THE CRIMINAL AND LABOR MARKET IMPACTS OF INCARCERATION

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ABSTRACT. This paper investigates the pre- and post-release impacts of incarceration on criminal behavior, economic wellbeing and family formation using new data from Harris County, Texas. The research design identifies exogenous variation in the extensive and intensive margins of incarceration by leveraging the random assignment of defendants to courtrooms. I develop a new data-driven estimation procedure to address multidimensional and non-monotonic sentencing patterns observed in the courtrooms in my data. My findings indicate that incarceration generates modest incapacitation effects, which are offset in the long-run by an increased likelihood of defendants reoffending after being released. Additional evidence finds that incarceration reduces post-release employment and wages, increases take-up of food stamps, decreases likelihood of marriage and increases the likelihood of divorce. Based on changes in defendant behavior alone, I estimate that a one-year prison term for marginal defendants conservatively generates $56,200 to $66,800 in social costs, which would require substantial general deterrence in the population to at least be welfare neutral.

Keywords: incarceration, recidivism, labor market outcomes, family formation, monotonicity

JEL: J24, K42, J62

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The United States currently has the highest incarceration rate in the world (Walmsley [2009]), a consequence of three decades of dramatic growth in the prison population since the late 1970s (Carson [2013]). Over this same time period governmental expenditures on police protection, judicial and legal systems, and corrections also surged (Bureau of Justice Statistics [1980] and Kyckelhahn [2013]). Recent estimates indicate that the annual U.S. correctional population included over 7 million adults (Glaze and Herberman [2013]), and combined federal, state and local expenditures on justice-related programs topped $260 billion per year. Despite the reach and cost associated with these changes to criminal justice policy, causal evidence on how this use of incarceration has impacted the population remains scarce (see Donohue III [2009]).

To help address this gap in the literature, I investigate the impacts of incarceration using original data from Harris County, Texas. The new data is comprised of over 2.6 million criminal court records accounting for 1.1 million unique defendants, which I collected and processed into an empirical dataset. It captures the universe of misdemeanor and felony criminal charges between 1980 and 2009 regardless of final conviction status. What makes the data especially unique is the ability to link the court records to a variety of other sources of administrative data including state prison and county jail data, unemployment insurance wage records, public assistance benefits, marriage and divorce records as well as future criminal behavior using individual identifiers available in the data. Taken together, the combined data allows me to estimate impacts on a broad range of policy-relevant outcomes, promoting a better understanding of the potential mechanisms underpinning the treatment effects and providing for a more complete accounting in the cost benefit analysis of incarceration.

The research design leverages the random assignment of criminal defendants to courtrooms as a source of exogenous variation in both the extensive and intensive margins of incarceration. The courts are staffed by judges and prosecutors who differ in their propensity to incarcerate. As a result, which courtroom a defendant is randomly assigned to strongly predicts whether he will be incarcerated and for how long.[1] This increasingly popular identification strategy has been used in a

[1]Even though parole boards may adjust some sentences ex-post, my evidence indicates that the courts exert influence over actual time served.

number of applications where judges, case workers, or other types of programs administrators are given discretion on how to respond to a randomly assigned caseload.[2]

The application considered in this paper is moderately more complex than standard uses of this research design. Sentencing takes on multiple dimensions (e.g. incarceration, fines, drug treatment, etc.) and judges display non-monotonic tendencies (e.g. a judge may incarcerate drug offenders at a relatively higher rate but property offenders at a relatively lower rate). Since failure to account for these features of the data could lead to violations of the *exclusion restriction* and *monotonicity assumption*, a new estimation procedure is developed.[3] In this new approach, I first construct instruments for each observable aspect of sentencing, not just incarceration, in order to control for court tendencies on non-focal sentencing dimensions. I also relax the first stage equation to allow the impact of court assignment on sentencing outcomes to flexibly respond to observed defendant characteristics. Because this second modification can generate many instruments due to the curse of dimensionality, the least absolute selection and shrinkage operator (LASSO) is used in conjunction with cross validation as a data-driven tool to achieve disciplined dimension reduction without skewing statistical testing.

My empirical findings indicate that incarceration for marginal defendants is less attractive from a policy perspective than has been shown in prior work. I measure modest incapacitation effects while defendants are in jail or prison: felony defendants are 6 percentage points less likely to be charged with a new criminal offense while incarcerated. This benefit, however, is offset by increases in post-release criminal behavior: each additional year that a felony defendant was incarcerated increases the probability of facing new charges post-release by 5.6 percentage points per quarter. What is particularly concerning about these results is that the incapacitation effect is disproportionately driven by misdemeanor charges, while the post-release criminal behavior shows mainly increases in felony offenses. Partially driving this result is a pattern of former inmates being charged with new crime types. In particular, I find that former inmates are especially likely to

[2]For studies specifically related to incarceration, see Kling [2006], Di Tella and Schargrodsky [2004], or Aizer and Doyle [2013]. For research in other fields, see Doyle [2007, 2008], Autor and Houseman [2010], Belloni et al. [2012], Munroe and Wilse-Samson [2012], Doyle et al. [2012], French and Song [2012] Maestas et al. [2013], Autor et al. [2013] and Dahl et al. [2013].

[3]Prior researchers have acknowledged the potential for these features to also affect their findings, but data limitations have generally limited their ability to address these concerns in any formal way.

commit more property (e.g. theft or burglary) and drug-related crimes after being released, even if these crimes were not their original offenses.

In contrast with prior work, I find strong evidence that incarceration has lasting negative effects on labor market outcomes after defendants have been released. I find that each additional year of incarceration reduces post-release employment by 3.6 percent points. Among felony defendants with stable pre-charge income incarcerated for one or more years, reemployment drops by at least 24 percent in the 5 years after being released. Misdemeanor defendants show a small increase in take-up of cash welfare payments, and felony defendants show increases in Food Stamps benefits, which provide further evidence of lasting economic hardship post-release.

The impacts of incarceration extend beyond recidivism and labor market outcomes. Incarceration appears to negatively impact family formation and stability as measured through marriage and divorce activity. While incarcerated, young felony defendants exhibit significantly lower rates of marriage that is not compensated post-release indicating a net decline in marriage rather than a temporal shift. Further supporting this conclusion, I find that divorce rates among older felons increase while in prison and post-release.

Using these new estimates, I reevaluate the welfare impacts of incarceration. Because I cannot measure general deterrence effects in my research design, the cost benefit exercise is partial in nature and only accounts for the administrative expenses, criminal behavior effects and economic impacts associated with the defendant's own outcomes. Using the most conservative estimates, I find that a one-year prison term for marginal defendants decreases social welfare by \$56,200 to \$66,800 of which negative impacts to economic activity account for 41 to 48 percent of overall costs. In order for this sentence to be neutral in social welfare terms, a one-year prison term for a marginal (low-risk) offender 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.[4]

The remainder of this paper organized into 8 sections. Section 1 briefly discusses the literature. Section 2 describes the setting of this study in Harris County, Texas. Section 3 documents the sources of data. Section 4 illustrates how multidimensional and non-monotonic sentencing

[4]This ignores potential intangible benefits of incarceration that might arise if victims gain utility from seeing their offender punished.

patterns create opportunities for bias, and Section 5 proposes an alternative estimation strategy to address these concerns. Section 6 describes the panel model used in this study to estimate both the contemporaneous and post-release effects of incarceration. Section 7 reports the empirical results and discusses the robustness exercises. Section 8 conducts a cost benefit exercise using the newly estimated parameters. Section 9 concludes.

1. Related literature

Economic research on the incarceration has primarily focused on measuring its impacts on future criminal behavior. Incapacitation, in particular, has received significant focus. Credible estimates range from 2.8 to 15 crimes prevented per year of incarceration (see Levitt [1996], Owens [2009], Johnson and Raphael [2012], Buonanno and Raphael [2013], Kuziemko [2013]). Lower estimates generally rely on inmate records that are matched to their own future criminal activity, while larger estimates allow for incapacitation effects to also measure potential multiplier effects in the population. The potential for diminishing returns to incarceration as incarceration rates have increased over time has also been put forth as a potential explanation for the variation in the estimates (see Liedka et al. [2006], Johnson and Raphael [2012]).

Existing work presents conflicting views on the degree to which general and specific deterrence inform criminal decision making. Poor prison conditions and three strikes laws appear to discourage criminal behavior (see Katz et al. [2003] and Helland and Tabarrok [2007]), yet sharp changes in the severity of sentencing at age of maturity and actual experiences of incarceration seem to have zero or positive effects on recidivism (see Lee and McCrary [2009] and McCrary and Sanga [2012], Chen and Shapiro [2007], Di Tella and Schargrodsky [2004], Green and Winik [2010], Nagin and Snodgrass [2013]). Perhaps at issue is the salience of the criminal penalty. Drago et al. [2009]'s analysis of a collective pardon in Italy that allowed inmates to be released under the explicit condition that any future reoffense would reinstate the remainder of their original sentence finds that each additional month carried over to future potential sentencing decreases future criminal activity by 0.16 percentage points. Conversely, when offenders appear to get off easy on the terms of their original sentence through either early release or changes in sentencing guidelines, recidivism rates tend to increase (see Maurin and Ouss [2009], Bushway and Owens [2012], Kuziemko [2013]).

An emerging agenda has begun to show that peer effects play an important role in criminality. Bayer et al. [2009] and Ouss [2013] both find evidence that inmate interactions influence their post-release criminal activity through encouraging new criminal patterns. Drago and Galbiati [2012] similarly find that inmates stimulate the criminal behavior of their non-incarcerated peers after being released. Yet Ludwig and Kling [2007]'s evaluation of the Moving to Opportunity experiment, on the other hand, found no measured correlation between the future criminality of the relocated study participants and the ambient levels of crime in their destination neighborhoods.

Data constraints have limited the ability of researchers to study outcomes beyond criminal activity. As a result, there is less rigorous evidence on the non-criminal effects of incarceration (see Donohue III [2009] for discussion). Several studies consider whether incarceration and criminal history generates 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 whether income increased after being released from incarceration (see Grogger [1996], Cho and Lalonde [2005], Western [2006], Sabol [2007], Pettit and Lyons [2007] and Raphael [2007]).

Two recent studies in particular are closely related to this paper. First, Kling [2006] studies the impact of incarceration length on labor market outcomes by linking inmate records of state and federal prisoners from Florida and California, respectively, to their labor market outcomes. He finds no evidence that longer prison sentences adversely affected labor market outcomes. His conclusions were based on panel data with individual fixed effects and an instrumental variable strategy using the average incarceration length for each defendant's randomly assigned federal court judge as an instrument for his actual incarceration length. Second, Aizer and Doyle [2013] study the impact of incarceration among juvenile offenders in Chicago also using an instrumental variable strategy based on randomized judges. While their data does not allow them to evaluate labor market impacts, they find that being sentenced to a juvenile delinquency facility reduces the likelihood of high school graduation and increases the likelihood of adult incarceration. Since these two studies evaluate different populations (i.e. adult versus juvenile offenders) and margins of incarceration (extensive versus intensive) their disparate findings are not necessarily inconsistent. For instance, incarceration may have a particularly harmful effect on youth who are still in the midst of building their human capital. The stark divergence in their findings, however, is still surprising and raises the need for further investigation.

2. The Harris County Criminal Justice System

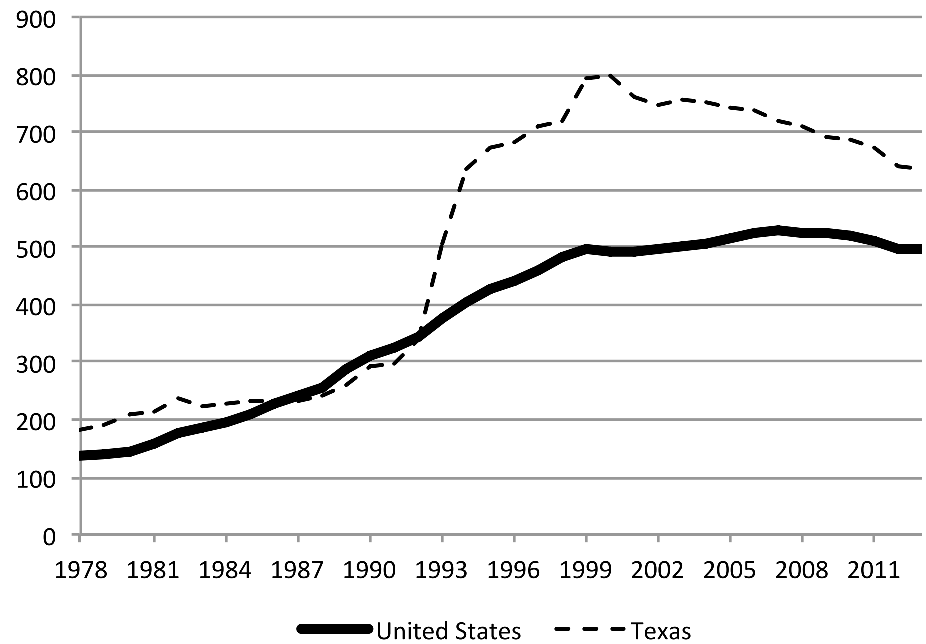
The setting for this study is Harris County, Texas, which includes the city of Houston as well as several surrounding municipalities. The Houston metropolitan statistical area has the fifth largest population in the United States and encompasses a geographical area slightly larger than the state of New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

Texas is known for being particularly tough on crime. Figure 1 plots the imprisonment rate per 100,000 residents in the United States and Texas. Throughout the 1980's, Texas was actually close to the national trend due to overcrowding within the Texas prison system, but in the early 1990's, the newly elected governor, Ann Richards, began a massive prison expansion program relaxing the capacity constraint and resulting in sustained higher imprisonment rates. The widespread use of incarceration in Texas will imply that defendants on the margin of incarceration 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, given that Texas accounts for roughly 12 percent of the non-federal institutional population, a group that is understudied in general, this population is important to study in and of itself.

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 per year.[5] 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 litigate cases

[5]In 1980, only 10 CCLs were active. Additional courts were added in 1983, 1985 and 1995.

FIGURE 1. National versus Texas imprisonment rate per 100,000 residents



Source: Bureau of Justice Statistics, Corrections Statistical Analysis Tool.

involving felony charges and serve roughly 1,800 cases per court per year.[6] Typical cases include possession of controlled substances, drug manufacture and distribution, larceny involving more than $1,500 in stolen property, residential or vehicular burglary, aggravated assault as well as more heinous offenses like murder, rape and child abuse. 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).[7]

Table 1 shows summary statistics for misdemeanor and felony defendants. Both the misdemeanor and felony caseloads are predominantly male with mean age around 30 years old. Individuals facing misdemeanor charges have been charged with and convicted of fewer previous

[6]In 1980, only 18 SDCs were active. Additional courts were added in 1982 and 1984.

[7]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.

crimes compared to felony defendants; 60 percent of the misdemeanor cases are first-time offenders while only 45 percent of the felony caseload are. 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 seriousness of the illicit drugs), more costly property crimes, and aggravated assault.

Roughly equal shares of non-Hispanic Caucasian, non-Hispanic African American and Hispanic defendants are represented in both caseloads. Misdemeanor cases have a relatively larger proportion of non-Hispanic Caucasians, whereas felony cases are more likely to be African Americans. A number of other physical descriptors are available in the data including: skin tone, height, weight, body type, eye color and hair color. These are mainly recorded in the event a warrant needs to be issued for the defendant. Coverage of these variables is much more reliable for cases from 1985 and onwards when record keeping in the court files improved.

When criminal charges are filed against a defendant in Harris County, his case is randomly assigned to a courtroom.[8] Randomization is viewed as an impartial 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 manipulated by internal actors, the Harris County District Clerk's office, which is both physically and administratively segregated from the criminal court system, is solely responsible for courtroom assignment.

When a case is randomly assigned to a courtroom, a defendant is assigned to the jurisdiction of a specific judge and team of assistant district attorneys. The judges are elected to serve a specific bench and are responsible for presiding over all cases assigned to their courtroom while in office. Elections occur every two years, and the vast majority of judges are successfully reelected. As a

[8]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.

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	0.8	1.2
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	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
Skin tone		
Fair	0.14	0.13
Light	0.06	0.05
Light brown	0.07	0.06
Medium	0.22	0.22
Medium brown	0.09	0.1
Olive	0.03	0.03
Dark	0.04	0.07
Dark brown	0.09	0.14
Black	0.04	0.07
Missing	0.21	0.13
Height (in.)	68.13	68.4
Weight (lbs.)	169.82	172.51
Body type		
Skinny, light	0.11	0.13
Medium	0.6	0.66
Heavy, obese	0.08	0.09
Missing	0.21	0.13
Eye color		
Green, blue	0.18	0.15
Brown, black	0.65	0.75
Missing	0.17	0.1
Hair color		
Blonde, red	0.07	0.06
Black, brown	0.75	0.83
Bald, grey	0.02	0.02
Missing	0.17	0.1
Total cases	1,449,453	775,576

Source: Author's calculations using Harris County District Clerk's criminal court records.

Notes: Calculations do not include sealed court records, juvenile offenders or defendants charged with capital murder.

result, a defendant's initial court assignment will likely determine the judge who presides over the entirety of his trial.

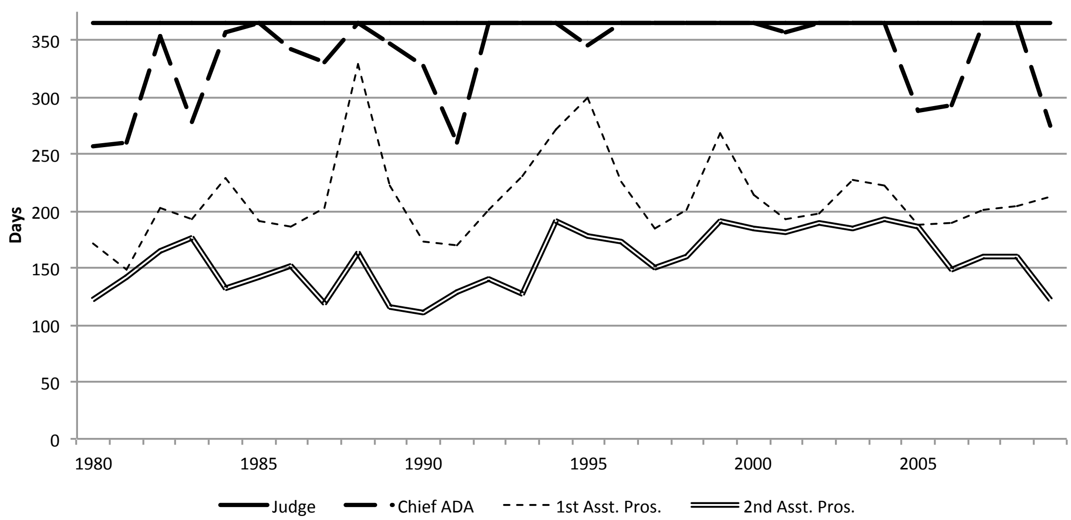
The Harris County District Attorney's office stations a team of three assistant district attorneys (one chief ADA and two subordinate ADAs) to each CCL and SDC. This team prosecutes all cases assigned to their courtroom with broad discretion over how to divide the workload within the team and the desired sentencing outcome.[9] The teams work in their assigned courtroom until staffing needs or promotions reallocate them. Generally, ADAs serve anywhere between a couple months to several years in the same courtroom before receiving a new assignment.

Overall, there were 111 elected judges operating in the Harris County criminal court system between 1980 and 2009. In this same time period, 1,262 individuals worked as assistant district attorneys (ADAs) in Harris County. Among these, 923 worked at some point in their career in the felony courts while 1,154 spent time in the misdemeanor court system. Figure 2 shows the median tenure per assignment among judges and ADAs staffed in a given court per year. Due to the infrequency of elections and high likelihood of re-election, the median judge in both court systems spent the entire year in her courtroom. ADAs working in the state district court exhibit a high degree of stability in their staffing, with the median chief ADA spending generally over 300 days in his court each year. First and second assistant prosecutors generally worked between 150 and 250 days in a given court. The misdemeanor courts also exhibit the same pattern of judge stability, with the median judge spending the entire year in her courtroom. ADAs, however, have a higher degree of turnover with all team members generally spending between 75 and 200 days in their respective courtrooms.

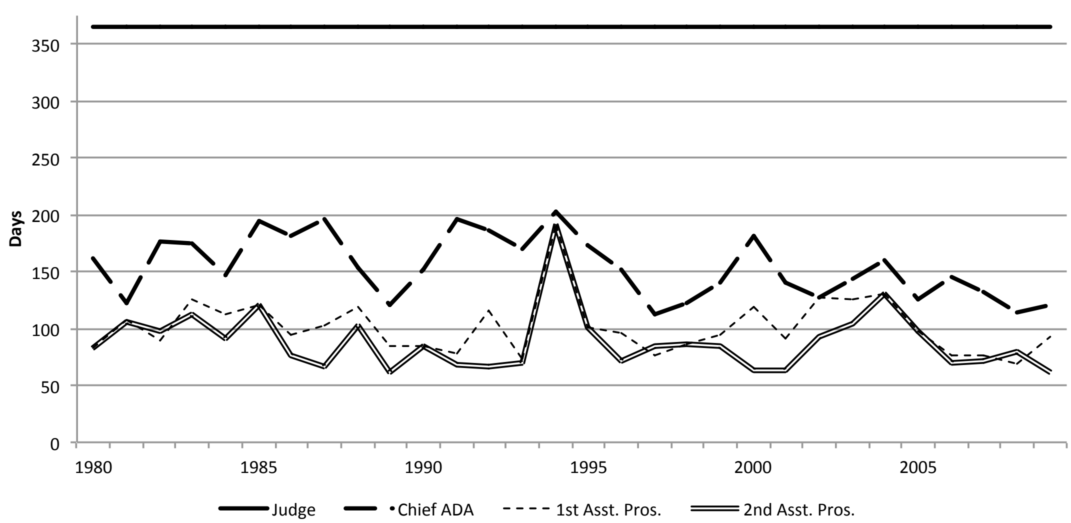
Defendants court outcomes heavily depend of the discretion of the specific judge and prosecutor assigned to their case in Texas. Sentencing guidelines established by the Texas Penal Code (see Appendix A) provide broad recommendations on maximum and minimum sentencing for defendants based on the degree 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

[9]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.

FIGURE 2. Median days staffedin a specific court per year for judges and assistant district attorneys between 1980 and 2009



(A) State District Courts (felony offenses)



(B) Criminal Courts at Law (misdemeanor offenses)

Source: Author's calculations using the Harris County Criminal Courts at Law, State District Courts, and District Attorney's staffing records.

can receive up to a year in county jail. Despite the potential for mandatory minimums, the court can choose to suspend any sentence of 10 years or less in favor of probation for defendants convicted of non-aggravated felonies or misdemeanors. This allows the court to release defendants to community supervision and forgo incarceration altogether under terms similar to parole.

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; the only way to modify these sentences is by court order. In contrast, indeterminate sentences can be changed after the fact by the Texas Department of Criminal Justice and the Texas Board of Pardons and Parole after taking into account an inmate's behavior and his participation in education and training programs. Sentence adjustments come in the form of granting "good time" credits to inmates and permitting supervised early release through parole. The court retains broad influence over incarceration duration however through establishing the maximum sentence length and, as a result, a corresponding minimum sentence length due to Texas's "truth in sentencing" law. Truth in sentencing requires that inmates serve a minimum percentage of their sentence prior to being eligible for early release.[10]

Several additional features give judges and prosecutors broad influence over court outcomes. These include determining the admissibility of evidence, prosecution strategy, sentencing enhancements and plea bargain terms. Local judges also play an important role in the indigent defense system. Until 2011 when a public defenders office was first opened in Houston, a defendant who could not afford legal representation would be appointed a lawyer by his trial judge. Bright [2000] describes judges in Harris County as "treating the appointment of counsel to defend poor defendants as political patronage and [...] assigning lawyers not to provide zealous advocacy but to help move their dockets." In fact, popular press in the early 2000's documented cases in Harris County where appointed counsel were under-qualified, intoxicated, and/or asleep at the time of trial (see Rimer and Bonner [2000]).

3. Sources of Data and Matching Methods

This project uses several 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. Initial filings of felony and misdemeanor charges between 1980 and 2009

[10]The specific percent of the sentence that must be spent in jail or prison depends on the laws in effect when the inmate committed his offense and what type of crime he was convicted of. It can range between 25 and 100 percent.

are included in the data regardless whether the case resulted in a guilty or innocent 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, not were included in the data. Criminal appeals cases were also 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. For these cases, only the earliest filing date and original sentencing outcomes outcomes were retained. For all defendants, sentencing modifications were eliminated from the data (e.g. a defendant who violated the terms of his probation after three years and was incarcerated as a result was only coded as only receiving probation in his original sentencing).

Administrative identifiers in the court data link defendants to their full historical criminal record in Harris County, allowing the research to evaluate local recidivism outcomes. 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. Data on actual incarceration spans between 1978 and 2013 were acquired through Public Information Act requests from the Texas Department of Criminal Justice for state prisons and from the Harris County Sheriff's Office for the Harris County Jail, and matched using the defendant's full name and date of birth.

Quarterly unemployment insurance wage records for the entire state of 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 a combination of full name, sex, exact date of birth and social security number depending on what variables were available in each specific dataset.

Public marriage and divorce indices were also collected from the Texas Department of State Health Services. Unfortunately, this data is only identified at the full name and age at marriage or divorce level, making it prone to mismatch. Incorrect data linkages should be orthogonal to courtroom assignment which should lead to classic measurement error and push estimated coefficients towards zero.

4. The Complications of Multidimensional and Non-monotonic Sentencing

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 two 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.[11] The program effect can potentially be heterogeneous so $\beta_1(X_i)$ is allowed to depend on defendant 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 (see Imbens and Angrist [1994], Angrist et al. [1996]). First, the exclusion restriction requires that $E[Y_i|D_i, X_i, J_i] = E[Y_i|D_i, X_i, J_i']$. This means that judge assignment can only impact the final outcome through

[11] 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 that are attached to a specific courtroom.

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 $E[D_i|X_i, J_i = j] \leq E[D_i|X_i, J_i = k]\ \forall i\}\ \forall j, k$. This means that defendants assigned to judges with higher overall incarceration rates must also be at weakly higher risk for incarceration if assigned to their caseload.

The parsimony of this standard model makes it quite appealing. The source of identification is intuitive, and the estimation is generally straightforward, particularly in settings where the researcher is constrained by data availability. The fact that my data exhibits multidimensional and non-monotonic sentencing patterns, however, limits the plausibility of satisfying the necessary assumptions for unbiasedness. Instead, application of the standard methods in this context results in two distinct biases which for the sake of clarity I term *omitted treatment bias* and *non-monotonic instruments bias*. The nature of these biases are described below.

Omitted treatment bias. In the Texas criminal justice system, judges and prosecutors have influence over several aspects of trial outcomes (e.g. guilt or innocence, incarceration versus probation, duration of punishment, amount of fine, etc.). I may, however, only be interested in a subset of the full range of sentencing outcomes just as incarceration is the focus of this present study. To distinguish between these sets, I define $D_i^f \subset D_i$ as the *focal* set of sentencing outcomes, while the remaining elements are the *non-focal* set D_i^n.

Omitted treatment bias is the result of neglecting of D_i^n when estimating the causal effect of D_i^f on Y_i. Judicial tendencies on focal and non-focal sentencing outcomes may be correlated leading to violations of the exclusion restriction. For instance, if judges who have higher than average incarceration rates also are more likely to impose fines (and the estimated model omits fines), the estimated impact of incarceration 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 potential treatments a defendant may received. For instance, a judge may speak sternly to the defendant, which would likely not be measured in the data. But, to the extent that unmeasured treatments play minor roles in producing final outcomes and are uncorrelated with other judicial tendencies, the potential bias is minimal.[12]

[12]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

Omitted treatment bias can be easily avoided by estimating to the full model, inclusive of both D_i^f and D_i^n. In this scenario, both the focal and non-focal elements of D_i would be simultaneously instrumented using random assignment of judges as the source of exogenous variation. This would ensure that, for instance, the impact of incarceration is identified off of judges who tend to incarcerate relatively more after accounting for their other sentencing tendencies. This approach, however, may undermine joint tests of the first stage and weak instrument robust inference for the focal variables since some non-focal sentencing outcomes may not exhibit significant differences between judges.

To solve this problem, this study proposes constructing predicted values of $E[D_i^n|J_i, X_i]$ and adding them to the second stage equation to eliminate the omitted treatment bias. In practice, this entails estimating the first stage equation for each element of D_i^n, and then adding the predicted values to the second stage equation as reduced form controls.

Non-monotonic instruments bias. Non-monotonic sentencing patterns also create opportunities for bias. Some judges, for instance, are observed to have higher than average incarceration rates for specific subsets of their caseload like drug offenders while also exhibiting lower than average incarceration rates for other groups like property offenders. This creates a situation where it is no longer necessarily true that being randomly assigned to a judge with a higher overall rate of incarceration actually increases the probability of incarceration for every defendant. Violations of the monotonicity assumption lead to an unsigned bias making it difficult to determine if the estimates under- or over-estimate the true effect.

To the extant that this complexity responds to observable characteristics, it is not insurmountable. The standard approach might result in biased estimates, but that is a consequence of the fact that the standard model is misspecified in this context. Judges form expectations as to what will best maximize their objective function given the facts of the case before them and their own subjective information. They don't simply see defendants interchangeably, but respond to the specific

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.

context of each case. This is not saying that judges are inconsistent in their application of the law; instead, their decision rules are just more complex that mean shifts between judges.

The alternative first stage equation I propose for this sentencing model is:

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

In contrast to Equation 2, Equation 3 allows judicial preference to flexibly adjust according to defendant characteristics. The implication is that the monotonicity of the impact of judge assignment no longer need hold across all defendants, but instead the impact of judge assignment must only remain consistent among a group of peers with similar observable characteristics (e.g. Caucasian male drug offenders or African American females convicted of driving while intoxicated).[13] While the modified approach adds complexity to the model, it relaxes the assumptions necessary for unbiased results.

Empirical examples. 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 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; more refined estimates using the full sample of data are reserved for Section 7.

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 controls in this example are constructed using the judge-specific mean of each non-focal court outcome. The results are shown in Table 2.

[13]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, 2003]), but is beyond the scope of this study.

TABLE 2. Estimating the "causal effect" of incarceration in the presence of 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

Source: Harris County District Clerk's criminal court records (cases filed in State District Court between 2005 and 2006).
Notes: "Sentenced to incarceration" is instrumented using fixed effects for the assigned judge at the time of charge. *** p<0.01, ** p<0.05, * p<0.1.

In the first specification, the impact of incarceration is estimated without controlling for judicial tendencies on any other court outcomes. The estimated coefficient 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 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.

The second and third specification also produce positive and significant coefficients, but now the estimated impact of being incarcerated increases dramatically, up to 125 to 300 percent larger. The smaller coefficient observed in 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

TABLE 3. 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%			

Source: Author's calculations using Harris County District Clerk's criminal court records (driving while intoxicated and possession of marijuana cases filed in County Criminal Courts between 2005 and 2006).

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 3 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 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.[14] 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

[14]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 7.

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 4. 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 being charged with a new crime within one year by 32 percentage points, which is significant at the five percentage point level. The effect for those charged with drug possession is even larger at 51 percentage points although only significant at the 10 percentage point 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:

$$\left(\hat{\beta}_1^{LATE} - \beta_1^{LATE}\right) = \frac{Pr[\text{Defier}]}{Pr[\text{Complier}] - Pr[\text{Defier}]} \times \left(\beta_1^{\text{Complier}} - \beta_1^{\text{Defier}}\right) \quad . \tag{4}$$

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.

TABLE 4. 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

Source: Author's calculations using Harris County District Clerk's criminal court records (driving while intoxicated and possession of marijuana cases filed in County Criminal Courts between 2005 and 2006).
Notes: *** p<0.01, ** p<0.05, * p<0.1.

Given this formula, I can directly compute the magnitude of the bias from using the judge assignment without crime type interactions 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 (0.0748) times the percent of the sample that is charged with DWI (0.557). The defier rate is equal to difference in the incarceration rates between the judges for drug possession (0.0726) times the percent of the sample that is charged with DWI (0.443). For the remaining two parameters, $\hat{\beta}_1^{\text{Complier}}$ is shown in the second column of Table 4, while $\hat{\beta}_1^{\text{Defier}}$ is listed in the third column. This results in the following calculation:

$$\begin{aligned}\text{Bias} &= \frac{0.0726 \times 0.443}{0.0748 \times 0.557 - 0.0726 \times 0.443} \times (0.3248 - 0.5147) \\ &= -0.6367\end{aligned}$$

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 4 (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 5 shows the results of separate estimations of the impact of incarceration based on splitting the sample by crime type, sex, first time offender status, age and race. Each reported coefficient is the result of a separate regression. The first column shows the effects estimated off of judge fixed effects within the given subgroup. The second column shows the effects estimated using judge fixed effects interacted with crime type within the given subgroup as the instrumental variable. The first versus the second columns present starkly divergent conclusions regarding the impacts of being incarcerated. When using uninteracted judge fixed effects, only the coefficients for the crime type subgroups are found to be statistically significant, which happen to be equivalent to allowing judge fixed effects to interact with crime type. In addition, coefficients vacillate between positive and negative and a chi-squared test of the joint significance across all specifications fails to reject the null hypothesis. In contrast, the second column shows systematic positive effects of incarceration on short-run recidivism across all subgroups, with the joint test strongly rejecting the null. The problem with the subgroup analysis here is that splitting the sample by sex or age eliminates potential violations of monotonicity only along those specific dimensions, but fails to correct the known violation based on crime type.

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 in which decision makers demonstrate non-uniform within-caseload preferences (see Korn and Baumrind [1998], Korn et al. [2001], Waldfogel [1998], Abrams et al. [2010] and Price and Wolfers [2010]). These settings include medical care, criminal law and professional sports. This is not to say that non-monotonicities necessarily bias studies based on this type of research design; even when non-uniform preferences exist, to the extent they represent a small portion of the overall variation (i.e. $Pr[\text{Defier}] \approx 0$) or that the range of potential treatment effects is not very

TABLE 5. 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

Source: Author's calculations using Harris County District Clerk's criminal court records (driving while intoxicated and possession of marijuana cases filed in County Criminal Courts between 2005 and 2006).
Notes: *** p<0.01, ** p<0.05, * p<0.1.

large, the resulting bias will be minimal. It does, however, provide motivation for developing more rigorous empirical methods.

5. ESTIMATING INSTRUMENTAL VARIABLE MODELS IN THE PRESENCE OF NON-MONOTONIC INSTRUMENTS

The solution to non-uniform judge preferences is straightforward if judges base their decisions on a single defendant characteristic that is categorical in nature (e.g. crime type). Using the judges' crime-specific incarceration rates in lieu of their average incarceration rates as the source of exogenous variation in sentencing will eliminate monotonicity violations resulting from the non-uniform preferences. But, the answer becomes more complicated when the data contains many defendant characteristics some of which might be categorical (e.g. sex, race or skin tone) and others might be continuous (e.g. age, time since last criminal charge or total prior convictions) and it is unknown over which judges base their decisions.

A fully non-parametric estimation of $\Gamma_1(X_i)$ from Equation 3 would be the most straightforward approach from a theoretical perspective. Multivariate regression including screener fixed effects fully interacted with all potential pre-existing covariates and combinations thereof would provide consistent estimates of the flexible judge-specific decision rules. But, due to the curse of dimensionality such models often are not practical. 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 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,

$$\Gamma_1(X_i)J_i = \sum_{k=1}^{K} \omega_k b_k(X_i, J_i) + \eta_i, \tag{5}$$

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 here. The functions I focus on measure how judicial preferences deviate 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.[15] For continuous characteristics, 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 [2013]). 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 be 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

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

and the overall caseload for various subgroups in the population:

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

In this notation, κ represents the potential values that the categorical variable x_i takes and J_i 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 are 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.[16]

The full set of basis functions could be used jointly as instrument variables without explicitly estimating their respective weights ω_k. However, since I explicitly do not want to arbitrarily constrain the set of defendant traits that may influence judicial preferences, the set of constructed preference measures can be very large and may lead to many instruments bias (see Hansen et al. [2008]). This problem is easily solved by invoking a cross validation sample splitting technique (see 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.

[16]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.

The difficulty with cross validation in this context 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. For instance, after controlling for judicial preference by crime type, it is unlikely that measured preference by crime type interacted with eye color adds a significant amount of new variation to the estimation. 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, the potential variance reduction has generally been shown to result in improved prediction accuracy (Leeb and Pötscher [2008a, 2008b]), which is precisely the goal given that this being implemented in a first stage equation using cross validation. Many procedures have been explored in the context of instrumental variables including boosting (Bai and Ng [2009]), common factor analysis (Bai and Ng [2010]), and ridge regression (Hansen and Kozbur [2013]). I adopt the *least absolute shrinkage and selection operator* (Lasso) originally proposed in Tibshirani [1996], which has received growing interest in recent years in the literature (see Belloni et al. [2014] for discussion of recent work).

I follow Belloni et al. [2012]'s implementation of Lasso by estimating following objective function to solve for ω:

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

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 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 $\lambda_k^o = \sqrt{\mathbf{E}\left[f_k(x_{k,i})^2 \eta_i^2\right]}$ are infeasible since η_i, the error term from Equation 5, 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 $\hat{\lambda}_k = \sqrt{\frac{1}{N}\sum_i [f_k(x_{k,i})^2\hat{\eta}_i^2]}$. 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 7 are constructed using Post-Lasso coefficients rather than Lasso coefficients.[17]

[17] 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.

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.

6. A Panel Model of the Impact of Incarceration

In contrast to existing work using judge randomization, this study adopts a panel framework to estimate both the immediate and lasting 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 specific variables: (1) the percent of days in a quarter that a defendant was incarcerated, (2) whether the defendant was previously incarcerated if he is not currently incarcerated, and (3) the total amount of time the defendant has spent incarcerated if he is not currently incarcerated. Quantifying these variables on the quarterly versus monthly or weekly basis may introduce measurement error into the analysis; this is unavoidable, however, as several outcome variables are only measured on the quarterly basis.

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 deal with this issue, I recode incarceration status to zero once in the days after new charges are filed until a new quarter has started. This breaks the mechanical relationship 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 incarcerations spells is 10 days.

To account for any unobserved changes 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:

$$(6) \quad Y_{i,t+q} = \delta_1 Incarcerated_{i,t+q} + \delta_2 Released_{i,t+q} + \delta_3 \left(Released_{i,t+q} \times Exposure_{i,t+q}\right) + \delta_{4,q}\hat{D}_i^n + \delta_{5,q}X_i + \mu_t + \mu_q + \xi_{i,t+q} \ ,$$

where the primary variables of interest are defined as:

$$\begin{aligned}
Incarcerated_{i,t+q} &= \frac{\text{Days Incarcerated}_{i,t+q}}{\text{Days in Quarter}_{t+q}} \ , \\
Released_{i,t+q} &= 1\left[\sum_{\tau=1}^{T} Incarcerated_{i,t+q-\tau} > 0\right] \times 1\left[Incarcerated_{i,t+q} < 1\right] \ , \\
Exposure_{i,t+q} &= \sum_{\rho=1978q1}^{t+q} Incarcerated_{i,\rho} \ .
\end{aligned}$$

To compute $Released_{i,t+q}$, a maximum retrospective window (denoted by T) is necessary. When a more narrow window is used, δ_2 and δ_3 will capture primarily short-run effects. A longer window will average short-run impacts with medium-term and long-term impacts. In order to strike a balance between short and medium-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, which is when the state prison data first begins. I cap the total exposure variable to 5 years to reflect the likely diminishing returns to incarceration length, and to improve the precision in the construction of the instruments. The model only allows total exposure to impact outcomes once an inmate has been released in order to avoid confounding the impact of duration after being released with any potential incapacitation effects.

Five years of post-charge outcomes are included in the panel data. In order to account for repeated observations for the same defendant over time as well as the same defendant appearing in the data for multiple charges, the standard errors are clustered at the defendant level.

The primary variables of interest will be instrumented using the methodology discussed in Section 5. 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 around the time charges were filed (e.g. a "tough" judge becomes relatively less tough when all of the "easy" judges are elected out of office and replaced with other "tough" judges).

Unlike fixed court outcomes such as guilt or fines, incarceration status and history change with time. As a result, instruments for these variables must be recalculated each quarter since the date of charge. This amounts to comparing the relative portion of each court's caseload that is incarcerated as of one quarter after charges were filed, two quarters after charges were filed and so on. A benefit of recalculating the instruments over time in the panel framework is that the estimation will leverage non-linear differences in the distribution of sentencing outcomes between courtrooms rather than focus on average differences in overall sentence length. As an example, some courts may be characterized by bimodal distributions of primarily short-term and long-term incarceration, whereas others might utilize a more 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 severely limits the feasibility of estimating the full panel model proposed. Instead, for this caseload, I will estimate the same model but exclude the $[Released_{i,t+q} \times Exposure_{i,t+q}]$ variable. This means that the misdemeanor analysis will be unable to speak to the post-release impact along the intensive margin, and instead will only measure the incapacitation effects and the post-release effects on the extensive margin.

7. The contemporaneous and post-release effects of incarceration

This section presents the empirical findings of the study. It begins with a descriptive analysis of how incarceration status, criminal activity and employment develop over time for defendants. I then report several results to confirm the validity of the proposed research design. Finally, I present the instrumental variable analysis which estimates the pre- and post-release effects of incarceration on defendant and household outcomes. Particular attention is paid to distinguishing mechanisms that underlie these results where possible. The results of my robustness exercises are discussed at the end of the section.

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, which is separated out into being in county jail and state prison, as well as criminal charges are measured on a weekly basis, whereas employment is measured quarterly. In order to preserve the scale of the figures, criminal charges in Week 0 are excluded since by definition all defendants would be charged in this week.

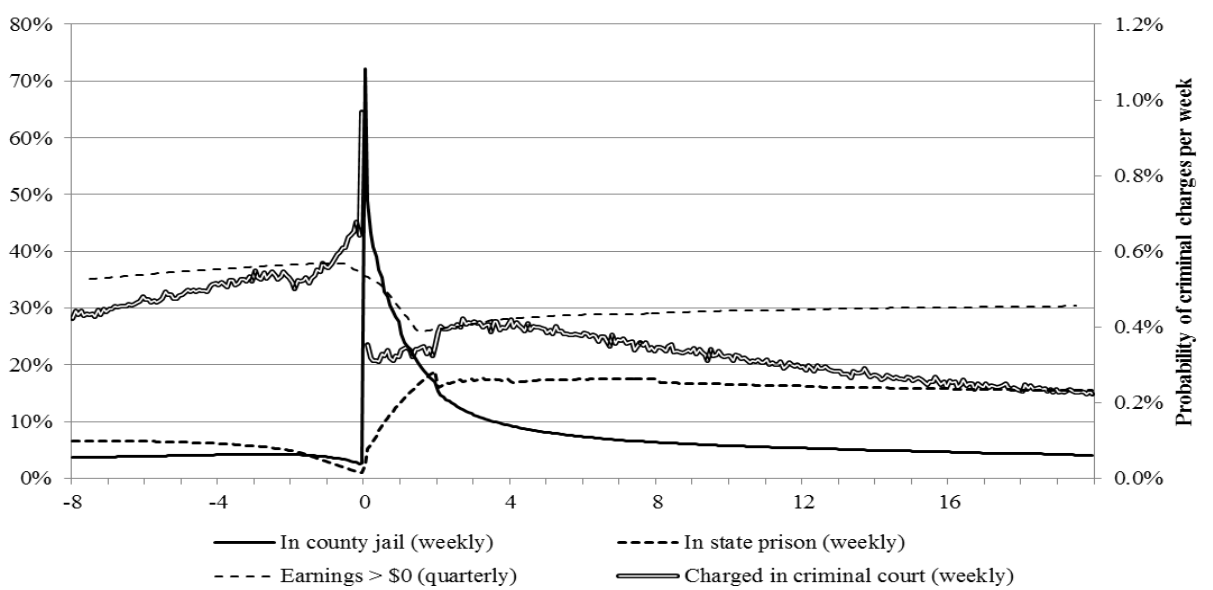
In the run up to Week 0, there is a relative decline in the incarceration rate of both felony and misdemeanor defendants. This is mirrored by an increase in criminal activity. Once charges are filed, there is an immediate increase in the likelihood of being jailed which is later displaced by prison for felony defendants. These increases in incarceration coincide 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 potentially upon release. In the 5 years of post-charge data, employment does not ever return to pre-charge levels.

This study relies on the fact that defendants are randomly assigned to courtrooms. While there is no reason to doubt the random assignment because it was implemented by an external office, the fact that the data only includes non-sealed records means that the observed caseloads for each courtroom may be censored in a non-random way. This can be tested by estimating the following equation.

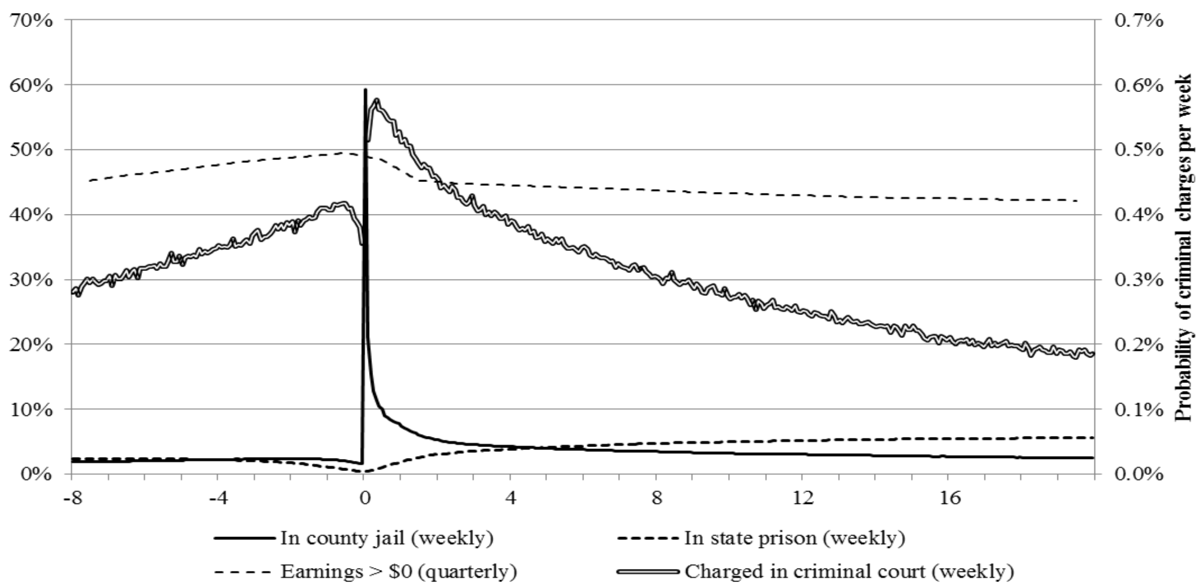
$$x_{i,t} = \alpha + \tau_t + \beta Court_{i,t} \otimes \tau_t + \epsilon_{i,t}$$

In this equation, $x_{i,t}$ is a defendant covariate, $Court_{i,t}$ is a vector of court assignment dummy variables and τ_t is a vector of charge year dummy variables. Because Harris County introduced several new courtrooms in response to growing caseloads over time, it is necessary to include time dummies to absorb this variation. In addition, since the composition of courtroom actors (i.e. judge,

FIGURE 3. Timeline of incarceration, additional criminal charges, and employment



(A) Felony defendants



(B) Misdemeanor defendants

Source: Author's calculations using Harris County District Clerk's criminal court records (1980-2013), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice's state prison records (1980-2013), and Texas Workforce Commission's unemployment insurance wage records (1994-2012).

chief prosecutor, etc) is changing over time, I fully interact courtroom and year fixed effects so that courtroom deviations are not arbitrarily constrained over time. In order to evaluate if the observed caseloads are statistically equivalent, I conduct an F-test of the joint significance of β. This procedure can be repeated using a sentencing outcome instead of a defendant covariate to establish a baseline of the instrument relevance if only using average differences between courtrooms.

Table 6 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. 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.

While there is significant average differences between courtrooms, particularly with respect to being sentenced to incarceration, I also rely on characteristic-specific differences in order to avoid non-monotonic instruments bias. Figure 4 shows the distribution of constructed instrument values for sentenced incarceration status and length for the felony and misdemeanor caseloads. Instruments for the felony caseload have more variation compared to the misdemeanor caseload, particularly with regard to incarceration length.

In order to document which traits have the most influence on relative court tendencies, Table 7 reports the ten strongest predictors of incarceration status and length selected by Post-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 court actors exhibit the most divergent preferences, which will have greatest influence over the final constructed instrument.[18]

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

[18]Instruments for actual incarceration status over time instead of static sentenced incarceration status and length are used in my main analysis. Because the Post-Lasso selects similar predictors for both sets of variables, sentenced incarceration is presented here for simplicity. The sample splitting technique was not employed in the construction of this table in order to avoid adding unnecessary complexity to the discussion.

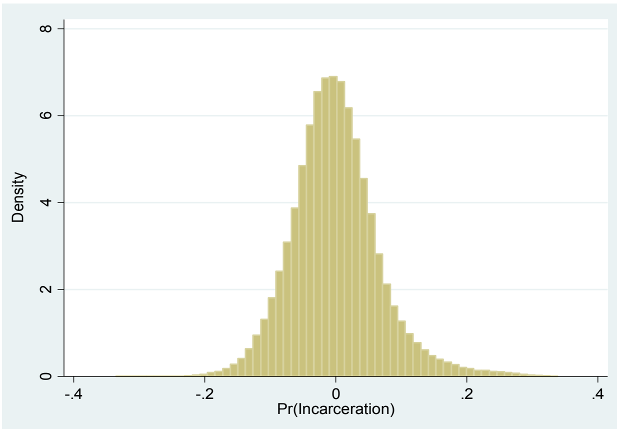
TABLE 6. Testing for significant differences between courts

	F-Test	
	Felony	Misd.
Panel A: Defendant Characteristics		
Female	0.9	1.1
Race/Ethnicity = Caucasian	1.1	1.2
Race/Ethnicity = African American	1.2	1.2
Race/Ethnicity = Hispanic	1.1	1.3
Age	2.3	1.0
Weight	1.0	1.1
Height	1.0	1.0
First Time Offender	1.3	1.2
Crime = Driving while intoxicated	1.2	1.1
Crime = Drug Possession	1.3	1.1
Crime = Traffic	1.4	1.1
Crime = Property Crime	1.4	1.2
Crime = Violent Crime	1.2	1.0
Panel B: Sentencing Outcomes		
Verdict = Guilty	14.0	15.0
Verdict = Deferred Adjudication of Guilt	19.3	22.8
Sentenced to Incarceration	14.8	19.7
Incarceration Length	3.4	11.0
Given Fine	24.5	8.1
Fine Amount	10.7	242.5
Sentenced to Probation	18.8	26.3
Probation Length	18.7	24.3
Felony Conviction Reduced to Misdemeanor	7.4	-
Sentenced to Drug Rehabilitation Program	9.5	4.9
Sentenced to Boot Camp Program	6.3	5.5
Sentenced to Incarceration during Off-Work Hours	6.0	27.6
Enrolled in Traffic School	1.0	11.6
Enrolled in Drug Education	7.1	12.6
Ignition Device Required	2.8	35.7
Electronic Monitoring	1.6	24.1

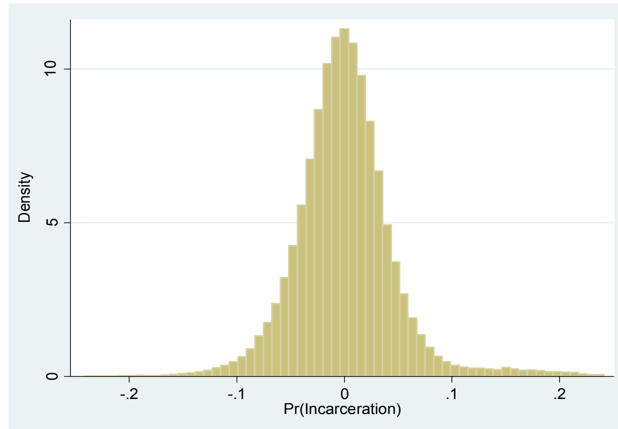
Source: Author's calculations using Harris County District Clerk's criminal court records (1980-2009).

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 the joint tendencies of judge and chief prosecutor. The defendant characteristics that most heavily influence relative court opinion are interactions between the defendant's type of crime, degree of charge, and criminal history. Felony courts pay closer attention to prior felonies while the lower courts rely more on misdemeanor records. Defendants' age and race also appear to systematically influence felony court decision making, while age, sex, and skin tone are more prominent in the misdemeanor caseload. For each

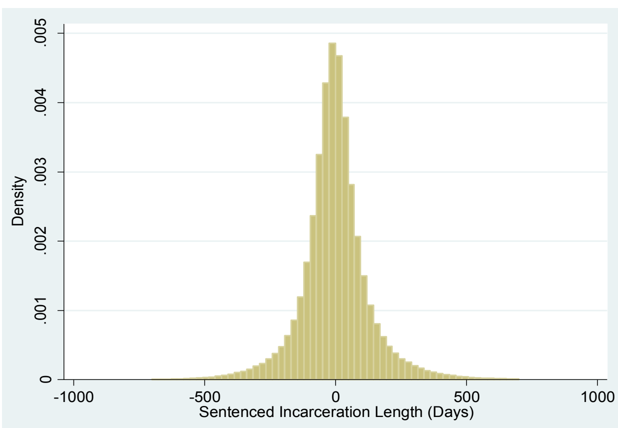
FIGURE 4. Histograms of constructed instruments for intensive and extensive margins of incarceration



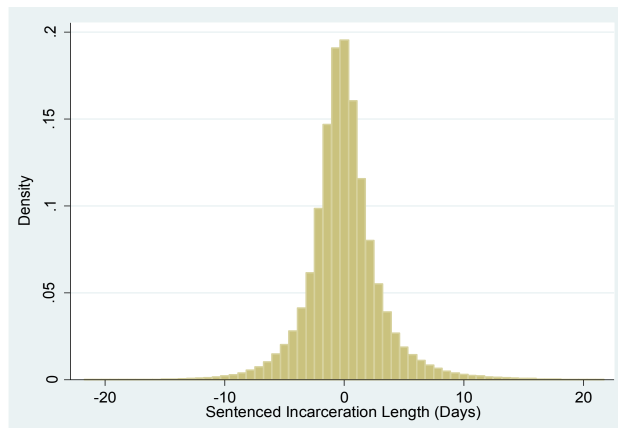
(A) Extensive margin, felony defendants



(B) Extensive margin, misdemeanor defendants



(C) Intensive margin, felony defendants



(D) Intensive margin, misdemeanor defendants

Source: Author's calculations using Harris County District Clerk's criminal court records (1980-2009).

sentencing outcome, when the algorithm is given the opportunity to select among conditional and unconditional sentencing propensities, unconditional sentencing propensities were never chosen.

One might be concerned that allowing for flexible interactions will overfit the first stage, even if sparse and cross-validation methods are employed. One strategy to evaluate this concern is to examine whether the proposed methodology constructs instruments are predictive of pre-charge incarceration status. Because judges and prosecutors have no jurisdiction over a defendant until he

TABLE 7. Ten strongest normalized predictors selected by Post-Lasso for constructed instrumental variable

Court Actors	Defendant/Crime Characteristic	$\hat{\omega}$
Panel A: Incarceration status, felony caseload		
2nd Asst. Pros.	Defendant age × Crime type	0.008
Judge	Total prior felony convictions × Crime type	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
Judge	Total felony convictions × Defendant race	0.005
1st Asst. Pros.	Crime type × Defendant race	0.004
2nd Asst. Pros.	Total prior felony charges × Charge degree	0.004
1st Asst. Pros.	Defendant age × Crime type	0.004
Judge	Total prior felony charges × Defendant Race	0.004
Panel B: Incarceration length, felony caseload		
1st Asst. Pros.	Total prior felony convictions × Time since last charge	0.225
Chief Pros.	Time since last conviction × Charge degree	0.097
2nd Asst. Pros.	Defendant age × Crime type	0.085
2nd Asst. Pros.	Total prior felony charges × Crime type	0.055
2nd Asst. Pros.	Crime type × Charge degree	0.054
2nd Asst. Pros.	Defendant age × Charge degree	0.051
Chief Pros.	Defendant age × Crime type	0.050
Judge	Crime type × Charge degree	0.043
Judge	Total prior felony charges × Charge degree	0.043
1st Asst. Pros.	Crime type × Charge degree	0.042
Panel C: Incarceration status, misdemeanor caseload		
2nd Asst. Pros.	Total prior misd. charges × Time since last charge	0.025
Judge	Crime type × First-time/Repeat offender	0.007
Chief Pros.	Crime type × Charge degree	0.004
2nd Asst. Pros.	Defendant age × Crime type	0.004
1st Asst. Pros.	Crime type × Charge degree	0.004
Chief Pros.	Total prior misd. charges × Time since last charge	0.004
Judge	Total prior misd. convictions × Crime type	0.004
Judge	Time since last conviction × Crime type	0.003
Chief Pros.	Total prior misd. charges × Skin tone	0.003
2nd Asst. Pros.	Crime type × Charge degree	0.003
Panel D: Incarceration length, misdemeanor caseload		
Chief Pros.	Time since last charge × Time since last conviction	0.005
Judge	Total prior misd. convictions × Crime type	0.002
2nd Asst. Pros.	Total prior misd. convictions × Sex	0.001
2nd Asst. Pros.	Total prior misd. charges × Crime type	0.001
Chief Pros.	Total prior misd. charges × Time since last charge	0.001
1st Asst. Pros.	Total prior misd. charges × Sex	0.001
Chief Pros.	Total prior misd. convictions × Charge degree	0.001
Chief Pros.	Total prior felony charges × Time since last conviction	0.001
Chief Pros.	Total prior misd. convictions × Skin tone	0.001
Chief Pros.	Total prior misd. convictions × Crime type	0.001

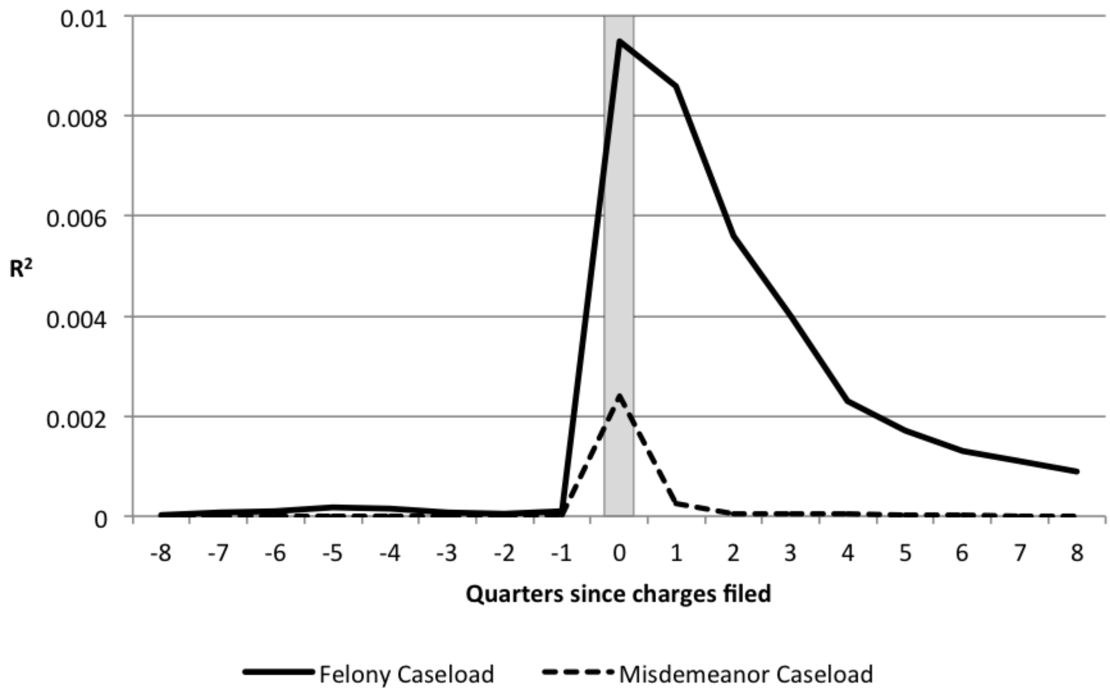
Source: Author's calculations using Harris County District Clerk's criminal court records (1980-2009).

is assigned to their courtroom, courtroom assignment should have no predictive power before the filing of charges.

To evaluate this hypothesis I construct "instruments" for the defendant's incarceration status for the 8 quarters before and 8 quarters after the filing of the defendant's charge using my proposed algorithm from Section 5. I then regress the constructed instruments on actual incarceration status and record the R^2 for each quarter. Figure 5 shows the results of this exercise. In the lead up to criminal charges, the constructed instruments have effectively zero explanatory power in both the felony and misdemeanor caseloads. There is a sharp break, however, once charges are filed indicating that court assignment has a clear albeit modest impact on incarceration status. Court influence is most pronounced during the first year after charges are filed. At its peak, the R^2 is 0.095 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. This pattern is precisely what should be expected for this research design.

I now turn to discussing the main findings of this paper. The first set of outcomes I consider is the impact of incarceration on future criminal activity in the 5 years after the defendant's charge date. Criminal activity is measured using three different variables: being booked into county jail for a new arrest, being charged in a Harris County criminal court with a new offense, and being convicted of a criminal offense anywhere in the state of Texas. Each of these measures comes from a different data source, and are not perfectly nested as a result. Table 8 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 (booking for new arrest, new criminal charges in the county court, and any statewide convictions), and each controls for time and relative quarter fixed effects as well as interactions between relative quarter fixed effects and defendant covariates and non-focal sentencing outcomes (or non-focal instruments in the case of IV). To account for repeated observations of defendants across multiple criminal episodes and over time, the standard errors are clustered at the defendant level.

FIGURE 5. R^2 of incarceration status first stage regression, by quarter relative to charges



Source: Author's calculations using Harris County District Clerk's criminal court records (1980-2009), Harris County Sheriff's county jail records (1980-2013), and Texas Department of Criminal Justice's state prison records (1980-2013).

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 based on my three measures, especially for longer incarceration durations. The incapacitation effects measured here, however, likely underestimate the true effect of incarceration as those with the greatest probability of reoffending are usually incarcerated longer. Likewise, post-release estimates may be biased upwards given that who are incarcerated also are thought to have unobserved characteristics that increase their probability of committing crimes.

The IV estimates show an incapacitation rate of 3 to 6 percentage points per quarter for marginal felony defendants depending on the measure. This decline in criminal activity, however, is offset

TABLE 8. 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

Source: Harris County District Clerk's criminal court records (1980-2013), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice state prison records (1978-2013), Texas Department of Public Safety statewide criminal conviction history database (1980-2013).
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. *** p<0.01, ** p<0.05, * p<0.1.

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 first finding that jumps out is that marginal offenders are more likely to be booked in a county jail for a new arrest or charged in court while incarcerated.

Taken literally, these results are misleading. The median incarceration sentence in the misdemeanor caseload is only 10 days, which is much shorter than the resolution at which the data is constructed.[19] While it is not impossible, it is extremely uncommon in the data for inmates to be charged with a new crime while in county jail. Instead, what this coefficient is measuring is the combined effect of temporary incapacitation as well as the immediate post-release effect in the remaining weeks or months in the quarter. Because the median defendant spends only slightly more than one-tenth of a quarter incarceration, the coefficient should be scaled down substantially for accurate interpretation. With this scaling adjustment, the measured effect is essentially brought in-line with the post-release coefficient estimates. Given this interpretation, it appears that, like the felony caseload, marginal misdemeanor defendants tend to become more criminally involved as a result of incarceration rather than less.

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 fact that I measure incapacitation using bookings, court charges and statewide convictions, the post-release increases in criminality suggest an alternative explanation. The strongest evidence in the literature on incapacitation comes from research designs that rely on quasi-random variation in sentence reductions among inmates who are already in jail or prison. Given their exposure to incarceration leading up to the potential sentence reduction, their likelihood of reoffending increases upon release which would translate into higher measured incapacitation rates. In comparison, the counterfactual in the study are individuals released on probation or not convicted due to their court assignment, whose natural rate of criminal behavior will be lower due to their insulation from the prison system.

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 9). The crimes encouraged through incarceration's impact on post-release behavior, on the other hand, tend to be primarily concentrated in felony-level crimes. This is particularly concerning because this indicates that criminal activity not only appears to be going up on net, but also appears to be getting

[19]Efforts to reestimate the model at the weekly level were explored, but not feasible due to computational constraints.

TABLE 9. 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

Source: Harris County District Clerk's criminal court records (1980-2013), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice state prison records (1978-2013).
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. *** p<0.01, ** p<0.05, * p<0.1.

more serious in nature. The misdemeanor caseload does not follow this trend. Instead, the increase in criminal activity overall tends to more weighted towards new misdemeanor charges. This could explain why no significant effects were observed when using statewide convictions as the dependent variable since the TDPS dataset has relatively 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 10 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 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 this table 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 to income stability, the effect on drug offenses, which are the most common crime type among inmates, suggests a distinct possibility for criminal learning in this context. 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 11 shows how incarceration impacts quarterly employment, income and log income. The first and third columns show the OLS coefficients for felony and misdemeanor defendants respectively, while the second and fourth columns show the IV estimates. 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 on post-release outcomes. The estimates based on the OLS estimates are larger in magnitude, likely drive 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, 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 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 were 3.6 percentage

TABLE 10. 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

Source: Harris County District Clerk's criminal court records (1980-2013), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice state prison records (1978-2013).
Notes: Each column excludes defendants originally charged with the type of crime being considered as the outcome variable. 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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

points less likely to be employed and earned 0.34 less log income. 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.

To further explore the impact on labor market outcomes, Table 12 breaks out the impacts on employment and log income 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

TABLE 11. 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

Source: Harris County District Clerk's criminal court records (1989-2009), Harris County Sheriff's county jail records (1994-2012), Texas Department of Criminal Justice state prison records (1994-2012), Texas Workforce Commission's unemployment insurance records (1994-2012).
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. *** p<0.01, ** p<0.05, * p<0.1.

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.

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 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 be unemployed even 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.

To determine whether incarceration increased or decreased dependence on government programs, Table 13 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.

Whether incarceration also effects social capital in addition to human capital is an important question. Marriage rates have significantly declined in recent decades, particularly among low-income and African American communities (Stevenson and Wolfers [2007]). Researchers point to the growth in mass incarceration as an important contributor to this trend (Western and Wildeman [2009]). Table 14 reports the effects of incarceration on marriage and divorce activity. The estimates are separately estimated for those under 25 years of age at time of charge and those over 40 years of age at time of charge. The purpose of these breakouts is to target the timing of when marriage and divorce are most likely to occur. Significant impacts are observed in the felony caseload.

TABLE 12. 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

Source: Harris County District Clerk's criminal court records (1994-2009), Harris County Sheriff's county jail records (1994-2012), Texas Department of Criminal Justice state prison records (1994-2012), Texas Workforce Commission's unemployment insurance records (1994-2012).
Notes: Pre-charge income calculated using up to 12 quarters of pre-charge data. 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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Individuals under 25 years old are 1 percentage point less likely to get married while incarcerated. While the absolute magnitude of this coefficient is relatively small, the relative effect is substantial given that defendants face a 0.27 percent likelihood of getting married each quarter in the 2 years leading up to charges. The fact that these estimates are substantially larger than the average marriage rate indicates that marginal defendants likely exhibit a higher likelihood of marriage than non-marginal defendants.

TABLE 13. Incarceration and public benefit receipt

Criminal Caseload	Felony		Misdemeanor	
	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

Source: Harris County District Clerk's criminal court records (1987-2009), Harris County Sheriff's county jail records (1992-2012), Texas Department of Criminal Justice state prison records (1992-2012), Texas Health and Human Service Commission's Food Stamps records (1994-2011), Texas Health and Human Service Commission's AFDC/TANF records (1992-2011).
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. *** p<0.01, ** p<0.05, * p<0.1.

The decline in marital activity does not appear to be simply a temporal displacement as post-release effects are quite small if not negative. In fact, among the felony defendants over 40 years old, who have the highest pre-charge marriage rate, the probability of divorce increases by 1 percentage point while incarcerated with positive coefficients on the post-release measures. I find that defendants serving three or more years in prison are at a statistically significant elevated risk

TABLE 14. The effects of incarceration on marriage and divorce activity

Caseload	Felony		Misdemeanor	
	OLS	IV	OLS	IV
Panel A: Marriage, Defendant Age at Charge < 25				
In jail or prison	-0.003***	-0.012**	-0.003***	0.00038
	(0.0001)	(0.006)	(0.0001)	(0.0081)
Released from incarceration	-0.001***	-0.008	-0.001***	-0.0016
	(0.0001)	(0.006)	(0.0001)	(0.0018)
[Released × Duration]	-0.0001	0.0004		
	(0.0001)	(0.003)		
Kleibergen-Paap rk LM stat.		135.9		573.6
Kleibergen-Paap rk Wald F stat.		45.3		100.6
Unique defendants	175,609	175,609	374,636	374,636
Total observations	4,447,204	4,446,062	10,848,746	10,847,455
Panel B: Divorce, Defendant Age at Charge >= 40				
In jail or prison	-0.0006***	0.011**	-0.0007***	-0.003
	(0.0001)	(0.005)	(0.0001)	(.009)
Released from incarceration	-0.0004***	0.002	-0.0006***	0.001
	(0.0001)	(0.006)	(0.0001)	(0.002)
[Released × Duration]	-0.00002**	0.002		
	(0.00002)	(0.001)		
Kleibergen-Paap rk LM stat.		64.8		111.9
Kleibergen-Paap rk Wald F stat.		21.8		56.1
Unique defendants	83,962	83,962	188,833	188,833
Total observations	2,307,326	2,304,959	5,103,583	5,101,709

Source: Harris County District Clerk's criminal court records (1980-2009), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice state prison records (1978-2013), Texas Department of State Health Services marriage and divorce indices (1980-2012).
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. *** p<0.01, ** p<0.05, * p<0.1.

of divorce post-release.[20] This further solidifies the argument that incarceration appears to play a causal role in preventing and dissolving marriages.

Robustness Exercises. Several robustness exercises, which can be found in Appendix C, were conducted to confirm the stability of the results. The results are generally robust, but some instability is observed in the incapacitation effects measured in the misdemeanor caseload. As such, these results should be interpreted with caution.

[20]Test not shown but available upon request.

In the first robustness check, 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[21], but is generally thought to capture the most serious crimes. While I do not observe the incapacitation effects in this data, 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 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 estimates virtually unchanged.

The third exercise intentionally omits crime type from the construction of the instruments. This explores the extent to which other covariates can account for variation that is of first order importance with regard to non-monotonic sentencing yet omitted from the estimation. This is an important concern considering that there are a number of potentially important defendant traits like educational attainment or marital status that are not included in my data. Reestimating my findings with the new set of instruments does not significantly change my results, indicating that an incomplete set of covariates still can potentially capture the important dimensions of non-uniformity.

The fourth robustness check 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.

[21]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

The fifth exercise 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 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 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.

8. Reexamining the costs and benefits of incarceration

A common exercise when presented with incapacitation effects is to evaluate whether the cost savings from crimes prevented outweigh the expense of keeping the inmate incarcerated (see Levitt [1996] or Owens [2009]). Without taking into account general deterrence effects, this type of calculation has been interpreted as a lower bound on the social gain from incarceration. But, such an exercise is not necessarily so straightforward; 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 that could increase the overall costs. 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 gaps in prior estimates. I cannot conduct a full cost benefit analysis as my research design does not measure general deterrence effects, and even if I could, there is no clear measure for other intangible benefits of punishment like a retributive utility gain for victims. But, 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 of incarceration I can provide improved partial estimates.[22] The remaining question is then to ask whether general deterrent 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 (Bartley [2000]), compensating wage differentials (Viscusi [2000]), estimates based on jury awards (Miller et al. [1996]) and contingent valuation studies (Cohen et al. [2004]). Where necessary, these estimates are supplemented by simple accounting exercises. Additional complications arise when considering whether to include or exclude property transfers from theft in the calculation since criminals gain utility from consuming stolen goods. I follow Donohue III [2009] in using the costs proposed in jury award studies, which are the most commonly used strategy in the literature, excluding transfers as lower bound estimates and contingent valuation prices as an upper bound estimates. Fewer crimes have been priced by the contingent valuation methodology[23], and so jury award prices inclusive of the value of stolen property supplement these figures.[24]

Resources are also expended in the criminal justice and legal system in order to arrest, charge, convict and punish individuals who commit crimes. In the absence of such crimes, these resources could reallocated for more constructive uses. Additionally, facing new criminal charges increases

[22] Because there is no clear way to value the impacts to marriage and divorce, these are excluded from the calculation.

[23] Crimes that have been explicitly priced by the contingent valuation methodology are murder, rape, assault, robbery and burglary.

[24] Neither approach has priced the cost of drug consumption. To address this gap, I construct a naive price using aggregate cost estimates from the Department of Justice and aggregate usage rates rom the National Survey on Drug Use and Health. National Drug Intelligence Center [2011] estimates that the economic impact of illicit drug use in the United States in 2007 was $193 billion. This number is inclusive of impacts to criminal justice expenditures, defendant productivity and health. Because criminal justice expenditures and defendant wage losses (due to incarceration) are accounted for elsewhere in the calculation, I exclude them which results in aggregate costs of $84.8 billion mainly attributed to decreases in productivity and increases in health expenditures. 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 frequency rate of 8.4 drug episodes per user. Finally, I divide aggregate costs by total estimated drug episodes in the year, which results in a price of $37 per act of drug consumption.

TABLE 15. 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

[a]Based on Miller et al. [1996] and exclude the average value of property transfer, [b]Based on Cohen et al. [2004]; Miller et al. [1996] (inclusive of the average value of property transfer). Both sets of estimates are inclusive of criminal justice costs originally taken from Donohue III [2009] but re-adjusted to eliminate arrest rate scaling. Both sets of estimates are inclusive of productivity losses using the author's estimates from Table 11. Figures reflect 5 percent discount rate.

the likelihood that a defendant is convicted and potentially incarcerated, which has implications for defendant productivity. Donohue III [2009] incorporates both of these features into his work, but relies on untested assumptions in their construction. I substitute my own IV estimates of the impacts to defendant productivity while incarcerated, which results in effects that are roughly half the size of what Donohue III [2009] proposes. I do, however, rely on his estimates regarding costs to the criminal justice system.[25] For the sake of simplicity, I ignore the recursive aspects of this calculation: being incarcerated increases the likelihood of criminal charges, which then increases the likelihood of additional incarceration, which further increases the likelihood of more criminal charges and so on. Leaving this component out of the calculation underestimates the true social costs but is of second-order importance. The final set of cost estimates are displayed in Table 15.

I complement the instrumental variable parameter estimates from Section 7 with detailed crime-specific estimates which can be found in Appendix D. These are used to determine the exact changes in behavior associated with each type of criminal activity listed in Table 15.[26] These costs

[25]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.

[26]Not all types of crimes are priced, and therefore are not estimated. This is equivalent to assuming that the social costs of unpriced crimes is zero. The vast majority of felony crimes are covered and the major types of misdemeanor crimes missing are traffic violations, public disturbance or disorderly conduct and fraud.

and savings 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.[27]

To evaluate whether savings or costs dominate in this exercise, I compute four estimates: 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 as not measured. This exercise is repeated for three candidate sentence lengths: 6 months in prison, 1 year in prison and 2 years in prison.[28] 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 (1.75 percent on a quarterly basis). The exercise is formalized in the following equations:

$$
\begin{aligned}
\text{Incapacitation} &= \sum_{q=1}^{Q}\left[(0.9825)^{q-1} \times \left(\sum_{\mathcal{C}}(cost_{\mathcal{C}} \times \hat{\delta}_1^{\mathcal{C}})\right)\right], \\
\text{Institutional Costs} &= \sum_{q=1}^{Q}\left[(0.9825)^{q-1} \times \left(\frac{\$21,390}{4}\right)\right], \\
\text{Post-Release Criminality} &= \sum_{q=1}^{20}\left[(0.9825)^{Q+q-1} \times \left(\sum_{\mathcal{C}} cost_{\mathcal{C}} \times \left(\hat{\delta}_2^{\mathcal{C}} + \hat{\delta}_3^{\mathcal{C}} \times \frac{Q}{4}\right)\right)\right], \\
\text{Economic Impacts} &= \sum_{q=1}^{Q}\left[(0.9825)^{q-1} \times \left(-\hat{\delta}_1^{Wage} + \hat{\delta}_1^{FS} + \hat{\delta}_1^{TANF}\right)\right] \\
&\quad + \sum_{q=1}^{20}\left[(0.9825)^{Q+q-1} \times \left(-\hat{\delta}_2^{Wage} + \hat{\delta}_2^{FS} + \hat{\delta}_2^{TANF}\right)\right] \\
&\quad + \sum_{q=1}^{20}\left[(0.9825)^{Q+q-1} \times \left(-\hat{\delta}_3^{Wage} + \hat{\delta}_3^{FS} + \hat{\delta}_3^{TANF}\right) \times \frac{Q}{4}\right],
\end{aligned}
$$

where the parameters refer to the various estimated coefficients from Equation 6, Q represents the length of the prison term (measured in quarters), $\mathcal{C}$ is the set of crime types, and $cost_{\mathcal{C}}$ refers to the lower or upper bound social costs of different crimes plus the criminal justice and productivity

[27] Owens [2009] uses an estimated marginal cost rather than the reported average cost of incarceration. She finds that the marginal cost is slightly over half of the average cost in Maryland where her study is located. If this also holds in my setting, the correctional costs and shares presented later in this section would tend to be overstated.

[28] This exercise could be replicated for longer prison terms like 5 years, 10 years or 20 years. The results of this exercise could be misleading as local average treatment effects could differ for more serious offenders, which this study is likely not identified off of. As such, these results are not reported.

TABLE 16. 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*** **	

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

costs added in. I compute the test statistics on the total measured change to evaluate if the estimates are significantly different from zero. This is accomplished using two stage least squares with seemingly unrelated regression employed in the second stage to allow from cross-equation correlation in the error terms. The results are presented in Table 16.

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 one year decreases welfare by roughly $56,000 to $67,000 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 depending on how costs are defined. The total measured change worsens with incarceration length, which should not be surprising given my results. The costs associated with a six-month prison term range between $37,000 to $45,000 and those of a two-year prison term range between $91,000 to $107,000.

To evaluate whether these measured costs can be justified based on a general deterrent effect alone, one can restate the cost benefit analysis in terms of the number of crimes that would need to be prevented in order for incarceration to be welfare neutral.[29] For instance, 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.[30] It is worth emphasizing that these are impacts that would need to be observed in the criminal behavior of the general population and not the defendant as his criminal behavior is already taken into account in this analysis. 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 to be welfare neutral. 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.

9. CONCLUSION

Criminal justice policy in the United States has grown increasingly reliant on incarceration in the past three decades. 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, should give pause to policy makers. Measured incapacitation rates in the context of this study are quite low. This might be a function of the fact that I mainly measure incapacitation using court charges instead of criminal acts, which has led to lower estimates in other settings. But, in spite of this caveat, I still find that incarceration led to increased criminality for inmates after re-entry, which calls into question whether incapacitation is actually achieving true cost savings.

A number of non-crime outcomes are also negatively impacted by incarceration further diminishing its appeal. Effects on employment, earnings, benefit take-up and family formation indicate

[29]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.

[30]A habitual drug user is defined as an individual who uses illicit drugs 200 or more times in a year.

that inmates face significant barriers to re-entry. Decreased economic self-sufficiency coincides with greater use of government safety net programs. Lower marriage rates and increased divorce rates indicate social isolation and raise important questions regarding potential spillovers to families and communities.

This study cannot provide any evidence regarding potential general deterrent effects of incarceration. However, based on the impacts to defendant outcomes alone, I estimate that incarceration generates significant 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.

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Appendices

A. The Texas Sentencing Guidelines

TABLE A.1. 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.
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.

B. Comparing estimated "impacts" of incarceration using average and interacted instrumental variables

TABLE B.1. 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	

Source: Harris County District Clerk's criminal court records (1980-2013), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice state prison records (1978-2013), Texas Workforce Commission unemployment insurance wage records (1994-2012), Texas Health and Human Service Commission's Food Stamps records (1994-2011).

Notes: Household members identified using Food Stamps case records prior to charges being filed. 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. *** p<0.01, ** p<0.05, * p<0.1.

C. Robustness exercises

Table C.1. Robustness Exercise 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 $\times$ 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

Source: Harris County District Clerk's criminal court records (1980-2013), Texas Department of Public Safety criminal conviction records (1980-2013), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice state prison records (1978-2013).
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. *** p<0.01, ** p<0.05, * p<0.1.

TABLE C.2. Robustness Exercise 2 - Impacts of incarceration while clustering at Court × 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 × 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

TABLE C.3. Robustness Exercise 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

TABLE C.4. Robustness Exercise 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 $\times$ 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

TABLE C.5. Robustness Exercise 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 $\times$ 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

TABLE C.6. Robustness Exercise 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

TABLE C.7. Robustness Exercise 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 × 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

D. Crime-specific estimates for Cost Benefit Exercise

Table D.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 $\times$ 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

Source: Harris County District Clerk's criminal court records (1980-2013), Harris County Sheriff's county jail records (1980-2013), Texas Department of Criminal Justice state prison records (1978-2013).
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. *** p<0.01, ** p<0.05, * p<0.1.