of incarceration as those not incarcerated have the lowest probability of reoffending. Likewise, post-release estimates may be biased upwards given that those who are incarcerated in the first place also are thought to have unobserved characteristics that increase their probability of committing crimes.

As expected the IV estimates show a higher incapacitation rate of 3 to 6 percentage points per quarter for marginal felony defendants.[23] This decline in criminal

[23]The measured incapacitation rate in this study is notably lower than other researchers' estimates. While this may be attributable to being a feature of the local context or the specific measures of recidivism, the post-release increases in criminality suggest an alternative explanation. The literature's strongest evidence on incapacitation comes from research designs that rely on quasi-random variation in sentence reductions among inmates who are already in jail or prison. Their estimates then are based on a group with a higher likelihood of reoffending compared to defendants who are never incarcerated in the first place.

TABLE 4. Impact of incarceration on criminal activity

Criminal Caseload	Felony		Misdemeanor	
	OLS	IV	OLS	IV
Panel A: Booked in county jail for new arrest				
In jail or prison	-0.023***	-0.033***	-0.035***	0.22***
	(0.00032)	(0.0080)	(0.00048)	(0.024)
Released from incarceration	0.023***	0.0038	0.033***	0.020***
	(0.00024)	(0.0074)	(0.00018)	(0.0046)
[Released × Duration]	0.025***	0.067***		
	(0.00021)	(0.0058)		
Panel B: Charged in Harris County criminal court with new offense				
In jail or prison	-0.023***	-0.060***	-0.031***	0.11***
	(0.00028)	(0.0068)	(0.00044)	(0.021)
Released from incarceration	0.018***	0.00092	0.028***	0.015***
	(0.00020)	(0.0066)	(0.00016)	(0.0041)
[Released × Duration]	0.020***	0.056***		
	(0.00020)	(0.0053)		
Panel C: Convicted of criminal offense in Texas				
In jail or prison	-0.0025***	-0.028***	-0.016***	-0.025
	(0.00029)	(0.0074)	(0.00034)	(0.020)
Released from incarceration	0.015***	-0.00071	0.015***	-0.0060*
	(0.00020)	(0.0058)	(0.00013)	(0.0036)
[Released × Duration]	0.012***	0.036***		
	(0.00019)	(0.0047)		
Kleibergen-Paap rk LM stat.		536.3		610.5
Kleibergen-Paap rk Wald F stat.		181.1		307.5
Unique defendants	462,377	431,422	897,934	887,019
Total observations	15,425,207	13,744,324	29,976,888	29,222,981

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Notes: Outcomes measured for up to 20 quarters after initial charges. Standard errors in parentheses clustered at defendant level. Quarter of charge fixed effects, quarters since charge fixed effects and defendant characteristics fully interacted with quarters since charge fixed effects included in all regressions.

activity, however, is offset by an increase in post-release criminal activity of 4 to 7 percentage points per quarter for each additional year spent incarcerated. The increase in future charges should be of particular concern since it rapidly reverses any cost savings from crime prevented.

In the misdemeanor caseload, the IV coefficients on incarcerated are noteworthy. Taken literally, these results suggest being incarcerated leads to criminal acts in jail. This interpretation, however, is likely incorrect as it is extremely uncommon in the data for inmates to be charged with a new crime while in county jail. Instead, what is at issue is the fact that the median incarceration sentence in this caseload is only 10 days, which is much shorter than the resolution at which the data is constructed.[24] As a result, the coefficient is measuring the combined effect of incapacitation as well as immediate reentry. Because the median defendant spends only a fraction of the quarter incarceration incarcerated, the coefficient should be scaled down by roughly one-tenth for accurate interpretation bring the measured effect essentially inline with the post-release coefficient.

Among felony defendants, the types of criminal charges prevented as a result of incarceration tend to be evenly split between misdemeanor and felony offenses (see Table 5). The crimes encouraged through incarceration's impact on post-release behavior, on the other hand, tend to be primarily felony-level crimes. This is particularly concerning because this indicates that criminal activity not only appears to be going up on net, but also becoming more serious. The misdemeanor caseload does not follow this trend. Instead, the increase in criminal activity overall tends to be more weighted towards new misdemeanor charges. This could explain why no statistically significant effects were observed for statewide convictions since the TDPS data has poor coverage of less serious crimes.

Several mechanisms could explain the increased likelihood of new criminal charges post-release. Incarceration may facilitate the transmission of criminal capital through peer interactions among inmates; penalties to labor market outcomes could increase material hardship, encouraging theft or pursuit of illegal income sources; or, diminished social capital may reduce one's incentives to avoid future incarceration. To evaluate the first of these hypotheses, Table 6 documents whether defendants were more or less likely to be charged with new types of crimes compared to their original offense. Each column in the table considers whether incarceration affected the likelihood of committing a specific type of crime (i.e. property, drug possession, drug manufacture or distribution, violent, and driving while intoxicated) for

[24]Estimating the model at the weekly level was not feasible due to computational constraints.

TABLE 5. Comparing impacts on felony versus misdemeanor charges

Criminal Caseload	Felony		Misdemeanor	
	OLS	IV	OLS	IV
Panel A: Charged in Harris County criminal court with misdemeanor offense				
In jail or prison	-0.013***	-0.031***	-0.022***	0.046***
	(0.00019)	(0.0048)	(0.00033)	(0.016)
Released from incarceration	0.012***	0.0049	0.017***	0.014***
	(0.00015)	(0.0044)	(0.00013)	(0.0034)
[Released × Duration]	0.0063***	0.014***		
	(0.00011)	(0.0033)		
Panel B: Charged in Harris County criminal court with felony offense				
In jail or prison	-0.011***	-0.034***	-0.010***	0.064***
	(0.00019)	(0.0047)	(0.00025)	(0.013)
Released from incarceration	0.0074***	-0.0022	0.013***	0.0032
	(0.00013)	(0.0046)	(0.000088)	(0.0023)
[Released × Duration]	0.015***	0.047***		
	(0.00015)	(0.0041)		
Kleibergen-Paap rk LM stat.		536.3		610.5
Kleibergen-Paap rk Wald F stat.		181.1		307.5
Unique defendants	462,377	431,422	897,934	887019
Total observations	15,425,207	13,744,324	29,976,888	29222981

*** p<0.01, ** p<0.05, * p<0.1.
Notes: See notes in Table 4.

the group of defendants not originally charged with this specific crime. These five crime groupings account for 70 percent of the charges in the data.

The first panel in Table 6 shows the results for felony defendants. I find that longer exposure to jail and prison increases the likelihood of new criminal behavior with the largest effects observed for drug possession and property crimes. While the increase in property crimes could be an indication that incarceration impacts income stability, the effect on drug offenses, which are quite among inmates, suggests a distinct possibility for criminal learning. Impacts on drug manufacture or distribution follow similar patterns. The second panel shows the results for misdemeanor defendants. Like the felony context, misdemeanor defendants are more likely to be charged with drug possession or dealing post-release, even if their prior offense did not relate to drugs. In addition, I also observe a small but significant increase in the likelihood of violent offenses post-release.

TABLE 6. Impact of incarceration on committing new types of offenses

Type of criminal offense:	Property	Drug poss.	Drug mfr. or distr.	Violent	DWI
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.011***	-0.013***	-0.0042***	-0.0059***	-0.0026**
	(0.0033)	(0.0030)	(0.0013)	(0.0021)	(0.0013)
Released from incarceration	-0.0035	-0.000052	-0.00015	0.0021	0.00065
	(0.0033)	(0.0030)	(0.0013)	(0.0018)	(0.0013)
[Released × Duration]	0.015***	0.013***	0.0045***	0.00085	-0.00095
	(0.0028)	(0.0031)	(0.0012)	(0.0014)	(0.00078)
Kleibergen-Paap rk LM stat.	390.0	286.4	433.2	504.6	518.9
Kleibergen-Paap rk Wald F stat.	131.5	96.0	146.0	170.4	175.2
Unique defendants	344,395	347,337	408,013	359,991	413,127
Total observations	10,228,285	9,829,092	12,458,737	11,355,229	13,157,796
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.0042	0.018**	0.0089**	0.010	-0.0017
	(0.011)	(0.0089)	(0.0045)	(0.0074)	(0.0046)
Released from incarceration	-0.00030	0.00027	0.00031	0.0032**	0.00046
	(0.0018)	(0.0016)	(0.00069)	(0.0013)	(0.0013)
Kleibergen-Paap rk LM stat.	415.0	576.3	607.7	524.5	491.6
Kleibergen-Paap rk Wald F stat.	208.6	290.4	306.0	264.0	247.6
Unique defendants	747,535	816,217	882,885	822,456	673,906
Total observations	23,525,669	25,709,334	29,088,997	26,299,327	21,806,616

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Notes: Each column excludes defendants originally charged with the type of crime being considered as the outcome variable. See additional notes in Table 4.

Table 7 shows how incarceration impacts quarterly employment, income and log income. While the specific magnitudes differ, the panels present similar stories: incarceration has a substantial impact on labor market outcomes while inmates are confined and a smaller but significant lasting negative impact after release. The OLS estimates are larger in magnitude, likely driven by omitted variable bias, but the IV results still remain negative and significant. Based on the IV estimates, felony and misdemeanor defendants were respectively 32 to 40 percentage points less likely to be employed while incarcerated. Stated another way, marginal defendants who were not incarcerated were roughly five times more likely to be gainfully employed than be charged with another criminal offense if not incarcerated.

In sharp contrast with prior research, I find the negative effect of incarceration extends beyond just the period of incapacitation. For each additional year of incarceration, felony defendants were 3.6 percentage points less likely to be employed

TABLE 7. Impact of incarceration on labor market outcomes

Criminal Caseload	Felony		Misdemeanor	
	OLS	IV	OLS	IV
Panel A: Quarterly employment				
In jail or prison	-0.40***	-0.32***	-0.41***	-0.40***
	(0.0019)	(0.037)	(0.0016)	(0.12)
Released from incarceration	-0.088***	-0.054	-0.082***	-0.045
	(0.0018)	(0.043)	(0.0012)	(0.031)
[Released × Duration]	-0.019***	-0.036*		
	(0.00053)	(0.019)		
Panel B: Quarterly log(earnings+1)				
In jail or prison	-3.30***	-2.59***	-3.30***	-3.25***
	(0.016)	(0.30)	(0.013)	(0.98)
Released from incarceration	-0.90***	-0.55	-0.86***	-0.42
	(0.015)	(0.35)	(0.010)	(0.27)
[Released × Duration]	-0.17***	-0.34**		
	(0.0042)	(0.16)		
Panel C: Total quarterly earnings				
In jail or prison	-2247.1***	-1632.1***	-2265.0***	-1641.0*
	(16.8)	(293.0)	(13.2)	(951.3)
Released from incarceration	-1119.3***	-683.5**	-1244.0***	-466.0
	(16.3)	(345.3)	(11.4)	(298.8)
[Released × Duration]	-140.5***	-246.5		
	(3.55)	(150.3)		
Kleibergen-Paap rk LM stat.		327.6		148.4
Kleibergen-Paap rk Wald F stat.		110.5		74.4
Unique defendants	259,698	243,491	424,306	419,432
Total observations	8,035,049	7,263,800	13,401,574	13,098,771

*** p<0.01, ** p<0.05, * p<0.1.
Notes: See notes in Table 4.

and earned 0.34 less log income. That outcomes decline with more time behind bars suggests a model of human capital erosion on top of potential labor market stigma. Misdemeanor defendants are 4.5 percentage points less likely to be employed and earn 0.42 less log income after being incarcerated, which are both marginally insignificant. As these magnitudes are well below the estimated incapacitation effects, many inmates likely return to pre-charge income levels.[25]

[25]Prior research has had difficulty establishing causal evidence of human capital atrophy from adult incarceration. One factor contributing to this may relate to what is observed for the majority

To further explore the impact on labor market outcomes, Table 8 breaks out the labor market impacts according to pre-charge income levels. Defendants were classified as either having \$0 in average annual income, between \$1 and \$17,050 (the cutoff for living below poverty level for a family of four), or having greater than \$17,050 in annual income. Prior earnings were calculating using up to 3 years of pre-charge data. A number of defendants were excluded from this analysis because their charge dates were before 1994 when the unemployment insurance wage records begin, making it impossible to calculate their pre-charge income level.

This table shows that labor market impacts for felony defendants are primarily concentrated among individuals with the strongest pre-charge earnings (see Panel A). The employment loss for individuals who previously earned over \$17,050 per year was 46 percentage points while incarcerated (i.e. in the absence of incarceration, about half of inmates of this type would have continued being employed). For those serving at least two years, at least 40 percent then fail to reintegrate into the labor market after release, resulting in long-term earnings loss. As a point of comparison, von Wachter et al. (2009) finds job displacements from mass layoffs result in an immediate loss of 30 percent in annual earnings and long-term loss of 20 percent after 15 to 20 years.

To determine whether incarceration affected dependence on government programs, Table 9 shows the impacts of incarceration on the take-up of the Food Stamps/Supplemental Nutrition Assistance Program as well as the take-up of Aid to Families with Dependent Children/Temporary Assistance for Needy Families. While policy dictates that inmates lose benefits while they are incarcerated, there is little evidence (based on the IV estimates) that incarceration terminates benefit take-up. Post-release, felony defendants were 5 percentage points more likely to receive Food Stamps benefits per quarter, while misdemeanor defendants were 1 percentage point more likely to receive cash welfare. This increased reliance on social programs serves as additional evidence that inmates struggle with self-sufficiency after being released from incarceration.

---

of inmates who earn little to no income prior to charges. The job loss rate during incarceration for marginal low-income defendants ranges from 8 to 38 percentage points, meaning that most very low-income defendants would not be employed byeven in the absence of incarceration. This indicates that most marginal defendants are only weakly attached to the formal labor force, and so earnings in the formal sector may be a poor proxy for human capital due to lack of variation.

TABLE 8. Labor market impacts by pre-charge income level

	Employment			Log Wages		
Panel A: Felony defendants, Instrumental variables						
In jail or prison	-0.080*	-0.38***	-0.46***	-0.60*	-2.91***	-4.23***
	(0.044)	(0.051)	(0.15)	(0.35)	(0.41)	(1.35)
Released from incarceration	-0.023	-0.067	-0.094	-0.16	-0.65	-1.11
	(0.063)	(0.060)	(0.10)	(0.49)	(0.48)	(0.96)
[Released × Duration]	0.0064	-0.020	-0.15	0.019	-0.19	-1.34
	(0.021)	(0.029)	(0.11)	(0.17)	(0.23)	(1.00)
Net post-release effect:						
6 months in prison	-0.02	-0.08	-0.17*	-0.15	-0.75*	-1.78**
1 year in prison	-0.02	-0.09*	-0.24**	-0.14	-0.85**	-2.45**
2 years in prison	-0.01	-0.11*	-0.39**	-0.12	-1.04**	-3.79**
Kleibergen-Paap rk LM stat.	119.7	142.3	20.1	119.7	142.3	20.1
Kleibergen-Paap rk Wald F stat.	40.3	47.8	6.73	40.3	47.8	6.73
Annual Pre-Charge Income	0	$1 - $17,050	$17,051+	0	$1 - $17,050	$17,051+
Unique defendants	65,334	132,042	25,963	65,334	132,042	25,963
Total observations	2,013,657	3,796,562	572,857	2,013,657	3,796,562	572,857
Panel B: Misdemeanor defendants, Instrumental variables						
In jail or prison	-0.046	-0.47***	-0.17	-0.037	-3.58***	-1.63
	(0.14)	(0.17)	(0.56)	(1.13)	(1.36)	(5.21)
Released from incarceration	-0.028	-0.010	-0.048	-0.36	-0.0098	-0.51
	(0.063)	(0.046)	(0.057)	(0.51)	(0.38)	(0.54)
Kleibergen-Paap rk LM stat.	40.0	71.8	14.1	40.0	71.8	14.1
Kleibergen-Paap rk Wald F stat.	20.0	36.0	7.06	20.0	36.0	7.06
Annual Pre-Charge Income	0	$1 - $17,050	$17,051+	0	$1 - $17,050	$17,051+
Unique defendants	92,526	228,499	70,048	92,526	228,499	70,048
Total Observations	2,712,784	7,088,968	1,714,330	2,712,784	7,088,968	1,714,330

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Notes: Pre-charge income calculated using up to 12 quarters of pre-charge data. See additional notes in Table 4.

Robustness. A number of robustness tests were conducted to confirm the stability of the results. These include a more conservative clustering of standard errors, intentional omission of important defendant characteristics in the first stage, testing for sensitivity to first stage misspecification, trimming extreme values in the instruments, using Lasso-weighted instruments instead of Post-Lasso, and dropping the shrinkage procedure altogether. The results are quite robust across the different specifications. See Online Appendix D for further details.

TABLE 9. Incarceration and public benefit receipt

	Felony Caseload		Misd. Caseload	
	OLS	IV	OLS	IV
Panel A: Quarterly Food Stamps receipt				
In jail or prison	-0.026***	-0.0087	-0.045***	-0.016
	(0.00090)	(0.018)	(0.00077)	(0.068)
Released from incarceration	0.037***	0.049**	0.033***	0.024
	(0.00089)	(0.020)	(0.00058)	(0.015)
[Released × Duration]	0.0023***	-0.016		
	(0.00031)	(0.011)		
Kleibergen-Paap rk LM stat.		464.4		186.1
Kleibergen-Paap rk Wald F stat.		157.1		93.3
Unique defendants	358,619	333,888	654,624	645,576
Total observations	9,785,345	8,864,396	17,982,294	17,583,624
Panel B: Quarterly cash welfare receipt (AFDC or TANF)				
In jail or prison	-0.0083***	-0.00049	-0.0088***	-0.024
	(0.00037)	(0.0084)	(0.00029)	(0.021)
Released from incarceration	0.0043***	0.0094	0.0039***	0.010*
	(0.00040)	(0.0093)	(0.00023)	(0.0061)
[Released × Duration]	-0.0015***	-0.0044		
	(0.000094)	(0.0039)		
Kleibergen-Paap rk LM stat.		505.5		413.4
Kleibergen-Paap rk Wald F stat.		171.0		207.7
Unique defendants	388,825	363,260	714,886	705,473
Total observations	10,955,406	9,879,373	20,165,101	19,700,866

*** p<0.01, ** p<0.05, * p<0.1.
Notes: See notes in Table 4.

7. REEXAMINING THE COSTS AND BENEFITS OF INCARCERATION

A common exercise in the literature is to compare the administrative costs of incarceration to the crime prevention savings from incapacitation. Without taking into account general deterrence, this calculation has been interpreted as a lower bound on the social gain from incarceration. But, this approach is not without critics; Donohue III (2009) compiles a detailed listing of additional mechanisms through which incarceration could impact welfare. At issue are concerns regarding losses to inmate productivity, spillovers to household members, and impacts on post-release behavior. Many parameters needed for this more detailed accounting have not been credibly estimated, and so attempts at evaluating this question are either incomplete or rely heavily on untested assumptions.

The new estimates developed in this paper address some of the prior gaps. Through aggregating the impacts on the defendants own pre- and post-release criminal charges, labor market outcomes and public assistance payments in addition known institutional costs I can provide improved partial estimates. The remaining question is then to ask whether general deterrence or other unmeasured benefits in society are large enough to justify these documented costs.

Researchers have used a number of ways to monetize the social cost of crimes. These include hedonic pricing models, compensating wage differentials,jury awards, and contingent valuation studies. I follow Donohue III (2009) in using the costs proposed in jury award studies excluding property transfers as lower bound estimates and contingent valuation prices as upper bound estimates. Fewer crimes have been priced by the contingent valuation methodology, and so jury award prices inclusive of the value of stolen property supplement these figures.[26]

Donohue III (2009) also takes into account two additional indirect costs: (1) the resources allocated to the legal system in order to arrest, charge and convict offenders, and (2) the productivity implications of being punished for a criminal act. While I rely on Doyle's estimates regarding costs to the legal system, I use my own IV estimates regarding defendant productivity, which results in effects that are roughly half the size of what Doyle proposes.[27] The final set of cost estimates are displayed in Table 10.

[26]Since neither approach has priced the cost of drug consumption I construct a naive price using aggregate cost and usage estimates. National Drug Intelligence Center (2011) estimates that the productivity and health costs of illicit drug use in the United States was $84.8 billion in 2007. Substance Abuse and Mental Services Administration (2011) findings indicate that roughly 22.6 million individuals in 2010 report having used illegal drugs in the prior month, and 39 percent used for 20 or more days. I conservatively assume that the remaining 61 percent of respondents only used drugs 1 day in the month, which generates an average rate of 8.4 drug episodes per user. I then divide aggregate costs by total estimated drug episodes in the year, which results in a price of $37 per act of drug consumption.

[27]Because I measure changes in criminal behavior with court charges rather than criminal activity I eliminate the arrest rate scaling used in his estimates. Comparable figures for drug possession and driving while intoxicated were added to complete the list.

TABLE 10. The Social Costs of Charged Criminal Activity (2010 USD)

Criminal Activity	Lower Bound[a] ($)	Upper Bound[b] ($)
Homicide	4,301,817	11,559,713
Rape	187,680	343,859
Robbery	73,196	333,701
Assault	41,046	109,903
Burglary	21,617	50,291
Larceny	9,598	9,974
Motor Vehicle Theft	10,590	15,192
Drug Possession	2,544	2,544
Driving while Intoxicated	25,842	25,842

Notes: [a]Miller et al. (1996) excluding the average value of property transfer, [b]Cohen et al. (2004); Miller et al. (1996) (inclusive of the average value of property transfer). Estimates include legal system costs taken from Donohue III (2009) but re-adjusted to eliminate arrest rate scaling, and productivity losses from Table 7. Estimates reflect 5 percent annual discount rate.

To determine the exact changes in behavior associated with each type of criminal activity listed in Table 10, I estimate detailed crime-specific coefficients which can be found in Online Appendix E.[28] These are added to direct impacts on earnings and public benefit receipt and the cost of incarceration. I use Vera Institute of Justice (2012)'s estimate that each year an inmate spends in prison in Texas costs $21,390.[29]

To evaluate whether savings or costs dominate in this exercise, I compute four statistics: the incapacitation benefits, the institutional costs of incarceration, the post-release costs of increased criminality and the total economic impact. The general deterrence effect is left explicitly unmeasured. The impacts take into account both the time served and five years of post-release outcomes and are discounted at a 5 percent annual discount rate. I compute the test statistics on the total measured change to evaluate if the measured costs are significantly different from zero. This is accomplished using two stage least squares with seemingly unrelated regression

[28]Crimes not are priced are excluded from the calculation, which is equivalent to assuming that such crimes have zero social costs. The vast majority of serious felony crimes are covered, but traffic violations, public disturbances and fraud are omitted.

[29]Owens (2009) uses an estimated marginal cost rather than the reported average cost of incarceration which shrinks costs by almost half. If this also holds in my setting, the correctional costs and shares presented would tend to be overstated.

TABLE 11. Partial net costs based on cost of incarceration and defendant criminal, labor and public benefit outcomes

	Lower Bound		Upper Bound	
	Benefits	Costs	Benefits	Costs
Prison Term: 6 Months				
Incapacitation	$774		$1,936	
General Deterrence	*Not Measured*		*Not Measured*	
Institutional costs		$10,601		$10,601
Post-Release Criminal Behavior		$6,078		$15,029
Economic Impacts		$21,433		$21,433
Total Measured Change	- $37,338***		- $45,127***	
Prison Term: 1 Year				
Incapacitation	$1,521		$3,805	
General Deterrence	*Not Measured*		*Not Measured*	
Institutional costs		$20,835		$20,835
Post-Release Criminal Behavior		$9,736		$22,615
Economic Impacts		$27,114		$27,114
Total Measured Change	- $56,164***		- $66,759***	
Prison Term: 2 Years				
Incapacitation	$2,939		$7,350	
General Deterrence	*Not Measured*		*Not Measured*	
Institutional costs		$40,249		$40,249
Post-Release Criminal Behavior		$16,281		$36,182
Economic Impacts		$37,653		$37,653
Total Measured Change	- $91,246***		- $106,735***	

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Notes: Estimates exclude murders, which are very expensive but noisily estimated. These estimates, which point to potentially substantially larger costs, are available upon request.

employed in the second stage to allow for cross-equation correlation in the error terms. The results are presented in Table 11.

Across all specifications, the estimated costs outweigh the short-run incapacitation benefits and these effects are significantly different from zero. I find that a prison term of generates $56,000 to $67,000 in costs based on correctional expenditures and defendant behavior. Close to half of these costs are driven by economic impacts, while post-release criminal behavior accounts for between one-fifth and one-third of costs. The total measured change worsens with incarceration length: a six-month prison term results in \$37,000 to \$45,000 in costs and a two-year prison term's costs range between \$91,000 to \$107,000.[30]

To evaluate whether these measured costs can be justified based general deterrence alone, one can restate the results in terms of the number of crimes that would need to be prevented in order for incarceration to be welfare neutral.[31] Based on the lower bound cost estimates a one-year prison term would be welfare neutral if it prevented 0.4 rapes, 2.2 assaults, 2.5 robberies, 62 larcenies or 4.8 habitual drug users in the general population.[32] The higher costs of crime associated with the upper bound estimates present a more modest picture: one year in prison would need to prevent 0.2 rapes, 0.7 assaults, 0.2 robberies, 52.5 larcenies or 5.7 habitual drug users. But, these estimates are still quite high, especially if we consider the thought experiment of imprisoning of a low-risk offender whose incarceration is unlikely to deter the high-cost crime categories like rape or assault.

8. Conclusion

Criminal justice policy in the United States has grown increasingly reliant on incarceration since the 1970s. Only in recent years has the incarcerated population begun to plateau. Previous work showing substantial incapacitation gains has helped encourage this trend. The findings of this study, however, suggest these conclusions were premature. Measured incapacitation rates in this study are quite low. In fact, criminal activity actually increases on net after accounting for post-release behavior. Those incarcerated go on to commit more serious offenses and are more likely branch into new types of crimes.

A number of non-crime outcomes are also shown to be impacted by incarceration. Negative effects on post-release employment and earnings indicate that inmates face significant barriers to re-entry. Decreased economic self-sufficiency coincides with greater use of government safety net programs. Together these suggest sustained economic vulnerability in the ex-offender population.

[30]This exercise could be replicated for longer prison terms but is not presented. The estimated treatment effects apply to less serious offenders and so such estimates could be misleading.

[31]Because general deterrence would impact crimes that both are and are not arrested, I used the arrest rate scaled estimates provided by Donohue III (2009) for this portion of the analysis.

[32]A habitual drug user is defined as an individual who uses illicit drugs 200+ times a year.

This study cannot provide evidence regarding the potential general deterrence effects of incarceration. However, based on the impacts to defendant outcomes alone, I estimate that incarceration generates sizable social costs to society. Unless the benefits of general deterrence are at the upper bound of estimates found in the literature or there are other sizable intangible benefits to incarceration, it is unlikely that incarceration for low-risk offenders in Texas is welfare improving.

LITERATURE CITED

Abrams, D. S., M. Bertrand, and S. Mullainathan (2010). Do Judges Vary in Their Treatment of Race? *Journal of Legal Studies*.

Aizer, A. and J. Doyle (2015). Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly Assigned Judges. *Quarterly Journal of Economics*.

Angrist, J., G. Imbens, and D. Rubin (1996). Identification of causal effects using instrumental variables. *Journal of the American Statistical Association 91*(434), 444–455.

Angrist, J. and A. Krueger (1995). Split-sample instrumental variables estimates of the return to schooling. *Journal of Business and Economic Statistics 13*(2), 225–235.

Autor, D. and S. Houseman (2010). Do temporary-help jobs improve labor market outcomes for low-skilled workers: Evidence from "work first". *American Economic Journal: Applied Economics 2*(3), 96–128.

Autor, D., N. Maestas, K. Mullen, and A. Strand (2015). Does delay cause decay? The effect of administrative decision time on the labor force participation and earnings of disability. *NBER Working Paper No. 20840*.

Barbarino, A. and G. Mastrobuoni (2014). The incapacitation effect of incarceration: Evidence from several italian collective pardons. *American Economic Journal: Economic Policy 6*(1), 1–37.

Bayer, P., R. Hjalmarsson, and D. Pozen (2009). Building criminal capital behind bars: Peer effects in juvenile corrections. *Quarterly Journal of Economics 124*(1), 105–147.

Belloni, A., D. Chen, V. Chernozhukov, and C. Hansen (2012). Sparse models and methods for optimal instruments with an application to eminent domain. *Econometrica 80*(6), 2369–2429.

Belloni, A., V. Chernozhukov, and C. Hansen (2014). High-dimensional methods and inference on structural and treatment effects. *Journal of Economic Perspectives 28*(2), 29–50.

Buonanno, P. and S. Raphael (2013). Incarceration and Incapacitation: Evidence from the 2006 Italian collective pardon. *American Economic Review 103*(6), 2437–2465.

Bushway, S. (2004). Labor market effects of permitting employer access to criminal history records. *Journal of Contemporary Criminal Justice 20*.

Bushway, S. and E. Owens (2013). Framing punishment: Incarceration, recommended sentences, and recidivism. *Journal of Law and Economics 56*(2), 301–331.

Bushway, S., E. Owens, and A. Piehl (2012). Sentencing guidelines and judicial discretion: Quasi-experimental evidence from human calculation errors. *Journal of Empirical Legal Studies 9*(2), 291–319.

Carson, E. A. (2013). Imprisonment rate of sentenced prisoners under the jurisdiction of state or federal correctional authorities per 100,000 U.S. residents, December 31, 1978-2012. National prisoner statistics program, Bureau of Justice Statistics.

Chen, K. and J. Shapiro (2007). Do Harsher Prison Conditions Reduce Recidivism? A Discontinuity-Based Approach. *American Law and Economics Review 9*(1), 1–29.

Cohen, M., R. Rust, S. Steen, and S. Tidd (2004). Willingness to pay for crime control programs. *Criminology 42*(1), 86–109.

Dahl, G., A. Kostol, and M. Mogstad (2014). Family welfare cultures. *Quarterly Journal of Economics 129*(4), 1711–1752.

Di Tella, R. and E. Schargrodsky (2004). Do police reduce crime? Estimates using the allocation of police forces after a terrorist attack. *American Economic Review 94*(1), 115–133.

Dobbie, W. and J. Song (2015). Debt relief and debtor outcomes: Measuring the effects of consumer bankruptcy protection. *American Economic Review 105*(3), 1272–1311.

Donohue III, J. (2009). Assessing the relative benefits of incarceration: Overall changes and the benefits on the margin. In S. Raphael and M. Stoll (Eds.), *Do Prisons Make Us Safer?* Russell Sage Foundation.

Doyle, J. (2007). Child protection and child outcomes: Measuring the effects of foster care. *American Economic Review 97*(5), 1583–1610.

Doyle, J. (2008). Child protection and adult crime: Using investigator assignment to estimate causal effects of foster care. *Journal of Political Economy 116*(4), 746–770.

Doyle, J., J. Graves, J. Gruber, and S. Kleiner (Forthcoming). Measuring Returns to Hospital Care: Evidence from Ambulance Referral Patterns. *Journal of Political Economy*.

Drago, F. and R. Galbiati (2012). Indirect effects of a policy altering criminal behavior: Evidence from the Italian prison experiment. *American Economic Journal: Applied Economics 4*(2), 199–218.

Drago, F., R. Galbiati, and P. Vertova (2009). The deterrent effects of prison: Evidence from a natural experiment. *Journal of Political Economy 117*(2), 257–280.

Finlay, K. (2009). Effect of employer access to criminal history data on the labor market outcomes of ex-offenders and non-offenders. In D. Autor (Ed.), *Studies of Labor Market Intermediation*. University of Chicago Press.

French, E. and J. Song (2014). The effect of disability insurance receipt on labor supply: A dynamic analysis. *American Economic Journal: Economic Policy 6*(2), 291–337.

Glaze, L. and E. Herberman (2013). Correctional Population in the United States, 2012. Technical report, U.S. Department of Justice, Bureau of Justice Statistics.

Green, D. and D. Winik (2010). Using random judge assignments to estimate the effects of incarceration and probation on recidivism among drug offenders. *Criminology 48*(2), 357–387.

Grogger, J. (1996). The effects of arrests on the employment and earnings of young men. *Quarterly Journal of Economics 110*, 51–72.

Hansen, C., J. Hausman, and W. Newey (2008). Estimation with many instrumental variables. *Journal of Business and Economic Statistics 26*(4), 398–422.

Hastie, T., R. Tibshirani, and J. Friedman (2009). *The Elements of Statistical Learning: Data Mining, Inference, and Prediction*. Springer Series in Statistics.

Heckman, J. and E. Vytlacil (1998). Instrumental variables methods for the correlated random coefficient model: Estimating the average rate of return to schooling when the return is correlated with schooling. *Journal of Human Resources 33*(4), 974–987.

Helland, E. and A. Tabarrok (2007). Does three strikes deter? A nonparametric estimation. *Journal of Human Resources 42*(2), 309–330.

Imbens, G. and J. Angrist (1994). Identification and estimation of local average treatment effects. *Econometrica 62*(2), 467–475.

Johnson, R. and S. Raphael (2012). How much crime reduction does the maringal prison buy? *Journal of Law and Economics 55*(2), 275–310.

Katz, L., S. Levitt, and E. Shustorovich (2003). Prison conditions, capital punishment, and deterrence. *American Law and Economics Review 5*(2), 318–343.

Kling, J. (2006). Incarceration length, employment and earnings. *American Economic Review 96*(3), 863–876.

Korn, E. and S. Baumrind (1998). Clinician preferences and the estimation of causal treatment differences. *Statistical Science 13*(3), 209–235.

Kuziemko, I. (2013). Should prisoners be released via rules or discretion? *Quarterly Journal of Economics 128*(1), 371–424.

Kyckelhahn, T. (2013). Justice expenditure and employment extracts, 2010 - preliminary. Technical report, U.S. Department of Justice, Bureau of Justice Statistics.

Lee, D. and J. McCrary (2009). The deterrence effect of prison: Dynamic theory and evidence. *unpublished manuscript*.

Leeb, H. and B. M. Pötscher (2008a). Can one estimate the unconditional distribution of post-model-selection estimators? *Econometric Theory 24*(2), 338–76.

Leeb, H. and B. M. Pötscher (2008b). Recent developments in model selection and related areas. *Econometric Theory 24*(2), 319–22.

Levitt, S. (1996). The effect of prison population size on crime rates: Evidence from prison overcrowding litigation. *Quarterly Journal of Economics 111*(2), 319–351.

Liedka, R., A. Piehl, and B. Useem (2006). The crime-control effect of incarceration: Does scale matter? *Criminology and Public Policy 5*(2), 245–276.

Ludwig, J. and J. Kling (2007, August). Is crime contagious? *Journal of Law and Economics 50*(3), 491–518.

Maestas, N., K. Mullen, and A. Strand (2013). Does disability insurance receipt discourage work? Using examiner assignment to estimate causal effects of SSDI receipt. *American Economic Review 103*(5), 1797–1829.

Maurin, E. and A. Ouss (2009). Sentence reductions and recidivism: Lessons from the Bastille day quasi experiment. *IZA Discussion Papers 3990*.

McCrary, J. and S. Sanga (2012). Youth offenders and the deterrence effect of prison. *unpublished manuscript*.

Miller, T., M. Cohen, and B. Wiersema (1996). Victim costs and consequences: A new look. National Institute of Justice Research Report, NCJ-155282, U.S. Department of

Justice.
Nagin, D. and M. G. Snodgrass (2013). The effect of incarceration on re-offending: Evidence from a natural experiment in Pennsylvania. *Journal of Quantitative Criminology 29*(4), 601–642.
National Drug Intelligence Center (2011). The economic impact of illicit drug use on american society. Technical report, U.S. Department of Justice.
Ouss, A. (2013). Prison as a school of crime: Evidence from cell-level interaction.
Owens, E. (2009). More time, less crime? Estimating the incapacitative effect of sentence enhancements. *Journal of Law and Economics 52*(3), 551–579.
Pager, D. (2003). The mark of a criminal record. *American Journal of Sociology 108*, 937–975.
Price, J. and J. Wolfers (2010). Racial discrimination among NBA referees. *Quarterly Journal of Economics 125*(4), 1859–1887.
Raphael, S. (2007). Early incarceration spells and the transition to adulthood. In S. Bushway, M. Stoll, and D. Weiman (Eds.), *Barriers to Reentry? The Labor Market for Released Prisoners in Post-Industrial America*. Russell Sage Foundation.
Substance Abuse and Mental Services Administration (2011). Results from the 2010 national survey on drug use and health: Summary of national findings. Technical report, U.S. Department of Health and Human Services.
Texas Code of Criminal Procedure (2014). Accessed on March 31, 2014.
Tibshirani, R. (1996). Regression shrinkage and selection via the lasso. *Journal of the Royal Statistical Society 58*(1), 267–288.
Vera Institute of Justice (2012). The price of prisons: What incarceration costs taxpayers. Technical report, Center on Sentencing and Corrections.
von Wachter, T., J. Song, and J. Manchester (2009). Long-term earnings losses due to mass layoffs during the 1982 recession: An analysis using u.s. administrative data from 1974 to 2004.
Waldfogel, J. (1998). Does inter-judge disparity justify empirically based sentencing guidelines? *International Review of Law and Economics 18*(3), 293–304.
Walmsley, R. (2009). *World Prison Population List (8th edition)*. International Centre for Prison Studies.
Western, B. (2006). *Punishment and Inequality in America*. Russell Sage Foundation.
Wooldridge, J. M. (1997). On two stage least squares estimation of the average treatment effect in a random coefficient model. *Economics Letters 56*(2), 129–133.

Online Appendices

ONLINE APPENDIX A. THE TEXAS SENTENCING GUIDELINES

Charges, Crimes and Recommended Sentences

Charge	Typical Crimes	Eligible Penalty	Sentencing System
Capital Felony	Murder of a public safety officer, Multiple Murders, Murder of a child	Death, Life in Prison or Life in Prison without Parole	Indeter./Deter.
First-degree Felony	Murder, Possession of a controlled substance (CS) with intent to distribute, Theft over $200,000	5 to 99 years in a state prison and/or a fine of not more than $10,000	Indeterminate
Second-degree Felony	Possession of a CS $>$ 4 grams and $\leq$ 200 grams, Aggravated Assault with a deadly weapon, Indecency with a child (by contact), Intoxicated Manslaughter	2 to 20 years in a state prison and/or a fine of not more than $10,000	Indeterminate
Third-degree Felony	Possession of CS $>$ 1 gram and $\leq$ 4 grams, Aggravated Assault, DWI (3rd Offense), Solicitation of a minor	2 to 10 years in a state prison and/or a fine of not more than $10,000	Indeterminate
State jail Felony	Possession of CS $\leq$ 1 gram, DWI with a minor under the age of 15 in the vehicle, Third theft conviction of any amount	180 days to 2 years in a state jail and/or a fine of not more than $10,000	Determinate
Class A Misdemeanor	DWI (2nd offense), Assault causing bodily injury, Possession of marijuana (between 2 oz. and 4 oz.), Illegal possession of prescription drugs	Not more than 1 year in a county jail and/or a fine of not more than $4,000	Determinate[a]
Class B Misdemeanor	DWI (1st offense), Possession of Marijuana (less than 2 oz.), Prostitution	Not more than 180 days in a county jail and/or a fine of not more than $2,000	Determinate[a]
Class C Misdemeanor	Assault by contact, Drug paraphernalia, Disorderly conduct, Theft under $50	A fine of not more than $500	Not Applicable

Source: Texas Code of Criminal Procedure (2014).
Notes: (a) In 2010, the Harris County Sheriff's Department enacted an Early Release Program that allows inmates to earn "good time" for participation in education, employment or community service related activities. This technically makes sentencing of misdemeanor crimes indeterminate since 2010.

Online Appendix B. Detailed empirical examples of omitted treatments bias and non-monotonic instruments bias

To illustrate how the multidimensional and non-monotonic sentencing affect my estimates, I construct two examples using actual court data from Harris County, TX. The first example considers the impact of accounting for additional degrees of treatment while the second demonstrates how non-uniformities in sentencing can generate bias. The estimates shown in these examples are given to illustrate the features of the data; when documenting omitted treatment bias, non-monotonic instruments bias is ignored and vice versa. Estimates that tackle these challenges simultaneously using the full sample of data are reserved for Sections 5 and 6.

The first example estimates the "causal" impact of incarceration on one year recidivism rates in the felony caseload. The analysis uses all individuals who were charged with felony crimes between 2005 and 2006, and their court sentence is instrumented using their randomly assigned judge. While the coefficient of primary interest is a dummy variable measuring whether or not a defendant was incarcerated for any period of time, each specification progressively adds more controls for non-focal dimensions of sentencing to the model. The results are shown in Table B.1.

In the first specification, the impact of incarceration is estimated without controlling for judicial tendencies on any other court outcomes. The second column adds judicial tendencies on incarceration length to the model and the third specification adds judicial tendencies for guilt, deferred adjudication of guilt, fine status and amount as well as probation status and length. To be clear, court tendencies are not the same as the sentencing outcomes themselves. These variables are constructed by estimating the first stage equation for each sentencing outcome and then using the predicted value as a control.[33]

The estimated coefficient in the first column is positive and significant indicating that defendants assigned to incarceration are 6 percentage points more likely to be charged with a new crime in the year after charges were filed. The second and third specifications also produce positive and significant coefficients, but now

[33]This exercise is isomorphic to simultaneously instrumenting for all of the (focal and non-focal) sentencing outcomes at the same time when estimating and drawing inference on the focal coefficient.

TABLE B.1. The "causal effect" of incarceration with omitted treatment bias

	New criminal charges within 1 year		
	(1)	(2)	(3)
Sentenced to Incarceration	0.06**	0.15***	0.26***
	(0.03)	(0.03)	(0.07)
Total Observations	66,335	66,335	66,335
Judicial Tendency Controls:	No controls	Incar. length	Incar. length, guilt, def. adj. of guilt, fine status/amount, probation status/length
Testing equality of coefficients:	(1) = (2)	(1) = (3)	(2) = (3)
Chi-squared test	44.32	9.06	3.35
P-value	0.00	0.00	0.07

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

the estimated impact of being incarcerated increases dramatically, up to 125 to 300 percent larger. The smaller coefficient observed in the first specification is due to the fact that judges who tend to have relatively higher rates of incarceration also tend to exhibit longer average incarceration lengths in their caseloads. Judicial tendencies on incarceration length are negatively correlated with short run recidivism (not shown), which results in coefficient in specification (1) being negatively biased. Similar mechanisms explain the difference between specifications (2) and (3). Statistical tests reject the null hypothesis that the estimated effects are equal.

To illustrate the consequences of non-monotonic instruments bias, I construct an empirical example using two years of the misdemeanor court data. The exercise uses data for two courtrooms between 2005 and 2006. For the entirety of the period, each court is served by a single elected judge (one Democrat, one Republican) and the cases are randomly assigned. To simplify the example, I have limited the caseload composition to two prominent crime types: driving while intoxicated and possession of marijuana. The total number of observations is 4,548 criminal cases.

Table B.2 shows the incarceration rates by judge as well as their corresponding crime-specific incarceration rates. Judge A exhibits a higher overall incarceration rate and defendants randomly assigned to this courtroom are roughly 1 percentage point more likely to be incarcerated. This aggregate statistic, however, masks substantial subgroup variation. When looking by crime type, Judge A remains the

TABLE B.2. Incarceration rates per judge, overall and by crime type

	Incarceration rate			Caseload size		
	DWI & Drug Poss.	DWI	Drug Poss.	DWI & Drug Poss.	DWI	Drug Poss.
Judge A	65.7%	66.6%	64.6%	2,271	1,274	997
Judge B	64.8%	59.1%	71.9%	2,277	1,261	1,016
Difference	0.9%	7.5%	-7.3%			

tougher judge for individuals charged with driving while intoxicated (+7.5 percentage points); this relationship, however, is reversed for individuals charged with marijuana drug possession, where now Judge A is 7.3 percentage points less likely incarcerate relative to Judge B.

Knowing that the impact of judge assignment depends on crime type, I compute four estimates of "the causal effect" of incarceration on short-run recidivism.[34] In the first estimation, I use an indicator variable for judge assignment as an instrument for incarceration status in the overall caseload. In the second and third estimations, I continue to use an indicator variable for judge assignment as an instrument for incarceration status, but I split the sample by crime type and estimate the impact separately. In the final estimation, I use interactions between judge assignment and crime type as instruments for incarceration.

The results of this exercise are presented in Table B.3. When I use judicial assignment as an instrument in the overall caseload, ignoring potential crime type interactions but still controlling linearly for type of crime, I find a negative correlation between incarceration and recidivism within one year. The estimate is noisy and I cannot reject the null hypothesis that there is zero correlation. In columns 2 and 3, where I separate by subgroup, the estimated coefficients for both subgroups are positive and significant however. For defendants charged with driving while intoxicated, I find that being sentenced to incarceration increases the likelihood of

[34]The maximum duration of incarceration in the county jail system is 1 year, so this should capture the short-run net effects of incarceration on criminal activity collapsing both the incapacitation and post-release effects. To the extent that these two judges adjust other dimensions of sentencing (e.g. sentence length, fines, or use of other alternative sentencing programs), these estimates will be biased. The purpose of this example is not to improve our understanding of the relationship between incarceration and recidivism, but instead illustrate the consequences of failures in monotonicity in a straightforward example. More refined estimates on the impact of incarceration on future criminal behavior are presented in Section 6.

TABLE B.3. Estimating the "causal effect" of incarceration in the presence of non-monotonic instruments bias

	New criminal charges within 1 year			
Sentenced to Incarceration	-0.31	0.32**	0.51*	0.41***
	(1.31)	(0.16)	(0.28)	(0.15)
Crime type = DWI	-0.21***			-0.17***
	(0.072)			(0.014)
N	4,548	2,535	2,013	4,548
Sample	DWI and Drug	DWI	Drug	DWI and Drug
Instrument	Judge	Judge	Judge	Judge × Crime
Anderson canon. Correlation LM statistic	0.46	15.2	12.2	27.4
Cragg-Donald Wald F statistic	0.46	15.3	12.3	13.8

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

being charged with a new crime within one year by 32 percentage points, which is significant at the five percent level. The effect for those charged with drug possession is even larger at 51 percentage points although only significant at the 10 percent level. Given that each subgroup shows significant and positive impacts of incarceration on recidivism, it is surprising that the results from the overall sample were negative and insignificant. What explains this pattern is the fact that the judges' rank ordering changes when looking at the incarceration rates for specific subgroups. In fact, when I return to the pooled sample and allow the impact of judge assignment to vary according to crime type, I find a strong correlation between incarceration and short-run recidivism (41 percentage points), significant at the 1 percent level, that is a weighted average between the effect for drug offenders and DWIs.

The magnitude of the bias depends on the degree to which monotonicity is violated and the treatment effect for the group that defies treatment:

$$(6) \quad \left(\hat{\beta}_1^{LATE} - \beta_1^{LATE}\right) = \frac{Pr[\text{Defier}]}{Pr[\text{Complier}] - Pr[\text{Defier}]} \times \left(\beta_1^{\text{Complier}} - \beta_1^{\text{Defier}}\right) \ .$$

If the probability of being a defier is close to zero, then the bias will also be close to zero. Likewise, if the treatment effects for the group of compliers and defiers is similar, the bias will also be negligible. Problems arise, however, when the ratio of defiers to compliers grows and the treatment effects for the two groups systematically differ.

Given this formula, I can directly compute the magnitude of the bias from using the judge assignment without allowing flexibility by crime type as an instrument. This requires estimating four parameters: $Pr[\text{Complier}]$, $Pr[\text{Defier}]$, $\beta_1^{\text{Complier}}$ and β_1^{Defier}. The compliers in this example are a subset of the individuals charged with driving while intoxicated while the defiers are those charged with possession of marijuana. The complier rate will be equal to difference in the incarceration rates between the judges for DWI's (0.07) times the percent of the sample that is charged with DWI (0.56). The defier rate is equal to difference in the incarceration rates between the judges for drug possession (0.07) times the percent of the sample that is charged with DWI (0.44). For the remaining two parameters, $\hat{\beta}_1^{\text{Complier}}$ is shown in the second column of Table B.3, while $\hat{\beta}_1^{\text{Defier}}$ is listed in the third column. This results in the following:

$$\text{Bias} \quad = \quad \frac{0.07 \times 0.44}{0.07 \times 0.56 - 0.07 \times 0.44} \times (0.32 - 0.51) \quad = \quad -0.62$$

When adding together the impact of incarceration for individuals charged with driving while intoxicated (e.g. the compliers in the example) with the estimate of the bias, I recover the point estimate recorded in Column 1 of Table B.3 (i.e. $\hat{\beta}^{\text{DWI}} + \text{Bias} = -0.31$).

Subgroup analysis based on the standard model, however, is not sufficient to eliminate this bias. Table B.4 shows the results of separate regressions after splitting the sample by crime type, sex, first time offender status, age and race. The first column shows the effects estimated off of judge fixed effects and the second column allows for interactions with crime type. The two columns present starkly divergent results. When using uninteracted judge fixed effects only the coefficients for the crime type subgroups are found to be statistically significant, which are equivalent to allowing the instrument to vary by crime type. The remaining coefficients range between positive and negative values and a test of the joint significance across all specifications fails to reject the null hypothesis. In contrast, the second column shows systematic positive coefficients across all subgroups, with the joint test strongly rejecting the null.

TABLE B.4. Estimated impact of incarceration using Judge versus Judge × Crime fixed effects as instrumental variables

Sugroup	N	New criminal charges within 1 year	
DWI	2,535	0.32**	0.32**
		(0.16)	(0.16)
Drug Poss.	2,013	0.51*	0.51*
		(0.28)	(0.28)
Female	682	0.88	0.31*
		(0.66)	(0.17)
Male	3,866	0.27	0.48**
		(0.56)	(0.20)
First	2,434	0.087	0.19
		(0.73)	(0.14)
Repeat	2,114	-0.065	0.77*
		(1.23)	(0.46)
Age < 25	1,919	0.75	0.45*
		(0.67)	(0.24)
Age >= 25	2,625	0.33	0.37*
		(0.32)	(0.20)
White	1,656	-0.36	0.23
		(1.34)	(0.19)
Black	1,195	2.30	1.01*
		(3.95)	(0.61)
Hispanic	1,697	0.90	0.26
		(1.38)	(0.18)
Chi-squared test of joint significance		10.87	89.11
P-value		0.45	0.00
Instrumental Variable:		Incarceration rate by Judge	Incarceration rate by Judge × Crime type

*** p<0.01, ** p<0.05, * p<0.1.

As a final exercise I reestimate two of the main findings in the paper, recidivism and labor market impacts, using the standard methodology and compare the estimates to my preferred specifications. Table B.5 shows the results of this exercise. I find that in my context relying on average judge tendencies as instruments tends to overstate the recidivism effects while underestimating the labor market impacts. Both of these departures would have warranted a different interpretation of the results.

TABLE B.5. Comparing estimates between standard and new methodology

	Charged in Harris County criminal court with new offense			
In jail or prison	-0.060***	-0.027**	0.11***	0.69***
	(0.0068)	(0.011)	(0.021)	(0.12)
Released from incarceration	0.00092	0.047***	0.015***	-0.014
	(0.0066)	(0.015)	(0.0041)	(0.014)
[Released × Duration]	0.056***	0.055***		
	(0.0053)	(0.012)		
Kleibergen-Paap rk LM stat.	536.3	97.8	610.5	46.2
Kleibergen-Paap rk Wald F stat.	181.1	32.6	307.5	23.1
Unique defendants	431,422	462,374	887019	897,934
Total observations	13,744,324	15,425,102	29222981	29,976,867
Instrument type	Interacted	Average	Interacted	Average
Caseload	Felony		Misdemeanor	

	Quarterly log(earnings+1)			
In jail or prison	-2.59***	-1.98***	-3.25***	-1.57
	(0.30)	(0.39)	(0.98)	(3.26)
Released from incarceration	-0.55	-0.55	-0.42	-0.27
	(0.35)	(0.65)	(0.27)	(0.45)
[Released × Duration]	-0.34**	-0.015		
	(0.16)	(0.39)		
Kleibergen-Paap rk LM stat.	327.6	65.7	148.4	23.7
Kleibergen-Paap rk Wald F stat.	110.5	21.9	74.4	11.9
Unique defendants	243,491	259,698	419,432	424,306
Total observations	7,263,800	8,035,049	13,098,771	13,401,574
Instrument type	Interacted	Average	Interacted	Average
Caseload	Felony		Misdemeanor	

*** p<0.01, ** p<0.05, * p<0.1. *Notes*: Outcomes measured for up to 20 quarters after initial charges. Standard errors in parentheses clustered at defendant level. Quarter of charge fixed effects, quarters since charge fixed effects, instrumental variable controls for non-focal treatments and defendant characteristics fully interacted with quarters since charge fixed effects included in all regressions.

ONLINE APPENDIX C. METHODOLOGICAL DETAILS

This appendix fleshes out two specific aspects of the methodology used in this paper. First, it details how the basis functions are constructed for the semi-parametric estimation of $\Gamma_1(X_i)$. Second, it documents my implementation of the Lasso and Post-Lasso estimators.

Basis Functions. To estimate judge or ADA preferences with regard to continuous characteristics like age or total prior convictions, two equations are estimated. The first equation is the caseload-wide relationship between the sentencing outcome and the trait, and the second re-estimates the model allowing the parameters to vary by judge. The equations are parameterized using an indicator function for the value being non-zero to deal with potential censoring and a second order polynomial to allow for some curvature in preferences:

$$\begin{aligned} D_i &= \phi_0 1[x_i > 0] + \phi_1 x_i + \phi_2 x_i^2 + e_i \ , \\ D_i &= \sum_{j \in \mathcal{J}} \left[\phi_0^j 1[x_i > 0] + \phi_1^j x_i + \phi_2^j x_i^2\right] \times 1[\mathrm{J}_i = j] + \mathrm{e}_i \ . \end{aligned}$$

The candidate basis function $b_k(\cdot)$ is then computed by taking the difference between the predicted value of D_i based on the judge-specific and general model. To avoid any degree of mechanical correlation in the first stage, several researchers have recommended using "leave-one-out" or "jackknife" estimators wherein data for all defendants except for individual i are used to estimate $b_k(\cdot)$ for individual i (see Kling (2006), Doyle (2007), and Aizer and Doyle (2015)). One can implement this strategy without having to reestimate the two models for each observation by simply computing the diagonal elements of the Hessian matrix H_k. The value $h_{k;ii}$, which represents the ith diagonal element of H_k, measures the impact that observation i has on his predicted value, which is known in statistics as i's *leverage*. The jackknife residual is then reverse engineered by dividing the fitted residual from the full regression by $(1 - h_{k,ii})$. This results in the following formula to estimate the jackknife version of $b_k(\cdot)$:

$$b_{k,\not{i}}(\cdot) \;=\; \left[D_i - \frac{\hat{\mathrm{e}}_i}{1 - \hat{\mathrm{h}}_{ii}}\right] - \left[D_i - \frac{\hat{e}_i}{1 - \hat{h}_{ii}}\right] \ ,$$

where $\not{i}$ reflects the fact that the parameter has been stripped of all information from individual i.

The basis function for categorical characteristics are much more straightforward. Rather than estimating multiple regressions, $b_k(\cdot)$ is implemented as the difference in means between the judge and the overall caseload for various subgroups in the population:

$$b_{k,\not{i}}(\cdot) = \sum_{\kappa}\sum_{j} 1[x_i=\kappa, \mathrm{J}_i=j] \times \left(\sum_{\iota=1,\iota\neq i}^{N} 1[x_\iota=\kappa] \times \left[\frac{1[\mathrm{J}_\iota=1]\times D_\iota}{\sum_{\iota,\iota\neq i} 1[\mathrm{J}_\iota=j]} - \frac{D_\iota}{\sum_{\iota,\iota\neq i} 1} \right] \right) .$$

In this notation, κ represents the potential values that the categorical variable x_i takes and j records judge assignment. Again to avoid a mechanical correlation in the first stage the sentencing means are calculated over all observations except for individual i. The resulting estimator will be numerically equivalent to but computationally faster than the prior strategy of estimating caseload-wide and judge-specific regressions models and using the leverage to remove individual i's data from the estimates.

Estimating preferences based on interactions of defendant characteristics (e.g. crime type by race) requires only trivial adjustments to the formulas described above and is not described in detail. To set an upper limit on the total number of potential basis functions to be constructed, the analysis presented in this study only uses two-way interactions among defendant characteristics. While this will limit the flexibility of the estimated decision rule, which could have implications for non-monotonicity, it is assumed that mismeasurement at this point will merely be an approximation error.[35]

C.1. **Lasso and Post-Lasso Implementation.** In my construction of optimal cross-validated instrumental variables, I follow Belloni et al. (2012)'s implementation of Lasso by estimating following objective function to solve for ω:

$$\hat{\omega}^{\text{Lasso}} \in \operatorname*{argmin}_{\omega\in R^p} \sum_{i\in\mathcal{C}} \left[\left(D_i - \sum_k \omega_k b_k(X_i, J_i) \right)^2 \right] + \frac{\lambda}{N} ||\Lambda\omega||_1 .$$

The objective function tries to minimize the sum of the squared residuals, but is penalized by the weighted sum of the absolute value of the coefficients. This creates

[35]To the extent that remaining violations of monotonicity are between defendants with similar local average treatment effects, the impacts of this assumption should be minimal.

a kink at zero in the domain of the objective function which forces coefficients that would otherwise be close to (but not exactly) zero under ordinary least squares (OLS) to be exactly zero under Lasso. Among the full set of p potential instruments, only s optimal instruments exhibit non-zero coefficients which are referred to as the *sparse* set.

In order to estimate this equation, both a penalty level (λ) and a penalty loading matrix ($\Lambda \equiv diag(\lambda_1, \lambda_2, \ldots)$) need to be specified. The elements of the optimal penalty loading matrix Λ^o defined as $\lambda_k^o = \sqrt{\mathbf{E}\left[b_k(x_i, j_i)^2 \eta_i^2\right]}$ are infeasible since η_i, the error term from Equation 4, is not observed in practice, but Λ^o can be approximated through an iterative process wherein conservative values initialize the Λ matrix. Given the initial penalty loadings, estimates of $\hat{\eta}_i$ can be recovered which can then be used to produce new penalty loadings based on $\hat{\lambda}_k = \sqrt{\frac{1}{N}\sum_i \left[b_k(X_i, J_i)^2 \hat{\eta}_i^2\right]}$. The process is repeated until the penalty loadings stabilize and converge.

The penalty level determines the degree of the kink in the objective function. Higher values of λ will result in relatively more coefficients being set to exactly zero. Belloni et al. (2012) recommend setting $\lambda = c\, 2\sqrt{N}\Phi^{-1}\left(1 - \gamma/(2p)\right)$, where the constant $c = 1.1$ and $\gamma = 0.1/\log(p \vee N)$. The combination of the iterated penalty loadings and this penalty level ensure that the Lasso estimator obeys the following near-oracle performance bounds,

$$||\hat{\Gamma}_1(X_i) - \Gamma_1(X_i)||_{2,N} \lesssim_P \sqrt{\frac{s\log(p \vee N)}{N}}\ ,$$

meaning that estimates will coincide up to a $\sqrt{\log(p)}$ factor with the bounds achievable when the correct sparse set of significant variables is known ex-ante.

The traditional implementation of Lasso generally assumes there exists only a fixed number of optimal instruments, which is known as *exact sparsity*. Belloni et al. (2012) show that their implementation of Lasso can relax this assumption to an *approximate sparsity* assumption, which states that $\frac{s^2\log^2(p \vee N)}{N} \to 0$. Instead of setting a fixed bound on the number of optimal instruments, this assumption places an upper bound on the growth rate of the number of optimal instruments relative to the sample size. They show this assumption can be relaxed even further when employing a sample splitting procedure (as used in this study) to $s\log(p \vee N) =$

$o(N)$, which effectively allows for an even faster growth rate of s in the sample size.

A closely related estimator known as the *Post-Lasso* estimator takes the sparse subset of instruments selected by Lasso and re-estimates their coefficients using OLS. This addresses a known issue in the Lasso estimator that non-zero coefficients are biased towards zero. Post-Lasso eliminates some of this shrinkage bias, and achieves the same rates of convergence without requiring additional assumptions. It is for this reason that the preferred estimates of $\hat{\Gamma}_1(X_i)$ used in Section 6 are constructed using Post-Lasso coefficients rather than Lasso coefficients.[36]

Compared to other shrinkage procedures, Lasso and post-Lasso are particularly interesting because they identify a subset of variables that have high explanatory power. Isolating these variables gives the researcher an opportunity to learn about the dimensions over which judges exhibit differential behavior. Thus, the algorithm not only increases the power of our instruments, but also improves our understanding of judicial decision making.

[36]In practice, the Lasso and Post-Lasso predictions of $\hat{\Gamma}_1(X_i)$ are very similar and this choice does not substantively alter the conclusions of this paper.

Online Appendix D. Robustness exercises

This appendix documents the numerous robustness checks explored in this project. A number of robustness exercises were conducted to confirm the stability of the results. These include a more conservative clustering of standard errors, intentional omission of important defendant characteristics in the first stage, testing for sensitivity to first stage misspecification, trimming extreme values in the instruments, using Lasso-weighted instruments instead of Post-Lasso, and dropping the shrinkage procedure altogether. The results are quite robust across the different specifications.

In the first robustness check (Table D.1), I replicate the findings on criminal behavior using a statewide criminal conviction database maintained by the Texas Department of Public Safety. It is known that this database has incomplete coverage,[37] but is generally thought to capture the most serious crimes. While the incapacitation effects measured in this data are weaker from a statistical perspective, I do capture the increase in criminality post-release for felony defendants. This ensures that these findings are not the result of differential intra-state mobility. The estimates for the misdemeanor caseload are much less precise, but this could be a function of the fact that fewer misdemeanor crimes are reported by counties to this statewide database.

The second robustness exercise (Table D.2) re-estimates my results using a more conservative level of clustering: the court interacted with quarter of charge. Clustering at the defendant level accounts for correlation in the error term between repeated observations in the panel, but fails to account for correlation between defendants charged in the same courtroom. One example that could generate this relationship is if defendants generate peer effects while in courtroom. Taking this more conservative approach, however, leaves my standard errors virtually unchanged.

The third exercise (Table D.3) intentionally omits crime type from the construction of the instruments. Reestimating the model after excluding crime type can help determine how sensitive the results are to the specific set of covariates that are allowed to affect the first stage. This is an important concern as there are a number of unobserved defendant traits like educational attainment or marital status that could affect judicial decision making. If omitting crime type, which has been shown to

[37] State auditors have generally found the submission rates from local authorities to the statewide repository to be roughly 60 to 70 percent over the years

TABLE D.1. Impacts of incarceration on criminal activity using Texas Department of Public Safety statewide criminal conviction database

Type of criminal offense:	Property	Drug poss.	Drug mfr. or distr.	Violent	DWI
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.0038	-0.0097**	-0.0029	-0.0047*	-0.0035**
	(0.0038)	(0.0039)	(0.0019)	(0.0024)	(0.0016)
Released from incarceration	0.00048	0.00083	-0.0013	-0.0017	0.00090
	(0.0029)	(0.0027)	(0.0012)	(0.0017)	(0.0013)
[Released × Duration]	0.0090***	0.016***	0.0040***	0.0010	-0.00076
	(0.0022)	(0.0026)	(0.0012)	(0.0012)	(0.00089)
Underidentification statistic	536.3	536.3	536.3	536.3	536.3
Weak Identification statistic	181.1	181.1	181.1	181.1	181.1
Unique defendants	431,422	431,422	431,422	431,422	431,422
Total observations	13,744,324	13,744,324	13,744,324	13,744,324	13,744,324
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.020*	-0.0036	0.0034	-0.0080	0.0074
	(0.010)	(0.0085)	(0.0029)	(0.0064)	(0.0068)
Released from incarceration	0.0014	-0.0022	0.00025	-0.00023	0.00016
	(0.0016)	(0.0015)	(0.00046)	(0.00095)	(0.0011)
Underidentification statistic	610.5	610.5	610.5	610.5	610.5
Weak Identification statistic	307.5	307.5	307.5	307.5	307.5
Unique defendants	887,019	887,019	887,019	887,019	887,019
Total observations	29,222,981	29,222,981	29,222,981	29,222,981	29,222,981

*** p<0.01, ** p<0.05, * p<0.1

exert important non-monotonicities in the first stage, does not dramatically change the results, we can be more confident that the results are robust to the exclusion of unobserved defendant covariates. Using the new set of instruments constructed without crime type does not statistically or substantively change my results, indicating that an incomplete set of covariates still can potentially capture the important dimensions of non-uniformity.

The fourth robustness check (Table D.4) employs a two-step procedure wherein first stage residuals were estimated using OLS and then used to construct a fourth order polynomial control function that was added to outcome equation. The IV coefficients were then re-estimated using two-step GMM to ensure the estimates were insensitive to potential misspecification in the first stage. The magnitudes of the coefficients do not change noticeably and in fact the statistical precision in these specifications generally improves.

TABLE D.2. Impacts of incarceration while clustering at Court $\times$ Quarter of Charge Level

	Any Criminal Court Charge	Employment	Log Income	Food Stamps Receipt	TANF Receipt
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.060***	-0.32***	-2.59***	-0.0087	-0.00049
	(0.0074)	(0.037)	(0.31)	(0.019)	(0.0091)
Released from incarceration	0.00092	-0.054	-0.55	0.049**	0.0094
	(0.0070)	(0.043)	(0.36)	(0.022)	(0.010)
[Released $\times$ Duration]	0.056***	-0.036*	-0.34**	-0.016	-0.0044
	(0.0059)	(0.020)	(0.16)	(0.011)	(0.0043)
Total clusters	2,613	1,848	1,848	1,738	1,980
Total observations	13,744,324	7,263,800	7,263,800	8,864,396	9,879,373
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.11***	-0.40***	-3.25***	-0.016	-0.024
	(0.030)	(0.12)	(0.99)	(0.077)	(0.025)
Released from incarceration	0.015***	-0.045	-0.42	0.024	0.010
	(0.0048)	(0.030)	(0.26)	(0.017)	(0.0070)
Total clusters	1,738	1,235	1,235	1,165	1,319
Total observations	29,222,981	13,098,771	13,098,771	17,583,624	19,700,866

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

The fifth exercise (Table D.5) trimmed the top 99th and bottom 1st percentiles in the instrument values to ensure that extreme values did not drive the results. I find the precision of the point estimates for the $[Released \times Duration]$ variable are somewhat sensitive to the trimming exercise with some loss of significance on specific coefficients, but this is not entirely surprising because I am eliminating variation. But taken as a whole, the general conclusions appear qualitatively similar.

The sixth check (Table D.6) replaces the Post-Lasso coefficients with the original Lasso coefficients to weight the basis functions in the instrument construction. This should demonstrate that the results are not an arbitrary artifact of the specific estimation process I used. What I find in this exercise is that the estimates are very close together with similar magnitudes and precision, indicating that the Lasso versus Post-Lasso distinction in this application is somewhat arbitrary.

The final robustness (Table D.7) exercise drops the shrinkage procedure entirely and uses only cross-validated OLS to weight the basis functions. These estimates do not deviate substantively from my main results.

TABLE D.3. Impacts of incarceration excluding crime type in instrument construction

	Any Criminal Court Charge	Employment	Log Income	Food Stamps Receipt	TANF Receipt
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.059***	-0.32***	-2.59***	-0.012	-0.0018
	(0.0069)	(0.037)	(0.31)	(0.018)	(0.0085)
Released from incarceration	0.0014	-0.059	-0.58	0.049**	0.0091
	(0.0067)	(0.043)	(0.35)	(0.021)	(0.0093)
[Released × Duration]	0.056***	-0.034*	-0.31*	-0.017	-0.0047
	(0.0055)	(0.020)	(0.16)	(0.011)	(0.0040)
Unique defendants	431,387	243,467	243,467	333,853	363,235
Total observations	13,741,071	7,261,945	7,261,945	8,862,474	9,877,134
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.11***	-0.37***	-3.01***	-0.019	-0.027
	(0.021)	(0.12)	(1.01)	(0.070)	(0.021)
Released from incarceration	0.016***	-0.044	-0.41	0.024	0.0100
	(0.0042)	(0.031)	(0.27)	(0.015)	(0.0061)
Unique defendants	887,016	419,421	419,421	645,564	705,463
Total observations	29,219,846	13,097,438	13,097,438	17,582,142	19,699,189

*** p<0.01, ** p<0.05, * p<0.1.

TABLE D.4. Impacts of incarceration after controlling for a quartic in the first stage residuals

	Any Criminal Court Charge	Employment	Log Income	Food Stamps Receipt	TANF Receipt
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.074***	-0.28***	-2.23***	0.0071	0.0041
	(0.0054)	(0.028)	(0.23)	(0.015)	(0.0070)
Released from incarceration	0.0074	-0.033	-0.37	0.037**	0.0085
	(0.0048)	(0.034)	(0.28)	(0.017)	(0.0078)
[Released × Duration]	0.055***	-0.042***	-0.39***	-0.013*	-0.0045
	(0.0037)	(0.014)	(0.11)	(0.0079)	(0.0029)
Unique defendants	431,422	243,491	243,491	333,888	363,260
Total observations	13,744,324	7,263,800	7,263,800	8,864.396	9,879,373
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.074***	-0.32**	-2.42**	-0.025	-0.021
	(0.021)	(0.13)	(1.03)	(0.071)	(0.021)
Released from incarceration	0.018***	-0.043	-0.38	0.025	0.012*
	(0.0043)	(0.032)	(0.27)	(0.015)	(0.0062)
Unique defendants	887,019	419,432	419,432	645,576	705,473
Total observations	29,222,981	13,098,771	13,098,771	17,583,624	19,700,866

*** p<0.01, ** p<0.05, * p<0.1.

TABLE D.5. Impacts of incarceration after trimming extreme valued instruments

	Any Criminal Court Charge	Employment	Log Income	Food Stamps Receipt	TANF Receipt
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.053***	-0.35***	-2.82***	-0.012	-0.0043
	(0.0079)	(0.044)	(0.36)	(0.022)	(0.010)
Released from incarceration	0.0072	-0.091*	-0.81**	0.049**	0.0067
	(0.0074)	(0.049)	(0.41)	(0.024)	(0.011)
[Released × Duration]	0.022***	-0.021	-0.24	-0.022	-0.0051
	(0.0084)	(0.037)	(0.30)	(0.020)	(0.0075)
Unique defendants	431,299	243,422	243,422	333,700	363,115
Total observations	13,099,543	6,944,516	6,944,516	8,468,617	9,433,805
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.13***	-0.54***	-4.35***	0.079	-0.023
	(0.030)	(0.20)	(1.62)	(0.11)	(0.030)
Released from incarceration	0.0087*	-0.0091	-0.15	0.025	0.0075
	(0.0049)	(0.035)	(0.30)	(0.017)	(0.0065)
Unique defendants	886,545	418,793	418,793	644,465	704,903
Total observations	28,098,153	12,594,146	12,594,146	16,904,904	18,942,217

*** p<0.01, ** p<0.05, * p<0.1.

TABLE D.6. Impacts of incarceration using Lasso-weight instruments

	Any Criminal Court Charge	Employment	Log Income	Food Stamps Receipt	TANF Receipt
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.057***	-0.32***	-2.57***	-0.0079	0.00020
	(0.0070)	(0.035)	(0.28)	(0.018)	(0.0082)
Released from incarceration	-0.0089	-0.043	-0.49	0.041**	0.0073
	(0.0070)	(0.042)	(0.35)	(0.020)	(0.0094)
[Released × Duration]	0.071***	-0.041**	-0.36**	-0.010	-0.0028
	(0.0054)	(0.018)	(0.14)	(0.0096)	(0.0036)
Unique defendants	431,422	243,491	243,491	333,888	363,260
Total observations	13,744,324	7,263,800	7,263,800	8,864,396	9,879,373
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.18***	-0.30**	-2.40**	-0.035	-0.025
	(0.022)	(0.12)	(0.95)	(0.064)	(0.022)
Released from incarceration	0.014***	-0.029	-0.27	0.021	0.0097
	(0.0044)	(0.033)	(0.28)	(0.016)	(0.0063)
Unique defendants	887,019	419,432	419,432	645,576	705,473
Total observations	29,222,981	13,098,771	13,098,771	17,583,624	19,700,866

*** p<0.01, ** p<0.05, * p<0.1.

TABLE D.7. Impacts of incarceration using cross validation without shrinkage procedure

	Any Criminal Court Charge	Employment	Log Income	Food Stamps Receipt	TANF Receipt
Panel A: Felony defendants, Instrumental variables					
In jail or prison	-0.049***	-0.35***	-2.89***	0.0014	-0.012
	(0.0086)	(0.041)	(0.33)	(0.021)	(0.0099)
Released from incarceration	0.019**	-0.026	-0.39	0.064***	-0.0010
	(0.0074)	(0.043)	(0.35)	(0.021)	(0.0100)
[Released $\times$ Duration]	0.034***	-0.036**	-0.33***	-0.014	-0.0058
	(0.0048)	(0.015)	(0.12)	(0.0093)	(0.0037)
Unique defendants	421,679	237,414	237,414	325,879	355,050
Total observations	13,183,828	6,977,260	6,977,260	8,548,485	9,509,945
Panel B: Misdemeanor defendants, Instrumental variables					
In jail or prison	0.021	-0.38***	-2.93***	-0.053	-0.0071
	(0.016)	(0.067)	(0.55)	(0.041)	(0.015)
Released from incarceration	0.022***	-0.032	-0.31	-0.0065	0.0054
	(0.0050)	(0.033)	(0.28)	(0.024)	(0.0065)
Unique defendants	885,565	418,474	418,474	644,099	703,984
Total observations	29,094,032	13,040,814	13,040,814	17,514,605	19,619,405

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

ONLINE APPENDIX E. CRIME-SPECIFIC ESTIMATES FOR COST-BENEFIT EXERCISE

TABLE E.1. Impacts of incarceration on specific types of criminal charges

Type of criminal offense:	Murder	Sexual Assault	Robbery	Assault	Burglary	Larceny	Drug Possession	Driving While Intoxicated
In jail or prison	0.00076	0.00080*	-0.0014	-0.0026	-0.0076***	-0.0043	-0.023***	-0.0032**
	(0.00046)	(0.00045)	(0.00093)	(0.0017)	(0.0021)	(0.0027)	(0.0032)	(0.0014)
Released from incarceration	0.00022	0.00024	0.00099	0.0011	-0.0022	0.0030	-0.0044	0.00077
	(0.00041)	(0.00042)	(0.00086)	(0.0015)	(0.0020)	(0.0024)	(0.0031)	(0.0014)
[Released × Duration]	0.00038	0.00021	0.00038	0.00094	0.0099***	0.0086***	0.026***	-0.0014
	(0.00027)	(0.00028)	(0.00069)	(0.0012)	(0.0018)	(0.0019)	(0.0029)	(0.00083)
Kleibergen-Paap rk LM stat.	536.3	536.3	536.3	536.3	536.3	536.3	536.3	536.3
Kleibergen-Paap rk Wald F stat.	181.1	181.1	181.1	181.1	181.1	181.1	181.1	181.1
Unique defendants	431,422	431,422	431,422	431,422	431,422	431,422	431,422	431,422
Total observations	13,744,324	13,744,324	13,744,324	13,744,324	13,744,324	13,744,324	13,744,324	13,744,324

*** $p<0.01$, ** $p<0.05$, * $p<0.1$