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

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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 experience distinctly different court verdicts and sanctions. The first natural experiment follows a 1994 state-wide reform after which sanctions became discontinuously more punitive for first-time felony drug defendants; the second follows a 2007 failed initiative to expand the county jail which caused courts to become discontinuously more lenient to avoid costly overcrowding. Using administrative data and regression discontinuity methods, we find consistent, robust evidence that offenders subject to lenient regimes—primarily characterized by higher rates of deferred adjudication of guilt which provide offenders an opportunity to avoid a felony criminal record—exhibit substantially lower patterns of future offending compared to those who experience the more punitive sanctions (i.e., guilty verdicts and incarceration sentences).

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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.[1] As a result, it is estimated that nearly 10 percent of the U.S. adult population (and 33 percent of African American adult males) have felony records, the majority with felony drug convictions.[2] 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. In fact, more than 30 states passed laws between 2009 and 2013 decreasing sanctions towards drug offenses within their criminal justice systems (Subramanian and Moreno 2014).

Many states have implemented deferred prosecution/adjudication options for drug defendants to decrease the rates of incarceration and reduce institutional overcrowding. In contrast to the establishment of specialized drug courts and drug treatment programs, these options require fewer resources since they typically rely on established community supervision programs (e.g. probation) and can be rapidly implemented. Moreover, the deferral of judgement allows an offender to avoid incarceration as well as a criminal felony conviction conditional on satisfying the requirements of their probation. Despite the increasing prevalence of deferred prosecution/adjudication options and the potential long-lasting impacts of a criminal conviction and incarceration, little is known about the impact of these options on rates of offending and drug use for treated offenders. These options could potentially have large effects on a wide array of outcomes given recent 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)[3], 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).[4]

This study seeks to provide empirical evidence of the effect of deferred adjudications for drug felony offenders on recidivism by exploiting two natural experiments in Harris County, Texas (TX) which dramatically altered the treatment of drug offenders in ways directly analogous to recent changes across the United States. 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. 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 increase in deferred adjudications for first-time felony drug offenders. What is particularly attractive about these policy shifts from a research perspective is that

[1] 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).

[2] 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).

[3] 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 (a similar strategy as used by Aizer and Doyle (2015) and Di Tella and Schargrodsky (2013)) 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.

[4] 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.

each was implemented quite rapidly such that defendants charged or convicted one day versus the next experience 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 latent trends in recidivism.

Because of the immediate nature of these changes, this paper employs a regression discontinuity (RD) research design. We show conviction rates and criminal sanctions change discontinuously across the thresholds and present reduced form evidence on how these changes affected future criminal behavior. 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. This stands in stark contrast to recent evidence from Hansen (2015) which found stricter penalties applied to drunk drivers reduced recidivism.

The types of crimes encouraged as a result of harsh treatment of drug offenders are remarkably similar across the two natural experiments. Both samples exhibit large discontinuities with regard to drug possession crimes. Additionally, we observe statistically and economically meaningful differences on future property crimes. In general, we find larger effects in 2007 compared to 1994. Given the proximity to the Great Recession, it is possible that harsh sanctions have more wide-ranging implications during periods of poor economic conditions.

We do not find evidence of 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 depend on any specific implementation design. 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 given that a theory of general deterrence predicts changes in the behavior of would-be criminals. This finding, however, is consistent with suggestive evidence that general deterrence plays a minor, if any, role in determining future criminal behavior (Lee and McCrary 2009, McCrary and Sanga 2012).

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. 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 3rd degree felony; and, Senate Bill 532 created "state jails," correctional institutions set up specifically for individuals who had been convicted of state jail felonies.[5] Individuals convicted of a state jail felony would still be

[5]State jails were neither a state prison or a county jail. The intent was to shield non-violent offenders from the more serious criminal population found in state prisons. The second aim was to save money through dormitory-style designs (similar to some county jails) which could quickly increase capacity at low cost. The change in the physical configuration of and peer composition in state jails could have altered the transmission

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.

The new laws only applied to defendants who had committed their offenses on or after September 1, 1994.[6,7,8] An array of new charge codes were adopted, replacing many of the original offense categories, which forced the courts into the new regime when it came into effect. Since defendants were required to be charged within 48 hours of arrest, prosecutors had minimal leeway to manipulate who was subject to which regime.

While the new legislation was intended to relieve the burden of overcrowding, it had several unanticipated consequences that made the new regime tougher on defendants in practice. One such issue was the use of deferred adjudication of guilt.[9] This intermediate conviction status, which is neither a guilty verdict nor a dismissal, allows courts to impose modest sanctions (usually community supervised probation) without permanently impacting a defendant's criminal record. Upon successful completion of the required punishment, the case is dismissed by the court and the offender's record will not include a felony conviction. If the defendant, however, fails to satisfy the probation's terms the deferred status can be revoked, a final conviction established, and harsher sentencing (typically incarceration) imposed ex-post.

Prior to the reform, roughly 50 percent of first time drug offenders in Harris County received deferred adjudication, yet this plummeted dramatically to less than 20 percent after the new policies went into effect (Figure 4). At issue was the fact that prosecutors had lost the ability to use incarceration as a penalty to enforce the terms of deferred adjudications since state jail probation was required before the execution of incarceration sanctions for first-time drug felony offenders. In fact, this precise issue was raised in October 1993 when policy makers conducted a simulated plea bargaining exercise with prosecutors and defense attorneys to evaluate the effect of the new penal code, yet no action was taken to amend the statutory language before it went into effect (Fabelo 1997).

Sentenced incarceration rates also unexpectedly increased after the September 1994 threshold and a modest but significantly smaller share were sentenced to drug rehabilation (Figure 5). The higher proportion of defendants with guilty verdicts resulted in more defendants being sentenced to incarceration. However, there was no significant discontinuity in the fraction of offenders administered probation supervision following the court's judgement on the focal drug felony charge (Figure 5). This reflects the fact that incarceration sentences were required to start with probation after 1994 (a probated incarceration sentence) and so the post period exhibits high levels of sanction mixing.

An added issue is that the state jail system introduced determinant sentencing requiring inmates to serve the full duration of their sentence regardless of behavioral considerations. This was a stark contrast compared to the indeterminate structure in operation in the rest of the state

of criminal capital among inmates, but we do not explicitly address this in this draft.

[6] 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.

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

[8] 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.

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

prison system. Prior to the reform, non-violent offenders serving time in state prisons were eligible for paroled release after serving 25 percent of their sentence and could receive up to an additional day and a half's credit for each day served without disciplinary incidents. In practice a two year determinant sentence equated to roughly the same amount of time served as a 12 year indeterminate sentence. So despite the shorter maximum sentence length recommended in the guidelines for state jail felonies, many offenders likely would have served less time behind bars under the indeterminate system.

1.2 The 2007 Failed Jail Expansion Ballot Initiative.

Problematic overcrowding remained an important concern in the 2000s throughout Texas although particularly in the Harris County Jail. This local jail—which served several functions including pre-trial detention, punishment for misdemeanor convictions and temporary 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.[10] In anticipation of the impending crisis, the state had sought to curb overuse of incarceration through passing House Bill 2668 in 2003 which mandated community service, drug treatment programs, or the suspension of incarceration sentences for certain types of convictions. Judges in Harris County, however, created a loophole through requiring county jail incarceration as a condition of probation to partially circumvent the new state rules.[11]

In 2006, the Texas Commission on Jail Standards ordered Harris County to relocate 500 jail inmates to contract housing over concerns of the inmates' health and safety with an estimated cost of roughly $30,000 per day.[12] The short-gap solution adopted by the county was to send several hundred inmates to a privately operated facility in northern Louisiana at a cost of $4 million per year.[13] The companion long-term solution was to expand the jail capacity by 2,500 beds with funds raised through county bonds for a new jail.

The proposed jail expansion 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. Proposition 3, which specifically dealt with the jail expansion, sought approval to issue $195 million in bonds.

In the final weeks before the election, a local campaign was mounted against Proposition 3.[14] Arguments against the proposition suggested that the intended location of the new jail would be bad for local economic development and that existing infrastructure needed to be used more efficiently with less reliance on pretrial detention. In spite of the challenge, local commentators

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

[11] https://web.archive.org/web/20060209193303/http://solutionsfortexas.info/id414.html

[12] http://www.chron.com/neighborhood/katy-news/article/State-orders-Harris-to-transfer-500-from-jail-1861887.php

[13] http://www.chron.com/opinion/editorials/article/Passing-the-inmate-Harris-County-has-chosen-an-1793007.php

[14] See: 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

expected the initiative to pass given Harris County's historical pattern of having "tough on crime" sentiments and support in local polling.

On election day, however, an unexpectedly large voter turnout led to a narrow defeat of the jail expansion 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 statewide bond to expand state prison capacity was approved (58.2 to 41.8).

There was an immediate response among most Harris county criminal courts. Cases being sentenced in the immediate aftermath of the election exhibited distinctly different sanctions relative to those disposed prior to the election (Figure 4). Guilty rates dropped from around 65 percent to 40 percent of the caseload. Those who would have originally been convicted were shifted into deferred adjudication of guilt allowing the county courts to postpone compounding the overcrowding crisis already playing out. With the drop in guilty rates, incarceration rates also immediately eased and were replaced by community-supervised probation (Figure 5). In spite of this, by the summer of 2008 the Harris County Jail had to transport an additional 1,130 inmates to Louisiana in order to stay in compliance with the Texas Commission on Jail Standards. With this addition, total annual costs for incarcerating inmates outside Harris County reached $24 million.[15]

2 Potential Mechanisms

The policy changes we study could impact recidivism through several distinct mechanisms, making theoretical predictions of lenient versus harsh punishment regimes ambiguous. Lower reoffense rates for individuals facing stricter sentencing outcomes would be suggestive of potential incapacitation as well as specific deterrence. Although there is some disagreement on the exact magnitude, numerous studies have documented that some criminal offenders do recidiviate in the absence of incarceration (Mueller-Smith 2015, Buonanno and Raphael 2013, Johnson and Raphael 2012, Owens 2009, Levitt 1996). Likewise, some evidence suggests that directly experiencing worse punishments decreases the likelihood of future criminal acts as future punishment becomes more salient (Hansen 2015).

At the same time, harsher regimes could generate the opposite effect and increase recidivism. Mounting evidence suggests 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).

While we do not have direct empirical evidence, it also is possible that those with and without a formal criminal conviction record are treated differently on an ongoing basis by the criminal justice system (e.g. police, prosecutor, judges, etc.). For instance, certain types of prior convictions can trigger aggravated (elevated) charges by TX statute upon reoffense which results in harsher and potentially more certain convictions. Additionally, defendants under the supervision of probation versus parole officers may face differing probabilities of arrest conditional on criminal activity.[16]

[15] http://www.chron.com/news/houston-texas/article/Packed-Harris-jail-may-ship-1-130-inmates-to-1672711.php

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

It is also worth mentioning that the transition in 1994 from indeterminate to determinate sentencing for those being charged with the newly created state jail felony offenses generates dynamics similar to Kuziemko (2013). These prior findings would suggest the harsher regime in this setting would likely generate higher rates of recidivism as inmates would have little incentive to invest in human capital or to avoid disciplinary infractions while incarcerated. At the same time, the new requirement for probated incarceration sentences for state jail felonies has features similar to Drago et al. (2009) which would predict lower rates of recidivism. These potential explanations, however, are unique to 1994 as the 2007 discontinuity exhibits no similar institutional changes.

3 Data Sources and Sample Restrictions

In order to empirically evaluate the effect of these regime changes, this project leverages several sources of administrative data. Adult criminal court records containing defendant traits, charges and court outcomes were collected from the Harris County District Clerk. These include all initial filings of felony and misdemeanor charges between 1980 and 2013 regardless of final verdict.[17]

We supplement the Harris County court record database with additional administrative data that provides both alternative measures of illegal activity as well as information on incarceration spells. Booking data was acquired from the Harris County Sheriff's Department from 1980 to 2013, providing an opportunity to observe arrests that did not progress to court charges. Records from the Computerized Criminal History Database, provided by the Texas Department of Public State, track statewide convictions in Texas from the mid-1970s up to the present. Incarceration data was collected from the Texas Department of Criminal Justice for state prisons and state jails and from the Harris County Sheriff's Office for the local county jail. These data cover all inmates incarcerated between 1978[18] and 2013. One major limitation of the incarceration data is that individual spells cannot be directly linked to specific court charges or convictions.

All county-level administrative data (criminal court records, bookings, and county jail records) are linked using a unique county identifier tied to an individual's fingerprint known as the SPN. The SPN allows us to link offenders within and across each Harris County criminal justice dataset. For the state-level administrative data (statewide convictions and state incarceration records), we match offenders to the county-level data 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. In the case of the 1994 penal code reform, we require that: (1) charges were filed between September 1, 1993 and September 1, 1995; (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.[20]

For the 2007 failed jail expansion, we require that: (1) charges were disposed between November

[17] 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.

[18] County jail data only starts in 1980

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

[20] 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).

7, 2006 and November 7, 2008; (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 twelve district courts that reacted to the election results the day following the election.[21,22] The remaining courts did not change their sentencing practice discontinuously in response to the failed jail election.This could be because they (1) increased their use of deferred adjudication in anticipation prior to the election in response to the pre-election public debate,[23] (2) took a "wait and see" approach through only adjusting their sentencing practices until several weeks after the election,[24] or (3) did not change their sentencing practices because they were already high users of deferred adjudication[25] or refused to use this alternative sentencing strategy.[26] The data for these non-responsive courts are analyzed as a placebo exercise with the results shown in Table 7.

4 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 generates two different mean squared error-optimal bandwidths (below and above the cutoff) for the RD treatment effect estimator. Our primary specification also adjusts for baseline covariates of age, gender, race/ethnicity, and prior number of convictions. [27]

We first measure the effect of the 1994 and 2007 changes on conviction status and type of sanction imposed to demonstrate a "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 3. 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 6, 2007.

The validity of this research design relies on the assumption that there is no selection across

[21] These are district courts 174, 179, 184, 208, 209, 230, 232, 248, 262, 263, 338, and 351.

[22] 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.

[23] Courts 178, 180, and 337.

[24] Courts 182, 183, 185 and 339.

[25] Courts 177 and 228.

[26] Court 176.

[27] Our Estimates rely on a Epanechnikov kernel and robust nonparametric confidence intervals from Calonico et al. (2014) and are robust to using the following alternative bandwidth selectors: MSE1 (one common mean squared error-optimal bandwidth selector for the RD treatment effect estimator); MSE2 (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). This empirical methodology was implemented using the *rdrobust* package available in STATA (Calonico et al. 2016).

the cutoff threshold. If potential criminals altered their illegal activity to anticipate new policies, police changed arresting patterns, prosecutors delayed filing charges, or judges adjusted sentencing dates, the viability of this assumption would be called into question. This is particularly concerning in the context of the 1994 reform when all relevant actors could fully anticipate the adoption of the new penal code. A strategy to address this concern is to examine whether caseload size or composition discontinuously changes at the threshold. As will be discussed in the following section, there is no evidence of sorting at either of the turning points.

5 Results

The validity of our research design hinges on the assumption that defendants are effectively randomly allocated before and after the two thresholds in our natural experiments. For 1994, 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 2007, 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.[28]

These identification assumption also imply that defendants do not discontinuously change their propensity to engage in criminal behavior in response to the change in the penal code or judicial behavior. If there exists a general deterrence effect, we would expect to see trends, and potentially a discontinuity, in the levels of criminal activity in our sample windows. Additional threats to our 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. To the extent that sorting across the threshold is taking place, we would expect to observe discontinuities in important predictors of future offending such as the prior misdemeanor records.

We do not observe discontinuities in caseload densities or defendant characteristics (Figures 1, 2, and 3) and detect no statistical differences using our primary empirical methodology (Table 1). In 1994, roughly 7 felony drug cases were charged each day.[29] These cases were comprised of individuals who had been convicted of slightly less than 1 misdemeanor crime prior to their current felony charge, were about 29 years old and were about 75 percent male. Slightly more than half were African American while non-Hispanic Whites (Hispanics) made up about one quarter (one fifth) of the caseload. The 2007 caseload was slightly higher at about 10 dispositions per day and the age profile shows a gradual trend upward in the running variable. The number of prior misdemeanor convictions and share male are roughly equivalent to what is observed in 1994. The racial composition exhibits fewer African Americans and more Hispanic defendants.

5.1 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

[28] Dispositions include case dismissals, guilty verdicts and deferred adjudications of guilt.
[29] Conditional on not having a prior felony conviction.

rate of deferred adjudications drops by over 30 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 25 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.

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 directly to incarceration and a drop in sentences involving drug rehabilitation programs.

In 2007, we observe a clear shift away from incarceration and towards probation in sentencing outcomes. At the discontinuity, we observe a drop of 25 percentage points in incarceration and a gain of 23 percentage points in probation.

5.2 Reoffending Outcomes

To assess the impact of the change in court judgements and sanctions on 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 arrests leading to convictions anywhere in the state of Texas, and total days incarcerated in the Harris County jail or a state prison. Overall, we find strong evidence that defendants subject the harsher policy regimes were more likely to reoffend during the followup period (Figure 6 and Table 3).

Across the board, we estimate higher rates of reoffending for the post-1994 reform group who received more punitive sanctions. While effects on statewide convictions in the 1994 sample are only marginally insignificant and effects on total days incarcerated is not significant, the direction and magnitude of these estimates are consistent with higher rates of reoffending among those subject to the more harsh punishments. We find that the new regime generated 0.8 additional charges per offender which is an 80 percent increase in reoffending from the pre-reform average of 0.99 total charges over 5 years. The magnitude of estimates on the other measures imply a 30 percent increase in jail bookings and a 50 percent increase in convictions. These effects are all very large in magnitude, are supported by visual evidence in Figure 6, and are suggestive of substantial changes in offender behavior.

We estimate comparable results in 2007. As a reminder, those disposed after the failed jail expansion ballot experienced more lenient court outcomes and so the sign of the reduced form effects should be expected to flip. 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.[30] We measure a decline of 0.62 Harris County Jail bookings, 0.99 Harris County criminal charges filed, 0.44 statewide convictions, and 83 days incarcerated. These estimates correspond to a 38 percent decrease in bookings, 73 percent decrease in county charges, 49 percent decrease in statewide convictions, and a 44 percent decrease in total days incarcerated. These effects are precisely estimated, supported by visual evidence in Figure 6, and are suggestive of substantial changes in offender behavior as a result of the change in court

[30]The increased precision in statewide conviction outcomes in 2007 relative to 1994 could partially be explained by an improvement in the coverage of the statewide database over time.

verdicts and court-imposed sanctions for first-time drug felony defendants.

The timing of the realization of these effects is traced out in Figure 7.[31] Both changes appear to have an immediate effect on offending behavior which begins to converge after 3 years. While most differences are not statistically significant 5 years from the initial charge (or date of disposal in 2007), the patterns suggest persistent effects.

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.[32]. We only find a significant difference in incarceration outcomes for the 1994 sample 3-4 years from the initial charge. This suggests that differences in behavior observed 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 in offenders sentenced to incarceration, the average sentence was only 34 days so this difference is also due to effects on future offenses. The sentence for the focal offense could explain the 30 day difference in incarceration during the first year, but would not explain the nearly 60 day difference in days incarcerated between the first and second year following the initial date of disposal depicted in Figure 7. Moreover, estimated differences in jail bookings, county charges, and state convictions 3 years following the initial date of disposal indicate changes in behavior not driven by incapacitation effects.

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 4 and by offender type in Table 5. In both 1994 and 2007, we see clear 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. Effects on property crimes are very large and statistically significant in 2007 (nearly a 100 percent decrease). Given the proximity to the Great Recession, the effect of harsh court sanctions at this time may be amplified due to economic conditions.

In Table 5, 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. One potential explanation for this pattern in the results is that leniency is most effective for individuals who have not yet established a habitual pattern of drug use. Younger individuals may also have more at stake in terms of labor market propsects or other outcomes through avoiding a felony conviction record. While in 2007, our estimates imply much larger effects among males but similar effects by age.

We present estimates from a fuzzy regression discontinuity design where the first stage outcome is an indicator for deferred adjudication of guilt in Table 6. While we prefer to focus on reduced form estimates previously discussed due to difficulties satisfying the exclusion restriction,[33] these estimates provide a sense of the magnitude of our estimated treatment effects on criminal justice outcomes. Overall, estimates imply substantial effects (typically 150 to 200 percent changes) of the reforms on future criminal offending.

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

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

[33] The reform may affect future behavior through channels other than effects on receiving a deferred adjudication of guilt verdict due to other types of changes and adjustments to the treatment of drug defendants.

5.3 Robustness Checks

We conduct several robustness checks to confirm the reliability of our results. Table 7 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 3) from the 2007 experiment. The goal in this exercise is to determine whether our findings could be picking up latent seasonal patterns. 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 8 and Table 9); models without covariates included (Table 10); models without the bias correction or robust standard errors as suggested by Calonico et al. (2014) (Table 11); and models based on outcomes aggregated to the week level (Table 12). Among these tables, there is no evidence to suggest our findings are driven by arbitrary implementation decisions.

6 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 harshness 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 guilty 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 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 substantial effects on property crimes.

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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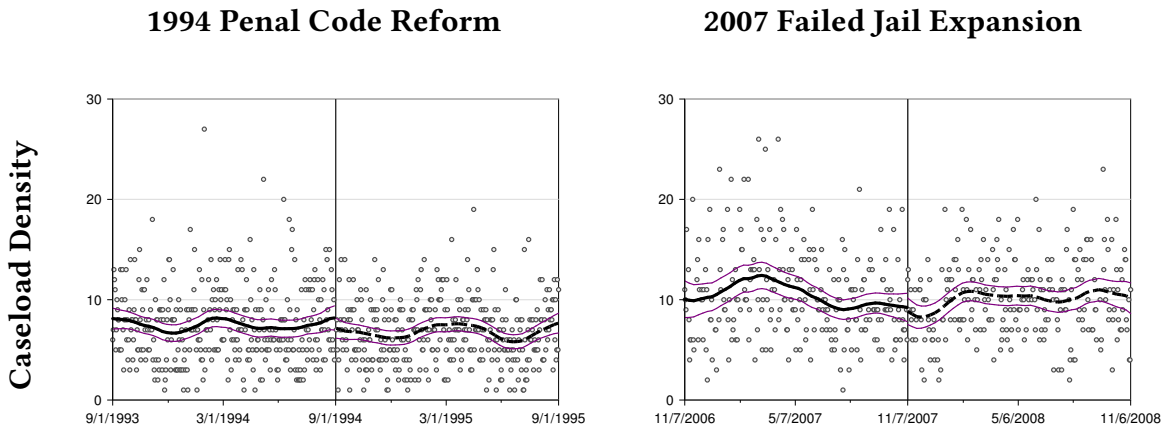


Figure 1: Discontinuities in caseload density

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.

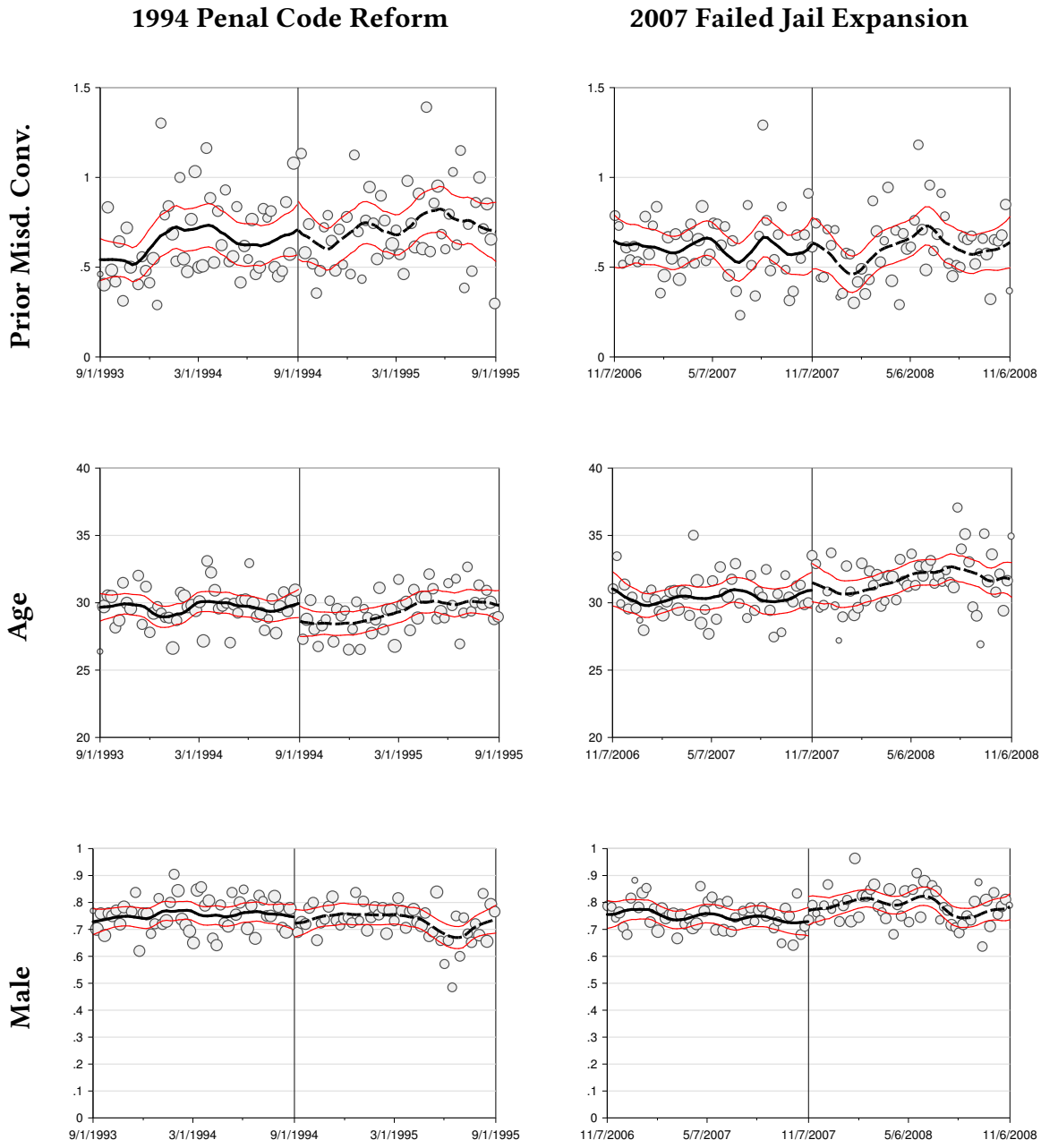


Figure 2: Discontinuities in baseline characteristics

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.

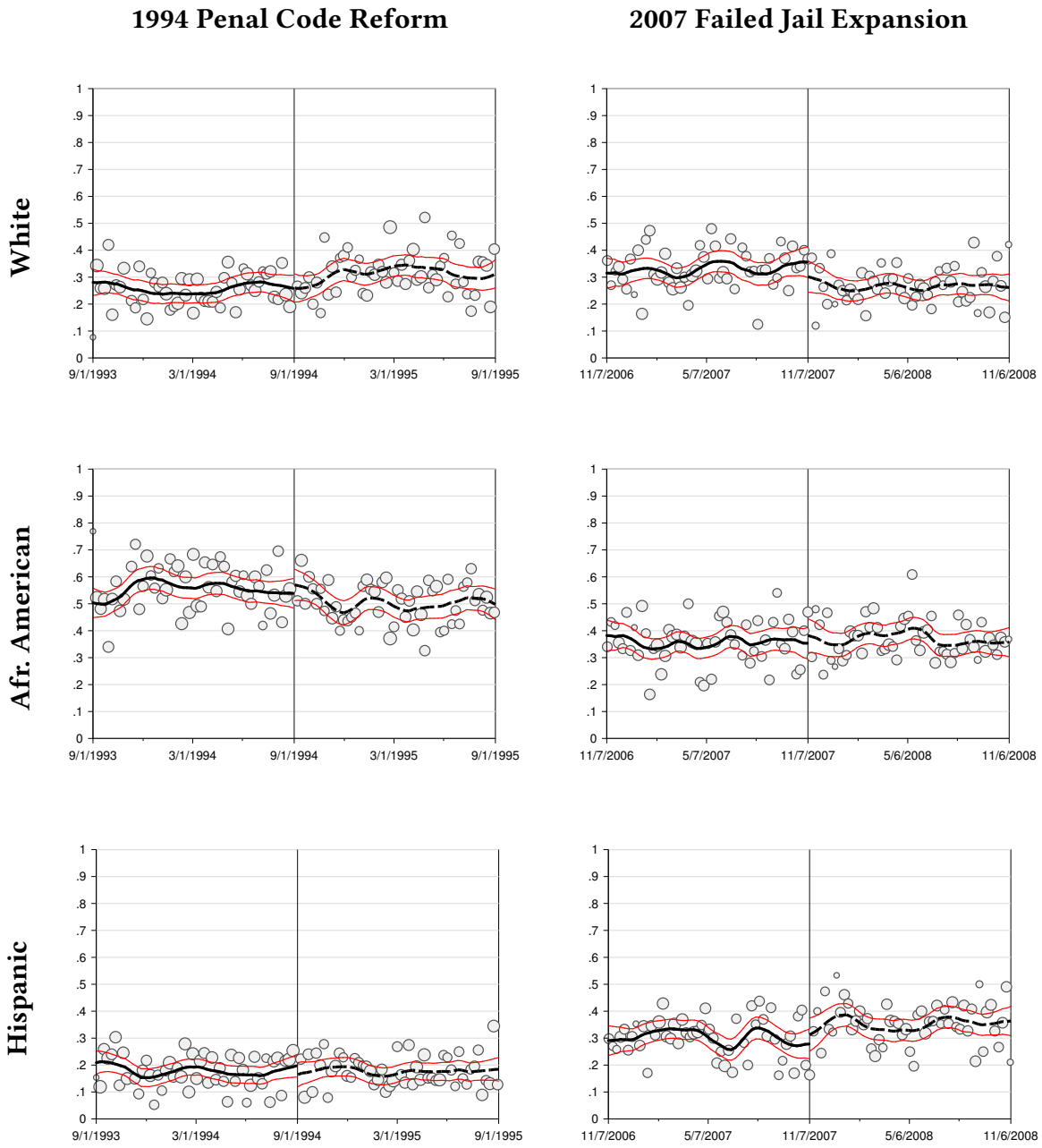


Figure 3: Discontinuities in baseline characteristics

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

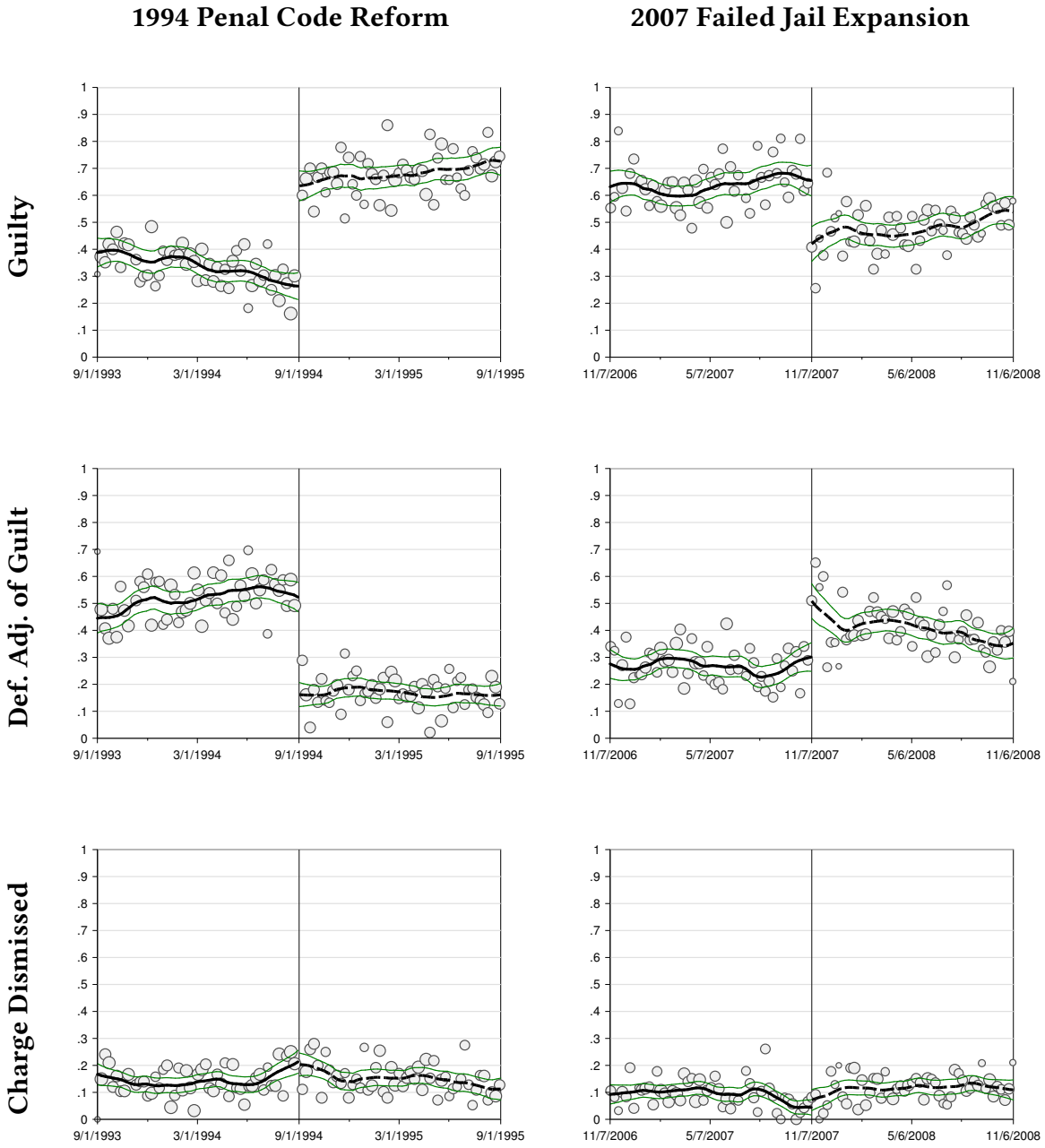


Figure 4: Discontinuities in court outcomes

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.

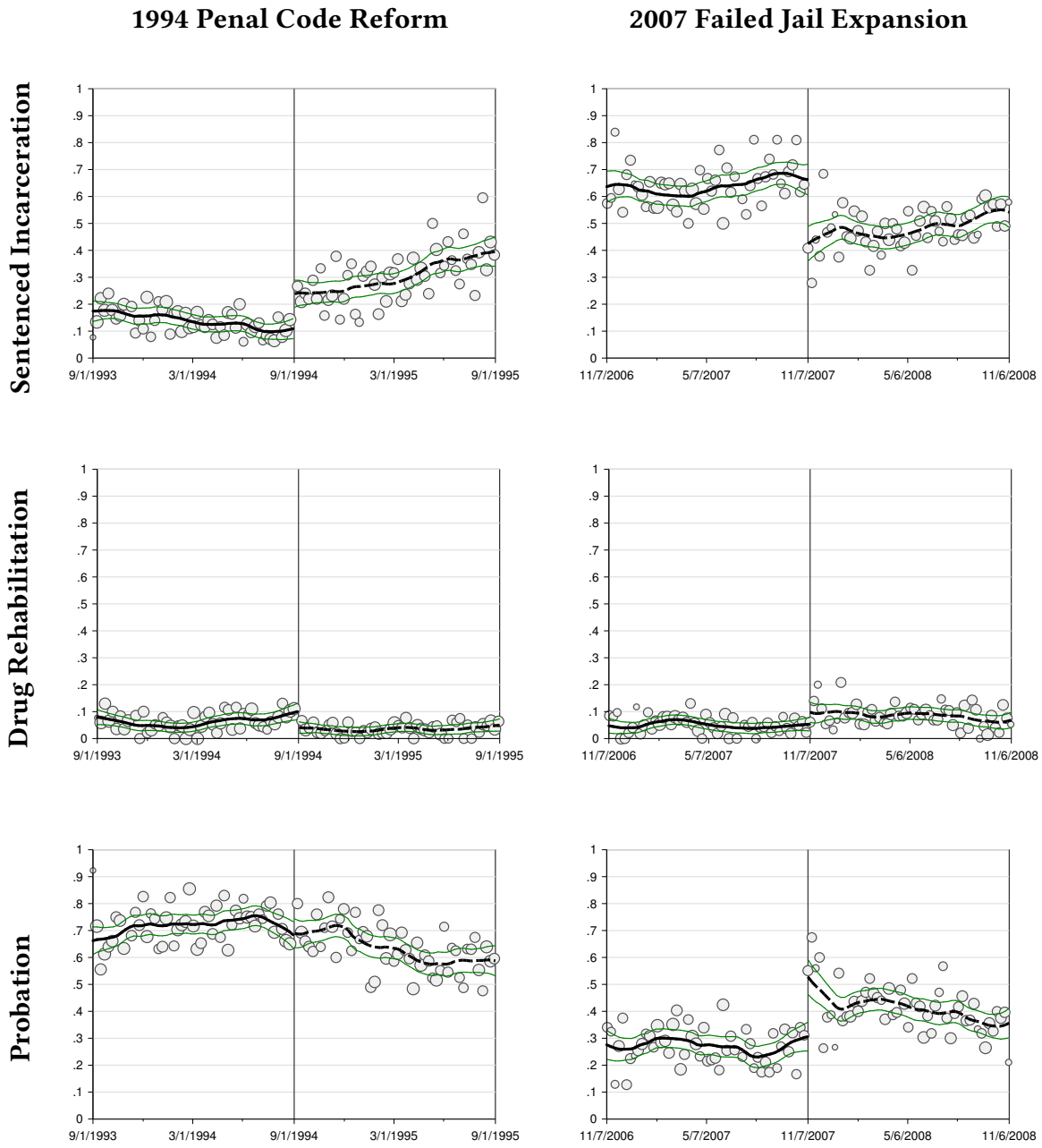


Figure 5: Discontinuities in status following court verdict

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.

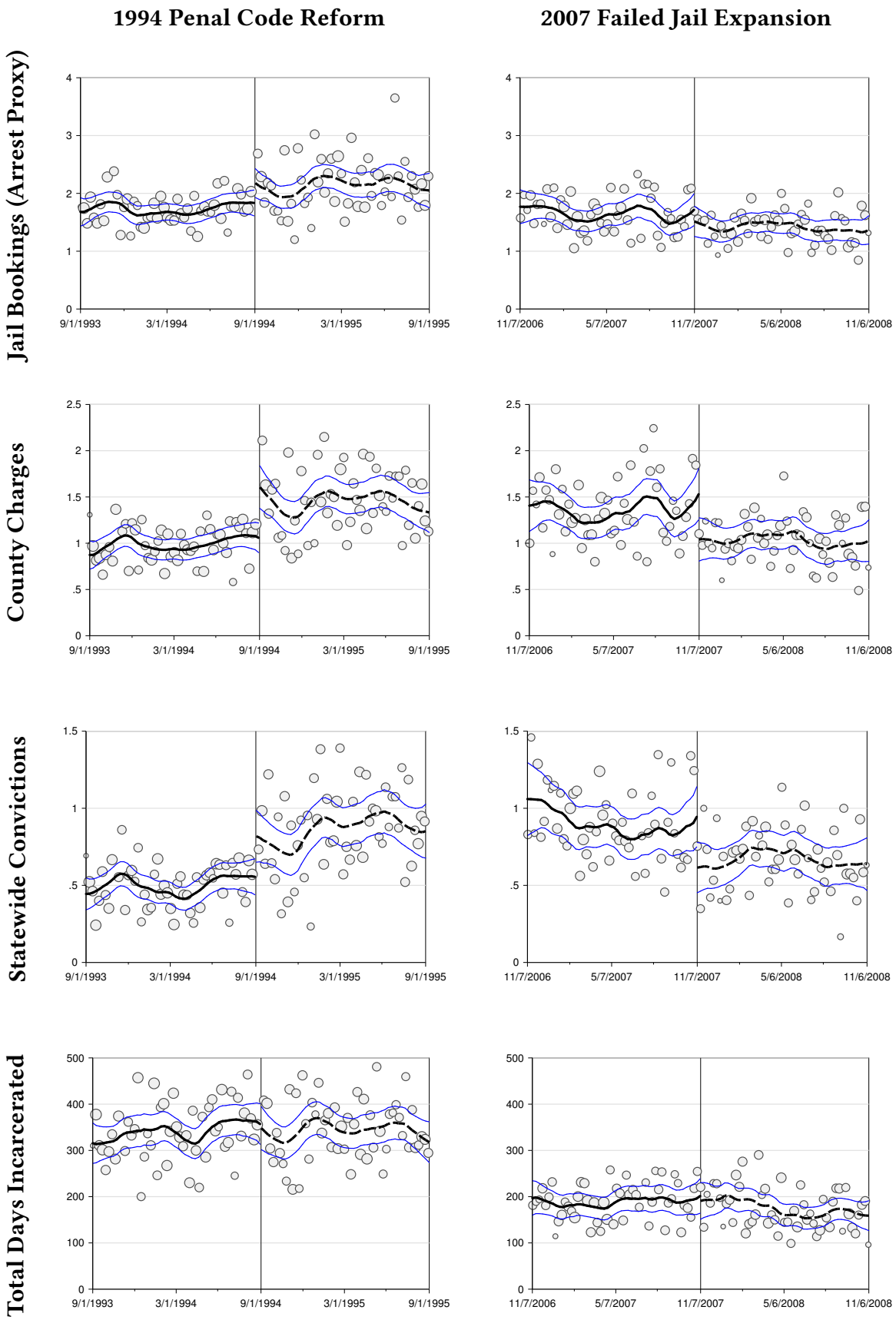


Figure 6: Five year criminal justice outcomes

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 statewide 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 3). General notes from Figure 2 apply.

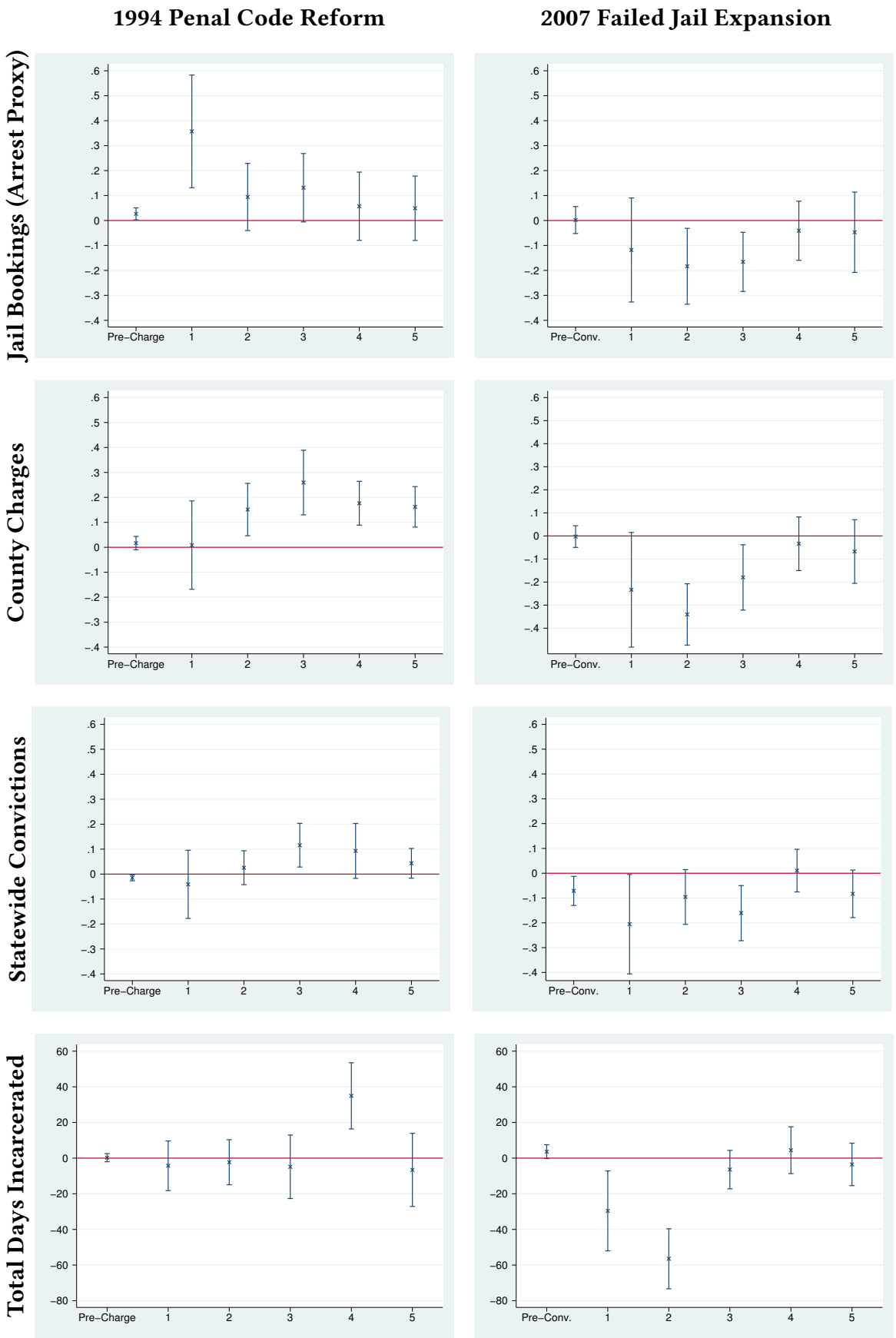


Figure 7: Estimated treatment effects over time, criminal justice outcomes

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.

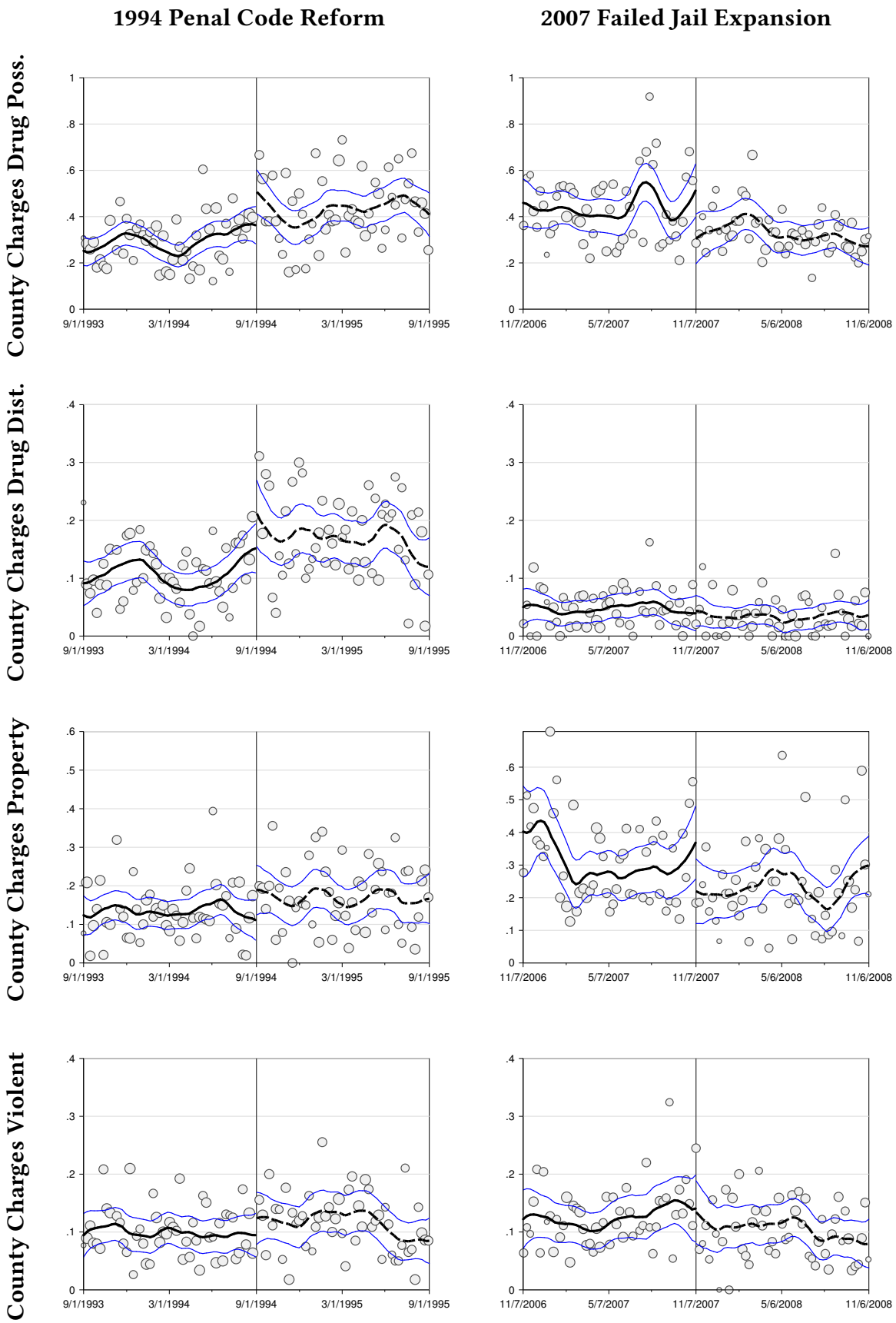


Figure 8: Five year recidivism outcomes, type of county charge

Note: This figure shows the relationship between the focal date on reoffending outcomes by type of county charge. General notes from Figure 2 apply.

Table 1: Discontinuities in background characteristics

	Caseload Density	Prior Misd.	Age	Male	Black	Hisp
1994: Post Reform	−0.853	0.146	−1.844	−0.037	0.060	−0.073
	(1.368)	(0.261)	(1.174)	(0.040)	(0.068)	(0.067)
Mean of Dep. Var. (Pre Reform)	9.35	0.64	29.67	0.75	0.55	0.18
Observations	5,193	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)	92 / 83	88 / 65	63 / 118	92 / 93	112 / 86	89 / 99
BW Bias (l/r)	157 / 126	139 / 119	127 / 206	169 / 175	178 / 129	154 / 151
2007: Post Reform	−1.416	0.101	0.034	0.029	0.050	−0.025
	(1.372)	(0.156)	(1.603)	(0.042)	(0.072)	(0.087)
Mean of Dep. Var. (Pre Reform)	12.38	0.61	30.42	0.75	0.36	0.31
Observations	5,128	5,128	5,126	5,128	5,128	5,128
Order Loc. Poly (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
BW Loc. Poly. (l/r)	64 / 114	108 / 76	76 / 94	157 / 98	113 / 82	85 / 68
BW Bias (l/r)	124 / 206	194 / 106	133 / 164	239 / 188	184 / 136	130 / 112

Notes: *** p<0.01, ** p<0.05, * p<0.10.Local-polynomial regression-discontinuity point estimates are bias-corrected and standard errors are robust and clustered at the week bin. Estimates rely on a Epanechnikov kernel and robust nonparametric confidence intervals from Calonico et al. (2014). Our primary specification also adjusts 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	0.346***	−0.339***	−0.025	0.130***	−0.072***
	(0.051)	(0.072)	(0.047)	(0.024)	(0.023)
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)	142 / 80	69 / 130	67 / 131	98 / 74	39 / 90
BW Bias (l/r)	225/151	124/195	125/216	169/148	108/154
2007: Post Reform	−0.250***	0.249***	0.013	−0.234***	0.046
	(0.092)	(0.092)	(0.039)	(0.088)	(0.037)
Mean of Dep. Var. (Pre Reform)	0.63	0.27	0.09	0.64	0.05
Observations	5,126	5,126	5,126	5,126	5,126
Order Loc. Poly (p)	1	1	1	1	1
Order Bias (q)	2	2	2	2	2
BW Loc. Poly. (l/r)	85 / 79	77 / 70	78 / 105	78 / 83	106 / 139
BW Bias (l/r)	152/134	137/128	163/174	141/139	171/202

Notes: *** p<0.01, ** p<0.05, * p<0.10.Local-polynomial regression-discontinuity point estimates are bias-corrected and standard errors are robust and clustered at the week bin. Estimates rely on a Epanechnikov kernel and robust nonparametric confidence intervals from Calonico et al. (2014). Our primary specification also adjusts 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	Statewide Convictions 5 years	Total Days Institutionalized 5 years
1994: Post Reform	0.547**	0.796***	0.263*	49.613
	(0.270)	(0.229)	(0.150)	(39.723)
Mean of Dep. Var. (Pre Reform)	1.73	0.99	0.50	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)	95 / 62	89 / 57	76 / 103	62 / 73
BW Bias (l/r)	169 / 131	151 / 121	123 / 191	113 / 148
2007: Post Reform	−0.622***	−0.992***	−0.442**	−82.556***
	(0.226)	(0.229)	(0.215)	(28.328)
Mean of Dep. Var. (Pre Reform)	1.65	1.35	0.90	188.35
Observations	5,126	5,126	5,126	5,126
Order Loc. Poly (p)	1	1	1	1
Order Bias (q)	2	2	2	2
BW Loc. Poly. (l/r)	48 / 57	63 / 96	104 / 133	58 / 69
BW Bias (l/r)	97 / 126	130 / 156	165 / 198	122 / 128

Notes: *** p<0.01, ** p<0.05, * p<0.10.Local-polynomial regression-discontinuity point estimates are bias-corrected and standard errors are robust and clustered at the week bin. Estimates rely on a Epanechnikov kernel and robust nonparametric confidence intervals from Calonico et al. (2014). Our primary specification also adjusts 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 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	0.205**	0.017	0.110*	0.027
	(0.095)	(0.055)	(0.061)	(0.034)
Mean of Dep. Var. (Pre Reform)	0.29	0.11	0.13	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)	70 / 65	87 / 138	87 / 132	130 / 95
BW Bias (l/r)	127 / 130	157 / 222	150 / 251	206 / 174
2007: Post Reform	−0.462***	0.004	−0.272***	0.025
	(0.121)	(0.027)	(0.096)	(0.066)
Mean of Dep. Var. (Pre Reform)	0.44	0.05	0.31	0.12
Observations	5,126	5,126	5,126	5,126
Order Loc. Poly (p)	1	1	1	1
Order Bias (q)	2	2	2	2
BW Loc. Poly. (l/r)	79 / 74	107 / 161	84 / 81	82 / 106
BW Bias (l/r)	154 / 121	174 / 233	148 / 130	149 / 164

Notes: *** p<0.01, ** p<0.05, * p<0.10.Local-polynomial regression-discontinuity point estimates are bias-corrected and standard errors are robust and clustered at the week bin. Estimates rely on a Epanechnikov kernel and robust nonparametric confidence intervals from Calonico et al. (2014). Our primary specification also adjusts 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, heterogeneous effects

	County Charges 5 years	County Charges 5 years	County Charges 5 years	County Charges 5 years	County Charges 5 years	County Charges 5 years	County Charges 5 years
	FULL	MALE	FEMALE	BLACK	HISP	AGE<30	AGE⩾30
1994: Post Reform	0.961***	0.882***	1.156***	0.675**	1.465***	1.362***	0.258
	(0.246)	(0.294)	(0.202)	(0.267)	(0.488)	(0.338)	(0.206)
Mean of Dep. Var. (Pre Reform)	0.99	1.06	0.77	1.20	0.71	1.23	0.72
Observations	5,193	3,860	1,333	2,758	925	2,765	2,428
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)	79 / 60	94 / 65	66 / 63	97 / 69	168 / 66	98 / 54	128 / 84
BW Bias (l/r)	138 / 119	154 / 125	121 / 119	161 / 138	241 / 120	160 / 102	182 / 166
2007: Post Reform	−1.107***	−1.251***	−0.053	−0.802	−0.790	−0.954**	−1.208***
	(0.256)	(0.269)	(0.449)	(0.571)	(0.495)	(0.392)	(0.214)
Mean of Dep. Var. (Pre Reform)	1.35	1.34	1.40	1.88	0.94	1.59	1.05
Observations	5,128	3,935	1,193	1,863	1,683	2,754	2,374
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)	47 / 67	59 / 66	96 / 140	148 / 129	81 / 67	71 / 78	57 / 123
BW Bias (l/r)	112 / 120	128 / 137	155 / 232	207 / 208	146 / 121	138 / 135	120 / 198

Notes: *** p<0.01, ** p<0.05, * p<0.10.Local-polynomial regression-discontinuity point estimates are bias-corrected and standard errors are robust and clustered at the week bin. Estimates rely on a Epanechnikov kernel and robust nonparametric confidence intervals from Calonico et al. (2014). Our primary specification also adjusts 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: Estimated Fuzzy RD treatment effects

	Jail Bookings 5 years	County Charges 5 years	Statewide Convictions 5 years	Total Days Institutionalized 5 years
1994: Def. Adj. of Guilt	−1.605	−2.390**	−0.780*	15.823
	(0.995)	(1.023)	(0.411)	(112.275)
Mean of Dep. Var. (Pre Reform)	1.83	1.04	0.56	364.60
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)	87 / 82	91 / 82	109 / 116	97 / 112
BW Bias (l/r)	153 / 127	149 / 129	215 / 184	144 / 182
2007: Def. Adj. of Guilt	−2.435**	−4.162**	−1.866*	−240.124**
	(1.172)	(1.677)	(1.069)	(106.442)
Mean of Dep. Var. (Pre Reform)	1.55	1.31	0.89	191.93
Observations	5,126	5,126	5,126	5,126
Order Loc. Poly (p)	1	1	1	1
Order Bias (q)	2	2	2	2
BW Loc. Poly. (l/r)	71 / 79	77 / 72	106 / 91	85 / 72
BW Bias (l/r)	126 / 135	131 / 126	161 / 145	148 / 133

Notes: *** p<0.01, ** p<0.05, * p<0.10.Local-polynomial regression-discontinuity point estimates are bias-corrected and standard errors are robust and clustered at the week bin. Estimates rely on a Epanechnikov kernel and robust nonparametric confidence intervals from Calonico et al. (2014). Our primary specification also adjusts 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: RD results using placebo samples

	Guilty	Deferred Adjudication of Guilt	Jail Bookings 5 years	County Charges 5 years	Statewide Convictions 5 years	Total Days Institutionalized 5 years
1993: Placebo	0.009	0.019	−0.126	−0.250*	−0.099	−6.716
	(0.046)	(0.054)	(0.212)	(0.146)	(0.128)	(34.668)
Mean of Dep. Var. (Pre Reform)	0.49	0.36	1.68	1.04	0.53	314.63
Observations	5,733	5,733	5,733	5,733	5,733	5,733
1995: Placebo	−0.010	−0.026	−0.084	0.040	−0.127	38.131
	(0.034)	(0.038)	(0.213)	(0.179)	(0.140)	(36.259)
Mean of Dep. Var. (Pre Reform)	0.68	0.17	2.15	1.47	0.87	344.60
Observations	5,060	5,060	5,060	5,060	5,060	5,060
2006: Placebo	−0.049	0.100***	−0.085	−0.164	0.082	−13.656
	(0.051)	(0.035)	(0.187)	(0.174)	(0.164)	(20.937)
Mean of Dep. Var. (Pre Reform)	0.59	0.29	1.68	1.41	0.93	205.56
Observations	9,866	9,866	9,866	9,866	9,866	9,866
2007: Placebo	0.022	−0.086	−0.279	−0.150	−0.182	58.259
	(0.098)	(0.105)	(0.265)	(0.191)	(0.177)	(35.730)
Mean of Dep. Var. (Pre Reform)	0.65	0.24	1.55	1.26	0.87	170.77
Observations	4,334	4,334	4,334	4,334	4,334	4,334
2008: Placebo	0.057	−0.005	0.192	0.148	0.008	50.964**
	(0.066)	(0.048)	(0.122)	(0.148)	(0.099)	(24.184)
Mean of Dep. Var. (Pre Reform)	0.47	0.39	1.39	1.03	0.67	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 standard errors are robust and clustered at the week bin. Estimates rely on a Epanechnikov kernel, two different mean squared error-optimal bandwidth selectors (below and above the cutoff) for the RD treatment effect estimator, and robust nonparametric confidence intervals from Calonico et al. (2014). Our primary specification also adjusts 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 8: RD results using alternative bandwidth selectors (1994)

	Guilty	Deferred Adjudication of Guilt	Jail Bookings 5 years	County Charges 5 years	Statewide Convictions 5 years	Total Days Institutionalized 5 years
1994: MSE1	0.357***	−0.354***	0.472*	0.723***	0.287*	50.792
	(0.059)	(0.069)	(0.261)	(0.218)	(0.156)	(36.890)
BW Loc. Poly. (l/r)	103 / 103	112 / 112	78 / 78	83 / 83	86 / 86	66 / 66
BW Bias (l/r)	166 / 166	182 / 182	164 / 164	176 / 176	171 / 171	143 / 143
1994: MSE2	0.358***	−0.334***	0.513*	0.817***	0.302*	47.686
	(0.059)	(0.076)	(0.283)	(0.251)	(0.163)	(37.664)
BW Loc. Poly. (l/r)	93 / 93	95 / 95	74 / 74	69 / 69	87 / 87	70 / 70
BW Bias (l/r)	167 / 167	152 / 152	138 / 138	128 / 128	153 / 153	130 / 130
1994: CER1	0.341***	−0.294***	0.690**	0.935***	0.246	121.047***
	(0.072)	(0.086)	(0.351)	(0.263)	(0.171)	(35.034)
BW Loc. Poly. (l/r)	67 / 67	73 / 73	51 / 51	54 / 54	56 / 56	43 / 43
BW Bias (l/r)	102 / 102	112 / 112	100 / 100	108 / 108	105 / 105	88 / 88
1994: CER2	0.335***	−0.285***	0.585	0.932***	0.309*	136.579***
	(0.061)	(0.083)	(0.382)	(0.294)	(0.172)	(34.046)
BW Loc. Poly. (l/r)	92 / 52	45 / 85	62 / 41	58 / 37	49 / 67	41 / 48
BW Bias (l/r)	138 / 92	76 / 119	104 / 80	92 / 74	75 / 117	69 / 91
1994: CER3	0.341***	−0.269***	0.719*	0.954***	0.244	118.245***
	(0.072)	(0.092)	(0.383)	(0.306)	(0.176)	(35.886)
BW Loc. Poly. (l/r)	60 / 60	62 / 62	48 / 48	45 / 45	57 / 57	46 / 46
BW Bias (l/r)	103 / 103	93 / 93	85 / 85	78 / 78	94 / 94	79 / 79

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Local-polynomial regression-discontinuity point estimates are bias-corrected and standard errors are robust and clustered at the week bin. Estimates rely on a Epanechnikov kernel and robust nonparametric confidence intervals from Calonico et al. (2014). Our primary specification also adjusts 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 (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 9: RD results using alternative bandwidth selectors (2007)

	Guilty	Deferred Adjudication of Guilt	Jail Bookings 5 years	County Charges 5 years	Statewide Convictions 5 years	Total Days Institutionalized 5 years
2007: MSE1	−0.246***	0.224**	−0.630***	−1.038***	−0.426*	−51.717*
	(0.090)	(0.087)	(0.220)	(0.232)	(0.218)	(26.902)
BW Loc. Poly. (l/r)	96 / 96	90 / 90	52 / 52	63 / 63	105 / 105	80 / 80
BW Bias (l/r)	141 / 141	138 / 138	101 / 101	129 / 129	162 / 162	161 / 161
2007: MSE2	−0.237***	0.235***	−0.646***	−0.978***	−0.406**	−67.956**
	(0.090)	(0.088)	(0.227)	(0.217)	(0.207)	(27.524)
BW Loc. Poly. (l/r)	88 / 88	75 / 75	45 / 45	70 / 70	119 / 119	74 / 74
BW Bias (l/r)	160 / 160	149 / 149	113 / 113	140 / 140	182 / 182	122 / 122
2007: CER1	−0.257**	0.288**	−0.591**	−1.097***	−0.670***	−98.260***
	(0.114)	(0.116)	(0.257)	(0.281)	(0.255)	(29.075)
BW Loc. Poly. (l/r)	62 / 62	59 / 59	34 / 34	41 / 41	69 / 69	52 / 52
BW Bias (l/r)	86 / 86	85 / 85	62 / 62	79 / 79	99 / 99	99 / 99
2007: CER2	−0.266**	0.271**	−0.547**	−1.053***	−0.645***	−122.364***
	(0.118)	(0.122)	(0.262)	(0.277)	(0.247)	(30.677)
BW Loc. Poly. (l/r)	56 / 52	50 / 46	31 / 37	41 / 62	68 / 87	38 / 45
BW Bias (l/r)	93 / 82	84 / 79	59 / 77	80 / 96	101 / 122	75 / 78
2007: CER3	−0.259**	0.268**	−0.576**	−1.144***	−0.622***	−121.661***
	(0.109)	(0.115)	(0.262)	(0.277)	(0.239)	(31.928)
BW Loc. Poly. (l/r)	57 / 57	49 / 49	30 / 30	46 / 46	78 / 78	48 / 48
BW Bias (l/r)	98 / 98	92 / 92	69 / 69	86 / 86	111 / 111	75 / 75

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Local-polynomial regression-discontinuity point estimates are bias-corrected and standard errors are robust and clustered at the week bin. Estimates rely on a Epanechnikov kernel and robust nonparametric confidence intervals from Calonico et al. (2014). Our primary specification also adjusts 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 (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 10: RD results without conditioning on covariate charactieristics

	Guilty	Deferred Adjudication of Guilt	Jail Bookings 5 years	County Charges 5 years	Statewide Convictions 5 years	Total Days Institutionalized 5 years
1994: Post Reform	0.361***	−0.321***	0.751**	0.961***	0.357**	87.347**
	(0.053)	(0.069)	(0.302)	(0.246)	(0.160)	(41.676)
Mean of Dep. Var. (Pre Reform)	0.34	0.52	1.73	0.99	0.50	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)	140 / 91	68 / 137	75 / 63	79 / 60	70 / 96	64 / 65
BW Bias (l/r)	219 / 165	121 / 205	142 / 124	138 / 119	117 / 176	116 / 134
2007: Post Reform	−0.242***	0.235***	−0.672***	−1.107***	−0.455*	−65.675*
	(0.084)	(0.084)	(0.231)	(0.256)	(0.245)	(36.136)
Mean of Dep. Var. (Pre Reform)	0.63	0.27	1.65	1.35	0.90	188.35
Observations	5,128	5,128	5,128	5,128	5,128	5,128
Order Loc. Poly (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
BW Loc. Poly. (l/r)	94 / 93	83 / 76	49 / 49	47 / 67	91 / 108	64 / 87
BW Bias (l/r)	168 / 148	153 / 134	109 / 106	112 / 120	153 / 155	127 / 144

Notes: *** p<0.01, ** p<0.05, * p<0.10. Local-polynomial regression-discontinuity point estimates are bias-corrected and standard errors are robust and clustered at the week bin. Estimates rely on a Epanechnikov kernel, two different mean squared error-optimal bandwidth selectors (below and above the cutoff) for the RD treatment effect estimator, and robust nonparametric confidence intervals from Calonico et al. (2014). These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016).

Table 11: RD results without bias correction or robust standard errors

	Guilty	Deferred Adjudication of Guilt	Jail Bookings 5 years	County Charges 5 years	Statewide Convictions 5 years	Total Days Institutionalized 5 years
1994: Conventional	0.354***	−0.346***	0.465**	0.715***	0.230*	33.097
	(0.044)	(0.059)	(0.225)	(0.194)	(0.130)	(31.162)
Mean of Dep. Var. (Pre Reform)	0.34	0.52	1.73	0.99	0.50	341.48
Observations	5,176	5,176	5,176	5,176	5,176	5,176
1994: Bias-corrected	0.346***	−0.339***	0.547**	0.796***	0.263**	49.613
	(0.044)	(0.059)	(0.225)	(0.194)	(0.130)	(31.162)
Mean of Dep. Var. (Pre Reform)	0.34	0.52	1.73	0.99	0.50	341.48
Observations	5,176	5,176	5,176	5,176	5,176	5,176
2007: Conventional	−0.246***	0.244***	−0.537***	−0.862***	−0.388**	−67.211***
	(0.082)	(0.081)	(0.183)	(0.195)	(0.183)	(23.540)
Mean of Dep. Var. (Pre Reform)	0.63	0.27	1.65	1.35	0.90	188.35
Observations	5,126	5,126	5,126	5,126	5,126	5,126
2007: Bias-corrected	−0.250***	0.249***	−0.622***	−0.992***	−0.442**	−82.556***
	(0.082)	(0.081)	(0.183)	(0.195)	(0.183)	(23.540)
Mean of Dep. Var. (Pre Reform)	0.63	0.27	1.65	1.35	0.90	188.35
Observations	5,126	5,126	5,126	5,126	5,126	5,126

Notes: *** p<0.01, ** p<0.05, * p<0.10. Local-polynomial regression-discontinuity point estimates rely on a Epanechnikov kernel and robust nonparametric confidence intervals from Calonico et al. (2014). Standard errors are clustered at the week bin. Our primary specification also adjusts 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). "Conventional" estimates omit both bias correction and robust standard errors. "Bias-corrected" estimates omit only robust standard errors.

Table 12: RD results after binning running variable by week

	Guilty	Deferred Adjudication of Guilt	Jail Bookings 5 years	County Charges 5 years	Statewide Convictions 5 years	Total Days Institutionalized 5 years
1994: Post Reform	0.393***	−0.369***	0.588***	0.757***	0.255	30.352
	(0.058)	(0.076)	(0.168)	(0.186)	(0.160)	(42.626)
Mean of Dep. Var. (Pre Reform)	0.34	0.52	1.73	0.99	0.50	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)	14 / 13	10 / 16	15 / 7	15 / 7	11 / 13	10 / 10
BW Bias (l/r)	25 / 24	19 / 26	28 / 15	24 / 16	18 / 26	17 / 21
2007: Post Reform	−0.397***	0.446***	−0.679**	−1.097***	−0.612**	−83.777**
	(0.098)	(0.067)	(0.282)	(0.333)	(0.239)	(35.276)
Mean of Dep. Var. (Pre Reform)	0.64	0.27	1.65	1.35	0.90	187.97
Observations	5,077	5,077	5,077	5,077	5,077	5,077
Order Loc. Poly (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
BW Loc. Poly. (l/r)	17 / 11	11 / 8	6 / 9	7 / 11	12 / 18	9 / 8
BW Bias (l/r)	30 / 19	22 / 16	15 / 20	16 / 21	20 / 29	19 / 15

Notes: *** p<0.01, ** p<0.05, * p<0.10. Local-polynomial regression-discontinuity point estimates are bias-corrected and standard errors are robust and clustered at the week bin. Running variable has been binned at the week level and the week of transition has been excluded from the estimation. Estimates rely on a Epanechnikov kernel and robust nonparametric confidence intervals from Calonico et al. (2014). Our primary specification also adjusts 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).