Punishment and (non-)Deterrence: Evidence on First-Time Drug Offenders from Regression Discontinuities

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Draft date: July 25, 2016

Abstract

Beginning in the 1970s with "The War on Drugs," the United States embarked on a sustained period of harsh, punitive sanctioning for drug offenses. Yet as local jurisdictions confront dire budget and overcrowding challenges, new proposals have emerged to roll back these policies and introduce more leniency into the criminal justice system. Despite the high stakes, little is known about the causal effect of these different approaches on future criminal behavior. In this paper, we exploit two natural experiments in Harris County, TX, in which felony drug defendants charged or convicted one day versus the next experienced abruptly different court outcomes. The first natural experiment follows a 1994 state-wide reform while the second follows a 2007 failed initiative to expand the county jail. In both instances, conviction rates abruptly changed by approximately 30 percentage points. Using administrative data and regression discontinuity methods, we find consistent, robust evidence that defendants who avoided a felony conviction exhibit substantially lower patterns of future offending over a 5 year follow-up period.

Keywords: criminal justice policy, drug offenders, recidivism

JEL classification codes: K14, K42

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Reducing the severity of punishment and the experience of incarceration for drug offenders has received significant attention in recent years following several decades of harsh sanctions during the "War on Drugs" in the United States.[2] As a result, felons account for nearly 10 percent of the U.S. adult population (and 33 percent of African American adult males) with the majority of their convictions due to the possession or distribution of illegal drugs.[3] At the federal level, the Fair Sentencing Act of 2010 (Public Law 111-220) and the Sentencing Reform & Corrections Act of 2015 (Senate Bill 2123) reduce criminal sanctions for certain types of drug offenders and include provisions to allow more judicial discretion when sentencing low-level drug offenders. This shift towards leniency is also observed at the state and local level with more than 30 states passing laws decreasing sanctions towards drug offenses since 2009 (Subramanian and Moreno 2014).

This recent shift is characterized by increases in options that allow defendants to avoid a criminal felony conviction by completing a probationary period without incident, such as a deferred prosecution or a deferred adjudication of guilt. In contrast to the establishment of specialized drug courts and treatment programs, these sanctions require fewer public resources since they typically rely on established community supervision programs (e.g. probation) and can be rapidly implemented.

Despite the increasing prevalence of deferral programs in the criminal justice system, little is known about their impact on reoffending. These options could potentially have large effects on a wide array of outcomes given a growing literature documenting strong negative effects of incarceration on criminal behavior, education, employment, and earnings (Mueller-Smith 2015, Aizer and Doyle 2015, Di Tella and Schargrodsky 2013)[4], as well as evidence that a felony conviction can negatively effect a wide range of outcomes (Raphael 2014, Lovenheim and Owens 2014, Finlay 2009, Pager 2008, 2003).[5]

To the best of our knowledge, this study provides the first quasi-experimental empirical evidence on the effect of felony convictions relative to deferred adjudications on recidivism for drug offenders in the U.S. We exploit two natural experiments in Harris County, Texas (TX) which dramatically altered the conviction rates of drug offenders. First, we follow a reform to the TX penal code in 1994 which decreased the use of deferred adjudication judgements for first-time felony drug offenders in favor of formal convictions with probation. The second experiment examines the fallout from an unexpected failure of a 2007 ballot initiative to expand local county jails that resulted in an immediate drop in conviction rates and a corresponding increase in deferred adjudications for first-time felony drug offenders.

What is particularly attractive about these policy shifts from a research perspective is that each was implemented quite rapidly such that defendants charged or disposed one day versus the

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[2]In 1971, President Richard Nixon labeled drug abuse "America's public enemy number one" and called for an "all-out offensive" (Subramanian and Moreno 2014).

[3]These estimates are based on a combination of current work by Christopher Uggen (available here http://paa2011.princeton.edu/papers/111687) and our calculations of the fraction of felony sentences which are drug-related from the State Court Processing Statistics (available here https://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/2038).

[4]Also relevant to our study, Green and Winik (2010) focuses on a sample of drug felony defendants and exploiting random assignment to judges differing in severity and do not find significant differences in recidivism among offenders incarcerated nor offenders sentenced to probationary supervision relative to those not receiving either of those sanctions.

[5]Interestingly, Uggen et al. (2014) find smaller effects on employment opportunities of low-level arrest records using a similar audit design as Pager (2003) suggesting that employers would respond to a difference between an arrest and a felony conviction when evaluating applicants.

next experienced distinctly different court verdicts and sanctions but do not differ across any observable characteristics. Additionally, through studying two experiments that together exhibit both increasing and decreasing harshness towards drug offenders, we can be more confident that our estimates are not simply capturing other unobserved changes contemporaneous with the sanction discontinuities.

Because of the immediate nature of these changes, we use a regression discontinuity (RD) research design to demonstrate that conviction rates and criminal sanctions change sharply across the thresholds and present reduced form evidence on how these changes affected future criminal justice outcomes. Our results are documented both graphically as well as through formal statistical tests. The common finding across these two quasi-experiments is that court leniency substantially decreases recidivism for drug offenders over a five year follow-up period.

We do not find evidence of any significant discontinuities in observable demographic characteristics, prior criminal histories, or the density of the criminal caseload which supports our argument that these two natural experiments present valid contexts for the use of regression discontinuity analysis. Several robustness exercises indicate that our results do not rely on any specific implementation design or functional form.

We contribute to a large literature investigating the impact of criminal sanctions on reoffending and other outcomes. Our results are consistent with recent evidence suggesting that punitive sanctions and criminal records may have a scarring effect on individuals (Mueller-Smith 2015, Aizer and Doyle 2015, Di Tella and Schargrodsky 2013, Shapiro and Chen 2007). This could operate directly through transmission of criminal capital amongst peers while incarcerated (Bayer et al. 2009, Ouss 2013) or penalties in the labor market (Raphael 2014, Finlay 2009, Pager 2008, 2003). Our results are also consistent with a scarring effect in the criminal justice system since those with and without a formal criminal conviction record may be treated differently on an ongoing basis by police, prosecutors, and judges. For instance, certain types of prior convictions can trigger aggravated (elevated) charges upon reoffense which results in more severe and potentially more certain convictions. Additionally, defendants under varying forms of supervision may face differing probabilities of arrest conditional on criminal activity.[6]

Our findings suggest that the increase in reoffending due to these scarring mechanisms outweigh any decrease through specific deterrence or incapacitation mechanisms. This result is in contrast to Hansen (2015) who finds that stricter penalties applied to drunk drivers reduced reoffending. It also conflicts with larger estimates from the incapacitation literature (Mueller-Smith 2015, Buonanno and Raphael 2013, Johnson and Raphael 2012, Owens 2009, Levitt 1996). Kuziemko and Levitt (2004) specifically evaluate the dramatic increase in incarceration rates for drug-related offenses in the U.S. during the 1980s and 1990s and find that the shift in policy was associated with a small aggregate decrease in property and violent crimes through an incapacitation effect.

It is interesting to note the lack of response in either the caseload trends or levels of criminal activity despite the large shift in the severity of punishment. While several studies find reductions in offending associated with increases in the severity of punishment (Helland and Tabarrok 2007, Drago et al. 2009, Abrams 2012), the trends in our setting are more consistent with evidence that general deterrence plays a minor, if any, role in determining future criminal behavior (Lee and McCrary 2009, McCrary and Sanga 2012).

The remainder of the paper is structured as follows: Section 1 describes the two sharp changes in criminal sanctions for drug offenders in Harris County, TX; Section 2 describes the admin-

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[6]We are not aware of any empirical evidence that confirms this to be the case.

istrative data sets used; Section 3 outlines our regression discontinuity empirical strategy; Section 4 presents and discusses our results and provides evidence supporting our identification assumptions; and, Section 5 includes concluding remarks.

1   History and Background on Two Natural Experiments Impacting Felony Drug Offenders in Harris County

1.1   1994 Penal Code Reform

In 1993 the Texas Legislature enacted its most sweeping sentencing reform in the history of the state in response to major overcrowding in its prisons and county jails.[7,8] The legislation was written such that the new sentencing regime only applied to defendants who had committed their offenses on or after September 1, 1994.[9,10,11] Prosecutors had little ability to manipulate defendants across this threshold since they were required to file charges within 48 hours of arrest and had to use the newly adapted charge codes.[12]

While the new legislation was intended to relieve the burden of overcrowding, it had the unanticipated consequence of dramatically increasing conviction rates and discouraging the use of deferred adjudication of guilt.[13] The rate of first time drug offenders receiving a deferred adjudication verdict plummeted from over 50 percent to less than 20 percent after the new policies went into effect (Figure 4).

The main determinant of this drop in use was the inability to use the threat of incarceration as leverage to enforce the terms of deferred adjudications. The new sentencing regime required probation before incarceration for most first-time felony drug offenders which meant that a second round of probation was required for a violation of the terms of a deferred adjudication prior to any incarceration spell. This precise issue was raised in October 1993 during a simulated plea bargaining exercise between prosecutors and defense attorneys, yet no action was taken to amend the statutory language before the changes were implemented (Fabelo 1997).

The majority of the would-be deferred adjudication cases were replaced with a guilty verdict for the newly created state jail felony. This new verdict involved a probated incarceration

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[7]Two pieces of legislation accomplished this overhaul: Senate Bill 1067 reclassified most non-violent felony crimes as "state jail felonies," a newly created offense level below a 3$^{\text{rd}}$ degree felony; and, Senate Bill 532 created "state jails," correctional institutions set up specifically for individuals who had been convicted of state jail felonies.

[8]Individuals convicted of a state jail felony would still be considered as having a felony record by the state but would be subject to different sentencing guidelines. These new guidelines limited the maximum incarceration sentence to two years and required a probated (conditional) incarceration sentence for defendants without prior state jail felony convictions.

[9]In practice, it appears that the courts used the charging date rather than the offending date as the key variable for determining which code applied.

[10]Offenses occurring prior to September 1, 1994, but not disposed until after this cutoff date were not grandfathered into the new policy regime.

[11]Additional provisions in the penal reform specific to only violent offenders (which are beyond the scope of this study) went into effect a year earlier on September 1, 1993.

[12]In our empirical estimation, we drop individuals charged on August 31, 1994 and September 1, 1994 to avoid any issues with sorting within the 48 hour threshold.

[13]Also known as "probation before judgment," "deferred disposition," or "deferred sentence" depending on the local jurisdiction.

sentence which explains both the change in sentenced incarceration and the lack of any change in probation in Figure 5 since the new verdict required both. We also observe a significant reduction in the probability of a drug rehabilitation sentence in Figure 5, but this option is not commonly imposed before or after the reform.

1.2 The 2007 Failed Jail Expansion Ballot Initiative.

Overcrowding in prisons and jails remained an important concern across Texas during the 2000s, especially in the Harris County Jail. This local jail—which houses inmates with shorter sentences and serves several other functions including pre-trial detention and holding for local inmates waiting to be transferred to the state prison system—had up to 1,900 inmates sleeping on mattresses on the floor by 2005.[14] To address overcrowding, the county sought to expand the jail capacity by 2,500 beds with $195 million raised through county bonds for a new jail.[15]

An unexpectedly large voter turnout on November 7, 2007 and a local campaign against the jail expansion led to a narrow defeat of the initiative by a vote of 50.6 to 49.4 percent. This outcome was particularly surprising given that all of the other local bonds were approved, and a $1 billion state-wide bond to expand state prison capacity was approved (58.2 to 41.8).

The local campaign against the jail expansion proposition suggested that the intended location of the new jail would be bad for local economic development and that existing infrastructure could be more efficiently used with less reliance on pretrial detention. Some commentators explicitly placed the responsibility of the overcrowding problem on the courts in Harris County, suggesting that they depended too heavily on incarceration at the cost of taxpayer funds.[16]

Most Harris County criminal courts responded immediately after the election. In the months following, guilty verdicts dropped from around 65 percent to 40 percent with most of these cases shifted into deferred adjudication of guilt (Figure 4). We also observe a significant increase in the fraction of charges dismissed (Figure 4). With the drop in guilty rates, incarceration rates decreased and were replaced by community-supervised probation (Figure 5).

2 Data Sources and Sample Restrictions

In order to empirically evaluate the effect of these large shifts in sanctions, this project utilizes several sources of administrative data including: criminal court records from the Harris County District Clerk, jail booking and spell data from Harris County Sheriff's Department, and state incarceration data from the Texas Department of Criminal Justice.[17]

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[14]http://www.chron.com/news/houston-texas/article/Revised-numbers-show-jail-crowding-is-worse-1525007.php

[15]The proposed jail expansion (Proposition 3) was part of a broader bond package being put to local voters in 2007 in response to the county's fast growing population. Together Harris County and the Port of Houston Authority added 6 local bond propositions to the November 6, 2007 election ballot at combined total of $880 million in potential bonds. The projects included upgrading roads and parks, expanding capacity at the port, building a new forensic lab and constructing a new family law center.

[16]See the following articles for discussions at the time of the election: http://www.chron.com/news/houston-texas/article/Picnickers-may-share-Buffalo-Bayou-with-inmates-1535996.php, http://gritsforbreakfast.blogspot.com/2007/10/texans-taxation-revulsion-vs-their.html, and http://gritsforbreakfast.blogspot.com/2007/11/kuff-new-jail-building-in-harris-county.html

[17]We also use information from the Computerized Criminal History Database, provided by the Texas Department of Public Safety, which tracks state-wide convictions in Texas from the mid-1970s up to the present to support

The criminal court record database contains felony and misdemeanor charges and court outcomes for all adults between 1980 and 2013 regardless of the final verdict.[18] The Harris County Jail booking data provides an opportunity to observe arrests that did not progress to court charges as well as any time spent in the jail between 1980 through 2013. We link these two Harris County data sets using a unique county identifier tied to an individual's fingerprint known as the SPN. We match the state-level data capturing all state prison or state jail incarceration spells between 1978 and 2013 to the county records using a defendant's full name and date of birth.[19]

We impose several sample restrictions in order to isolate the effect of the two natural experiments. For the 1994 sample, we require that: (1) charges were filed between September 1, 1993 and September 1, 1995;[20] (2) defendants had no prior felony charges in Harris County regardless of conviction status; and, (3) defendants were charged with felony drug crimes involving a Penalty Group 1 controlled substance.[21]

For the 2007 sample, we require that: (1) charges were disposed between November 7, 2006 and November 7, 2008;[22] (2) defendants had no prior felony charges in Harris County regardless of conviction status; (3) defendants were charged with a state jail felony drug offense; and, (4) defendants cases were sentenced in one of thirteen district courts that reacted to the election results the day following the election.[23,24] The remaining courts did not change their sentencing practice discontinuously in response to the failed jail election. These courts fall into three categories: one court increased their use of deferred adjudication in anticipation prior to the election;[25] some took a "wait and see" approach through only adjusting their sentencing practices until several weeks after the election;[26] and a few others simply did not change their sentencing practices.[27] We apply our regression discontinuity models to defendants from these excluded courts as a placebo exercise (Table 9).

As depicted in Figures 2 and 3, the average defendant in each sample is approximately 30 years old with a slight upwards trend for the 2007 sample. Both samples are predominately male (over 75%) with around 30% White defendants with similar rates of prior misdemeanor convictions. Half of the defendants are African American in the 1994 sample which declines to around one-third in the 2007 sample with a corresponding increase in the fraction Hispanic.

We limit our analysis to defendants without prior felony charges for two distinct reasons. First,

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our findings using county court records and to check for potential biases caused by offenders moving out of Harris County as a result of the court verdict.

[18] Cases sealed to the public by order of the court, which account for less than half of a percentage point of the overall caseload, and criminal appeals were not included in the data.

[19] In 1994 (2007), we match 84.8% (88.3%) to a valid incarceration spell.

[20] We exclude cases charged between August 31, 1994 and September 1, 1994 since the charging date could theoretically be sorted among those arrested in 48 hours prior to the new regime being implemented.

[21] Penalty Group 1 controlled substances are predominantly cocaine (both crack and non-crack), heroine, methamphetamine, ketamine, oxycodone and hydrocodone. A full listing can be found in the Texas Health and Safety Code (http://www.statutes.legis.state.tx.us/Docs/HS/htm/HS.481.htm).

[22] We exclude cases disposed between November 6-8, 2007 since the exact day that each court changed its behavior varied slightly among the set.

[23] These are district courts 174, 177, 180, 184, 185, 208, 230, 232, 248, 263, 337, 338, and 351.

[24] Defendants in Harris County are randomly assigned to district courts ensuring that the subset of defendants sentenced by these courts are representative of the defendant population as a whole.

[25] Court 209.

[26] Courts 178, 182, and 183.

[27] Courts 179, 228, 262, and 339 were already high users of deferred adjudication even prior to the election. Court 176 rarely ever used the alternative sentencing strategy either before or after the election.

we are concerned that if the policy changes we examine impact criminal recidivism then our study sample may be contaminated by endogenous entry that could differentially affect one side of the cutoff versus the other. Through imposing this restriction, we ensure that each defendant will only appear once in our estimation sample. Second, individuals without a felony record are particularly relevant in this setting since avoiding a criminal conviction (through a deferred adjudication of guilt or dismissed charge) will preserve their clean felony record.[28]

3 Empirical Strategy

We estimate the reduced form effect of the 1994 and 2007 changes in criminal sanctions for drug felony defendants using a regression discontinuity (RD) design. We present both graphical evidence as well as statistical tests to confirm the reliability of our results. For our statistical tests, we follow the approach of Calonico et al. (2014) to obtain bias-corrected point estimates using local linear functions, optimal bandwidths and valid confidence intervals. We opt for a bandwidth selector that selects the median bandwidth from three mean squared error-optimal methods for the RD treatment effect estimator.[29] Our primary specification also adjusts for baseline covariates of age, gender, race/ethnicity, and prior number of misdemeanor convictions.

We first measure the effect of the 1994 and 2007 changes on conviction status and type of sanction imposed to demonstrate our "first stage"—a discontinuous relationship between the running variable and these court outcomes and judgements. We then measure the reduced form effect of the transition between harsh and lenient regimes on future offending behavior as captured through the data sources described in Section 2. Our running (or forcing) variable differs across the two quasi-experiments due to the nature of each change: the 1994 Penal Code Reform changes applied to offenders based on the date the charge was filed; the 2007 change in judicial behavior was effective for offenders whose case had not been disposed prior to November 7, 2007.

To attribute a causal interpretation to our RD estimates, we must assume that defendants are effectively randomly allocated before and after the two thresholds. For the first estimation sample, individuals charged immediately after September 1, 1994 should be observationally equivalent to those charged before and we should not see a discontinuity in the total number of cases. For the second sample, defendants disposed immediately after November 6, 2007 should be observationally equivalent to those disposed before and we should not see a discontinuity in the total number of dispositions.[30]

Causal identification also requires that there is no discontinuous change in the likelihood of committing crimes in response to the change in sanction severity. If there exists a general deterrence effect, we expect to see trends, and potentially a discontinuity, in the levels of criminal activity within our sample windows. Additional threats to our empirical strategy include changes in policing practices or sorting of offenders by prosecutors/judges across thresholds in order to guarantee they face one punishment regime versus the other. This is

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[28]It is possible, however, that defendants have been charged with and convicted of misdemeanor level charges.

[29]We use the option *msecomb2* within the STATA *rdrobust* command described by Calonico et al. (2016) which uses the median bandwidth from the following methods: one common MSE-optimal bandwidth selector for the RD treatment effect estimator; two different bandwidth selectors (below and above); and one common MSE-optimal bandwidth selector for the sum of regression estimates. We present estimates using alternative bandwidth selectors in Tables 10 and 11.

[30]Dispositions include case dismissals, guilty verdicts and deferred adjudications of guilt.

particularly concerning in the context of the 1994 reform when all relevant actors could fully anticipate the adoption of the new penal code. To the extent that there was endogenous sorting across the thresholds, we expect to observe discontinuities in important predictors of future offending such as the prior misdemeanor records.

4 Results

4.1 Caseload Density and Baseline Characteristics

In support of causal identification, we do not observe discontinuities in caseload densities or in the majority of defendant characteristics (Figures 1, 2, and 3) and detect no statistical differences using our primary empirical methodology (Table 1). We do find marginally insignificant changes age in 1994 as well as sex in 2007. However, given our results demonstrate clear balance on the remaining demographic and prior criminal history characteristics and are robust to the inclusion or exclusion of controls, we do not feel that this apparent difference reflects any discontinuity in any unobserved determinants of recidivism.

4.2 Court Verdicts and Court-Imposed Sanctions

While the defendants appear observationally equivalent across the two thresholds, the court outcomes differ quite dramatically (Figure 4 and Table 2). In 1994, the share of cases receiving final convictions at disposition jumps from about 30 to 65 percent at the discontinuity, a gain representing more than a 100 percent increase over the pre-cutoff mean. At the same time, the rate of deferred adjudications drops by about 35 percentage points indicating a nearly one-for-one tradeoff.

The 2007 experiment documents the opposite phenomena; court decisions switch from harsh to lenient practices. In fact, we observe an almost mirrored reflection of the patterns observed in 1994. Convictions at disposition drop by 29 percentage points (compared to a 35 percentage point jump in 1994). Again, the majority of these marginal cases appear to switch to a deferred adjudication status (22 percentage points) although we do measure an increase in the number of case dismissals (7 percentage points) as well.

The differences in conviction status carry over to sentenced sanctions (Figure 5 and Table 2). In 1994, we observe a small increase in defendants being sentenced to incarceration and a drop in sentences involving drug rehabilitation programs.[31] In 2007, we observe a clear shift away from incarceration and towards probation in sentencing outcomes. At the discontinuity, we observe a drop of 28 percentage points in incarceration and a gain of 22 percentage points in probation. We also see a marginally insignificant increase in the reliance on drug treatment programs. The net decline in sanctions (i.e., incarceration or probation) reflects the higher dismissal rate observed during the post-period in 2007.

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[31]This increase in sentenced incarceration though was subject to the mandated community supervised release requirement in the new penal code and so does not necessarily represent higher rates of actual experience in jail or prison.

4.3 Reoffending Outcomes

To assess the impact of these policy shifts on future criminal justice outcomes, we present evidence for four separate measures of future offending over a 5 year period following the focal charge: total bookings in the Harris County Jail, total charges in the Harris County Criminal Courts, total convictions in the Harris County Criminal Courts, and total days incarcerated in the Harris County Jail or a state prison.[32] Overall, we find strong evidence that defendants who were more likely to receive a felony conviction in each experiment were also more likely to reoffend during the followup period (Figure 6 and Table 3).

We estimate higher rates of reoffending for the post-1994 reform group who who had significantly higher conviction rates. We find that the new regime generated 0.78 additional charges per offender representing a 78 percent increase over the pre-reform average of one total charge in the 5 year followup period. The magnitude of estimates on the other measures imply a 28 percent increase in jail bookings and an 86 percent increase in Harris County convictions. These effects are all very large in magnitude, are supported by visual evidence in Figure 6.

We estimate lower rates of reoffending for the post-2007 election group who who had significantly lower conviction rates. Among the offenders disposed during the more lenient period, we find point estimates that are close to what is observed in 1994 and internally consistent across the different measures. We measure a decline of 0.73 jail bookings, 0.89 criminal charges filed, 0.67 convictions, and 80 days incarcerated. These estimates correspond to a 45 percent decrease in bookings, 65 percent decrease in county charges, 58 percent decrease in convictions, and a 41 percent decrease in total days incarcerated. These effects are precisely estimated and are supported by visual evidence in Figure 6.

The pattern of results in both 1994 and 2007 are suggestive of substantial changes in offender behavior as a result of the change in court verdicts and court-imposed sanctions for first-time drug felony defendants. The consistency of our estimates across these two natural experiments is striking given the two changes are 13 years apart.

The timing of the realization of these effects is traced out in Figure 7.[33] Both changes appear to have a building effect on offending behavior during the initial followup period. The largest impacts are generally observed in both samples during the third year post-charge, although the direction of the impact remains persistent across the remainder of the 5 year post-charge period.

Differences in the incarceration status of offenders in our sample are both a function of the original court decision as well as a function of the offender's future behavior.[34] We only find a marginally insignificant difference in incarceration outcomes for the 1994 sample during the fourth year following the initial charge. This suggests that differences in offending are not driven by an incapacitation mechanism. For the 2007 sample, our estimated difference in total days incarcerated in Table 3 appears to be driven by differences within the first two years following the initial charge. While this difference can be partially attributed to the decrease

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[32]Days incarcerated will capture both the differential incarceration rates from the focal charge as well as new incarceration sentences resulting from future criminal activity. Later in this section, we examine the timing of the incarceration affects to assess the extent to which this total measure represents more of the former or latter mechanism.

[33]These figures plot out estimates using our primary RD specification described in Section 3 for specific time windows. The first estimate reflects pre-charge effects covering 5 years prior to the focal charge and the following estimates reflect time intervals of 0-1 year, 1-2 years, 2-3 years, 3-4 years and 4-5 years post-charge.

[34]We cannot tie a given incarceration spell observed to a specific conviction due to data limitations.

in offenders sentenced to incarceration, the average sentence was only 34 days. The sentence for the focal offense could explain the 20 day difference in incarceration during the first year, but would not explain the observed differences in later years depicted in Figure 7. Moreover, estimated differences in jail bookings, county charges, and state convictions 3 years following the initial date of disposal indicate differences in offending that do not coincide with the timing of potential incapacitation.

To further explore these criminal outcomes, we estimate the impact on binary measures of future criminal during the 5 year followup period (Table 4). These include any jail bookings, any county charges, any county convictions and ever being incarcerated. We find clear evidence that avoiding a felony conviction decreases the likelihood of ever receiving a future conviction (0.15 percentage points in both 1994 and 2007). The other coefficients in the table point in the same direction as the conviction results, but exhibit smaller magnitudes and generally lack statistical significance. The 2007 sample exhibits a particularly interesting pattern: the coefficients increase in magnitude when moving from booking to charges and from charges to convictions. This suggests that criminal convictions may have an impact on the likelihood of escalation from one stage to the next in the criminal justice system independent of any impacts to the underlying illegal activity itself.

To assess differential impacts across different types of reoffending outcomes and different types of offenders, we estimate effects for outcomes by crime type in Figure 8 and Table 5 and by offender type in Table 6. In both 1994 and 2007, we see evidence that future drug possession crimes account for a significant share of the overall effect on recidivism. Interestingly, we also observe significant coefficients with non-trivial magnitudes on property crimes. This suggests conviction status may be impacting self sufficiency. The examination of discontinuities in labor market outcomes for defendants across these two discontinuities will allow for an important investigation of the impact of the changes on economic stability.

In Table 6, we separately estimate effects for different types of offenders. In 1994, we find similar effects by gender, but find much larger effects among younger defendants. While in 2007, our estimates imply larger effects among males and older offenders.[35]

While our primary estimates focus on outcomes from the Harris County court records, we also test whether our results are robust to convictions recorded in the state-wide Computerized Criminal History (CCH) Database in Table 7. While we expect smaller magnitudes since not all crimes are reported to the state-wide database, the patterns of results remain consistent with those based on the county records. We observe an increase of 0.33 convictions in 1994 and a decrease of 0.46 convictions in 2007, representing a 64 percent growth and a 49 percent contraction respectively. The changes observed at the state-level are almost entirely driven by difference in convictions reported to the state by Harris County arresting agencies, limiting our concerns about intra-state mobility much less inter-state mobility, which we cannot formally test.

We present estimates from a fuzzy regression discontinuity design where the first stage outcome is an indicator for a guilty verdict (felony conviction) in Table 8. While we prefer to focus on reduced form estimates previously discussed due to difficulties satisfying the exclusion restriction,[36] these estimates provide a sense of the very large magnitude of our estimated

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[35]There is no strong first stage relationship for women in the 2007 sample which explains the lack of precision and the "wrong signed" result observed in the second panel of Table 6. As such, we cannot rule out that women in this sample are unaffected by conviction status in their future criminal behavior.

[36]The reform may affect future behavior through channels other than effects on receiving a felony conviction

treatment effects on criminal justice outcomes. Overall, estimates imply substantial effects (typically 150 to 200 percent changes) of the reforms on future criminal offending.

4.4 Robustness Checks

We conduct several robustness checks to confirm the reliability of our results. Table 9 show the estimated effects of a placebo experiment where we shift the cutoff dates to one year earlier (September 1, 1994 and November 6, 2006) and one year later (September 1, 1996 and November 6, 2008). We also estimate a placebo effect for non-responsive courts (as described in Section 2) from the 2007 experiment. The goal in this exercise is to investigate whether our findings could be picking up latent seasonal breaks.[37] Overall, we do not find any meaningful discontinuities in court outcomes or in future criminal activity in this placebo exercise. Only 2006 shows a statistically significant difference in the rate of deferred adjudications and it is much smaller than our estimated magnitudes.

We also provide a number of other robustness checks including: alternative bandwidth selection methods (Table 10 and Table 11); alternative variance estimation strategies (Table 12; models without covariates included (Table 13); models without the bias correction or robust standard errors as suggested by Calonico et al. (2014) (Table 14); and models based on outcomes aggregated to the week level (Table 15). Among these tables, there is no evidence to suggest our findings are driven by arbitrary implementation decisions.

5 Conclusion

We observe sharp discontinuities in future offending outcomes across two dramatic changes in the typical sanctions for first-time drug felony defendants in Harris County, TX. While these two changes occurred 13 years apart and have very different explanations, we find similar impacts on criminal sanctions a well as on recidivism outcomes. Through studying two natural experiments that together exhibit both increasing and decreasing conviction rates towards drug offenders in the same location, we can be more confident that our estimates represent a real behavioral impact and are not simply capturing latent trends in recidivism or other unobserved shocks contemporaneous with the experiments.

Through a set of data-driven regression discontinuity plots and statistical tests, we demonstrate that first-time drug felony defendants—who are statistically indistinguishable on observable characteristics—differ in rates of guilty convictions by approximately 30 percentage points in both scenarios. In the absence of a conviction, defendants were most likely to receive deferred adjudication of guilt in both 1994 and 2007, allowing them to avoid a permanent criminal record. Those who ended up on the more lenient side of the thresholds demonstrate consistently lower rates of reoffending—with estimated magnitudes ranging from roughly 30 to 80% decreases across various measures of future reoffending. These effects are largely driven by decreases in future drug possession outcomes, but we do find estimates suggesting effects on property crimes as well.

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due to other types of changes and adjustments to the treatment of drug defendants such as drug rehabilitation, probation, incarceration, or other unobserved changes in the treatment of offenders across the thresholds.

[37]For example, the beginning of the school year or the timing of elections may impact criminal behavior or unobserved sample characteristics.

Our conclusion that leniency decreases recidivism is in direct contrast with recent work by Hansen (2015). Different geographic locations (Washington versus Texas) as well as different types of offenders (drunk drivers versus drug offenders) likely explain the discrepancy in our results. Together, we believe these studies demonstrate the challenge and complexity of studying a diverse criminal offender population.

Substantial changes to laws and sanctions associated with illegal drug use have been made at the state and federal level over the past several years. The overarching trend is towards more leniency, especially for first time drug offenders. Our results suggest that these changes may lead to lower rates of reoffending (and higher rates of rehabilitation) over the coming years. Our results also suggest that reductions in reoffending may be achievable through modifying sanctions in ways that do not require significant investments or changes to the existing criminal justice infrastructure. Through the analysis of these two unique natural experiments, we find large decreases in future offending from providing first-time drug felony offenders an opportunity to avoid the potential damage inflicted by a felony criminal record and incarceration sentence.

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1994 Penal Code Reform   **2007 Failed Jail Expansion**
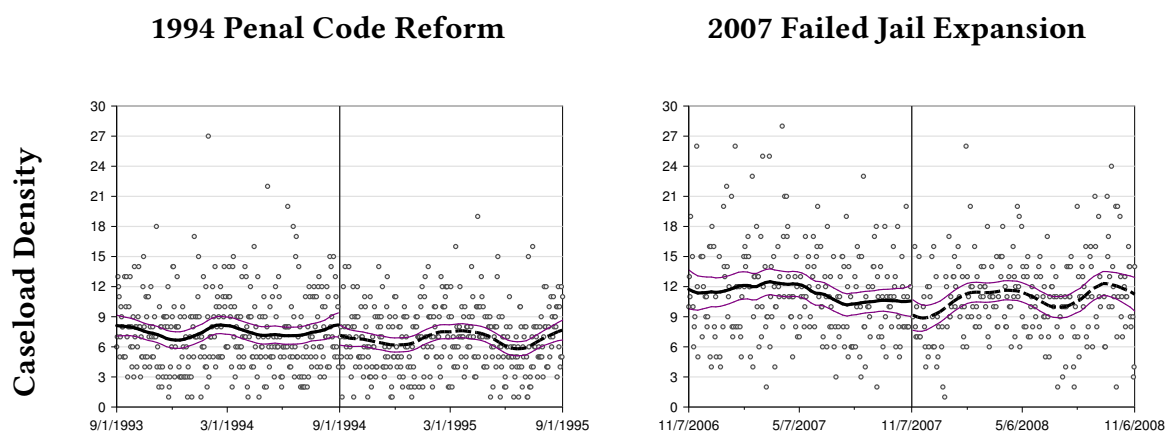
Figure 1: Discontinuities in caseload density

Note: This figure shows the relationship between the number of cases by date of filing (1994) or disposal (2007) and the focal date. The scatter plots depict the raw data relative to the running variable with kernel-weighted local-polynomial lines on either side of the cutoff. The scatter plots are pooled at the date level and the local-polynomial lines are based on the aggregated data at the exact date level.

1994 Penal Code Reform   **2007 Failed Jail Expansion**
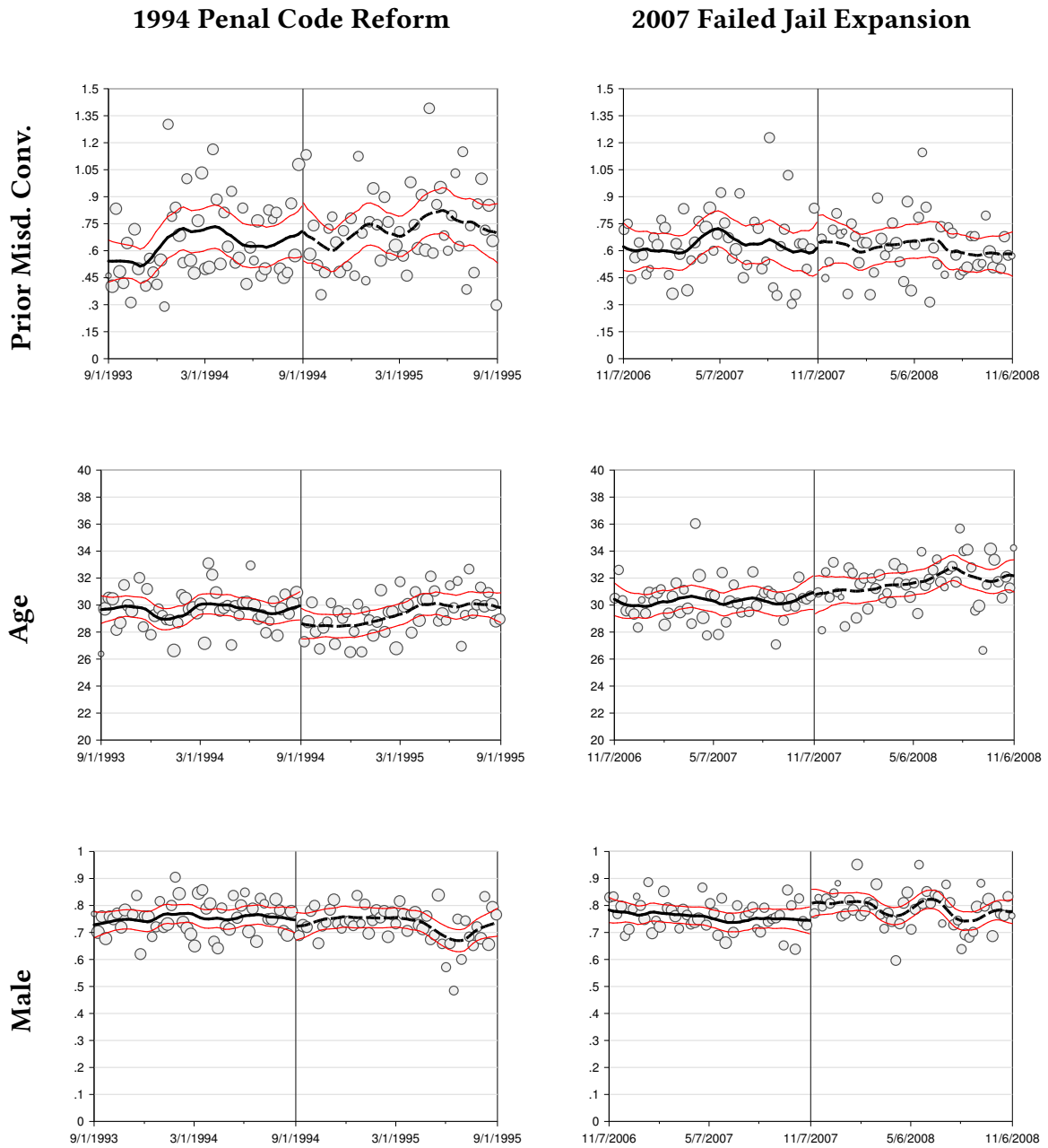
Figure 2: Discontinuities in baseline characteristics

Note: This figure shows the relationship between the focal date on baseline criminal history and demographic characteristics. The first row depicts the relationship between the total number of prior misdemeanor convictions and the running variable.

General graphing notes: All scatter plots depict the raw data relative to the running variable with kernel-weighted local-polynomial lines on either side of the cutoff. For readability, the scatter plots are pooled at the week level and weighted according to the total number of cases. However, the local-polynomial lines are based on the disaggregated data at the exact date level. Due to the nature of the changes, the date of filing is used as the running variable for the 1994 sample, while the date of disposition is used as the running variable for the 2007 sample. All plots and local-polynomial lines omit observations during the week of the cutoff.

1994 Penal Code Reform   **2007 Failed Jail Expansion**
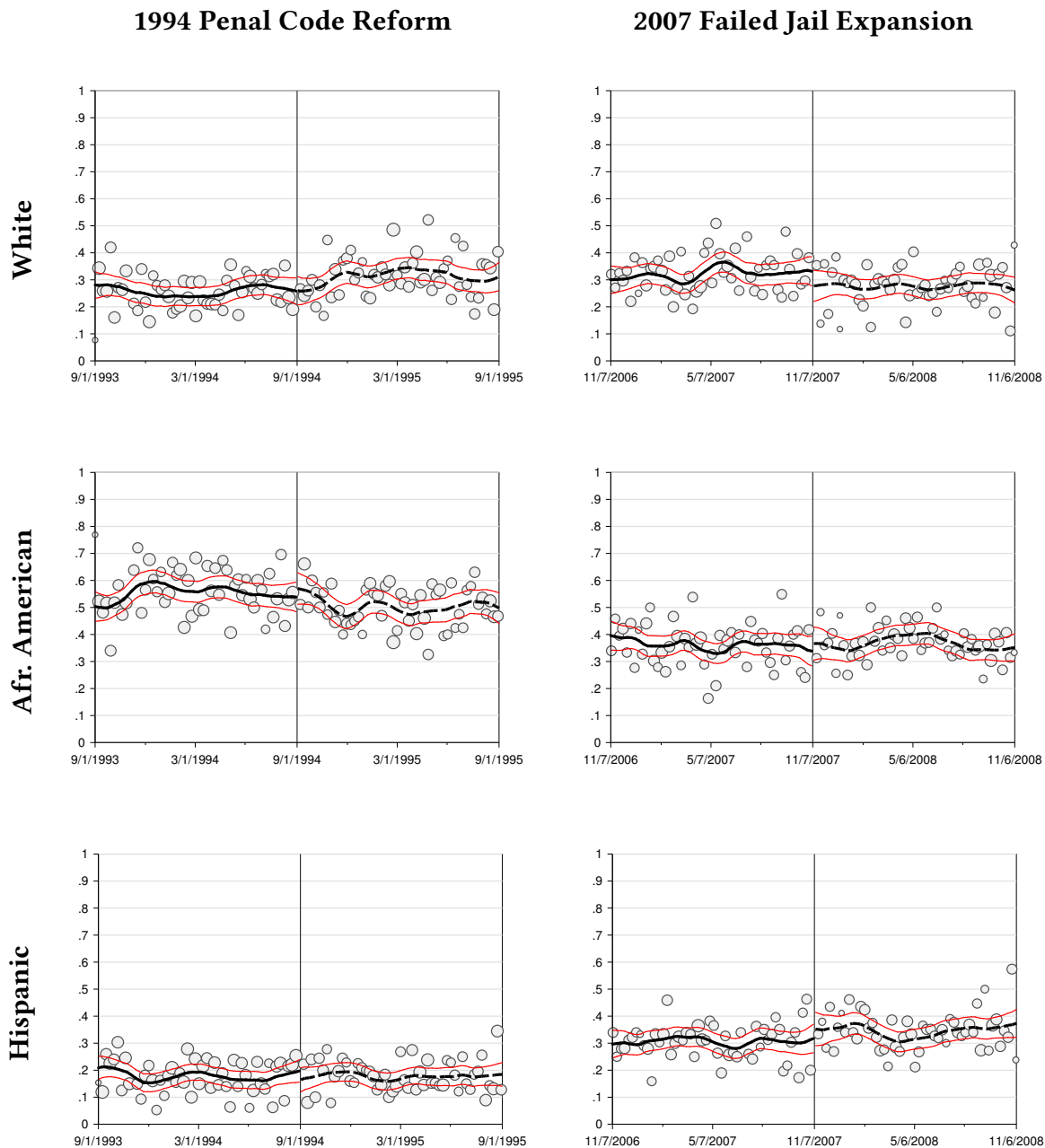
Figure 3: Discontinuities in baseline characteristics

Note: This figure shows the relationship between the focal date on indicators for race/ethnicity. General notes from Figure 2 apply.

1994 Penal Code Reform　　　　**2007 Failed Jail Expansion**
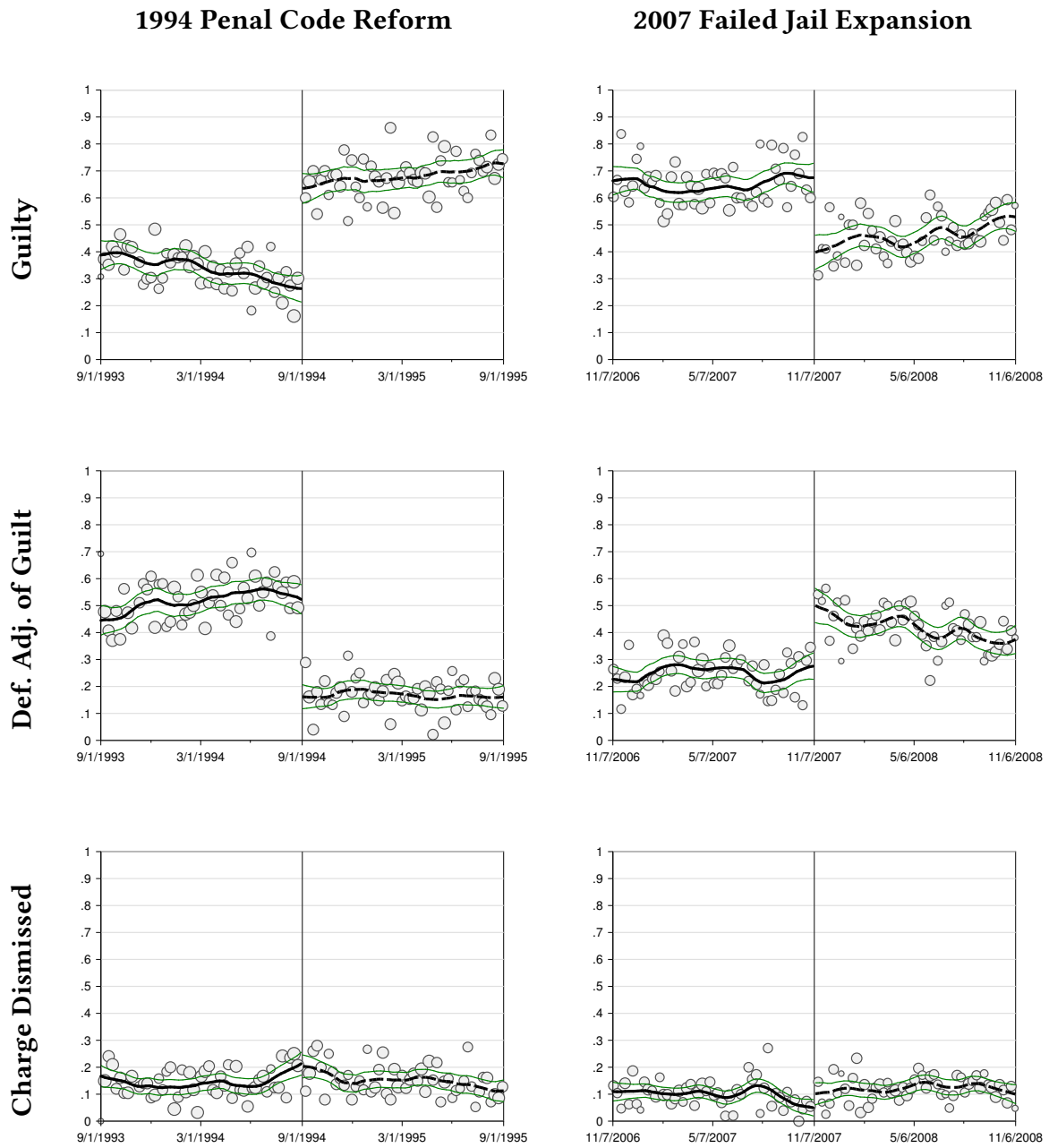
Figure 4: Discontinuities in court outcomes

Note: This figure shows the relationship between the focal date (date of court filing in left column; date of court disposition in right column) on court outcomes for criminal defendants charged with a drug felony. The first row shows the relationship between the focal date and a guilty verdict. In the case a criminal defendant does not receive a guilty verdict, he is either provided a deferred adjudication of guilt (second row) or the charge is dismissed (third row). A one year window on each side of the focal date thresholds is provided for all figures. Sample restrictions are based on the nature of the changes in 1994 and 2007 and are described in Section 1. General notes from Figure 2 apply.
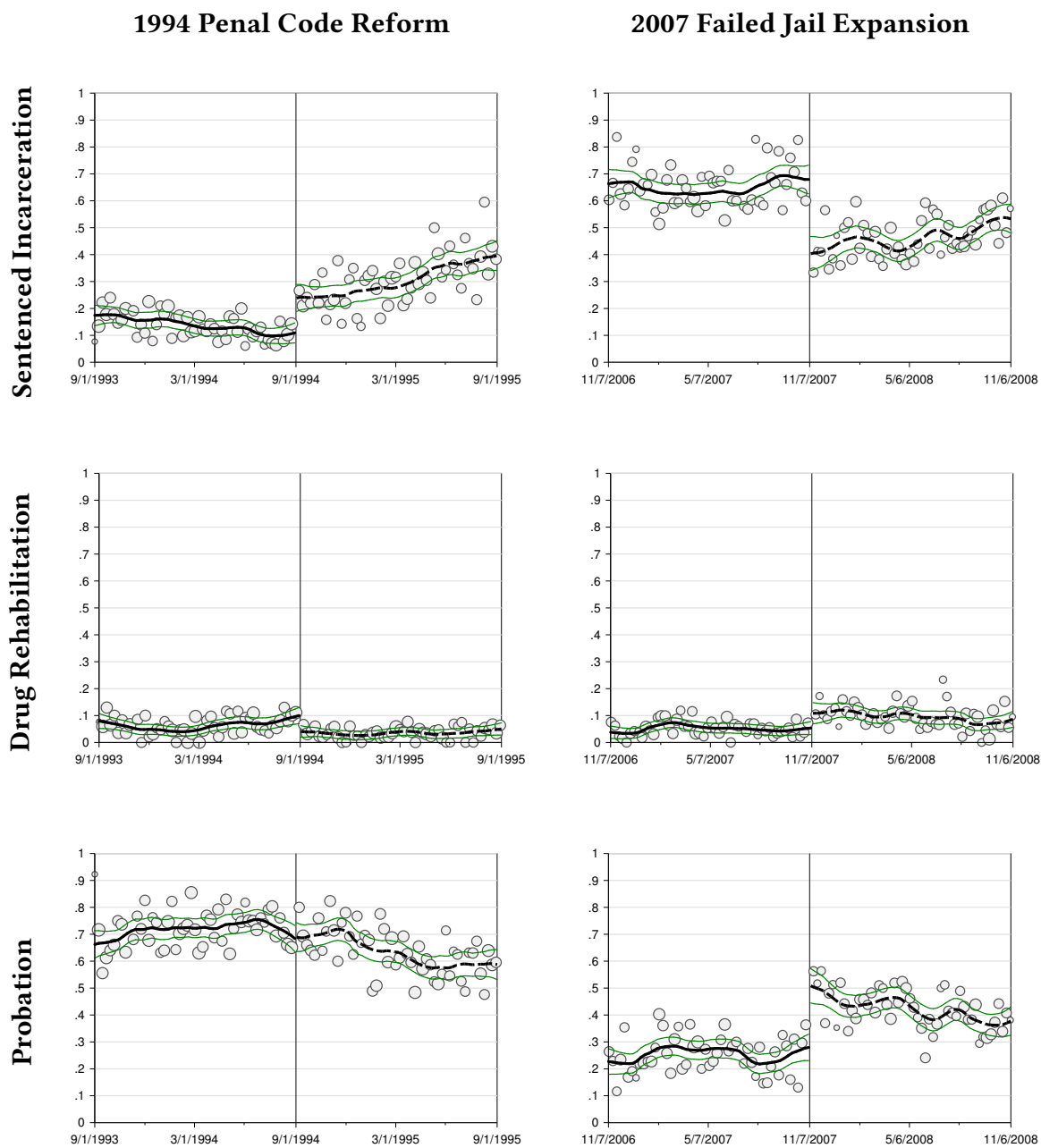
Figure 5: Discontinuities in status following court verdict

Note: This figure shows the relationship between the focal date on the immediate court imposed sanctions (or status following the court's decision) after considering the focal charge. The first row shows the relationship between the focal date and a direct (or immediate) incarceration sentence. The second row plots the proportion of cases assigned to a drug rehabilitation program. The third row depicts the relationship between the focal date and a required probation supervision. General notes from Figure 2 apply.
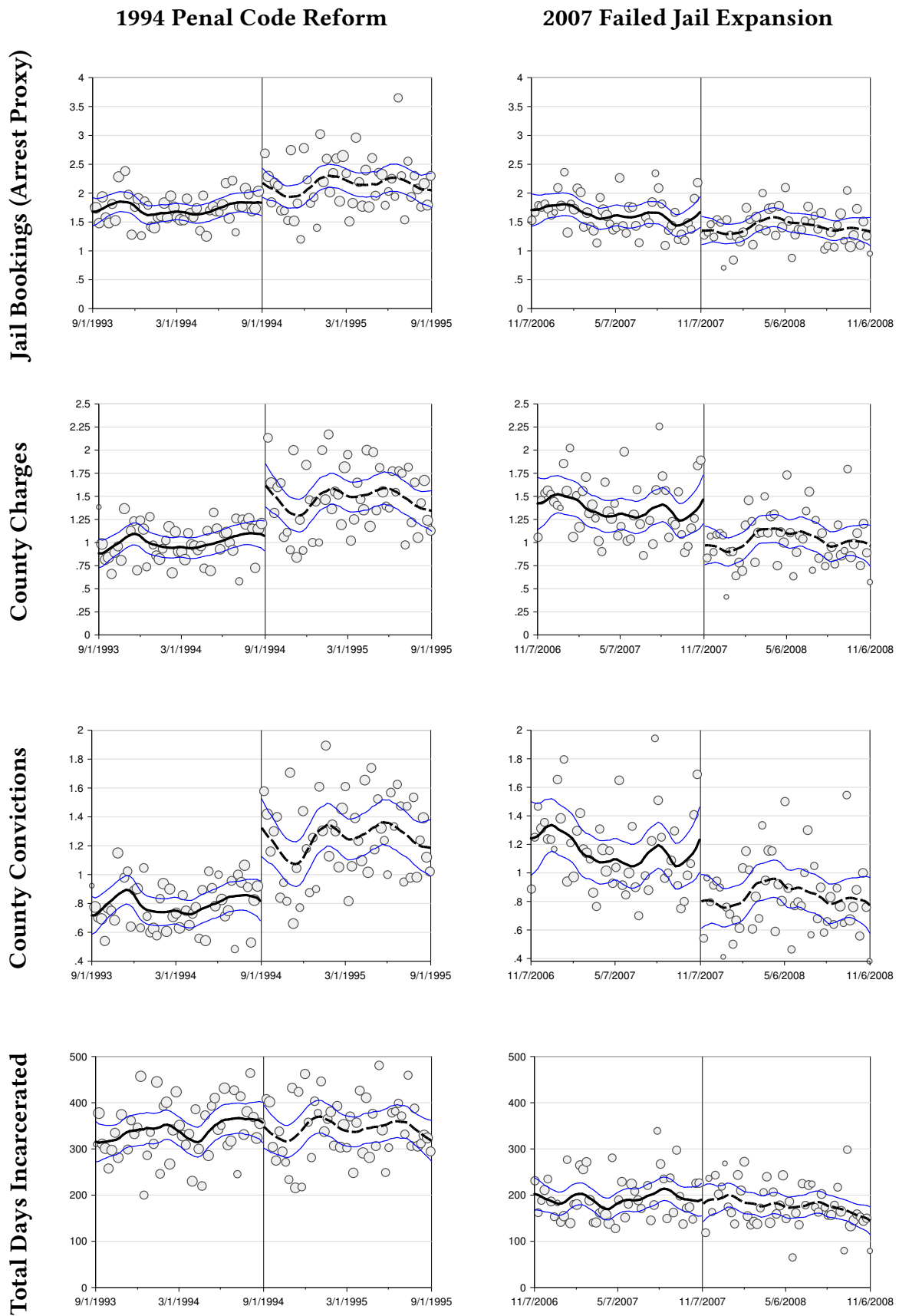
Figure 6: Five year criminal justice outcomes

Note: This figure shows the relationship between the focal date on reoffending outcomes. The first row shows the relationship between the focal date and the total number of jail bookings in Harris County for a five year follow-up period. The second row depicts the relationship between the focal date and the total number of charges filed with the Harris County criminal courts for a five year follow-up period. The third row plots the relationship between the focal date and the total number of state-wide convictions over a five year follow-up period. The fourth row depicts the relationship between the focal date and total time served in county or state confinement in the 5 years following the focal date. These figures reflect the total effect on incarceration including future recidivism since incarceration spells cannot be directly linked to court charges (described in Section 2). General notes from Figure 2 apply.

1994 Penal Code Reform   2007 Failed Jail Expansion
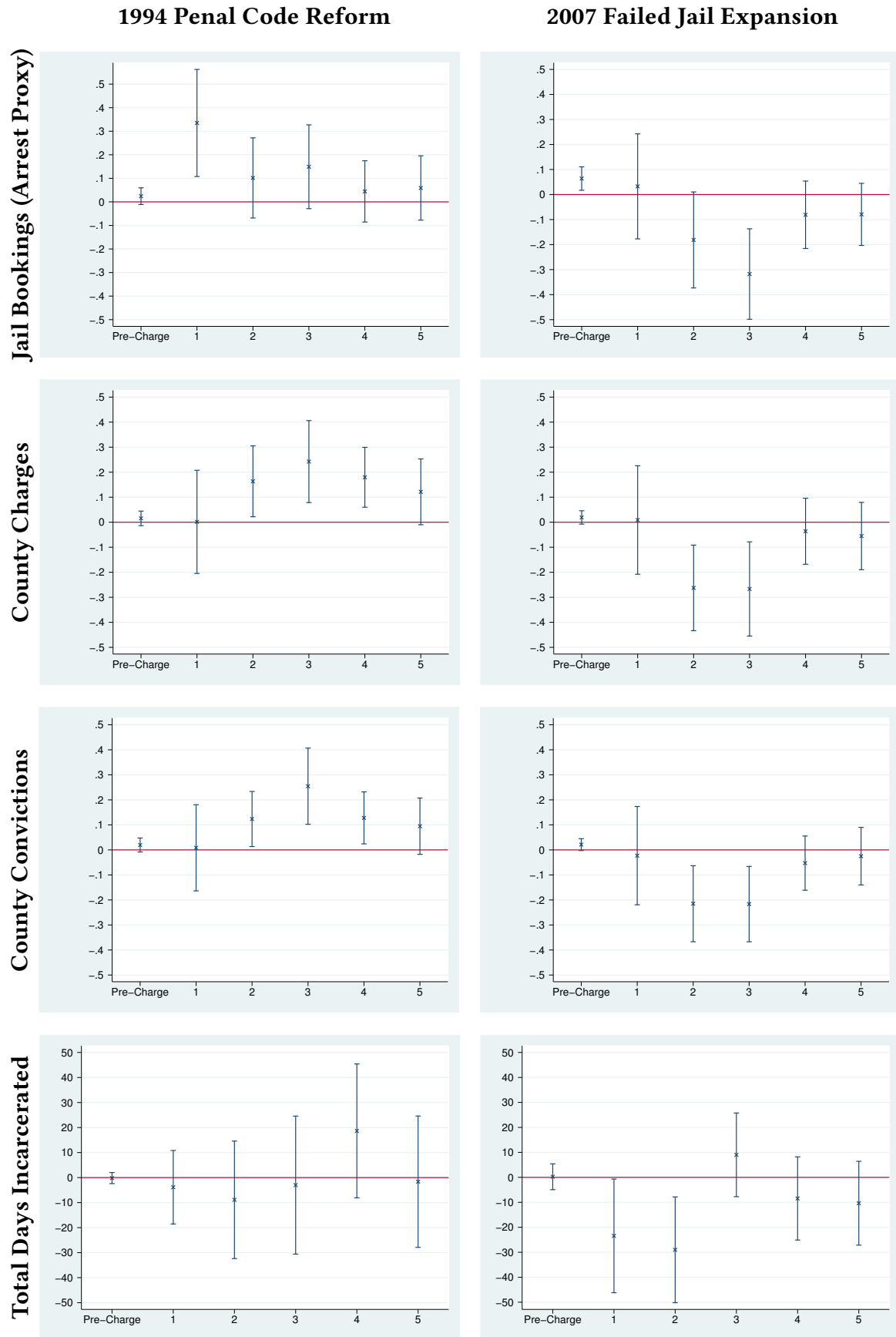
Figure 7: Estimated treatment effects over time, criminal justice outcomes

Note: This figure depicts estimated effects from our primary regression discontinuity specifications for pre- and post-reform time windows. The estimate for the five-year pre-reform period is depicted in the first estimate presented. Following, point estimates and associated confidence intervals are plotted for one-year intervals during the post-reform period.
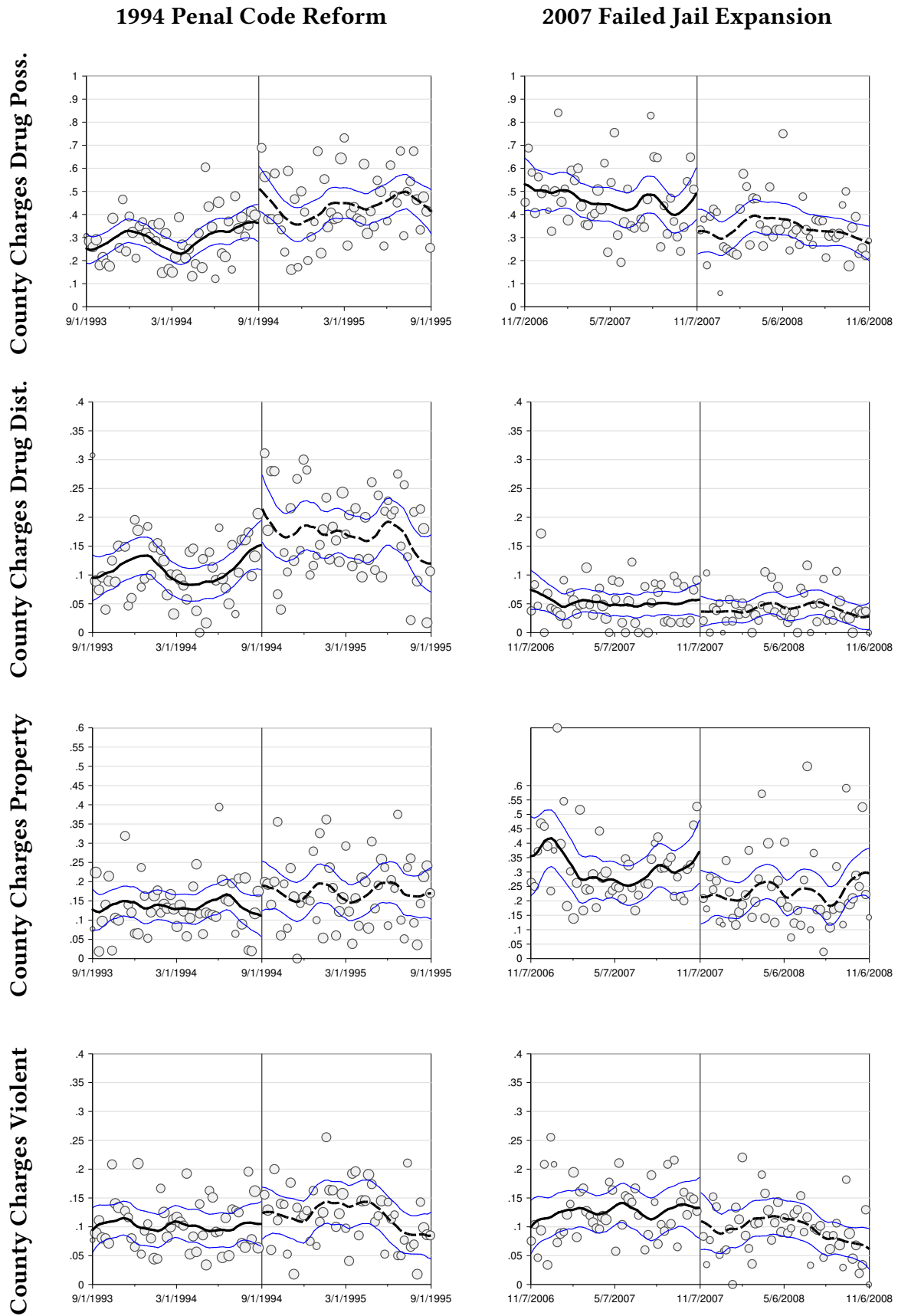
Figure 8: Five year recidivism outcomes, type of county charge

Table 1: Discontinuities in background characteristics

	Caseload Density	Prior Misd.	Age	Male	Black	Hisp
1994: Post Reform	−1.721	0.034	−1.709	−0.035	0.071	−0.079
	(1.444)	(0.234)	(1.342)	(0.055)	(0.068)	(0.056)
Mean of Dep. Var. (Pre Reform)	7.48	0.64	29.67	0.75	0.55	0.18
Observations	727	5,193	5,176	5,193	5,193	5,193
Order Loc. Poly (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
BW Loc. Poly. (l/r)	79 / 84	92 / 99	91 / 92	121 / 113	103 / 84	81 / 84
BW Bias (l/r)	137 / 131	148 / 168	149 / 154	198 / 186	173 / 146	136 / 143
2007: Post Reform	−2.083	0.075	−0.940	0.065	0.060	0.042
	(1.737)	(0.170)	(1.428)	(0.056)	(0.071)	(0.069)
Mean of Dep. Var. (Pre Reform)	11.34	0.63	30.33	0.76	0.36	0.31
Observations	497	5,524	5,523	5,524	5,524	5,524
Order Loc. Poly (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
BW Loc. Poly. (l/r)	98 / 97	103 / 123	111 / 100	109 / 122	100 / 92	107 / 93
BW Bias (l/r)	161 / 159	165 / 200	187 / 162	176 / 203	168 / 150	181 / 153

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Epanechnikov kernel. For bandwidth selection, we use a method which selects the median bandwidth from the set of MSE-optimal selection procedures. We use the HC_2 heteroskedasticity-robust variance estimation. We also adjust for baseline covariates (age, gender, race/ethnicity, and prior number of convictions). These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016).

Table 2: Discontinuities in court outcomes

	Guilty	Deferred Adjudication of Guilt	Charge Dismissed	Sentenced to Incarceration	Drug Treatment
1994: Post Reform	0.358***	−0.344***	−0.022	0.137***	−0.078**
	(0.061)	(0.061)	(0.057)	(0.048)	(0.036)
Mean of Dep. Var. (Pre Reform)	0.34	0.52	0.15	0.14	0.06
Observations	5,176	5,176	5,176	5,176	5,176
Order Loc. Poly (p)	1	1	1	1	1
Order Bias (q)	2	2	2	2	2
BW Loc. Poly. (l/r)	104 / 94	90 / 104	79 / 91	118 / 105	87 / 103
BW Bias (l/r)	173/171	150/175	148/148	194/169	150/160
2007: Post Reform	−0.286***	0.223***	0.073*	−0.276***	0.052
	(0.071)	(0.075)	(0.039)	(0.074)	(0.036)
Mean of Dep. Var. (Pre Reform)	0.65	0.25	0.10	0.65	0.05
Observations	5,523	5,523	5,523	5,523	5,523
Order Loc. Poly (p)	1	1	1	1	1
Order Bias (q)	2	2	2	2	2
BW Loc. Poly. (l/r)	93 / 93	87 / 87	92 / 116	89 / 89	121 / 119
BW Bias (l/r)	157/153	141/141	153/188	147/147	204/190

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Epanechnikov kernel. For bandwidth selection, we use a method which selects the median bandwidth from the set of MSE-optimal selection procedures. We use the HC_2 heteroskedasticity-robust variance estimation. We also adjust for baseline covariates (age, gender, race/ethnicity, and prior number of convictions). These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016).

Table 3: Discontinuities in criminal outcomes

	Jail Bookings 5 years	County Charges 5 years	County Convictions 5 years	Total Days Incarcerated 5 years
1994: Post Reform	0.493*	0.777***	0.682***	24.332
	(0.283)	(0.272)	(0.217)	(52.344)
Mean of Dep. Var. (Pre Reform)	1.73	1.00	0.79	341.48
Observations	5,176	5,176	5,176	5,176
Order Loc. Poly (p)	1	1	1	1
Order Bias (q)	2	2	2	2
BW Loc. Poly. (l/r)	84 / 83	80 / 80	86 / 86	77 / 82
BW Bias (l/r)	164 / 139	135 / 138	139 / 148	139 / 155
2007: Post Reform	−0.730**	−0.887***	−0.665***	−79.710**
	(0.313)	(0.311)	(0.251)	(40.122)
Mean of Dep. Var. (Pre Reform)	1.64	1.36	1.15	192.79
Observations	5,523	5,523	5,523	5,523
Order Loc. Poly (p)	1	1	1	1
Order Bias (q)	2	2	2	2
BW Loc. Poly. (l/r)	77 / 102	78 / 98	94 / 114	75 / 64
BW Bias (l/r)	142 / 166	146 / 163	163 / 181	144 / 127

Notes: *** p<0.01, ** p<0.05, * p<0.10. Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Epanechnikov kernel. For bandwidth selection, we use a method which selects the median bandwidth from the set of MSE-optimal selection procedures. We use the HC_2 heteroskedasticity-robust variance estimation. We also adjust for baseline covariates (age, gender, race/ethnicity, and prior number of convictions). These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016).

Table 4: Discontinuities in binary criminal outcomes

	Any Jail Bookings 5 years	Any County Charges 5 years	Any County Convictions 5 years	Ever Incarcerated 5 years
1994: Post Reform	0.072	0.041	0.147**	0.032
	(0.047)	(0.059)	(0.065)	(0.047)
Mean of Dep. Var. (Pre Reform)	0.69	0.49	0.44	0.84
Observations	5,176	5,176	5,176	5,176
Order Loc. Poly (p)	1	1	1	1
Order Bias (q)	2	2	2	2
BW Loc. Poly. (l/r)	132 / 132	107 / 126	79 / 104	85 / 79
BW Bias (l/r)	229 / 223	176 / 201	145 / 172	145 / 136
2007: Post Reform	−0.057	−0.102	−0.151**	−0.091*
	(0.066)	(0.068)	(0.063)	(0.050)
Mean of Dep. Var. (Pre Reform)	0.56	0.47	0.45	0.88
Observations	5,523	5,523	5,523	5,523
Order Loc. Poly (p)	1	1	1	1
Order Bias (q)	2	2	2	2
BW Loc. Poly. (l/r)	97 / 105	103 / 103	112 / 114	108 / 84
BW Bias (l/r)	179 / 179	184 / 167	186 / 198	174 / 149

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Epanechnikov kernel. For bandwidth selection, we use a method which selects the median bandwidth from the set of MSE-optimal selection procedures. We use the HC_2 heteroskedasticity-robust variance estimation. We also adjust for baseline covariates (age, gender, race/ethnicity, and prior number of convictions). These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016).

Table 5: Discontinuities in criminal outcomes, by charge type

	County Charges 5 years DRUG POSS.	County Charges 5 years DRUG DIST.	County Charges 5 years PROPERTY	County Charges 5 years VIOLENT
1994: Post Reform	0.184	0.026	0.136**	0.014
	(0.121)	(0.059)	(0.059)	(0.043)
Mean of Dep. Var. (Pre Reform)	0.30	0.11	0.14	0.10
Observations	5,176	5,176	5,176	5,176
Order Loc. Poly (p)	1	1	1	1
Order Bias (q)	2	2	2	2
BW Loc. Poly. (l/r)	84 / 82	107 / 134	75 / 95	125 / 111
BW Bias (l/r)	139 / 143	175 / 209	148 / 163	209 / 182
2007: Post Reform	−0.223*	−0.031	−0.160	−0.049
	(0.129)	(0.033)	(0.114)	(0.058)
Mean of Dep. Var. (Pre Reform)	0.46	0.05	0.31	0.13
Observations	5,523	5,523	5,523	5,523
Order Loc. Poly (p)	1	1	1	1
Order Bias (q)	2	2	2	2
BW Loc. Poly. (l/r)	105 / 109	112 / 113	114 / 100	135 / 133
BW Bias (l/r)	173 / 181	179 / 182	187 / 160	216 / 208

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Epanechnikov kernel. For bandwidth selection, we use a method which selects the median bandwidth from the set of MSE-optimal selection procedures. We use the HC_2 heteroskedasticity-robust variance estimation. We also adjust for baseline covariates (age, gender, race/ethnicity, and prior number of convictions). These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016).

Table 6: Discontinuities in criminal outcomes, heterogeneous effects

	County Convict. 5 years	County Convict. 5 years	County Convict. 5 years	County Convict. 5 years	County Convict. 5 years	County Convict. 5 years	County Convict. 5 years
	FULL	MALE	FEMALE	BLACK	HISP	AGE<30	AGE⩾30
1994: Post Reform	0.682***	0.606**	0.755**	0.650**	0.211	1.010***	0.349
	(0.217)	(0.257)	(0.326)	(0.314)	(0.335)	(0.354)	(0.227)
Mean of Dep. Var. (Pre Reform)	1.00	1.08	0.77	1.21	0.72	1.25	0.73
Observations	5,176	3,850	1,326	2,756	919	2,765	2,411
Order Loc. Poly (p)	1	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2	2
BW Loc. Poly. (l/r)	86 / 86	92 / 92	87 / 91	99 / 100	111 / 92	70 / 68	79 / 101
BW Bias (l/r)	139 / 148	155 / 156	144 / 163	153 / 167	186 / 161	128 / 122	135 / 160
2007: Post Reform	−0.665***	−0.916***	0.326	−0.598	−0.659*	−0.448	−1.016**
	(0.251)	(0.281)	(0.596)	(0.431)	(0.344)	(0.297)	(0.418)
Mean of Dep. Var. (Pre Reform)	1.36	1.34	1.43	1.86	0.98	1.60	1.06
Observations	5,523	4,265	1,258	2,014	1,798	3,006	2,517
Order Loc. Poly (p)	1	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2	2
BW Loc. Poly. (l/r)	94 / 114	91 / 91	116 / 121	138 / 130	77 / 74	127 / 109	71 / 72
BW Bias (l/r)	163 / 181	168 / 158	185 / 186	221 / 219	145 / 134	210 / 175	132 / 145

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Epanechnikov kernel. For bandwidth selection, we use a method which selects the median bandwidth from the set of MSE-optimal selection procedures. We use the HC_2 heteroskedasticity-robust variance estimation. We also adjust for baseline covariates (age, gender, race/ethnicity, and prior number of convictions). These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016).

Table 7: Discontinuities in criminal activity in state-wide computerized criminal history (CCH) file

	TX CCH Conviction 5 years	TX CCH Conviction Harris 5 years	TX CCH Conviction Not Harris 5 years
1994: Post Reform	0.325*	0.316*	0.006
	(0.179)	(0.168)	(0.056)
Mean of Dep. Var. (Pre Reform)	0.51	0.44	0.07
Observations	5,176	5,176	5,176
Order Loc. Poly (p)	1	1	1
Order Bias (q)	2	2	2
BW Loc. Poly. (l/r)	87 / 93	86 / 87	144 / 159
BW Bias (l/r)	151 / 165	143 / 166	233 / 249
2007: Post Reform	−0.455**	−0.388**	−0.079
	(0.184)	(0.167)	(0.062)
Mean of Dep. Var. (Pre Reform)	0.92	0.74	0.18
Observations	5,523	5,523	5,523
Order Loc. Poly (p)	1	1	1
Order Bias (q)	2	2	2
BW Loc. Poly. (l/r)	125 / 122	139 / 132	82 / 69
BW Bias (l/r)	219 / 201	222 / 222	149 / 143

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Epanechnikov kernel. For bandwidth selection, we use a method which selects the median bandwidth from the set of MSE-optimal selection procedures. We use the HC_2 heteroskedasticity-robust variance estimation. We also adjust for baseline covariates (age, gender, race/ethnicity, and prior number of convictions). These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016).

Table 8: Estimated Fuzzy RD treatment effects

	Jail Bookings 5 years	County Charges 5 years	County Convictions 5 years	Total Days Incarcerated 5 years
1994: Felony Conviction	1.229	2.180***	1.997***	−12.825
	(0.747)	(0.840)	(0.724)	(133.811)
Lenient Period Mean	1.80	1.06	0.83	364.30
Observations	5,176	5,176	5,176	5,176
Order Loc. Poly (p)	1	1	1	1
Order Bias (q)	2	2	2	2
BW Loc. Poly. (l/r)	109 / 92	90 / 79	81 / 79	93 / 106
BW Bias (l/r)	187 / 175	152 / 152	140 / 149	158 / 182
2007: Felony Conviction	2.489*	3.102**	2.308**	67.318
	(1.304)	(1.328)	(1.018)	(112.562)
Lenient Period Mean	1.30	0.91	0.74	192.92
Observations	5,523	5,523	5,523	5,523
Order Loc. Poly (p)	1	1	1	1
Order Bias (q)	2	2	2	2
BW Loc. Poly. (l/r)	81 / 83	81 / 81	92 / 83	111 / 115
BW Bias (l/r)	156 / 142	157 / 137	158 / 144	209 / 214

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Epanechnikov kernel. For bandwidth selection, we use a method which selects the median bandwidth from the set of MSE-optimal selection procedures. We use the HC_2 heteroskedasticity-robust variance estimation. We also adjust for baseline covariates (age, gender, race/ethnicity, and prior number of convictions). These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016).

Table 9: RD results using placebo samples

	Guilty	Deferred Adjudication of Guilt	Jail Bookings 5 years	County Charges 5 years	County Convictions 5 years	Total Days Incarcerated 5 years
1993: Placebo	0.024	0.004	−0.139	−0.305	−0.350*	−10.560
	(0.052)	(0.057)	(0.251)	(0.198)	(0.183)	(40.555)
Mean of Dep. Var. (Pre Reform)	0.49	0.36	1.68	1.05	0.86	314.63
Observations	5,733	5,733	5,733	5,733	5,733	5,733
1995: Placebo	−0.012	−0.021	0.005	0.029	−0.053	49.411
	(0.055)	(0.046)	(0.308)	(0.195)	(0.185)	(47.772)
Mean of Dep. Var. (Pre Reform)	0.68	0.17	2.15	1.49	1.26	344.60
Observations	5,060	5,060	5,060	5,060	5,060	5,060
2006: Placebo	−0.051	0.080**	−0.142	−0.271	−0.251	−21.160
	(0.044)	(0.039)	(0.230)	(0.223)	(0.202)	(28.813)
Mean of Dep. Var. (Pre Reform)	0.59	0.29	1.68	1.43	1.20	205.56
Observations	9,866	9,866	9,866	9,866	9,866	9,866
2007: Placebo	−0.011	−0.042	−0.019	−0.078	0.010	29.302
	(0.092)	(0.084)	(0.353)	(0.326)	(0.270)	(52.104)
Mean of Dep. Var. (Pre Reform)	0.63	0.26	1.55	1.27	1.06	162.39
Observations	3,886	3,886	3,886	3,886	3,886	3,886
2008: Placebo	0.044	−0.035	0.158	0.167	0.139	48.318*
	(0.049)	(0.052)	(0.183)	(0.172)	(0.140)	(27.618)
Mean of Dep. Var. (Pre Reform)	0.47	0.39	1.39	1.04	0.86	171.78
Observations	9,081	9,081	9,081	9,081	9,081	9,081

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Epanechnikov kernel. For bandwidth selection, we use a method which selects the median bandwidth from the set of MSE-optimal selection procedures. We use the HC_2 heteroskedasticity-robust variance estimation. We also adjust for baseline covariates (age, gender, race/ethnicity, and prior number of convictions). These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016). The 2007 placebo sample is composed on the non-responsive courts, specifically District Courts: 176, 177, 178, 180, 182, 183, 185, 226, 337, and 339. The 1993 and 1995 placebo samples apply the same sample restrictions as the 1994 sample but with the cutoff shifted forward or backwards by one year. The 2006 and 2008 placebo samples apply the sample restrictions as the 2007 sample (inclusive of all courts) but also with the cutoff shifted forward or backwards by one year.

Table 10: RD results using alternative bandwidth selectors (1994)

	Guilty	Deferred Adjudication of Guilt	Jail Bookings 5 years	County Charges 5 years	County Convictions 5 years	Total Days Incarcerated 5 years
1994: MSE1	0.360***	−0.349***	0.472*	0.639***	0.555***	30.877
	(0.060)	(0.057)	(0.271)	(0.230)	(0.184)	(51.682)
BW Loc. Poly. (l/r)	104 / 104	104 / 104	84 / 84	101 / 101	112 / 112	77 / 77
BW Bias (l/r)	171 / 171	177 / 177	164 / 164	194 / 194	208 / 208	155 / 155
1994: MSE2	0.349***	−0.345***	0.505*	0.782***	0.741***	18.232
	(0.058)	(0.061)	(0.296)	(0.275)	(0.226)	(53.558)
BW Loc. Poly. (l/r)	137 / 88	82 / 109	92 / 70	77 / 76	67 / 81	71 / 92
BW Bias (l/r)	230 / 159	148 / 175	164 / 131	134 / 138	122 / 148	122 / 163
1994: MSE3	0.359***	−0.331***	0.504*	0.779***	0.680***	27.019
	(0.061)	(0.062)	(0.288)	(0.274)	(0.222)	(52.799)
BW Loc. Poly. (l/r)	94 / 94	90 / 90	83 / 83	80 / 80	86 / 86	82 / 82
BW Bias (l/r)	173 / 173	150 / 150	139 / 139	135 / 135	139 / 139	139 / 139
1994: CER1	0.340***	−0.287***	0.686*	0.859***	0.712***	96.181
	(0.080)	(0.077)	(0.359)	(0.303)	(0.238)	(64.368)
BW Loc. Poly. (l/r)	68 / 68	68 / 68	55 / 55	66 / 66	73 / 73	50 / 50
BW Bias (l/r)	105 / 105	108 / 108	101 / 101	119 / 119	128 / 128	95 / 95
1994: CER2	0.342***	−0.274***	0.613	0.970***	0.855***	118.643*
	(0.076)	(0.081)	(0.389)	(0.368)	(0.297)	(66.157)
BW Loc. Poly. (l/r)	89 / 57	54 / 71	60 / 46	51 / 50	44 / 53	46 / 60
BW Bias (l/r)	141 / 97	91 / 107	101 / 80	82 / 85	75 / 91	75 / 100
1994: CER3	0.340***	−0.262***	0.714*	0.989***	0.852***	109.151*
	(0.081)	(0.084)	(0.381)	(0.366)	(0.295)	(65.799)
BW Loc. Poly. (l/r)	61 / 61	58 / 58	54 / 54	52 / 52	56 / 56	54 / 54
BW Bias (l/r)	106 / 106	92 / 92	85 / 85	83 / 83	85 / 85	86 / 86

Notes: *** p<0.01, ** p<0.05, * p<0.10. Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Epanechnikov kernel. We use the HC_2 heteroskedasticity-robust variance estimation. We also adjust for baseline covariates (age, gender, race/ethnicity, and prior number of convictions). These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016). The bandwidth selectors are coded as follows: MSE1 (one common mean squared error-optimal bandwidth selector for the RD treatment effect estimator); MSE2 (two different mean squared error-optimal bandwidth selectors (below and above the cutoff) for the RD treatment effect estimator); MSE3 (one common mean squared error-optimal bandwidth selector for the sum of regression estimates); CER1 (one common coverage error rate-optimal bandwidth selector for the RD treatment effect estimator); CER2 (two different coverage error rate-optimal bandwidth selectors (below and above the cutoff) for the RD treatment effect estimator); and, CER3 (one common coverage error rate-optimal bandwidth selector for the sum of regression estimates).

Table 11: RD results using alternative bandwidth selectors (2007)

	Guilty	Deferred Adjudication of Guilt	Jail Bookings 5 years	County Charges 5 years	County Convictions 5 years	Total Days Incarcerated 5 years
2007: MSE1	−0.284***	0.223***	−0.787**	−0.952***	−0.651**	−96.245**
	(0.071)	(0.075)	(0.323)	(0.320)	(0.259)	(41.665)
BW Loc. Poly. (l/r)	93 / 93	87 / 87	77 / 77	78 / 78	91 / 91	63 / 63
BW Bias (l/r)	152 / 152	141 / 141	142 / 142	141 / 141	156 / 156	127 / 127
2007: MSE2	−0.277***	0.224***	−0.810**	−0.897***	−0.665***	−64.580*
	(0.072)	(0.080)	(0.322)	(0.311)	(0.251)	(38.375)
BW Loc. Poly. (l/r)	82 / 89	67 / 78	66 / 102	77 / 98	94 / 114	98 / 64
BW Bias (l/r)	157 / 153	128 / 139	139 / 166	146 / 163	163 / 181	166 / 126
2007: MSE3	−0.322***	0.229***	−0.348	−0.608**	−0.507**	−61.575
	(0.061)	(0.064)	(0.262)	(0.264)	(0.220)	(38.991)
BW Loc. Poly. (l/r)	115 / 115	99 / 99	121 / 121	114 / 114	124 / 124	75 / 75
BW Bias (l/r)	211 / 211	208 / 208	208 / 208	195 / 195	207 / 207	144 / 144
2007: CER1	−0.257***	0.211**	−1.253***	−1.555***	−1.267***	−156.069***
	(0.096)	(0.102)	(0.414)	(0.406)	(0.333)	(50.969)
BW Loc. Poly. (l/r)	61 / 61	57 / 57	50 / 50	51 / 51	59 / 59	41 / 41
BW Bias (l/r)	93 / 93	86 / 86	87 / 87	86 / 86	95 / 95	78 / 78
2007: CER2	−0.251***	0.158	−1.169***	−1.432***	−1.106***	−145.002***
	(0.097)	(0.107)	(0.411)	(0.393)	(0.320)	(47.258)
BW Loc. Poly. (l/r)	53 / 58	44 / 51	43 / 67	50 / 64	61 / 74	64 / 41
BW Bias (l/r)	96 / 93	78 / 85	85 / 101	89 / 100	100 / 111	101 / 77
2007: CER3	−0.265***	0.219**	−0.854**	−1.097***	−0.866***	−142.648***
	(0.080)	(0.085)	(0.333)	(0.341)	(0.285)	(47.945)
BW Loc. Poly. (l/r)	75 / 75	64 / 64	78 / 78	74 / 74	80 / 80	49 / 49
BW Bias (l/r)	129 / 129	127 / 127	127 / 127	119 / 119	126 / 126	88 / 88

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Epanechnikov kernel. We use the HC_2 heteroskedasticity-robust variance estimation. We also adjust for baseline covariates (age, gender, race/ethnicity, and prior number of convictions). These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016). The bandwidth selectors are coded as follows: MSE1 (one common mean squared error-optimal bandwidth selector for the RD treatment effect estimator); MSE2 (two different mean squared error-optimal bandwidth selectors (below and above the cutoff) for the RD treatment effect estimator); MSE3 (one common mean squared error-optimal bandwidth selector for the sum of regression estimates); CER1 (one common coverage error rate-optimal bandwidth selector for the RD treatment effect estimator); CER2 (two different coverage error rate-optimal bandwidth selectors (below and above the cutoff) for the RD treatment effect estimator); and, CER3 (one common coverage error rate-optimal bandwidth selector for the sum of regression estimates).

Table 12: RD results using alternative variance estimators

	Guilty	Deferred Adjudication of Guilt	Jail Bookings 5 years	County Charges 5 years	County Convictions 5 years	Total Days Incarcerated 5 years
1994: Day Cluster	0.358***	−0.342***	0.495*	0.780***	0.688***	19.236
	(0.062)	(0.058)	(0.277)	(0.238)	(0.193)	(56.122)
BW Loc. Poly. (l/r)	103 / 96	86 / 101	85 / 82	80 / 76	81 / 81	80 / 84
BW Bias (l/r)	178 / 173	149 / 177	163 / 140	139 / 135	139 / 146	140 / 158
1994: Week Cluster	0.356***	−0.348***	0.493*	0.771***	0.688***	44.903
	(0.059)	(0.071)	(0.272)	(0.237)	(0.174)	(38.903)
BW Loc. Poly. (l/r)	103 / 93	95 / 112	78 / 74	89 / 70	78 / 70	66 / 70
BW Bias (l/r)	167 / 166	152 / 182	164 / 138	150 / 129	139 / 133	130 / 143
1994: Nearest Neighbor	0.358***	−0.344***	0.494*	0.774***	0.681***	25.671
	(0.061)	(0.061)	(0.285)	(0.276)	(0.220)	(51.718)
BW Loc. Poly. (l/r)	104 / 94	90 / 104	84 / 83	81 / 81	86 / 86	76 / 82
BW Bias (l/r)	172 / 170	150 / 175	164 / 139	136 / 139	140 / 149	139 / 155
2007: Day Cluster	−0.279***	0.224***	−0.787**	−0.995***	−0.745***	−79.582*
	(0.064)	(0.064)	(0.312)	(0.318)	(0.271)	(43.732)
BW Loc. Poly. (l/r)	87 / 87	82 / 82	70 / 97	70 / 90	81 / 102	74 / 65
BW Bias (l/r)	152 / 146	134 / 134	136 / 156	135 / 146	149 / 162	139 / 121
2007: Week Cluster	−0.291***	0.225***	−1.104***	−1.347***	−1.187***	−82.483**
	(0.064)	(0.048)	(0.189)	(0.224)	(0.173)	(41.008)
BW Loc. Poly. (l/r)	96 / 96	82 / 82	45 / 71	48 / 63	45 / 68	74 / 64
BW Bias (l/r)	160 / 153	137 / 137	109 / 136	108 / 126	104 / 129	131 / 111
2007: Nearest Neighbor	−0.287***	0.223***	−0.725**	−0.882***	−0.661***	−80.062**
	(0.071)	(0.076)	(0.313)	(0.309)	(0.249)	(39.545)
BW Loc. Poly. (l/r)	94 / 94	87 / 87	77 / 103	79 / 99	94 / 115	75 / 63
BW Bias (l/r)	157 / 153	142 / 142	143 / 167	146 / 164	163 / 182	144 / 127

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Epanechnikov kernel. For bandwidth selection, we use a method which selects the median bandwidth from the set of MSE-optimal selection procedures. We also adjust for baseline covariates (age, gender, race/ethnicity, and prior number of convictions). This table presents estimates using alternative variance estimators as indicated. These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016).

Table 13: RD results without conditioning on covariate charactieristics

	Guilty	Deferred Adjudication of Guilt	Jail Bookings 5 years	County Charges 5 years	County Convictions 5 years	Total Days Incarcerated 5 years
1994: Post Reform	0.367***	−0.320***	0.685**	0.915***	0.809***	59.500
	(0.058)	(0.063)	(0.330)	(0.317)	(0.259)	(56.619)
Mean of Dep. Var. (Pre Reform)	0.34	0.52	1.73	1.00	0.79	341.48
Observations	5,193	5,193	5,193	5,193	5,193	5,193
Order Loc. Poly (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
BW Loc. Poly. (l/r)	121 / 105	86 / 90	78 / 76	79 / 74	77 / 77	79 / 79
BW Bias (l/r)	195 / 182	147 / 160	150 / 130	136 / 129	129 / 134	138 / 147
2007: Post Reform	−0.275***	0.215***	−0.613*	−0.743**	−0.560**	−40.489
	(0.073)	(0.076)	(0.338)	(0.326)	(0.269)	(40.023)
Mean of Dep. Var. (Pre Reform)	0.65	0.25	1.64	1.36	1.15	192.79
Observations	5,524	5,524	5,524	5,524	5,524	5,524
Order Loc. Poly (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
BW Loc. Poly. (l/r)	90 / 90	86 / 86	82 / 86	84 / 83	94 / 92	82 / 75
BW Bias (l/r)	154 / 147	140 / 140	142 / 142	149 / 140	160 / 149	154 / 138

Notes: *** p<0.01, ** p<0.05, * p<0.10. Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Epanechnikov kernel. For bandwidth selection, we use a method which selects the median bandwidth from the set of MSE-optimal selection procedures. We use the HC_2 heteroskedasticity-robust variance estimation. These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016).

Table 14: RD results without bias correction or robust standard errors

	Guilty	Deferred Adjudication of Guilt	Jail Bookings 5 years	County Charges 5 years	County Convictions 5 years	Total Days Incarcerated 5 years
1994: Conventional	0.359***	−0.356***	0.376	0.662***	0.573***	6.445
	(0.052)	(0.051)	(0.239)	(0.230)	(0.184)	(45.315)
Mean of Dep. Var. (Pre Reform)	0.34	0.52	1.73	1.00	0.79	341.48
Observations	5,176	5,176	5,176	5,176	5,176	5,176
1994: Bias-corrected	0.358***	−0.344***	0.493**	0.777***	0.682***	24.332
	(0.052)	(0.051)	(0.239)	(0.230)	(0.184)	(45.315)
Mean of Dep. Var. (Pre Reform)	0.34	0.52	1.73	1.00	0.79	341.48
Observations	5,176	5,176	5,176	5,176	5,176	5,176
2007: Conventional	−0.290***	0.226***	−0.645**	−0.824***	−0.618***	−69.488**
	(0.059)	(0.062)	(0.268)	(0.268)	(0.211)	(35.444)
Mean of Dep. Var. (Pre Reform)	0.65	0.25	1.64	1.36	1.15	192.79
Observations	5,523	5,523	5,523	5,523	5,523	5,523
2007: Bias-corrected	−0.286***	0.223***	−0.730***	−0.887***	−0.665***	−79.710**
	(0.059)	(0.062)	(0.268)	(0.268)	(0.211)	(35.444)
Mean of Dep. Var. (Pre Reform)	0.65	0.25	1.64	1.36	1.15	192.79
Observations	5,523	5,523	5,523	5,523	5,523	5,523

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Local-polynomial regression-discontinuity point estimates rely on a Epanechnikov kernel. For bandwidth selection, we use a method which selects the median bandwidth from the set of MSE-optimal selection procedures. These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016). "Conventional" estimates omit both bias correction and robust standard errors. "Bias-corrected" estimates omit only robust standard errors.

Table 15: RD results after binning running variable by week

	Guilty	Deferred Adjudication of Guilt	Jail Bookings 5 years	County Charges 5 years	County Convictions 5 years	Total Days Incarcerated 5 years
1994: Post Reform	0.396***	−0.370***	0.451	0.696**	0.622**	2.674
	(0.063)	(0.062)	(0.330)	(0.309)	(0.246)	(56.052)
Mean of Dep. Var. (Pre Reform)	0.34	0.52	1.73	1.00	0.79	341.70
Observations	5,145	5,145	5,145	5,145	5,145	5,145
Order Loc. Poly (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
BW Loc. Poly. (l/r)	15 / 15	14 / 15	11 / 11	12 / 11	12 / 11	12 / 13
BW Bias (l/r)	26 / 26	24 / 25	22 / 20	24 / 21	22 / 22	21 / 24
2007: Post Reform	−0.321***	0.247***	−0.649**	−0.709**	−0.502**	−70.198
	(0.072)	(0.079)	(0.327)	(0.316)	(0.253)	(44.464)
Mean of Dep. Var. (Pre Reform)	0.65	0.25	1.64	1.36	1.15	192.32
Observations	5,502	5,502	5,502	5,502	5,502	5,502
Order Loc. Poly (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
BW Loc. Poly. (l/r)	15 / 14	12 / 12	11 / 13	12 / 13	15 / 15	11 / 9
BW Bias (l/r)	26 / 23	21 / 21	21 / 23	23 / 23	25 / 25	21 / 19

Notes: *** p<0.01, ** p<0.05, * p<0.10. Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Epanechnikov kernel. For bandwidth selection, we use a method which selects the median bandwidth from the set of MSE-optimal selection procedures. We use the HC_2 heteroskedasticity-robust variance estimation. We also adjust for baseline covariates (age, gender, race/ethnicity, and prior number of convictions). These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016).