Diversion in the Criminal Justice System

Michael Mueller-Smith* Kevin T. Schnepel†

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Abstract

This paper provides the first causal estimates on the popular, cost-saving practice of diversion in the criminal justice system, an intervention that provides offenders with a second chance to avoid a criminal record. We exploit two natural experiments in Harris County, Texas where first-time felony defendants faced abrupt changes in the probability of diversion. Using administrative data and regression discontinuity methods, we find robust evidence across both experiments that diversion cuts reoffending rates in half and grows quarterly employment rates by nearly 50 percent over 10 years. The change in trajectory persists even 20 years out and is concentrated among young black men. An investigation of mechanisms strongly suggests that stigma associated with a felony conviction plays a key role in generating these results. Other possible mechanisms including changes in incarceration, other universal adjustments in policy or practice, and differences in criminal processing are ruled out empirically.

Keywords: felony records, convictions, criminal justice, race, recidivism, labor market

JEL classification codes: J24, K14, K42

* *mgms@umich.edu*, Department of Economics, University of Michigan

† *kevin_schnepel@sfu.ca*, Department of Economics, Simon Fraser University

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The U.S. criminal justice caseload, whether defined in terms of arrestees, defendants, or detainees, has expanded dramatically over the last half century. As of 2010, more than 8 percent of the adult population in the U.S. had a prior felony conviction, compared with just 3 percent in 1980 (Shannon et al. 2017). As caseload growth has challenged system capacity, legal and procedural strategies have emerged to focus efforts on the most serious offenders. Often this means showing greater leniency toward individuals posing minimal risk to conserve resources. One popular practice is a class of interventions referred to as *diversion* wherein public officials choose to pause, terminate, or divert someone's progression through the justice system, usually with the aim of avoiding a criminal record (Center for Health and Justice 2013).

Among felony defendants in the 75 largest U.S. counties in 2009, 9 percent of criminal charges resulted in a formal diversion agreement (Reaves 2013).[1] An additional 10 percent of charges avoid convictions through case dismissals "in the interest of justice" (Frederick and Stemen 2012), suggesting that the effective rate of diversion (broadly defined) is even higher.[2]

Diversion is not a uniquely American phenomenon. One-fifth of criminal cases in England, France, and the Netherlands are discharged after successful completion of a probationary period (Jehle et al. 2008). In New Zealand, one-third of first-time offenders participate in diversion-like programs that withdraw criminal charges if probation requirements are satisfied (Triggs 1998).[3] While the nomenclature and requirements of such programs differ both within the U.S. and internationally, providing offenders with a second chance to avoid the lifelong stigma of a criminal conviction record has become regular practice within court systems around the world (National Association of Pretrial Service Agencies 2009).[4]

Knowledge about how diversion impacts future behavior is limited.[5] Recent findings document a range of ways in which the criminal justice system may harm employment,

[1] Table A.1 provides details on court-focused diversion programs we identified from 38 states in the U.S.

[2] Around 25% of the total cases in 2009 were dismissed (Reaves 2013), which is often done in consideration of factors unrelated to guilt or innocence, or "in the interest of justice".

[3] Jehle et al. (2008) studied the function of prosecution services in six European jurisdictions (England/Wales, France, Denmark, Netherlands, Poland, and Sweden) through a structured questionnaire. A conditional discharge is typically an agreement where a criminal defendant can avoid a conviction by fulfilling certain requirements such as paying a fine or community service. The study also documents rates of charges dismissed by prosecutors "in the interest of justice", another form of diversion, which are highest at 17% of the caseload in France and Denmark.

[4] Precisely quantifying the fraction of cases diverted in the U.S. is not possible with current statistical reporting practices as recently recognized by leading legal scholars (LaFave et al. 2018).

[5] Chiricos, Barrick, Bales and Bontrager (2007) find that two-year recidivism rates among offenders with a court deferral agreement are significantly lower compared with convicted offenders using data from Florida. While the authors control for offender and county characteristics, estimates may suffer from bias since variation in conviction status is endogenous.

education, and reoffending outcomes (Dobbie et al. 2018, Stevenson 2018, Mueller-Smith 2015, Aizer and Doyle 2015, Di Tella and Schargrodsky 2013, Raphael 2014, Lovenheim and Owens 2014, Finlay 2009, Pager 2008, 2003). To the extent that diversion protects individuals from the negative consequences of the criminal justice system, it may improve an offender's self-sufficiency and minimize further criminal activity. Recent evidence on the effectiveness of specific deterrence (Bhuller et al. 2019, Hansen 2015, Owens 2009)—the prediction that a past experience of a more severe sanction can decrease the probability of future offending—however, suggests impacts in the opposite direction. These ambiguous theoretical predictions demonstrate the need for convincing empirical evidence.

In this paper, we present the first causal evidence on the effect of diversion in the criminal justice system and examine a range of recidivism and labor market outcomes. To accomplish this, we use a regression discontinuity (RD) design to evaluate two discrete changes in the use of diversion by criminal courts in Harris County, Texas. The first discontinuity follows a Texas penal code reform in 1994 that reduced diversion rates by 24 percentage points for offenders charged with certain reclassified drug and property offenses. The second discontinuity follows the unexpected failure of a 2007 ballot initiative that resulted in an immediate 18 percentage point increase in the diversion rate for low-risk defendants. These policy shifts are attractive from a research perspective due to their rapid implementation, which caused defendants to experience abruptly different case decisions depending on whether they were charged before or after the change in policy.

Through a fuzzy RD framework, we quantify the impact of diversion on future behavioral outcomes among marginal defendants. These *compliers* are a non-trivial fraction of the caseload overall and likely represent the most relevant population for policy makers considering expanding (or contracting) diversion's use. Scaling our outcome estimates by the change in diversion rates is especially useful in the context of this study given that we examine two different natural experiments with different first stage magnitudes.[6]

Our findings indicate that diversion substantially improves behavioral outcomes over a 10-year follow-up period among first-time felony defendants. The probability of any future conviction declines by approximately 45 percent and the total number of future convictions falls by 75 percent for both the 1994 and 2007 samples. Among defendants diverted in the 1994 sample, quarterly employment rates improve by 49 percent (18 percentage points) and total earnings over the ten-year follow up period grow by $85,365 (93%). While estimated

[6]Reduced form point estimates for all of our main results are provided, although these could generate misleading conclusions when comparing across the 1994 and 2007 experiments due to the changing size of the first stage.

impacts of diversion on labor market outcomes are not statistically significant for the 2007 sample, the direction and magnitude of estimates are suggestive of large benefits.[7] In the 1994 sample, we can show that these positive effects persist and expand even 20 years out, suggesting that diversion permanently changes one's lifetime trajectory. This finding likely relates to the composition of our main study sample: individuals charged with, but not yet convicted of, their first felony offense.

In the local context of our study, criminal court diversion is typically administered as a *deferred adjudication of guilt* (which we generally refer to as a "deferral"), meaning that the defendant avoids a formal felony conviction by undergoing a period of community supervision that operates just like felony probation. Unlike an expungement approach—a legal process resulting in the removal of an existing conviction from record—the defendant has never been legally convicted, which may help him or her avoid the many restrictions and penalties associated with prior felony convictions (National Reentry Resource Center 2018). In contrast to other potential court interventions like specialized drug or veterans courts, deferred adjudication requires minimal additional public resources since it relies on an established community supervision program (e.g. probation).

While diversion can be used to reduce the incidence of incarceration, our evidence indicates that is not the case for our marginal defendants. In 1994, defendants who are not diverted as a consequence of the penal code reform are instead convicted and sentenced to community supervision. Consistent with this, we observe no differences in time served in prison or jail during the initial 5 years of follow-up data. In 2007, we do observe a change in incarceration sentences associated with the uptick in diversion, but this does not translate into overall differences in actual time served during our primary outcome windows. A detailed, week-by-week analysis of time-served in the first follow-up year shows a short-lived temporal displacement of incarceration in 2007 associated with the change in sentences; however, cumulative exposure is equalized within 19 weeks and thus differential exposure to incarceration does not appear to explain our estimated long-run impacts of diversion on reoffending and labor market outcomes.

Instead, the results appear driven largely by the protective effect of avoiding a felony conviction record. Nearly half of the defendants who receive deferrals successfully complete their term of community supervision, receive a case dismissal, and maintain a clean criminal record. Moreover, approximately one third of the marginal defendants from the 2007 experiment are diverted with an outright case dismissal, guaranteeing no felony record at least initially. We

[7]Notably, the two independent experiments yield estimated treatment effects that are highly consistent, often within several percentage points.

find that the increases in employment rates are largely due to diverted individuals finding work in industries that are less accessible to individuals with criminal records. We also re-run our analysis on repeat offenders, who are already saddled with a felony record regardless of diversion status, and fail to replicate our findings from the first-time offender caseload. These results build on an extensive literature, largely centered around audit and correspondence research designs, that demonstrates the negative repercussions of criminal convictions in the U.S (Pager 2003, Pager et al. 2009, Agan and Starr 2017).

Using diversion to study the effect of felony convictions overcomes three important limitations of prior research. First, using a natural experiment instead of an audit design allows our study to generate estimates based on final labor market outcomes rather than callback rates, net of any sorting behavior to non-discriminating employers present in the labor market (Becker 1971). Second, in contrast to work considering ex-post interventions like record clearing or expungement (Selbin et al. 2017, Prescott and Starr 2019), research evaluating diversion can measure the change in trajectory of future outcomes caused by disposition status before potential irreversible damage has accrued to labor market experience or compliance with the law. Finally, unlike research examining the impact of offense reclassification (e.g. reducing a felony charge to a misdemeanor, see Bird et al. (2018)), this study examines variation in conviction status that holds the realized correctional supervision requirements constant.

Although our analysis advances this literature on felony convictions, there remains the possibility that other mechanisms may contribute to our estimated impacts. For instance, among those with a deferred adjudication, re-sentencing deterrence for one's original offense—similar to the mechanism observed in Drago et al. (2009)—may also discourage criminal activity during the initial follow-up period. In our sample, successfully completed deferral agreements last 2.86 years on average, and while we observe significant impacts during this period, our effects persist well beyond these first few years that carry an additional incentive to refrain from criminal activity. In order for this mechanism to explain our results entirely, short-run deterrence would need to generate long-term behavioral changes (see Bell et al. (2018)), which we cannot rule out with full certainty. We also find no difference in the treatment effect of deferrals relative to case dismissals (who would not be influenced by re-sentencing deterrence), a result that is inconsistent with this potential hypothesis.

Another possible mechanism is that prosecutors may be more likely to eschew new charges and/or convictions since revoking an existing deferral agreement may allow the more immediate imposition of punishment not requiring the same evidentiary burden as a new conviction. This behavior could account for the lower future conviction rates observed in our findings. Our results, however, are consistent across many different measures of recidivism including jail bookings (which proxy for arrests) and including non-technical probation revocations, indicating that mechanical changes in criminal processing play a minimal role in practice.

Furthermore, we do not measure significant discontinuities in observable demographic characteristics, prior criminal histories, or the density of the criminal caseload, allaying concerns regarding endogenous sorting or sample imbalance. We conduct a number of robustness tests to demonstrate that our findings do not rely on any particular RD specification or bandwidth. We show that there are no changes in behavioral outcomes in response to the discontinuities for other parts of the felony caseload to ensure our results are not driven by caseload-wide policy changes unrelated to diversion. We demonstrate the robustness of labor market outcomes to specifications that account for missing earnings records as well as those that eliminate potential incapacitation effects arising from incarceration. Finally, we rule out artifacts of seasonality based on placebo exercises that apply our sample criteria and RD strategies to defendants charged around the same discontinuity dates shifted forward or back by one year to Sept. 1, 1993; Sept. 1, 1995; Nov. 7, 2006; and Nov. 7, 2008.

The largest treatment effects are measured for young black men with a misdemeanor record—a group that exhibits the highest likelihood of future criminal involvement. Because the criminal justice system disproportionately affects this population in the United States (Shannon et al. 2017), expanding the practice of diversion could help reduce the broad socioeconomic inequalities recently documented by Chetty et al. (2018).

The remainder of the paper is structured as follows: Section 2 describes the two discontinuities in diversion rates in Harris County, Texas that form the basis of our research design as well as describes the process of diversion studied in detail; Section 3 addresses the administrative datasets used to perform the analysis; Section 4 outlines our regression discontinuity empirical strategy; Section 5 presents our results and provides evidence that supports our identification assumptions; Section 6 discusses identification threats and robustness checks; Section 7 highlights mechanisms that explain our pattern of empirical results; and Section 8 provides concluding remarks. The appendix provide results from additional robustness exercises, alternative specifications, and subgroup analyses.

2 Institutional Background

2.1 Diversion practices in Texas and the U.S.

Diversion practices vary within and across legal jurisdictions. While many programs target specific populations such as individuals with substance abuse problems, most states also have programs available to broad groups of criminal defendants (National Conference of State Legislatures 2017, Center for Health and Justice 2013). Table A.1 describes a variety of diversion programs that we identified from 38 states in the U.S. Among these states, 92% offer programs that are eligible to felony defendants, and 55% have programs initiated within criminal court proceedings (as opposed to pre-charge actions by prosecutor offices). While programs differ in size and nature, each is designed to help individuals avoid criminal conviction records. With a recent caseload size of nearly a quarter-of-a million individuals, deferred adjudication in Texas is one of the largest diversion programs in the world.

In addition to formal diversion programs, defendants regularly avoid criminal convictions through discretionary case dismissals "in the interest of justice" by judges and prosecutors. In practice, these outcomes serve a similar purpose as diversion since the aim is generally to facilitate a low-risk offender avoiding a criminal conviction record. A recent survey investigating prosecutorial decision making, Frederick and Stemen (2012), found that 10% of felony charges were dismissed with this goal in mind with around half occurring at the charge screening stage and half dismissed by the court during proceedings.[8]

While some defendants have their cases dropped with the intent of diversion in Texas, deferred adjudication agreements are the most frequently used diversion option in this local context. A deferred adjudication is an intermediate plea arrangement where defendants admit guilt without receiving a formal criminal conviction record and complete a probationary period of community supervision. It is broadly available across less serious felony offenses,[9] and usually targets first-time felony defendants. Charges relating to aggravated violence or sexual conduct are typically ineligible (Tex. Code Crim. Proc. §42A.101(a)).[10]

The duration of the required community supervision period can vary by defendant. A maximum length of 10 years of probation can be imposed for a deferred adjudication, but, on average, defendants are sentenced to 2.35 (3.87) years of community supervision in our 1994 (2007) study sample.[11] They join a larger community corrections caseload supervised by the Harris County Community Supervision and Corrections Department that includes felony convicts who have received a probation sentence. Observationally, deferred defendants'

[8]We are not able to quantify the fraction of case dismissals for diversionary versus other reasons in Harris County. However, the discontinuous increase in case dismissals following the 2007 failed jail expansion ballot initiative below suggests a non-trivial fraction of diversionary case dismissals in this jurisdiction since there was no change in evidentiary thresholds.

[9]Driving while intoxicated charges are ineligible for deferred adjudication by state law.

[10]While defendants can face charges for multiple offenses, it is very rare in this context that a defendant would be convicted of one charge and deferred for another. In fact, we only observe this situation for 0.002% of our estimation sample in 1994 (less than 50 individuals) and do not observe any such instances in 2007.

[11]The actual amount of time on community supervision can be reduced ex-post at the court's discretion, which is why the average length of completed agreements is lower than the noted sentence ranges.

experience of community supervision does not differ from that of their peers with official convictions.

All individuals sentenced to community supervision may be subject to judge-ordered conditions, including community service, a short period of confinement in a county jail, or assignment to a substance abuse treatment facility (Tex. Code Crim. Proc. Â§42A.301).[12] One basic condition for all community supervision is that the defendant does not commit any new criminal offense. When a defendant violates any condition of the community supervision—either through a technical violation or a non-technical (new offense) violation—a prosecutor can file a motion to revoke community supervision, which triggers an administrative hearing.[13]

If a defendant successfully completes the community supervision, a judge must dismiss the original offense (Tex. Code Crim. Proc. §42A.701(e)). The defendant's criminal history at no point includes a felony conviction unless the deferral has been revoked and he can legally claim to not have a felony conviction record (Court of Appeals of Texas 2006). We plot the standing of deferral agreements in the years following the case disposition for each of our samples in Figure 1. In the 1994 sample, approximately 40% of the deferral agreements are successfully completed and, in the 2007 sample, over 50% are successfully completed. Once the agreements are completed, there is no way to reverse the case dismissal.

Those with a deferral legally have to respond affirmatively to questions regarding past deferred adjudication agreements or pleading guilty to a crime,[14] although such non-conviction based questions are much less common in practice.[15] Official criminal background checks provided by state and county agencies cover individuals who have pled guilty to a felony offense, including those with deferred adjudications (Gaebler 2013). Since 2004, the Texas Department of Public Safety has provided online access to the Criminal Conviction History (CCH) database, where individual criminal histories can be acquired for a small fee through a search requiring identifying information such as name and date of birth. Prior to this period, access to the CCH and the quality of the data were thought to be more cumbersome and lower quality, which likely discouraged employer-initiated background checks (Finlay 2009).[16] In

[12]The short periods of confinement used in conjunction with an order of community supervision (sometimes referred to as *shock probation*) is our best understanding for why exposure to incarceration equalizes within several months in the 2007 sample in spite of the stark difference in sentencing. See Section 7.2 for further discussion.

[13]The implication of revocations in our analysis is further discussed in Section 7.2.

[14]Legally, a deferred adjudication agreement requires the admission of guilt.

[15]Vuolo et al. (2017) found only 3 job applications from a random sample of 416 that contained questions about criminal records including non-conviction outcomes; it is unknown what share of employers ask about deferral agreements in Texas.

[16]Based on a Freedom of Information Act Request in August 2019 to the Texas Department of Public Safety, we learned the following information: The TDPS CCH database was created 1976. Employers wanting

the context of our study, this would make the labor market effects in 1994 relatively stronger and in 2007 relatively weaker since the post-2004 diversion agreements would less effectively shield defendants from having a publicly searchable criminal record.

Since deferral is not considered a felony conviction in Texas (Tex Code Crim Proc. 42.12 Sec. 5), an individual will not lose the right to vote or access to occupational licenses that carry conviction restrictions.[17] While a deferral does not affect most civil liberties or government program eligibility, it can be used in the same way as a felony conviction for immigration deportation proceedings and could also be used to deny federally subsidized public housing. We summarize this comparison for important civil liberties and assistance programs in Table A.2.

2.2 The 1994 Penal Code Reform

In 1993, the Texas Legislature enacted a reform of its penal code that affected defendants who had committed offenses on or after Sept. 1, 1994.[18] The reform sought to "get smart" on incarceration through reducing sanctions for low-risk offenders while increasing time served before parole eligibility for aggravated violent offenders.

A major component of the reform was the creation of a probation-before-incarceration requirement for low-risk, first-time felony offenders. The specific legal wording of this provision, however, disincentivized prosecutors' use of diversion. At issue was the removal of the ability to threaten incarceration to ensure compliance with a diversion agreement, which prosecutors felt severely undermined its effectiveness. Under the new penal code, a second round of probation was required for defendants who violated their diversion agreements since they had not yet been legally convicted. The fact that the language of this provision weakened the credibility of diversion was raised by attorneys in October 1993, yet no action was taken to amend the statutory language before the law was implemented (Fabelo 1997).

Not all crimes were impacted by the probated incarceration requirement. The change affected only the lowest felony drug and property crimes, specifically possession of less than 1 gram of a controlled substance[19] and property offenses totaling less than $20,000 in damages.

to perform a criminal background check would need to make requests by phone or mail (associated costs were unable to be confirmed). In 1998, an initial secure online platform was launched, but the CCH underwent a significant overall in 2004 and was relaunched.

[17]There are a few legislated exceptions to occupational license restrictions noted below Table A.2.

[18]Two pieces of legislation accomplished this overhaul: Senate Bills 1067 and 532. In general, prosecutors in Texas are required to file charges within 48 hours of arrest, which limited the opportunity for manipulation around the introduction of this penal code reform.

[19]Controlled substances covered under these statutes include crack cocaine and cocaine as well as heroin, methamphetamine, and other serious drugs. Marijuana possession was covered by a separate part of the penal code and was not impacted by this specific change.

Diversion rates in the affected caseloads dropped significantly with the new penal code. In lieu of diversion, defendants were typically convicted and sentenced to probation as required by the statute. Both before and after the Sept. 1, 1994 cutoff, marginal defendants were subjected to community supervision; the main difference from the defendant's perspective was that before the cutoff, one could avoid a felony conviction whereas afterwards, a felony conviction was non-negotiable.

Deferred adjudication rates remained low throughout our two year sample window following the September 1, 1994 reform date but did begin to increase following an amendment in 1997 that provided judges the ability to directly sentence individuals with diversion revocations to incarceration (Fabelo 1997).

2.3 The 2007 Failed Ballot Initiative

During the 2000s, overcrowding in the Harris County Jail was a widespread concern. This local jail—which houses inmates with shorter sentences and serves several other functions including detention of individuals either waiting for trial or to be transferred to the state prison system—had up to 1,900 inmates sleeping on mattresses on the floor by 2005 (Hughes 2005). To address overcrowding, the county sought to expand capacity by 2,500 beds with $195 million to be raised through county bonds for construction of a new jail facility. Before issuing the bonds, the county first sought permission from local voters on the Nov. 6, 2007 ballot.[20]

A local campaign against the jail expansion combined with a large voter turnout led to a narrow and unexpected defeat of the initiative by a vote of 50.6 to 49.4 percent. This outcome was particularly surprising given that all the other local bonds were approved, and a $1 billion state-wide bond to expand state prison capacity was overwhelmingly 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 incarceration. However, some commentators explicitly placed the responsibility of the overcrowding problem on the courts in Harris County, citing an over-use of incarceration at the cost of taxpayer funds (Snyder 2007, Henson 2007a,b).

[20] The proposed jail expansion (Proposition 3) was part of a broader bond package put to voters in 2007 in response to the county's fast growing population. Together Harris County and the Port of Houston Authority added six local bond propositions to the Nov. 6, 2007 election ballot at a 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.

Most Harris County criminal courts exhibited a discernible change in their caseload outcomes immediately following the election. Diversion among first-time felony defendants increased by 18 percentage points, from less than 50 percent of the caseload to roughly 65 percent (Figure 2). It appears that the courts were responding to their critics given that both the district attorney and criminal court judges are publicly elected officials and would face the electorate in the coming year.[21]

The fact that the defeat of the ballot initiative was unanticipated limits the likelihood of systematic sorting. Furthermore, disposition dates are typically scheduled well in advance, constraining the ability to sort. As shown in Section 5, it does not appear that widespread sorting occurred around the election. Instead, it appears that some defendants were simply lucky to have been scheduled to be disposed after the election rather than before.

3 Sample Construction and Measurement of Outcomes

3.1 Administrative Data Sources

We rely on several county and state-wide sources of administrative data in this study. From the criminal justice system, these include criminal court records from the Harris County District Clerk, jail entry and exit data from the Harris County Sheriff's Department, state prison data from the Texas Department of Criminal Justice, and the Texas Department of Public Safety's Computerized Criminal History (CCH) database, which tracks state-wide convictions in Texas.

The Harris County criminal court records contain charges (felony and misdemeanor) and court outcomes for all adults between 1980 and 2017 regardless of the final verdict.[22] The records contain longitudinal case notes on major events in the court proceedings, including

[21] We have also considered an alternative theory for the 2007 change in conviction rates. On October 31, 2007, the Harris County District Attorney received a subpoena associated with the *Ibarras v Harris County Texas* civil lawsuit regarding improper coaching of defense witness testimony. On Nov 5, 2007, an inventory of his official e-mail was collected and some messages were improperly released to the public on December 22, 2007 showing the DA was having a romantic affair with his executive secretary (Rogers et al. 2007). Additional emails were released on January 8, 2008, which showed the DA had exchanged dozens of pornographic, racist and political emails on his office computer (Rogers et al. 2008), which led to his resignation on February 15, 2008 (Associated Press 2008). While the initiation of these events in the DA's office coincide with the 2007 cutoff we examine, it is unclear how they could lead to lasting changes in diversion practices in the courts extending out to 2009. Furthermore, we do not see any discontinous change in practice at key dates when e-mails were released (December 22, 2007 or January 8, 2008) nor at the time of DA Rosenthal's resignation (February 15, 2008).

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

initial and revised disposition and sentencing outcomes. We use this data to construct the core analysis sample, measure our source of identifying variation (date of charge or disposition), document first-stage diversion outcomes and their associated sanctions, and track future criminal activity. To assess the robustness of our recidivism measure, we also examine Harris County Jail bookings as a proxy for arrests and the state-wide CCH conviction database to evaluate spatial spillovers. While each data source has potential limitations,[23] the findings remain remarkably consistent.

We are also able to observe time spent in Harris County Jail and the Texas prison system. However, we cannot reliably link time served to a specific charge or conviction. As a consequence, any changes in realized incarceration could be the result of initial sentencing disparities as well as future criminal behavior.

The two Harris County datasets are linked using a unique county identifier tied to an individual's fingerprint known as the "system person number" (SPN). We match this data to state-wide data (CCH and the state prison records) using a defendant's full name and date of birth.[24]

To evaluate the impact of these different sanctions on labor market outcomes, we also match offenders to administrative earnings and employment data drawn from quarterly unemployment insurance wage records between 1994 and 2017 from the Texas Workforce Commission. Wage and employment records were matched to criminal justice records using a Social Security Number (SSN).[25] To reduce the influence of extreme outliers, we cap quarterly earnings at $50,000.[26]

[23] The Harris County Jail records can also contain multiple bookings associated with the same offense that may inflate the degree of criminal activity, and previous audits have found the CCH data to have incomplete state-wide coverage, particularly prior to the early 2000s, due to the voluntary nature of reporting to the CCH.

[24] In 1994 (2007), 73.8% (81.8%) of the first-time felony offender sample ever match to a valid jail spell and 46.4% (30.0%) match to a valid prison spell.

[25] Approximately 77% (75%) of the 1994 (2007) sample has a recorded SSN in the case management system. Individuals without a SSN on file were dropped from the labor market analysis. The unemployment insurance wage records do not contain name and date of birth preventing any form of probabilistic matching for those defendants without SSNs. Further analysis imputing missing labor market outcomes based on observable traits for those without recorded SSNs show no meaningful differences with our main labor market impact findings (see Section 6.2). There is a higher likelihood of a SSN not being collected for individual with case dismissals which we discuss in Section 5.1. The rate of missing SSNs for case dismissals is 27% compared to 23% for convictions and deferrals.

[26] This affects less than 0.025% of person-quarters observed in the data.

3.2 Sample Restrictions

We restrict our sample to first-time felony defendants charged two years before and two years after the Sept. 1, 1994 threshold and defendants disposed two years before and after the Nov. 7, 2007 threshold. Through imposing the first-time offender restriction, we ensure that each defendant will only appear once in our estimation sample. In addition, individuals without a felony record are generally the target of diversion programs in the U.S., therefore providing evidence for this group is particularly important from a policy perspective.

In 1994, we further limit the analysis to consider those charged with affected statutes as described in Section 2.2. In 2007, we focus on low-risk defendants defined as those facing a sentence of county jail, felony probation, or no correctional supervision, who would be most likely to be impacted by the 2007 ballot result.[27] We also apply a donut procedure in 2007 and drop all observations with disposition dates between October 30, 2007 and Nov. 9, 2007 to account for a transition period in diversion rates during which some courts appear to be responding contemporaneously to their public critics.[28,29]

3.3 Imputation for Missing Follow-up Data

Although this paper leverages a wealth of administrative records, the timing of the available data makes it infeasible to observe outcomes over a full decade for each individual in the analysis. In the 1994 sample, we are missing initial labor market outcomes for those charged prior to Jan. 1, 1994; in the 2007 sample, we cannot observe end-of-decade criminal justice and labor market outcomes for those disposed after Oct. 1, 2007 and Jan. 1, 2008 respectively. While the missing data represents a minor fraction of the cumulative 10-year outcomes, we do not want to ignore this issue as it could generate misleading trends in the graphical analysis as well as impact the bandwidth selection and bias-correction procedure in our RD analysis.

We impute the missing data using two types of panel models. In 1994, we backwards "forecast" missing quarters of earnings data using a Tobit model based on observed defendant traits (sex, race, age, and misdemeanor record) and earnings starting in the first quarter of

[27]There is no significant change in the likelihood of being in the high or low-risk group across the 2007 threshold, which should assuage concerns of endogenous sample selection.

[28]An examination of the micro-data shows that different courts appear to have reacted to the pre-election debate and ballot outcome at different points in time which together create a transition period of approximately two weeks from one practice to another in the overall caseload. This may also relate to the events occurring in the District Attorney's office described in Footnote 21.

[29]Table A.8 shows that eliminating this donut procedure reduces the size of the first-stage relationship in 2007 by 33 percent.

1994.

$$Earnings_{i,t} = \begin{cases} Earnings^*_{i,t} & \text{if} \quad Earnings^*_{i,t} > 0 \\ 0 & \text{if} \quad Earnings^*_{i,t} \leq 0 \end{cases}$$

$$Earnings^*_{i,t} = \alpha^{94} + \sum_{q=1}^{6} \beta_q^{94} Earnings_{i,t+q} + \sum_{q=1}^{6} \gamma_q^{94} Work_{i,t+q} + \delta^{94} X_i + \psi^{94} t + \epsilon_{i,t}$$

In 2007, we utilize an equivalent procedure except we forecast earnings based on lagged values.

$$Earnings^*_{i,t} = \alpha^{07} + \sum_{q=1}^{6} \beta_q^{07} Earnings_{i,t-q} + \sum_{q=1}^{6} \gamma_q^{07} Work_{i,t-q} + \delta^{07} X_i + \psi^{07} t + \epsilon_{i,t}$$

The imputed values are iteratively computed by: (1) applying the fitted values for the α, β's, γ's, δ's, and ψ's to generate a linear prediction of $Earnings^*_{i,t}$, (2) adding a random shock $\hat{\epsilon}_{i,t} \sim N(0, \hat{\sigma}^2)$ based on the model's estimate of the variance of the error term to avoid overstating the precision of the estimates, and (3) censoring the results $\widehat{Earnings}^*_{i,t}$ at zero to generate $\widehat{Earnings}_{i,t}$.[30]

To assess whether these imputations play an outsized role in our main results, we compute the contemporaneous impact of diversion on future convictions and employment separately by follow-up year.[31] We find that the level and trend of the contemporaneous impacts are consistent whether or not the follow-up data includes imputed values demonstrating the robustness of our findings to the exclusion of the imputation procedure.

4 Research Design

We first estimate the impact of the 1994 and 2007 policy changes on diversion rates using a sharp 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, 2017) to obtain bias-corrected point estimates using local linear functions, optimal bandwidths and valid confidence intervals. Formally, we estimate the discontinuity ($\hat{\tau}$) based on the following model:

$$\tau = \mu_+ - \mu_-,$$

[30] In the case of the imputed criminal justice outcomes in 2007, the model is slightly modified due to the relatively infrequent nature of criminal activity in the data. Instead, we estimate an annual data model based on the follow latent variable: $Total\,Convictions^*_{i,t} = \alpha^{07} + \sum_{y=1}^{3} \beta_y^{07} Total\,Convictions_{i,t-y} + \sum_{y=1}^{3} \gamma_y^{07} Any\,Convictions_{i,t-y} + \delta^{07} X_i + \psi^{07} t + \epsilon_{i,t}$.

[31] See Figure A.2.

where,

$$\mu_+ = \lim_{x \to 0^+} \mu(x), \quad \mu_- = \lim_{x \to 0^-} \mu(x), \quad \text{and} \quad \mu(x) \equiv E[Y_i | X_i = x].$$

The parameters μ_+ and μ_- represent the limit of the expectation of Y_i given X_i as it approaches the cutoff threshold from above and below respectively. As a result, τ should be thought to measure the magnitude of the jump in the outcome variable at the point of the discontinuity. In this notation, X_i is the running variable that has a cutoff threshold at $X_i = 0$, which generates a discontinuity in the outcome variable of interest (Y_i). Our running (or forcing) variable differs across the two quasi-experiments due to the nature of each change: the 1994 penal code reform affected offenders based on the date the charge was filed; the 2007 change in court behavior was based on the date the case was first disposed by the court (i.e., given a verdict).

We parameterize $\mu(x)$ using a local linear function:

$$\hat{\tau}(h_n) = \hat{\mu}_{+,1}(h_n) - \hat{\mu}_{-,1}(h_n), \quad \text{where,}$$

$$\left(\hat{\mu}_{+,1}(h_n), \hat{\mu}^{(1)}_{+,1}(h_n)\right)' = \arg\min_{b_0, b_1 \in \mathbb{R}} \sum_{i=1}^{n} 1(X_i \geq 0)(Y_i - b_0 - X_i b_1)^2 K(X_i/h_n), \quad \text{and}$$

$$\left(\hat{\mu}_{-,1}(h_n), \hat{\mu}^{(1)}_{-,1}(h_n)\right)' = \arg\min_{b_0, b_1 \in \mathbb{R}} \sum_{i=1}^{n} 1(X_i < 0)(Y_i - b_0 - X_i b_1)^2 K(X_i/h_n).$$

K is the kernel function that determines the weighting scheme within a given bandwidth, while h_n represents the size of the bandwidth itself. We opt for a data-driven bandwidth selector that selects the median bandwidth from three mean squared error-optimal methods for the RD treatment effect estimator,[32] and utilize the uniform kernel function. Coefficients are estimated using a first-order local polynomial that has been bias-corrected using a second-order local polynomial with robust standard errors based on a heteroskedasticity-robust plug-in residuals variance estimator with HC_2 weights. Our primary specification adjusts for baseline covariates of age, gender, race/ethnicity, and prior number of misdemeanor convictions.[33] These parameterization decisions can be modified without impacting the substantive findings of this study as documented in Table A.8.

After quantifying the "first-stage" relationship, we then turn to a fuzzy RD design to measure the causal effect of the diversion ($\hat{\phi}$) on court sanctions, future offending behavior,

[32] We use the option *msecomb2* within the STATA *rdrobust* command described by Calonico et al. (2017), 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.

[33] See Calonico et al. (2019) for notation of this methodology including baseline covariates.

and labor market outcomes. In this last approach, the measured change in outcomes is scaled by the observed first-stage relationship to quantify the impact of diversion. In doing this, we assume that each threshold influences the outcomes measured only through its impact on diversion (exclusion restriction) and that there are not individuals for whom the threshold is associated with an increase in diversion in 1994 and a decrease in 2007 (monotonicity assumption). The specific context and institutional details discussed in Sections 2.2 and 2.3 support these assumptions. Further validation of the exclusion restriction assumption is provided in Section 6. This fuzzy RD procedure estimates a local average treatment effect (LATE) for defendants who are on the margin of receiving or not receiving diversion in each experiment.

We also compute sample averages, which are presented with our estimates in brackets, to benchmark our results. For our sharp RD estimates, we present the overall caseload average in the bandwidth window prior to the cutoff. For our fuzzy RD estimates, we show an adapted control complier mean (CCM) (Abadie 2003, Kling et al. 2007, Heller et al. 2017) for the RD setting. The CCM is the average outcome for the *complier* population (i.e. those caused to be diverted due to the discontinuity) in the absence of diversion (i.e. convicted). Please see Appendix A.1 for a discussion of our application of this method to the RD context.

5 Empirical Results

5.1 Caseload Density and Baseline Characteristics

To attribute a causal interpretation to our RD estimates, we must assume that defendants are randomly allocated before and after each threshold. Individuals charged immediately before Sept. 1, 1994 should be observationally equivalent to those charged after, and we should not see a discontinuity in the total number of cases; likewise, defendants disposed immediately on or after Nov. 7, 2007 should be observationally equivalent to those disposed before, and we should not see a discontinuity in the total number of dispositions.

A sharp general deterrence response to the change in diversion rates would violate this assumption. Additional threats to our empirical strategy include changes in policing practices and sorting of offenders by prosecutors/judges across the threshold dates to guarantee they face one punishment regime versus the other. This is of particular concern in the context of the 1994 reform since all relevant actors could fully anticipate the adoption of the new penal code. Each of these threats generate a testable prediction of discontinuous changes in either the size or composition of the criminal caseload across the discontinuity.

In spite of these potential threats to identification, we do not observe discontinuities in caseload densities or in baseline defendant demographic characteristics such as gender, age, race, and ethnicity or in misdemeanor criminal history. Balance across these factors is presented in Table 1, with supporting graphical evidence available for summary measures in Figure A.1.

As an additional test, we calculate a predicted recidivism risk score for each defendant using the baseline characteristics and prior number of misdemeanor convictions.[34] Since no information from the running variable or discontinuity is used in constructing this index, the assumptions of the RD research design would imply that no sharp changes in the predicted risk of recidivism should appear at the threshold, which is what we observe empirically in Table 1.

Continuity in the caseload density, demographic composition, and predicted recidivism risk all strongly support our identification assumption of continuity in unobserved determinants of our outcomes across the threshold. We do, however, estimate a significant difference in missing a SSN in the 2007 sample. Upon further investigation, this difference appears to be driven by an increase in the proportion of case dismissals following the Nov. 2007 ballot, which have a higher likelihood of missing a SSN. While any imbalance in observable characteristics across the threshold is a concern, this specific imbalance will only impact our labor market analysis. In Section 6, we discuss empirical strategies for dealing with these missing records.

5.2 Court Verdicts and Sentencing Outcomes

The exact meaning of and counterfactual to diversion varies between the 1994 and 2007 experiments. Panel A of Table 2 reports the impact of diversion on case dispositions, while Panel B shows the effect on court-ordered sanctions. These point estimates are calculated using a fuzzy RD design and should be interpreted as the causal impact of diversion on disposition and sentencing outcomes.

In 1994, a diversion is entirely characterized as a deferred adjudication of guilt. We find a

[34] We calculate our measure of recidivism risk from the predicted dependent variables for each individual from the following OLS regression: Total $\text{Charges}_i^{10 \text{ Years}} = \alpha + \mathbf{X}_i'\beta + \epsilon_i$, where $\mathbf{X}_i'$ is the set of the observable covariates (i.e., age, sex, race/ethnicity, and prior misdemeanor convictions — not the forcing variable) as well as the corresponding two-way interactions. The risk score is defined as $\hat{\alpha} + \mathbf{X}_i'\hat{\beta}$ and captures an offender's predicted rate of recidivism over ten years based on his or her observable characteristics. If police or prosecutors act in a discriminatory manner and monitor certain sub-populations at higher than average rates (e.g., African American men), then an alternative interpretation of this index would be having a higher or lower likelihood of involvement with the criminal justice system whether through differences in actual future behavior or differences in future monitoring.

modest decrease in the likelihood of receiving an incarceration sentence or being issued a fine, but do not estimate any difference in the likelihood of probation.[35] As can be seen from the CCMs, marginal defendants not diverted receive a felony conviction and a probation sentence, which is consistent with the probation-before-incarceration requirement created after the 1994 cutoff. While we observe a decrease in the likelihood of an incarceration sentence on file for those diverted, it is unlikely that these sentence differences reflect changes to sanctions in practice, which we confirm through our analysis of jail and prison records.

In 2007, what we term diversion is split roughly 60% and 40% between deferred adjudications of guilt and outright case dismissals respectively. While these are clearly different disposition outcomes, we have combined these rulings under the broader umbrella of diversion given that both avoid a criminal conviction and decrease further involvement in the criminal justice system. By 2007, the probation-before-incarceration requirement had been eliminated, and the primary counterfactual to diversion is a felony conviction with an incarceration sentence. Here there is an unambiguous change in incarceration sentences as a consequence of diversion. Whether this translates into meaningful differences in time served depends on the length of sentence (which are relatively short by sample construction), parole practices (which we cannot directly observe), and other factors that could contribute to spending time in incarceration like pre-trial detention and future bookings. From Table 2, it is clear that diversion in 2007 leads to an increase in probation and/or a fine.[36] These estimated effects are less than a one-for-one exchange with incarceration due to the non-trivial share of defendants who received a dismissal-based diversion where no sentence would be applied.

Throughout our analysis, we test the equality between the two experimental estimates (the third column of tables reporting our RD results).[37] Testing the difference between the coefficients quantifies the differences and similarities between the 1994 and 2007 estimates, which helps us distinguish between potential mechanisms driving the causal effects. Given the differences in the implementation of diversion across 1994 and 2007, it is unsurprising that our tests of equality are rejected across the board for case dispositions and sanctions in Table 2. Across the two natural experiments, deferred adjudications of guilt are the predominant case disposition associated with diversion.

[35] Fuzzy RD estimates using the dollar amount of fines as an outcome variable imply that diversion in 1994 was associated with an insignificant decrease of $82 in fines on the margin.

[36] Fuzzy RD estimates on the amount of fines show that diversion in 2007 is associated with an insignificant increase of $130 in fines on the margin.

[37] Given that the samples are independent and separately estimated, the tests of coefficient equality are conducted as follows: $Z = \frac{\phi_{1994} - \phi_{2007}}{\sqrt{SE^2_{1994} + SE^2_{2007}}}$.

5.3 Reoffending and Labor Market Outcomes

Our main recidivism analysis, which evaluates the 10-year impacts of diversion on future convictions by extensive and intensive margins, by type of crime, and by offense level, are presented in Table 3. As with the disposition and sanction outcomes, the coefficients are estimated using a fuzzy RD design and represent the causal impact of diversion.

In both 1994 and 2007, we find strong and consistent evidence that diversion leads to statistically significant and economically meaningful declines in recidivism (at both extensive and intensive margins). Diversion decreases reconviction rates by slightly more than 30 percentage points in the 1994 sample and by 26 percentage points in the 2007 sample, which reflects close to a 50 percent reduction relative to the CCM. On the intensive margin, diversion reduces the total number of future offenses by 1.6 to 1.7 new convictions (75 to 77 percent). Perhaps surprisingly, we cannot reject that diversion has the same causal effect in the two separate analysis samples in spite of the practical differences between the two experiments raised in Section 5.2.

The change in the types of crimes prevented show similarities and differences between 1994 and 2007. Consistent across both experiments, we see significant reductions in property offenses in the range of 0.5 to 0.57 convictions over 10 years, which represent 84% and 89% reductions respectively. In 1994, it appears this is accompanied by lower rates of drug offenses: -0.71 convictions. In 2007, we observe statistically significant declines in violent offenses: -0.28 convictions. Despite these differences, we cannot statistically distinguish effects for specific types of crimes across the two natural experiments (as reported in the third column).

By level of the offense, it appears more felony-level crimes were prevented through diversion in 1994 (91 percent decline) whereas more misdemeanor-level crimes were prevented through diversion in 2007 (64 percent decline). But, here again, statistical tests of the equality of the sample-specific coefficients fail to reject the null.

Our primary 10-year labor market results are presented in Table 4. This includes performance measured on several dimensions: employment rates (Panel A), total earnings (Panel B), employment rates by industry type (Panel C), unemployment duration (Panel D), and earnings stability (Panel E). Across both experiments and each group of outcomes, the evidence consistently shows that diversion has a positive causal impact on measures of labor market performance.

We find that diversion increases quarterly employment rates by 18 (15) percentage points in 1994 (2007), indicating nearly a 50 percent increase over the CCMs. In other words, diversion increases the average number of quarters with positive earnings from 13.6 quarters up to 20.2

quarters in the complier population over ten years (averaging across the two experiments). These employment gains are not confined to occasional, very-low-income work; the second row of Panel A indicates that greater than 80 percent of the employment gains accrue in jobs with earnings exceeding the federal poverty level for a single adult.[38] Table 4 also documents improved earnings overall as shown in Panel B. We find that diversion increases log total earnings by 1.95 (1.96) log points, and total earnings by $85,365 ($41,437) in 1994 (2007). Taking into account that the CCMs are in the range of $90,000 to $100,000 over this ten-year period, the results represent substantial improvements for UI-covered income over baseline rates.

In Panel C, we investigate the industrial concentration of the employment impacts. Employment restrictions typically occur by occupation rather than by industry, but unfortunately occupational information in not collected in our employment records. We classify each two digit NAICS industrial sector as having low or high employment penetration among felony convicts using the regression adjusted ratios of the sector-specific employment rate for those with felony convictions versus those without in our analysis samples (see Table A.3). Sectors with ratios of 1.5 or less are deemed high penetration industries (e.g. Agriculture, Construction, Manufacturing, Accommodation and Food Services) and should be thought to be relatively more accessible to those with criminal histories. Sectors with ratios greater than 1.5 are low penetration industries (e.g. Retail Trade, Finance, Management, Educational Services, Health Care and Social Assistance) and should be thought to be difficult to access with a criminal history. While the precision of our estimates is limited, the results point towards larger benefits accruing in low penetration industries. We find increases of 70% to 82% relative to the CCM for employment in the low penetration industries compared to just 15% to 28% in high penetration industries. This suggests that diversion receipt may be improving labor market outcomes through access to jobs that would otherwise be unattainable with a felony conviction history.

Diversion might also impact employment and earnings through influencing job finding rates and labor market churn. To explore these mechanisms, we create two measures of employment gaps (time to first employment and duration of first observed unemployment spell) and two outcome measures of employment stability (longest duration of continuous employment with the same employer and longest period of continuous earnings). We find statistically insignificant decreases of 40 to 50% relative to the CCM for both of our job finding measures and statistically significant increases of 45 to 95% for both of our job stability

[38] Although this result is encouraging, it is important to bear in mind that this represents a relatively modest target of just $3,035 per quarter in 2018.

measures in both analysis samples. This suggests diversion is helping defendants match with better, more stable jobs rather than minimizing periods of unemployment.

The 1994 experiment generally exhibits stronger statistical precision throughout Table 4. In particular, the impacts to employment rates in 2007 are marginally insignificant, which may raise questions about the robustness of these conclusions. Several pieces of evidence, however, stoke our confidence in these findings. First, across each of the labor market outcomes, we fail to reject the null of treatment effect equality between 1994 and 2007 for all considered outcomes. Second, a number of alternative specification choices presented in our robustness exercises do yield statistically significant improvements in employment rates as a result of diversion in 2007 (see Section 6.2). Finally, the timing of the impacts presented in Figure 3, which will be discussed next, demonstrate marginally significant effects on employment rates in 2007 between Years 1 and 8. This suggests the impact at Year 10 is somewhat less precise but unlikely to be the product of random noise.

Figure 3 plots the estimated cumulative impact of diversion on three outcomes (total future convictions, the quarterly employment rate, and total days incarcerated) over time. The first coefficient corresponds to the impact during the initial 365 days post-charge or disposition, and each sequential estimate adds another year of data to expand the total follow-up window up through 10 and 20 years for the 2007 and 1994 samples respectively.

Both 1994 and 2007 plots show similar trajectories. We observe an immediate impact on criminal activity and employment that starts in the first year and grows year by year as the follow-up window is expanded. The first five years show particularly rapid improvements in crime reductions and employment gains, yet the benefits continue to accrue as far out as we are able to track outcomes. Although our main analysis focuses on 10-year outcomes, the persistent and growing impact up to 20 years after the first felony charge in the 1994 sample is quite remarkable. We interpret this pattern to indicate that diversion, at least at the critical juncture of someone's first felony charge, has the potential to fundamentally alter an individual's trajectory in life.

The final row of Figure 3 depicts the cumulative effect on time served in jail or prison. Given the previous discussion on sentencing impacts, it is useful to evaluate the effects on actual time served to assess whether changes to incarceration is a key mechanism for achieving diversion's impacts (see also Section 7.2 for more exercises examining this hypothesis). The plots do not show any significant differences in cumulative time served until more than 10 years out in the 1994 sample; no significant effect is ever observed in the 2007 sample. Figure A.2, which shows the contemporaneous rather than cumulative effects on incarceration, documents an emergent trend starting in Year 6 for the 1994 sample. These impacts appear to be the byproduct of future criminal activity rather than a delayed implementation of differences in original sentencing (see Figure 4 for the impacts on the accumulation of new sentences stemming from future crime).[39]

Our previous analyses consider criminal and labor market outcomes independently, however, the incidence of these impacts could fall on mutually exclusive subgroups, a common population or some combination thereof. To distinguish between these possibilities, we create four mutually-exclusive outcomes for the 10-year follow-up period. We consider whether a defendant's quarterly employment rate is above or below 50% during the follow-up period and whether they have zero or at least one new conviction during the follow-up period. The sample-specific and pooled results indicate substantial changes on the extreme outcomes (see Table 5). We estimate that diversion increases the fraction of individuals who jointly experience higher levels of employment and no future convictions by 20 (24) percentage points, a gain of 143 (218) percent over the CCM for the 1994 (2007) sample. At the same time, diversion reduces the fraction of defendants with low employment levels and at least one new conviction by more than 20 percentage points in each sample. It is hard to know whether these results reflect a major improvement for a narrow share of the marginal caseload or an incremental improvement for a broader group. Regardless of the interpretation, however, the impacts demonstrate substantial improvements following modest (if any) costs to the justice system.

5.4 Heterogeneity Analysis

Certain subgroups may particularly benefit from diversion. To explore this possibility, we examine whether our estimated impacts differ over the spectrum of predicted recidivism risk.[40] We prefer this approach to the typical sub-group analysis because of the correlation between demographic traits in our sample. For example, black offenders are roughly two years younger than white offenders, so subgroup estimates by race may actually capture differences by age.

We implement this analysis by separately estimating local polynomial RD specifications for each percentile in the risk score quantile function. Due to sample size constraints, we utilize a 40 percentile uniform bandwidth centered at the focal percentile when estimating these coefficients. Since the quantile function is not defined below zero or above one hundred,

[39] In Section 7.1, we define this potential indirect mechanism of diversion as an amplification effect.

[40] See Section 5.1 for a description of the construction of this index. To account for the bias discussed in Abadie et al. (2018), the estimation of the risk score employs a leave-one-out or jackknife estimation procedure for the purpose of these exercises.

asymmetric bandwidths occur above the 80th and below the 20th percentiles.[41] Figure A.3 shows the prevalence of background characteristics over the smoothed risk score quantile function. Individuals at the highest predicted risk of recidivism are more likely to be young, African American, male, and have prior misdemeanor convictions.

The first row of Figure 5 presents the first-stage relationship between diversion and the threshold across the 1994 and 2007 sample thresholds. The first stage magnitude remains fairly constant throughout the distribution.[42] The second and third rows report the 10-year impacts on total convictions and average employment rates. The largest point estimates appear at the top end of the distribution in both samples, although improvements in reoffending behavior also appear to show up among low-risk defendants as well. These results are striking in the fact that observable characteristics would predict that high-risk defendants would have worse future outcomes, yet we find that diversion effectively closes the employment gap between the 90th and median risk percentiles, and halves the re-conviction gap between these groups. The findings are also consistent with a more standard subgroup analysis reported in Table A.4.

Two related mechanisms could generate these results. First, young African American men may be over-targeted by law enforcement leading to a disproportionate treatment response in the marginal subpopulation. Second, the subpopulation may suffer more on average from a felony conviction record due to statistical discrimination or animus.

6 Evaluating Potential Violations of the Exclusion Restriction and Robustness Checks

6.1 Potential Violations of the Exclusion Restriction

An important assumption underlying our research design is that no other factor that could impact defendants' future behavioral outcomes changed across our two thresholds other than the decrease or increase in the probability of diversion. We have observed and discussed how sentencing outcomes did respond, which could be thought to represent a violation of the exclusion restriction. We disagree with this interpretation, however, as we view these

[41] This exercise requires the stronger assumption that defendants before and after the discontinuities exhibit similar risk score distributions. Further investigation indicates recidivism risk remains balanced within subsets of the risk score distribution supporting this strong assumption. Results available upon request.

[42] An implicit assumption of our fuzzy RD analysis is that felony convictions are moved exogenously to either deferral agreements or case dismissals (or vice versa). This assumption would be violated if some individuals were exogenously moved between our two diversion categories: deferred adjudications and case dismissals. While our assumption is not directly testable, the stability of our first stage impact across the risk score distribution provides reassurance that this assumption is not unrealistic.

sentencing shifts as derivative of the initial change in case dispositions and therefore part of the overall treatment effect of diversion rather than violations of our identification assumptions.

In contrast, a more concerning pattern would be if we observed changes in behavioral outcomes in other parts of the felony caseload where there was no change in diversion rates. In this scenario it would be difficult to differentiate whether our study's findings were caused by the change in diversion practice or some other caseload-wide phenomena affecting defendants near the cutoff threshold. Table 6 reproduces our main results for all other felony defendants excluding those used in our main analysis.[43] For both the 1994 and 2007 samples, we find no evidence of a change in future behavioral outcomes among other parts of the felony caseload and can thus reject that the reduced form change is equal to the results for our main study sample.

Unobserved seasonal breaks that coincide with the timing of our cutoffs such as the start of the school year or holiday season could be another route to violating the exclusion restriction. While we would also expect these patterns to cause differences among the non-focal caseload outcomes in Table 6, we provide additional support in Table A.5, which presents estimated effects of a placebo experiment where we adjust the cutoff dates to one year earlier (Sept. 1, 1993 and Nov. 7, 2006) or one year later (Sept. 1, 1996 and Nov. 7, 2008) than our actual threshold dates. Overall, we do not find any meaningful discontinuities in initial court outcomes, future reoffending, or future labor market outcomes in these placebo exercises.

Because we observe two discontinuous changes in the likelihood of diversion, we can pool the two samples, estimate an instrumental variables model and test for over-identifying restrictions to evaluate whether our instruments are uncorrelated with the structural error term. Panel A in Table 7 shows the estimated treatment effect of diversion using different specifications including variables for sample-specific intercepts, sample-specific universal trends, and sample-specific post-trends.[44] Across all specifications, our point estimates replicate our main results, and more importantly, we see strong failure to reject correlation between the instruments and the error term, which should further address concerns regarding violations of the exclusion restriction.

6.2 Robustness Checks

The main reoffending analysis is based on Harris County conviction records. Table A.6 demonstrates the robustness of our results to a variety of alternative recidivism measures:

[43] This combines repeat offenders as well as first-time offenders for non-impacted crime types. For results that specifically compare the estimates for first-time versus repeat offenders, see Table 8.

[44] The bandwidth is selected using a pooled RD model examining the sharp change in diversion.

county jail bookings, county charges, county convictions including non-technical violations, and state-wide convictions. There does not appear to be any significant difference in the point estimates depending on the measure of recidivism. Given differences in the CCM by outcome, with bookings having the highest prevalence and convictions the lowest, the percent change does increase the closer one gets to conviction, but these changes are statistically indistinguishable.

To address concerns regarding differential mobility out of Harris County, we compare Harris County and non-Harris County reoffending outcomes in Texas using the state-wide Computerized Criminal History (CCH) Database. If diversion displaces criminal activity to other counties in Texas, we should expect to see a negative impact in Harris County and a positive impact elsewhere in the state. By contrast, however, the results show a decline in reoffending both within and outside Harris County. Although the Harris County-specific convictions make up the majority of the observed impact in the state-wide CCH data, the effects elsewhere in the state suggest that our county-specific measures, if anything, underestimate the total gains from diversion.[45]

We also challenge the robustness of our labor market analysis. To do so, we assess whether the missing earnings information for those without a SSN recorded in the case management system may bias our findings. This is particularly important for the 2007 analysis where we see a small but discrete jump in the likelihood of having a SSN recorded. Table A.7 shows the estimated impacts for employment rates, total earnings, and log total earnings under two assumptions. In the first column, we drop all observations that have a missing SSN, which is a reproduction of our main estimates. In the second column, we impute labor market outcomes for defendants with missing SSNs based on their observable characteristics and the timing of their charge or disposition. The results show no meaningful change regarding labor market activity, suggesting the missing SSNs are not significantly confounding our findings.

Finally, our results are also robust to alternative data-driven bandwidth selection methods (Table A.8), which is not entirely surprising given the stability of the estimates over a range of fixed bandwidths starting from 90 days up to 720 days (see Figure A.4).[46] They are also robust to the use of alternative variance estimation strategies, and Table A.8 reports results for several other modifications including models without the donut restriction, without including

[45] While these state-wide results partially alleviate mobility concerns, there could be differences in defendants being forced to leave Texas. If Hispanic immigrants are more or less likely to be deported, for example, this could bias our estimated effects. However, we expect this bias to go in the opposite direction of our estimated effects on reoffending outcomes since those receiving a felony conviction would be more likely to be deported compared with those with a diversion.

[46] For reference, the vertical red line in Figure A.4 indicates the position of the optimal bandwidth chosen by the Calonico et al. (2014) methodology for our primary results.

covariates, with alternative kernels, and without using the robust standard errors or bias correction as suggested by Calonico et al. (2014). We also provide estimates from models based on outcomes aggregated to the week level. Among these tables, there is no evidence to suggest our findings are driven by any choices we made in implementing the RD methodology.

7 Discussion of Potential Mechanisms

An important question remains: how does this low-cost intervention generate such large and long-term improvements? The magnitude of our estimates are particularly surprising given that prior evaluations of reentry services (as recently reviewed by Doleac (2018)) or summer jobs (Heller 2014, Gelber et al. 2015) have failed to yield impacts with similar magnitudes or degrees of persistence despite requiring substantially more resources.

7.1 Mechanisms Supported by the Data

Growing evidence suggests that a felony conviction carries sharp penalties for employment opportunities (Raphael 2014, Finlay 2009, Pager et al. 2009, Pager 2003). A recent nationally representative survey found that over 70 percent of those who applied for a job in the past year were asked whether they had a criminal record at some point during the job search (Denver et al. 2018). A stigma effect may also exist within the criminal justice system if police, prosecutors, or judges react to prior felony convictions. This may operate mechanically due to penal code escalations or risk classification algorithms that depend on criminal conviction history.[47] Our analysis sample is composed of individuals with no prior felony charges, and so diversion can safeguard them against the lifelong mark of a felony record.

Our sector-specific employment analysis already provides strong evidence to suggest diversion helps defendants avoid the felony conviction stigma. Another natural way to evaluate this channel is to compare our estimated impacts with the corresponding coefficients for defendants already holding a felony record. Diversion, however, is rarely offered to those with a prior felony, which limits our ability to implement this strategy. Table 8 compares our main RD estimates (both sharp and fuzzy) with the corresponding coefficients for defendants with 1 to 3 prior felony convictions. The exercise suggests that defendants without a felony

[47] A deferral can legally be used by a judge in a subsequent punishment proceeding in Texas (Tex Code Crim Proc. 42A.111). We do not have evidence, however, of how often this is the case. In either case, certain types of prior convictions more often trigger aggravated (elevated) charges upon re-offense which results in more severe and potentially more certain future convictions. Additionally, defendants under varying forms of supervision may face differing probabilities of arrest conditional on criminal activity. We are not aware of any empirical evidence that confirms this to be the case, however.

record disproportionately benefit from diversion, which would be consistent with the stigma mechanism.[48]

The second direct channel that we find evidence for in the data is a form of deterrence that we term *re-sentencing deterrence*. If a defendant violates his diversion agreement, he can be convicted and sentenced at that time for the original offense.[49] This threat would discourage reoffending during the agreement period, which lasts on average 2.86 years in our sample, especially for minor non-felony crimes.[50]

The estimated behavioral impacts of diversion remain persistent well after the expiration of the deferral agreements, indicating that re-sentencing deterrence may play a secondary role relative to to felony conviction stigma. In order for re-sentencing deterrence to explain our results entirely, short-run deterrence would need to generate long-term behavioral changes (Bell et al. 2018), which we cannot rule out with full certainty. Results presented in Panel B of Table 7 show, however, that we cannot reject equality in the estimated treatment effect of deferred adjudication and case dismissals where no re-sentencing is possible due to double jeopardy.[51] While not definitive, it does strongly suggest felony conviction stigma is the more consequential of the two potential direct mechanisms.

An indirect mechanism, which we term an *amplification effect*, likely also contributes to the long-run impacts. Any initial changes to criminal or labor market activity may dynamically influence future outcomes. For instance, if a felony conviction causes additional criminal activity and this results in additional sanctions against the individual, the initial treatment differences between diversion and conviction dispositions may be reinforced or amplified. Stated differently, the dosage of justice system involvement may grow over time as a consequence of the future behavioral outcomes.

Figure 4 shows direct evidence that amplification is plausible. This figure tracks the cumulative impact of diversion on new sanctions for future criminal activity and shows

[48]This exercise is limited by two key factors. First, in 1994, there is no discernible first-stage relationship. Consequently, there is no way to compute treatment effect estimates for this cohort. In 2007, we do observe a statistically significant yet modest first-stage relationship in the repeat offender caseload. In fact, the point estimates suggest that diversion does not generate the same gains for this population, however, large standard errors, which are partially a consequence of the modest first stage, limit our ability to make this claim definitively.

[49]This only applies to individuals who receive a deferred adjudication of guilt disposition.

[50]A new minor misdemeanor conviction (e.g., shoplifting) would result in a comparatively outsized sanction (felony conviction record and potentially time in prison) due to the sentence imposed by the violation of the deferred adjudication agreement, whereas a new serious felony conviction (e.g., aggravated assault) would trigger a felony conviction and harsh sanctioning regardless of whether a deferred adjudication was active at the time.

[51]This exercise (described in further detail in Section 6.1) relies on the strong assumption that diversion exhibits a common treatment effect in 1994 and 2007 compared with case dismissal.

that diversion decreases the total number of future incarceration and probation sentences. The effects are larger for incarceration, but both impacts grow over the follow-up period which parallel the cumulative impacts to behavioral outcomes. The precise nature of an amplification effect is hard to pin down, though. Evolving differences in incarceration raise the possibility of incapacitation and post-release effects. Changing labor market opportunities alter the opportunity cost of criminal activity. A host of other interventions and incentives may also come into play. Without further structure imposed on the analysis, we are not able to disentangle these channels.

7.2 Potential Mechanisms Not Supported by the Data: Incarceration, Fines, and Probation Revocations

A number of alternative channels could theoretically explain our results but are not supported by the data. For instance, mechanisms implying a change in the composition of offenders at the threshold dates such as a general deterrence effect from the changes in expected punishment or any manipulation of charge or disposition dates are empirically ruled out in Section 5.1.

Our analysis of sentencing outcomes raises incarceration as a potential mechanism. Vetting this hypothesis is critical for an accurate interpretation of our findings. Section 5.3 raises evidence that the change in incarceration sentences at our discontinuity plays a minimal role in generating our observed behavioral impacts.

The null impact of diversion on time served in jail or prison for the 2007 sample is surprising though given the clearly documented change in sentencing outcomes and the related overcrowding concerns in the Harris County jail. We further investigate this issue in Figure A.5 using high frequency (weekly) data on time served in the first year following disposition. The week-by-week results yield additional insight missed by the prior annual estimates: consistent with the change in sentencing outcomes for the 2007 sample, diverted defendants are significantly less likely to be incarcerated immediately following disposition. However, these defendants start experiencing higher rates of incarceration roughly two months after disposition, which effectively equalizes cumulative exposure to incarceration over time; in fact, there is no statistical difference in cumulative exposure to incarceration by the fourth month. While deferral revocations could partially contribute to this pattern, our analysis of diversion durations shows that only 5 percent of deferments are typically revoked by the three-month mark.[52] Instead, this pattern is more consistent with the use of shock probation—

[52]See Figure 1.

short periods of incarceration scheduled in the beginning of community supervision or applied by a judge to punish minor technical violations without triggering a revocation. In summary, we observe only a temporal displacement of short incarceration spells from immediately after disposition to a few months following disposition and no statistical difference in cumulative exposure within 4 months of disposition for the 2007 sample.[53]

It is plausible that longer-term (>5yrs) employment and offending differences following the 1994 change in diversion are affected by incarceration patterns. This conjecture is consistent with the evidence on amplification previously discussed. An incapacitation channel would dampen the difference in reoffending outcomes but could exacerbate differences in employment and earnings. We quantify the potential role of incarceration-related incapacitation in our employment effect estimates in Table A.9. The table reports estimates where we recode employment status during quarters of incarceration to shut down the mechanical incapacitation channel. In column 2, we recode employment status to be equal to the average employment rate in the year prior to the current incarceration spell. In column 3, we recode employment status to be equal to 1 for all quarters with some incarceration. In this last exercise, we make the strong and arguably disproven assumption that all inmates in the absence of incarceration would have secured gainful employment in the formal sector (see Mueller-Smith (2015)). While our results decrease if we assume the counterfactual to incarceration is guaranteed employment, our effects are very similar if we make the more realistic assumption that employment rates would have persisted at pre-incarceration rates in the absence of incarceration.

We also find that diversion significantly impacts financial penalties, yet the direction of the effects go in opposite directions in 1994 as opposed to 2007. Unless the causal effect of financial sanctions changed signs in the 13 years between the two experiments, it is unlikely this channel is of first order importance in generating the consistently signed behavioral impacts across the two experiments.

Another potential mechanism by which diversion decreases future convictions could be through the treatment of non-technical violations and the revocation process. For example, if a deferred defendant commits a new criminal offense while under community supervision, the prosecutor can file for a motion to revoke probation which may result in an incarceration sentence that overshadows any sanction from separately pursuing a conviction for the new criminal offense. This type of behavior could suppress future conviction rates for defendants

[53] A parallel exercise for the 1994 sample in Figure A.5 yields results that are consistent with our understanding that the marginal defendants received community supervision regardless of their diversion status. We find no statistical difference in exposure to incarceration in the weeks following the charge for those diverted due to the 1994 penal code reform and no statistical difference in cumulative exposure throughout the entire first year.

under community supervision. To assess this concern, we incorporate case file data that captures probation and deferral revocations and evaluate whether our results hold after adding such revocations as "uncharged convictions" into our measure of recidivism.[54,55] Such adjustments, however, leave our recidivism impacts substantively unchanged (see Section 5.3) suggesting this channel plays a minimal role.

As discussed in Section 2.1, a felony conviction can impact certain civil liberties and eligibility for public assistance programs. Given low voter turnout rates and low levels of higher education enrollment among our individuals in our sample, especially those most at risk for recidivism for whom we find the largest effects, we find it implausible that differences in voting rights or pell grant eligibility play an important role in generating our results. Tuttle (2019) finds higher rates of recidivism among individuals banned from food stamp receipt for drug felony convictions. Within our analysis, the ban on welfare and food stamp assistance would only affect those convicted of drug felony offenses in the 2007 sample.[56] Given the consistency of our results across both the 1994 and 2007 samples and the fact that we observe larger declines in future convictions among non-drug offenders who are not subject to these bans in the 2007 sample,[57] our results do not point towards this channel as critical.

8 Conclusion

This paper studies two discontinuities in criminal court diversion — a cost-saving strategy that offers defendants a second chance to avoid a felony conviction record — among a large population of low-risk offenders in Harris County, Texas. While these two changes occurred 13 years apart and originate from different contexts, we find large and consistent impacts from both experiments: future recidivism roughly halves and employment rates improve by around 50 percent. To the best of our knowledge, no prior research has found similarly large and long-lasting effects for such a low-cost intervention in the justice system. Comparing these effects with those from the Mueller-Smith (2015) evaluation of the causal impact of

[54] While we do not observe the underlying reason for revocation (technical violation or a new offense), we convert a fraction of revocations based on rates of non-technical (criminal) community supervision revocations provided by the Texas Legislative Board (TX Legislative Budget Board 2005, 2008). This is a conservative approach given that many non-techincal revocations were actually separately prosecuted in practice and, as a result, would be double counted as both revocations and new convictions in this exercise.

[55] TX Legislative Budget Board (2008) reports that 36.8% of the 2007 revocations for felony community supervision arrangements were for non-technical (criminal violations). We were unable to find a similar statistic for Harris County in 1994 or 1995 and therefore use the state-wide average non-technical revocation rate of 45% between 1999 and 2005 as reported by TX Legislative Budget Board (2005).

[56] As noted in Table A.2, the food stamp ban only applied to drug felony offenses after August 22, 1996 which is well outside the bandwidth used around the 1994 change in diversion for our results.

[57] See Table A.4.

incarceration in Texas on employment and future criminal offending, our estimates imply that the impact of a conviction (relative to diversion) is similar to the impact of around 4 years in prison (relative to no incarceration).[58] A large impact of diversion on employment rates is perhaps not surprising thought considering recent evidence that job applicants with felony conviction records are 63% less likely to receive a call back for an interview compared to those without a record (Agan and Starr 2017).

An extensive empirical investigation of theoretical channels points to the stigma associated with a felony conviction as the focal mechanism. This finding contributes substantially to prior studies that discuss the impact of a prior conviction on re-offending and labor market outcomes. In particular, our estimates suggest that the negative impact of a conviction on employer callback rates, as found in the extensive audit-based literature, extends to actual employment and earnings outcomes and persists in spite of potential search behavior by convicts to match with non-discriminating employers.

A heterogeneity analysis demonstrates a striking pattern: those at the highest risk of recidivism gain the most from diversion. These individuals are typically young black men with one or more misdemeanor convictions, a group often discussed as over-policed in the United States. Our results indicate that intervening for this disadvantaged population at a critical moment (i.e., when they are being charged with their first felony offense) could significantly improve their life course. More broadly, these results suggest the criminal justice system likely plays a crucial role in the propagation of racial inequality in the U.S.

In contrast to prior work on diversion, our research design addresses selection bias without relying on controlling for observables or propensity score matching. The results are shown to be robust to a variety of specification choices and bandwidths. A number of exercises support the viability of our exclusion restriction assumption. The findings are replicated in two independent samples which together exhibit both increasing and decreasing rates of diversion. As such, it is unlikely that our analysis is biased by some unobserved shock contemporaneous with the discontinuous changes in diversion.

In 2010, annual justice expenditures eclipsed $250 billion in the U.S. (Kyckelhahn 2010). Given the impact on federal and state budgets, substantial efforts have been made to reform criminal justice policy. The general trend has been toward more leniency, especially for first-

[58] Mueller-Smith (2015) finds that each additional year of incarceration generates a reduction in post-release employment by 3.6 percentage points and increases post-release reoffending by 4 to 7 percentage points. We estimate that diversion generates an increase in employment of 18 (15) percentage points among the 1994 (2007) sample and a reduction in offending of 31 (26) percentage points for the 1994 (2007) sample. Extrapolating the results from Mueller-Smith (2015) to 4 years of prison is well beyond the identifying variation in the paper and should be interpreted with caution.

time and low-risk defendants, with diversion emerging as a popular option. Our results suggest that these changes may lead to lower rates of reoffending and higher rates of rehabilitation in the coming years.

To limit the stigma of a felony conviction in labor markets, "Ban-the-Box" policies that prohibit questions about criminal histories on employment applications have now been widely adopted across the U.S. (Denver et al. 2018).[59] Recent evaluations, however, suggest that such policies have failed to benefit individuals with conviction records (Rose 2017, Jackson and Zhao 2017).[60] Together with our findings, these results suggest that reducing the visibility of a felony conviction is not a close substitute to avoiding a felony conviction altogether.

Some policymakers, however, have sought to reverse course and reduce leniency (Sessions 2017). This could reflect an alternative, risk-averse perspective that seeks to minimize the possibility of high-cost rare events (e.g. inadvertently diverting a to-be murderer) even at the cost of average gains. Our study is not well-equipped to quantify the impacts of diversion on these rare events. We do find a decline in violent crime overall, however, which should be attractive to those coming from this perspective.

While much has been recently written on ineffective criminal justice policy in the U.S., this paper provides compelling evidence on a successful intervention that both improves defendant outcomes and saves public resources. What we find most appealing about diversion is that it can be feasibly implemented without significant investments or changes to current infrastructure, making it a practical solution for criminal justice reform. While it is not clear whether individuals with a prior felony conviction would similarly benefit from diversion, the results are clear for those facing their first felony charge. For all these reasons, diversion should be viewed as a promising option for jurisdictions seeking to reduce the fiscal cost and potential negative community impacts of the criminal justice system.

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[59]Houston city departments passed a ban-the-box policy effective in April 2015 for city workers `http://www.justicepoliciy.org/news/8915`[Accessed 9/18/2019]. We do not find a substantial change in employment outcomes around the eighth year for our 2007 sample in Figure A.2 which would correspond to the implementation of this policy for Houston city departments.

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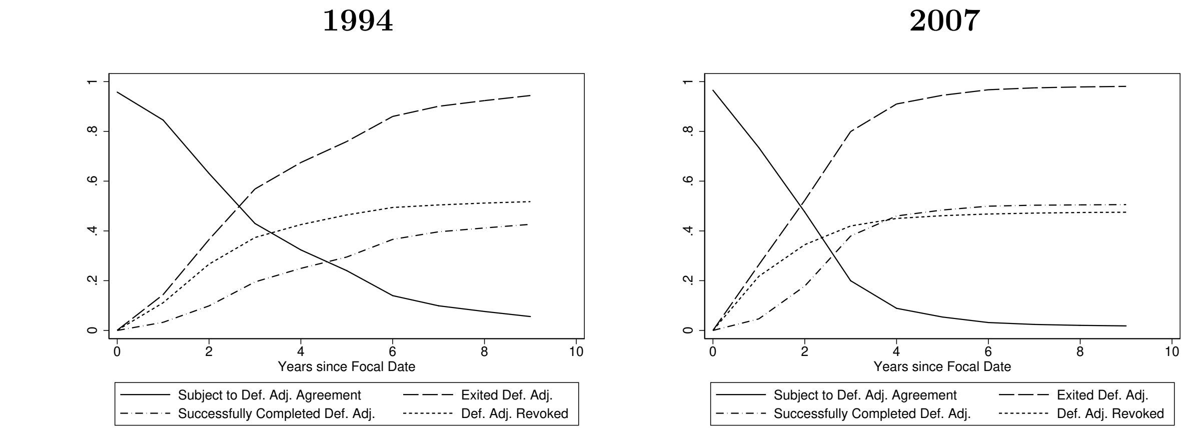


Figure 1: Duration of Deferred Adjudication Agreements

This figure describes the duration of diversion contracts (and associated community supervision) for those in our study sample whose case disposition is a deferred adjudication of guilt. The duration of completed agreements varies across individuals with an average length of 3.87 years for the 1994 sample and 2.35 years for the 2007 sample. Courts can provide early termination of agreements which is why durations can be shorter than sentenced probation lengths. The total proportion of active deferral agreements each year following the case disposition are depicted by the solid line and, it's converse, the proportion of agreements that are no longer active is shown by the long-dashed line. We also plot the cumulative fraction of agreements that have successfully completed without violation (dash-dot line) and those revoked due to a violation (short-dash) over time.

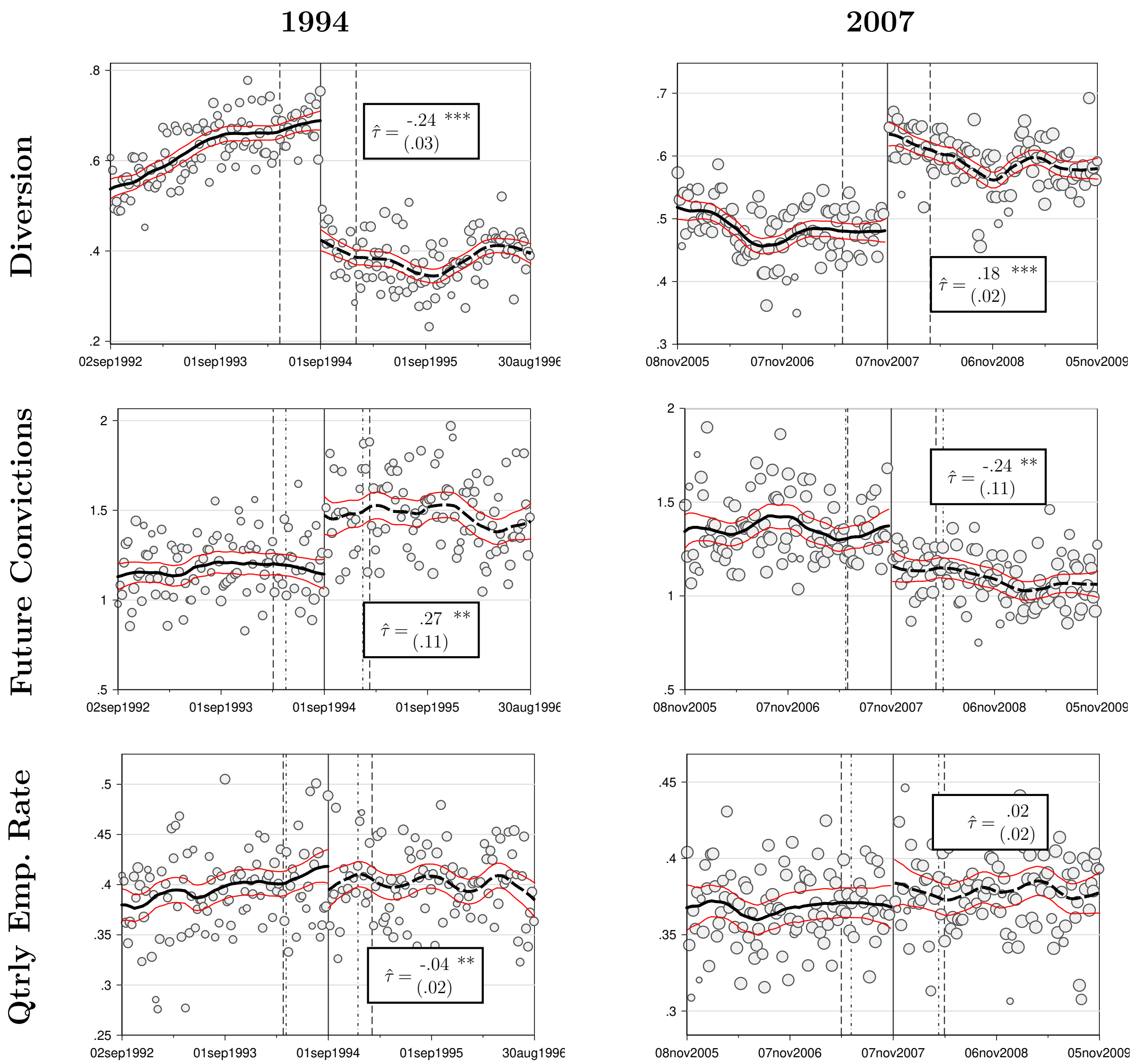


Figure 2: Graphical summary of the reduced form evidence in both the 1994 and 2007 samples

This figure presents reduced form graphical evidence of the changes in diversion, reoffending, and employment associated with the 1994 (left column) and 2007 (right column) natural experiments. The first row depicts the relationship between the running variable and diversion, defined as a court verdict of a deferred adjudication or guilt or a case dismissal. The next two rows plot our key outcomes measured over a ten-year follow-up period: the total number of future Harris County convictions; and, the average quarterly employment rate.

General RD Figure Notes: In general, we present reduced form (sharp RD) results in panels of figures with the left and right columns representing the 1994 and 2007 experiments, respectively. The threshold dates for each experiment are 9/1/1994 (based on the date the charge is filed) and 11/7/2007 (based on the date the case is disposed) and we include two years of data on each side of the threshold date. Scatter points represent week-level bin averages and are weighted by the total number of individual cases. We overlay local polynomial lines and their associated 95 percent confidence bands weighted using an Epanechnikov kernal and 90-day bandwidth. We also report reduced form local-polynomial regression-discontinuity point estimates ($\hat{\tau}$) and standard errors in each plot. The data-driven bandwidth chosen for the reduced form estimates on each side of the discontinuity is depicted by the dashed vertical line and the bandwidth chosen for the fuzzy RD estimates is depicted by the dash-dotted vertical line. RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2.

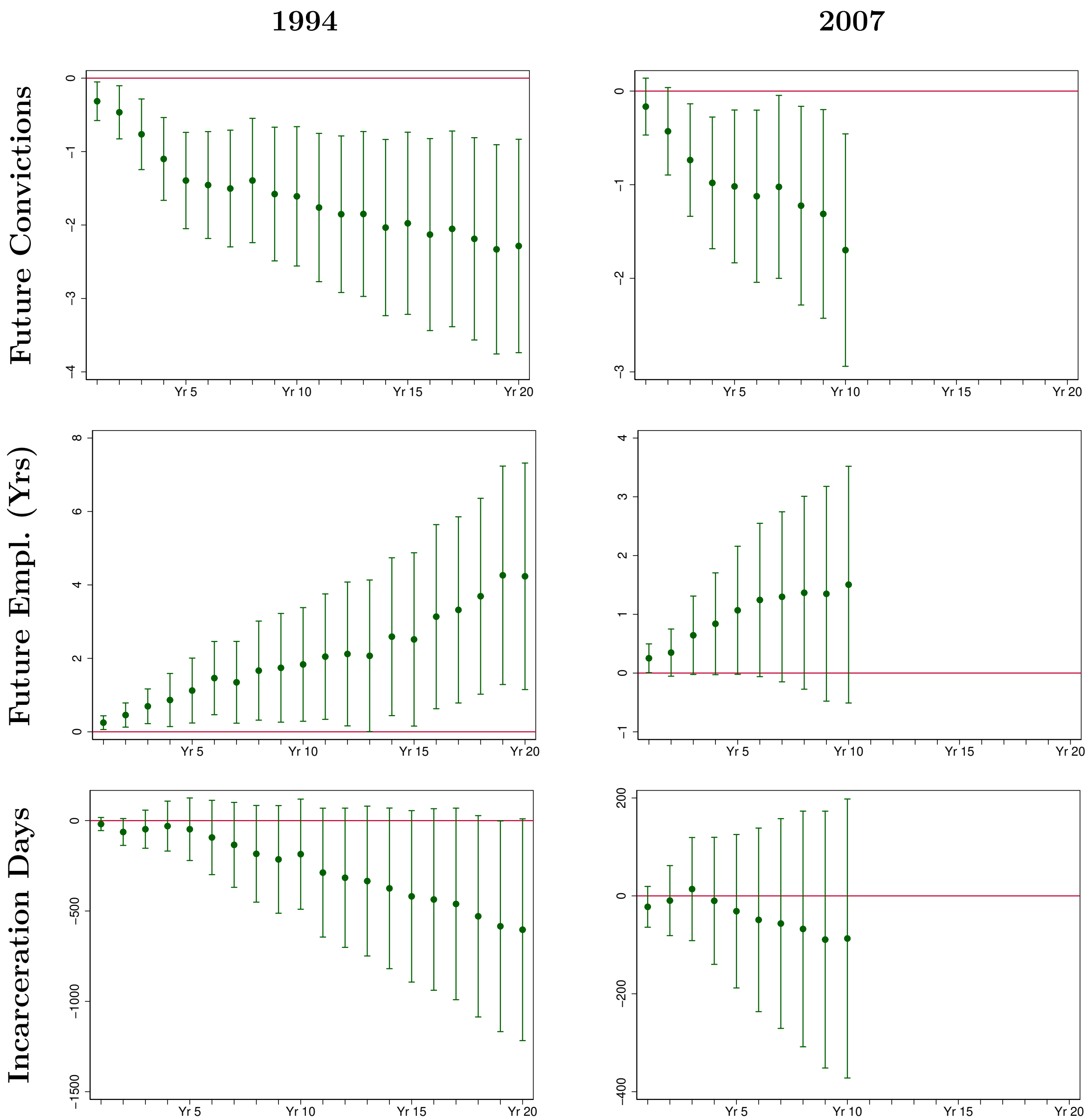


Figure 3: Timeline Impacts

This figure displays fuzzy RD estimates and associated 95% confidence bands for reoffending, employment, and incarceration outcomes that measure cumulative impacts of diversion after each year up through 20 years for the 1994 sample (left column) and 10 years for the 2007 sample (right column). The first two rows depict the estimated impact of diversion on our key outcomes up through each year indicated on the horizontal axis: the total number of future Harris County convictions; and, the average quarterly employment rate. The third row reports the estimated impact of diversion on incarceration days including both actual time served (as opposed to sentenced) in prison and jail. RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2. Contemporaneous (year-by-year) estimates are presented in Figure A.2.

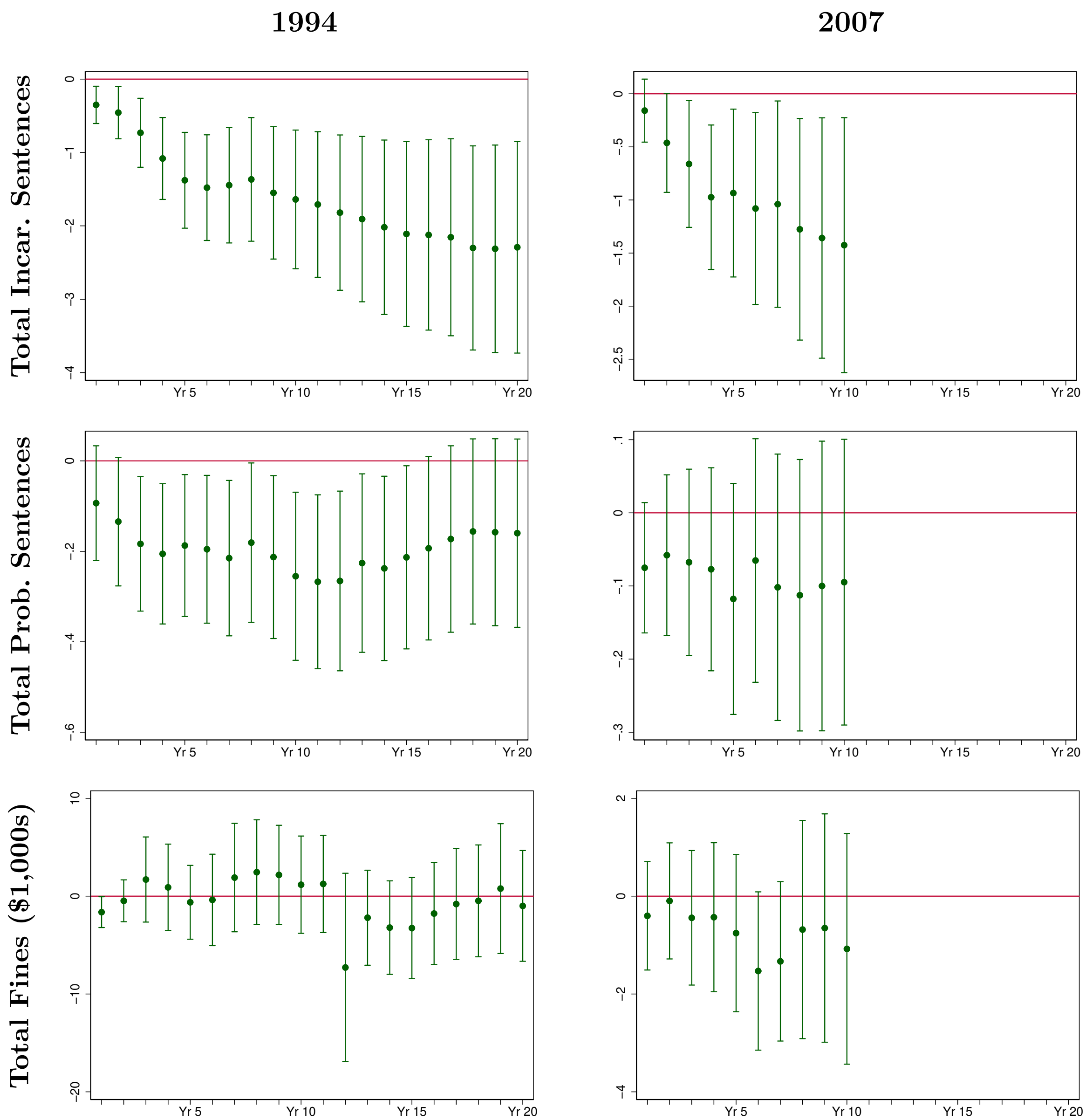


Figure 4: Accumulation of new sanctions for future criminal activity

This figure displays fuzzy RD estimates and associated 95% confidence bands for incarceration, probation and fine sanctions associated with future criminal activity. The coefficients measure cumulative impacts of diversion for each year period up through 20 years for the 1994 sample (left column) and 10 years for the 2007 sample (right column). The pattern of results are consistent with the amplification effect mechanism described in Section 7. RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2.

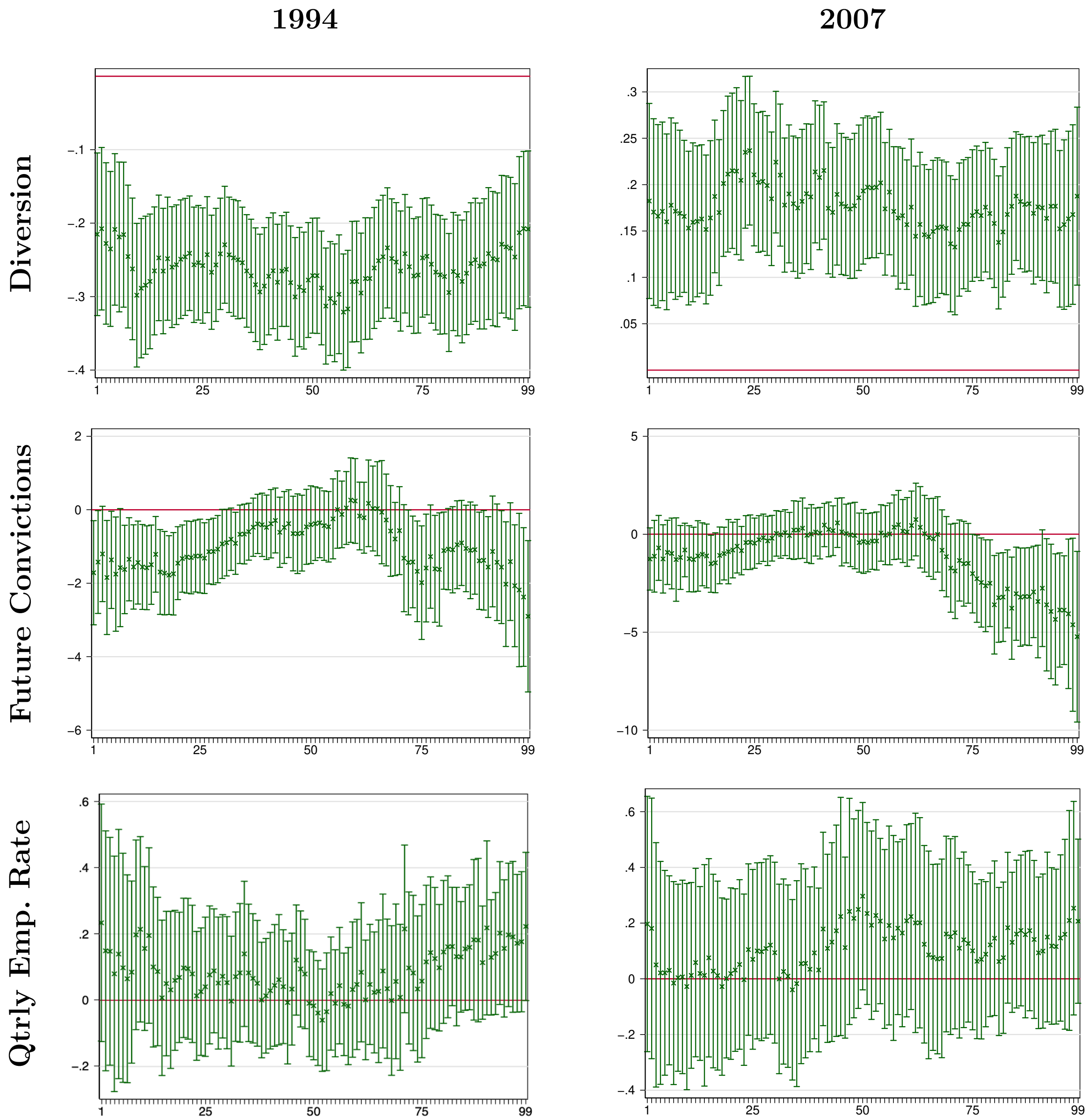


Figure 5: First-Stage and fuzzy RD Impacts over the Recidivism Risk Profile

This figure displays RD estimates and associated 95% confidence bands for diversion, reoffending, and employment outcomes calculated over the quantile function of the predicted recidivism risk score described in Section 5.4 for the 1994 experiment (left column) and 2007 experiment (right column). The first row presents first-stage estimates of the impact of the threshold date on the probability of diversion, defined as a court verdict of a deferred adjudication or guilt or a case dismissal. The next two rows plot fuzzy RD estimated impacts of diversion on our key outcomes measured over a ten-year follow-up period: the total number of future Harris County convictions; and, the average quarterly employment rate. Each coefficient reflects a distinct local polynomial RD estimate from regression centered at the focal percentile using a uniform kernel with a 40 percentile bandwidth. Estimates below the 20th and above the 80th percentiles will reflect narrower, asymmetric bandwidths. RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2.

Table 1: Balance Tests

	τ^{94}	τ^{07}
Caseload Size	-3.43	-2.66
	(2.49)	(3.54)
	[28.4]	[50.3]
Total Prior Misd. Conv.	-0.030	0.034
	(0.058)	(0.052)
	[0.55]	[0.70]
Age at Charge	-0.46	0.50
	(0.49)	(0.55)
	[28.8]	[29.4]
Sex = Male	0.040	0.029
	(0.027)	(0.021)
	[0.68]	[0.73]
Race/Ethn. = Black, not Hisp.	-0.038	-0.030
	(0.027)	(0.022)
	[0.46]	[0.38]
Race/Ethn. = Hispanic	-0.019	0.027
	(0.023)	(0.022)
	[0.21]	[0.31]
Crime Type = Property	-0.0022	0.0095
	(0.027)	(0.022)
	[0.54]	[0.28]
Crime Type = Drug	-0.0029	-0.0090
	(0.028)	(0.026)
	[0.46]	[0.45]
Crime Type = Violent		-0.0068
		(0.014)
		[0.10]
Recidivism Risk Score	0.066	-0.031
	(0.044)	(0.043)
	[1.27]	[1.29]
Employed in quarter of or preceding focal date	0.0028	0.033
	(0.049)	(0.027)
	[0.51]	[0.54]
Social Sec. Number Unrecorded	0.0013	0.048**
	(0.023)	(0.022)
	[0.22]	[0.22]
Observations	31,131	52,792

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

This table presents sharp RD estimates for pre-determined characteristics as recorded in the criminal court records from the Harris County District Court. The calculation of the recidivism risk score is described in Section 5. The first two columns report results from the 1994 and 2007 experiments, respectively.

General RD Table Notes: Unless otherwise noted, all tables present local-polynomial regression discontinuity point estimates, standard errors in parentheses, and control-group means in square brackets. For all sharp RD estimates, control group means represent the average for defendants just to the left of the threshold dates within the data-driven bandwidth. For all fuzzy RD estimates, we report *complier control means* (CCM) as described in Section 4. RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2.

Table 2: Impact of Diversion on Disposition and Sentencing Outcomes

	τ^{94}	τ^{07}	$H_0 : \tau^{94} = \tau^{07}$
Panel A: Case Disposition			
Court Deferral	1.08***	0.59***	0.49***
	(0.098)	(0.10)	(0.14)
	[0.00]	[0.00]	
Case Dismissal	-0.10	0.41***	-0.52***
	(0.10)	(0.099)	(0.14)
	[0.00]	[0.00]	
Panel B: Sentencing			
Incarceration Sentence	-0.19**	-0.97***	0.79***
	(0.086)	(0.056)	(0.10)
	[0.16]	[1.02]	
Probation Sentence	-0.017	0.67***	-0.69***
	(0.11)	(0.11)	(0.16)
	[1.09]	[-0.037]	
Fined	-0.30**	0.46***	-0.76***
	(0.12)	(0.13)	(0.18)
	[0.96]	[-0.0027]	
Drug Treatment	0.032	0.083	-0.051
	(0.045)	(0.068)	(0.081)
	[0.030]	[-0.0021]	
Observations	31,131	52,792	

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

This table presents fuzzy RD estimates of the effect of diversion on case dispositions and sentencing for the focal felony charge. The first two columns report results from the 1994 and 2007 experiments respectively. The third column calculates the average impact across the two experiments. *General RD Table Notes* from Table 1 apply.

Table 3: Impact of Diversion on Reoffending Behavior over 10 years

	τ^{94}	τ^{07}	$H_0 : \tau^{94} = \tau^{07}$
Panel A: Overall			
Any Convictions	-0.31***	-0.26**	-0.052
	(0.12)	(0.13)	(0.17)
	[0.70]	[0.56]	
Total Convictions	-1.61***	-1.70***	0.090
	(0.48)	(0.63)	(0.80)
	[2.08]	[2.27]	
Panel B: Crime Type			
Drug Convictions	-0.71***	-0.25	-0.46
	(0.26)	(0.23)	(0.35)
	[0.80]	[0.68]	
Property Convictions	-0.50**	-0.57*	0.069
	(0.22)	(0.32)	(0.39)
	[0.56]	[0.68]	
Violent Convictions	-0.11	-0.28*	0.17
	(0.099)	(0.14)	(0.18)
	[0.24]	[0.34]	
Panel C: Charge Level			
Misdemeanor Convictions	-0.64**	-0.79**	0.15
	(0.29)	(0.40)	(0.49)
	[0.94]	[1.24]	
Felony Convictions	-1.07***	-0.29	-0.78
	(0.30)	(0.37)	(0.48)
	[1.17]	[0.97]	
Observations	31,131	52,792	

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

This table presents fuzzy RD estimates of the impact of diversion on reoffending outcomes measured using a 10-year follow-up period. The first panel reports results with the total number of convictions in the ten-year follow-up period at the extensive (*Any Conviction*) and intensive (*Total Convictions*) margins. Panel B splits total convictions by type: drug, property, and violent. Panel C reports results separating convictions by severity. *General RD Table Notes* from Table 1 apply. Visual evidence and reduced form coefficients are provided in Figure 2.

Table 4: Impact of Diversion on Labor Market Outcomes over 10 years

	τ^{94}	τ^{07}	$\text{H}_0 : \tau^{94} = \tau^{07}$
Panel A: Employment			
Qtrly Employment Rate	0.18**	0.15	0.033
	(0.079)	(0.10)	(0.13)
	[0.37]	[0.31]	
Earn ≥ 100% Fed. Pov. Level	0.19**	0.12	0.067
	(0.077)	(0.095)	(0.12)
	[0.23]	[0.22]	
Panel B: Earnings			
Log Total Earnings	1.95**	1.96	-0.010
	(0.96)	(1.30)	(1.62)
	[8.75]	[7.95]	
Total Earnings	85,365**	41,438	43,927
	(37,033)	(52,626)	(64,350)
	[91,496]	[102,735]	
Panel C: Industry Effects			
Empl. in Low Penetration Industries	0.099*	0.082	0.017
	(0.055)	(0.075)	(0.093)
	[0.14]	[0.10]	
Empl. in High Penetration Industries	0.036	0.064	-0.028
	(0.062)	(0.082)	(0.10)
	[0.24]	[0.23]	
Panel D: Employment Gaps			
Time to first employment (Quarters)	-5.22	-6.05	0.83
	(3.41)	(4.68)	(5.79)
	[10.3]	[12.6]	
Duration of first unemployment period (Quarters)	-4.84	-3.99	-0.85
	(3.43)	(4.57)	(5.72)
	[11.3]	[15.1]	
Panel E: Tenure			
Max Employment Spell (Quarters).	2.34	5.64*	-3.29
	(1.70)	(2.89)	(3.35)
	[6.27]	[5.96]	
Max Cont. Earning Spell (Quarters)	4.47*	6.14*	-1.67
	(2.54)	(3.48)	(4.31)
	[9.93]	[8.32]	
Observations	24,088	39,765	

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

This table presents fuzzy RD estimates of the impact of diversion on labor market outcomes measured using a ten-year follow-up period. The labor market outcomes vary across each panel. Panel A presents estimated effects on the quarterly employment rate in the first row and quarterly employment at or above the federal poverty level in the second row. All wage outcomes are adjusted to 2018 US dollars using the Houston MSA CPI index and the 2018 Federal Poverty Level for a single adult is ($12,140). Panel B presents results on a total earnings earnings dependent variable in log form (first row) and the raw level (second row). Panel C presents the impact on quarterly employment rates broken out by industries that do and do not tend to hire individuals with felony convictions (see Table A.3 for classification of each industry). Panel D presents the time to first employment post-disposition and the length of the first observed unemployment spell. Finally, Panel E presents results for two dependent variables measuring employment stability by calculating the longest spell (measured in total quarters) of uninterrupted employment at a single employer (first row) or consecutive earnings (second row) during the follow-up period. *General RD Table Notes* from Table 1 apply. Visual evidence and reduced form coefficients are provided in Figures 2.

Table 5: Impact of Diversion on Intersection Outcomes over 10 years

	τ^{94}	τ^{07}	$H_0 : \tau^{94} = \tau^{07}$
Qtrly Employment Rate $\geq$ 50%; No Convictions	0.20**	0.24*	-0.041
	(0.094)	(0.13)	(0.16)
	[0.14]	[0.11]	
Qtrly Employment Rate $\geq$ 50%; 1+ Convictions	-0.036	0.038	-0.074
	(0.088)	(0.10)	(0.14)
	[0.22]	[0.14]	
Qtrly Employment Rate < 50%; No Convictions	0.060	0.099	-0.038
	(0.094)	(0.13)	(0.16)
	[0.14]	[0.35]	
Qtrly Employment Rate < 50%; 1+ Convictions	-0.22**	-0.34**	0.11
	(0.093)	(0.14)	(0.17)
	[0.50]	[0.40]	
Observations	24,088	39,765	

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

This table presents fuzzy RD estimates of the impact of diversion on four mutually-exclusive categories considering labor market outcomes and criminal offending outcomes jointly over the ten-year follow-up period. The first row estimates the probability of more-than 50% employment *AND* no future convictions over the ten-year follow-up period while the fourth row reports estimates of the impact of diversion on a less-than 50% employment rate *AND* at least one future conviction. The other two rows report effects on different combinations of these employment and reoffending measurements. *General RD Table Notes* from Table 1 apply.

Table 6: Comparison of Reduced Form Estimates for Study Sample and other Felony Defendants in Harris County

	τ^{94}	τ^{07}	τ^{94}	τ^{07}
Panel A: Disposition and Sentencing				
Court Deferral	-0.27***	0.12***	-0.018	0.020
	(0.027)	(0.025)	(0.016)	(0.013)
	[0.53]	[0.35]	[0.25]	[0.12]
Case Dismissal	0.0040	0.040**	0.0026	0.048***
	(0.020)	(0.016)	(0.019)	(0.016)
	[0.16]	[0.14]	[0.26]	[0.15]
Incarceration Sentence	0.047*	-0.17***	-0.014	-0.070***
	(0.025)	(0.025)	(0.018)	(0.016)
	[0.17]	[0.50]	[0.39]	[0.73]
Probation Sentence	-0.0044	0.12***	0.053**	0.032**
	(0.028)	(0.025)	(0.021)	(0.014)
	[0.67]	[0.37]	[0.35]	[0.14]
Fine Sentence	0.081***	0.066***	0.049**	0.015
	(0.031)	(0.025)	(0.020)	(0.013)
	[0.45]	[0.27]	[0.29]	[0.10]
Panel B: Reoffending Behavior				
Any Convictions	0.071***	-0.053**	0.013	0.0088
	(0.023)	(0.022)	(0.018)	(0.018)
	[0.46]	[0.47]	[0.50]	[0.57]
Total Convictions	0.27**	-0.24**	0.084	0.022
	(0.11)	(0.11)	(0.065)	(0.099)
	[1.14]	[1.38]	[1.11]	[1.89]
Panel C: Labor Market Activity				
Qtrly Employment Rate	-0.043**	0.025	0.026*	0.0015
	(0.020)	(0.018)	(0.016)	(0.013)
	[0.42]	[0.37]	[0.34]	[0.25]
Total Earnings	-18,589*	5,789	6,294	-9,119
	(9,748)	(9,751)	(7,239)	(6,997)
	[112,204]	[108,891]	[93,087]	[70,110.3]
Observations	31,131	52,792	55,784	75,955
Sample Criteria	Main Study Sample		Other Felony Defendants	

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

This table presents reduced form estimates quantifying how disposition, recidivism and labor markets change across the 1994 and 2007 discontinuities for our main study sample and all other felony defendants in Harris County. In order to avoid defendants repeatedly appearing in the analysis when examining behavior in the broader caseload, which is not constrained to first time defendants, we collapse observations associated with the same individual to their earliest date when multiple charges or dispositions occur within a 1 year period. *General RD Table Notes* from Table 1 apply.

Table 7: Testing Overidentification and Equality of Def. Adj. and Case Dismissal Impacts

Panel A: Overidentification Tests			
	Total Future Convictions (N = 16,039)		
Diversion	-1.05***	-1.22***	-1.23***
	(0.15)	(0.32)	(0.32)
Hansen J statistic (P-value)	0.65	0.90	0.91
	Qtrly Employment Rate (N = 12,306)		
Diversion	0.036	0.16***	0.16***
	(0.026)	(0.052)	(0.053)
Hansen J statistic (P-value)	0.37	0.94	0.93
Panel B: Comparing Impacts of Def. Adj. and Case Dismissals			
	Total Future Convictions (N = 16,039)		
Def. Adj. of Guilt	-1.04***	-1.24***	-1.24***
	(0.15)	(0.33)	(0.33)
Case Dismissal	-1.71	-1.05	-1.08
	(1.49)	(1.44)	(1.42)
H_0: Def. Adj = Dismissal (P-Value)	0.66	0.90	0.91
	Qtrly Employment Rate (N = 12,306)		
Def. Adj. of Guilt	0.030	0.16***	0.16***
	(0.028)	(0.054)	(0.054)
Case Dismissal	0.30	0.14	0.14
	(0.30)	(0.23)	(0.23)
H_0: Def. Adj = Dismissal (P-Value)	0.39	0.95	0.93
Specification Details:			
Sample-specific Intercepts	x	x	x
Sample-specific Trend		x	x
Sample-specific Post-Trend			x

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

This table presents IV estimates from various specifications using the pooled 1994 and 2007 samples to further evaluate the validity of our research design and to compare differential effects across the two types of diversion (deferred adjudication and case dismissals). In Panel A, we estimate the effects of our endogenous variable, diversion, using the threshold date indicators as two exogenous instruments. This specification allows us to assess the joint validity of the instruments through a Hansen J Statistic test. In Panel B, we use the two threshold-date indicators as exogenous instruments for two endogenous case disposition variables, deferred adjudication and case dismissal. The baseline specification in the first column includes our basic set of individual-level controls and an indicator variable for whether the observation is part of the 1994 or 2007 study sample; the second column allows for 1994 and 2007-specific trends in the running variable in addition to the controls from the first column; and the third column adds 1994 and 2007-specific post-threshold trends in the running variable in addition to the regressors previously noted. A single common bandwidth for these exercises is selected using a pooled RD regression quantifying the joint first stage relationship of the effect of the cutoffs on diversion.

Table 8: Comparison between First Time Felony Defendants and Repeat Felony Defendants

	1994 First Time	1994 Repeat	2007 First Time	2007 Repeat
Sharp RD: Diversion	-0.24***	-0.0061	0.18***	0.12***
	(0.028)	(0.034)	(0.025)	(0.025)
	[0.69]	[0.29]	[0.49]	[0.12]
Fuzzy RD: Total Convictions	-1.61***		-1.70***	0.12
	(0.48)		(0.63)	(1.98)
	[2.20]		[2.12]	[3.86]
Fuzzy RD: Qtrly Employment Rate	0.18**		0.15	0.082
	(0.079)		(0.10)	(0.15)
	[0.37]		[0.31]	[0.15]
Observations	31,131	15,724	52,792	26,851

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

This table compares RD estimates for our estimation sample of first-time felony defendants with samples of individuals with prior felony convictions, but who meet all other sample requirements, for each experiment. Sharp RD estimates are reported for all groups in the first row. We then report fuzzy RD estimates for our primary reoffending and employment outcomes in the second and third rows. We do not report the second-stage fuzzy RD estimates for the repeat offender group in 1994 since effectively there is zero first-stage relationship. *General RD Table Notes* from Table 1 apply.

A Print Appendix

A.1 Control Complier Means in the RD Design

For simplicity, let us assume that approaching the discontinuity from below represents the low diversion regime (i.e. control) and approaching the discontinuity from above represents the high diversion regime (i.e. treatment). Let C correspond to whether an observation is a complier or not. The CCM can thus be defined as:

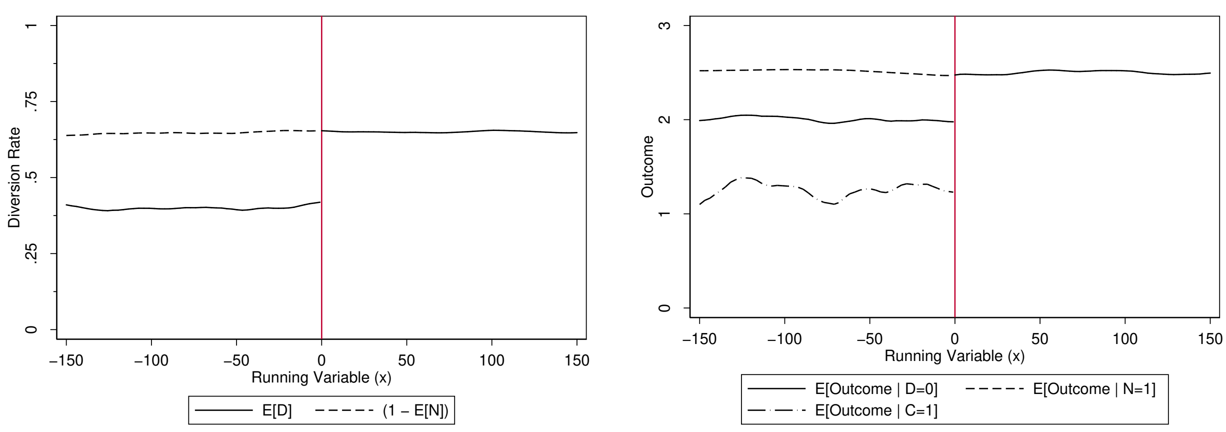
$$CCM \equiv \lim_{x \to 0^-} \mu(x|C=1)$$

The challenge in calculating this estimator is that complier status is not directly observable. What is observable, however, is the average outcome among the non-diverted population (i.e. $D=0$) approaching the discontinuity from below. This will be a weighted average of the outcomes among compliers and *never-takers* (i.e. those never diverted regardless of the discontinuity), which we define as $N=1$.

$$\lim_{x \to 0^-} \mu(x|D=0) = \lim_{x \to 0^-} \mu(x|C=1)\,(1-\omega) + \lim_{x \to 0^-} \mu(x|N=1)\,(\omega)$$

The weight (ω) indicates the never-taker share of the non-diverted population: $\frac{\lim_{x \to 0^-} E(N)}{1-\lim_{x \to 0^-} E(D)}$.

While we can calculate the proportion $(1-E(D))$ and outcome averages $(\mu(x|D=0))$ for the non-diverted population as we approach the threshold from below, we cannot disentangle the contribution of never takers from that of the compliers. However, we can instead use information from those who are not diverted above the threshold since, by the monotonicity assumption of our RD design, this is the population of never-takers. This simply involves replacing $\lim_{x \to 0^-} E(N)$ and $\lim_{x \to 0^-} \mu(x|N=1)$ with $\lim_{x \to 0^+} E(N)$ and $\lim_{x \to 0^+} \mu(x|N=1)$ respectively. For the sake of simplicity and transparency, we approximate the local average approaching the cutoff in the equations discussed using a fixed bandwidth of 2.5 months and a uniform kernel.



(a) Diversion rates in the overall caseload (b) Average outcomes in the non-diverted populations

An illustration of the control complier mean procedure

Our illustration on the next page depicts the CCM procedure applied to a RD setting as described in Section 4. It utilizes artificial data for clarity. Solid black lines represent information that is directly observable to the econometrician. Dashed and dotted lines represent statistics that are not observable, but can be inferred based on the assumptions of the research design. Recall,

$$CCM \equiv \lim_{x \to 0^-} \mu(x|C=1) = \frac{\lim_{x\to 0^-} \mu(x|D=0) - \lim_{x\to 0^-} \mu(x|N=1)\left(\frac{\lim_{x\to 0^-} E(N)}{1-\lim_{x\to 0^-} E(D)}\right)}{\left(1 - \frac{\lim_{x\to 0^-} E(N)}{1-\lim_{x\to 0^-} E(D)}\right)}.$$

Two of the right-hand side terms can be directly estimated without assumptions: $\lim_{x\to 0^-} \mu(x|D=0)$ and $\lim_{x\to 0^-} E(D)$. The first term is the average outcome for those not diverted approaching the discontinuity from below. The second is the rate of diversion approaching the discontinuity from below. Both can be observed because these are caseload-wide statistics and do not rely on unobservable statuses like being a *complier* or *never-taker*.

The remaining terms, $\lim_{x\to 0^-} \mu(x|N=1)$ and $\lim_{x\to 0^-} E(N)$, cannot be directly observed in the data. We can, however, instead approximate their values at cutoff using observations approaching the discontinuity from above: $\lim_{x\to 0^+} \mu(x|N=1)$ and $\lim_{x\to 0^+} E(N)$ respectively. This is feasible because only never-takers will not be diverted after the threshold based on the monotonicity assumption.

The figure highlights the pieces of information that are used to execute the CCM calculation. Together, these present our best estimate of the potential outcome that compliers would have experienced in the absence of diversion. The approach yields similar estimates to methods proposed in Abadie (2003). Differences arise though in that our CCM calculation utilizes a stable bandwidth definition as the treatment effect estimate, which we find to be an attractive feature.

A.2 Appendix Figures and Tables

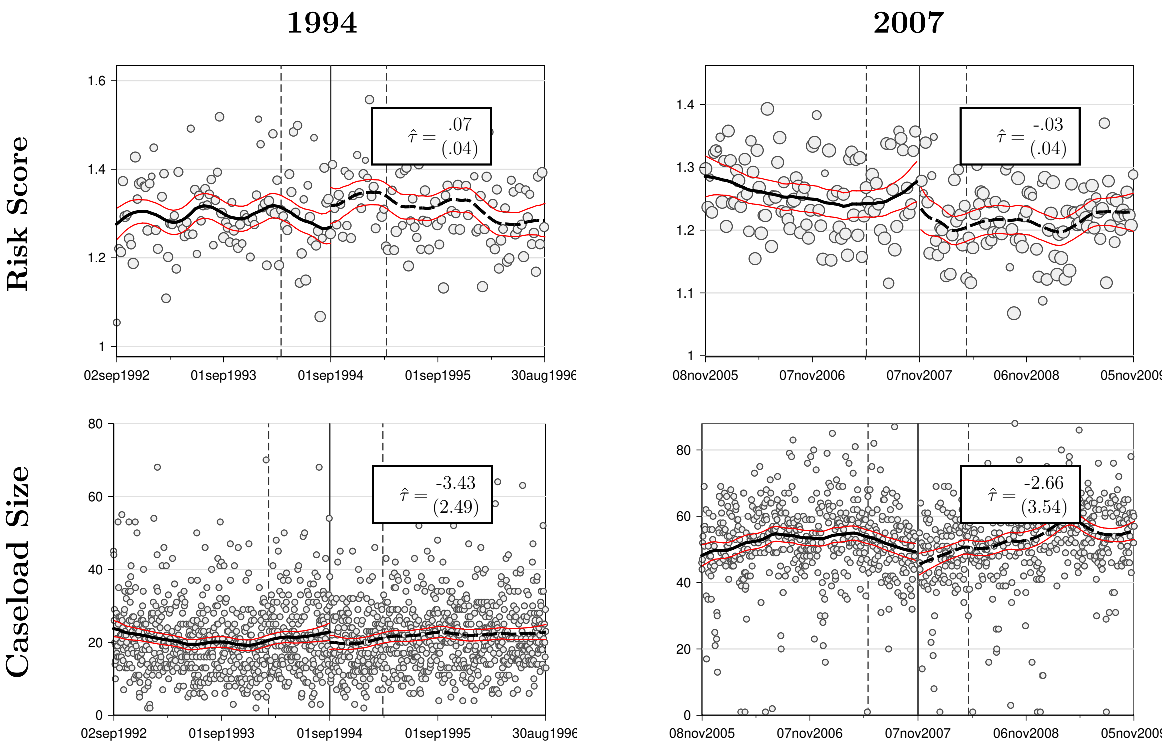


Figure A.1: Baseline comparison

This figure presents reduced form graphical evidence of changes in pre-determined characteristics and caseload size as recorded in the criminal court records from the Harris County District Court associated with the 1994 (left column) and 2007 (right column) natural experiments. The calculation of the recidivism risk score is described in Section 5 and summarizes cumulative changes in sex, race/ethnicity, age, and misdemeanor criminal history. Caseload scatter plots show daily caseload sizes. Point estimates correspond to coefficients presented in Table 1. All *General RD Figure Notes* from Figure 2 apply.

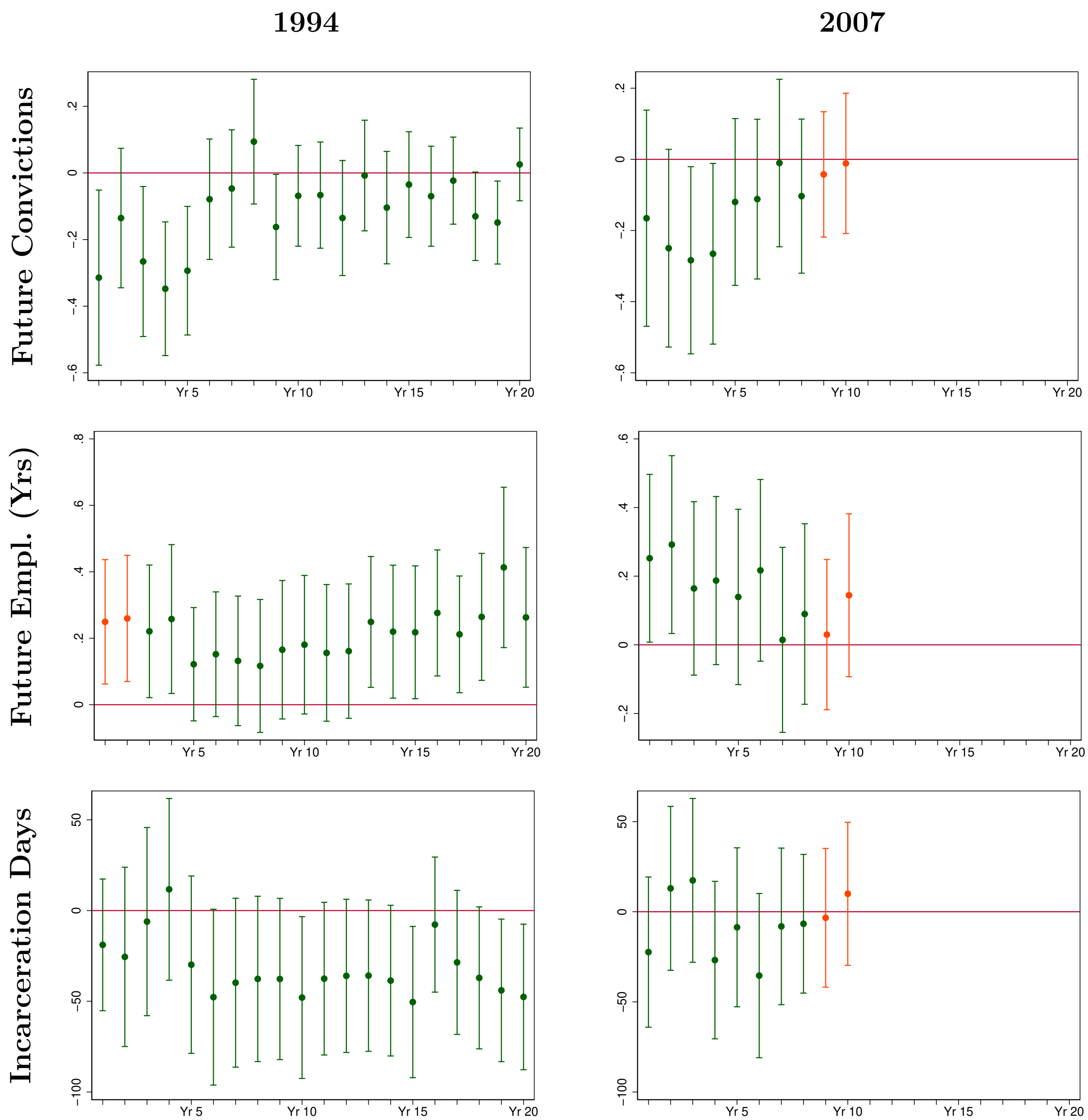


Figure A.2: Contemporaneous impacts by year of follow-up

This figure displays fuzzy RD estimates and associated 95% confidence bands for reoffending, employment, and incarceration outcomes that measure year-by-year (contemporaneous) impacts of diversion for each year period up through 20 years for the 1994 sample (left column) and 10 years for the 2007 sample (right column). The first two rows depict the estimated impact of diversion on our key outcomes up through each year indicated on the horizontal axis: the total number of future Harris County convictions; and, the average quarterly employment rate within each follow-up year. The third row reports the estimated impact of diversion on the number of incarceration days each year. RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2. Cumulative estimates are presented in Figure 3.

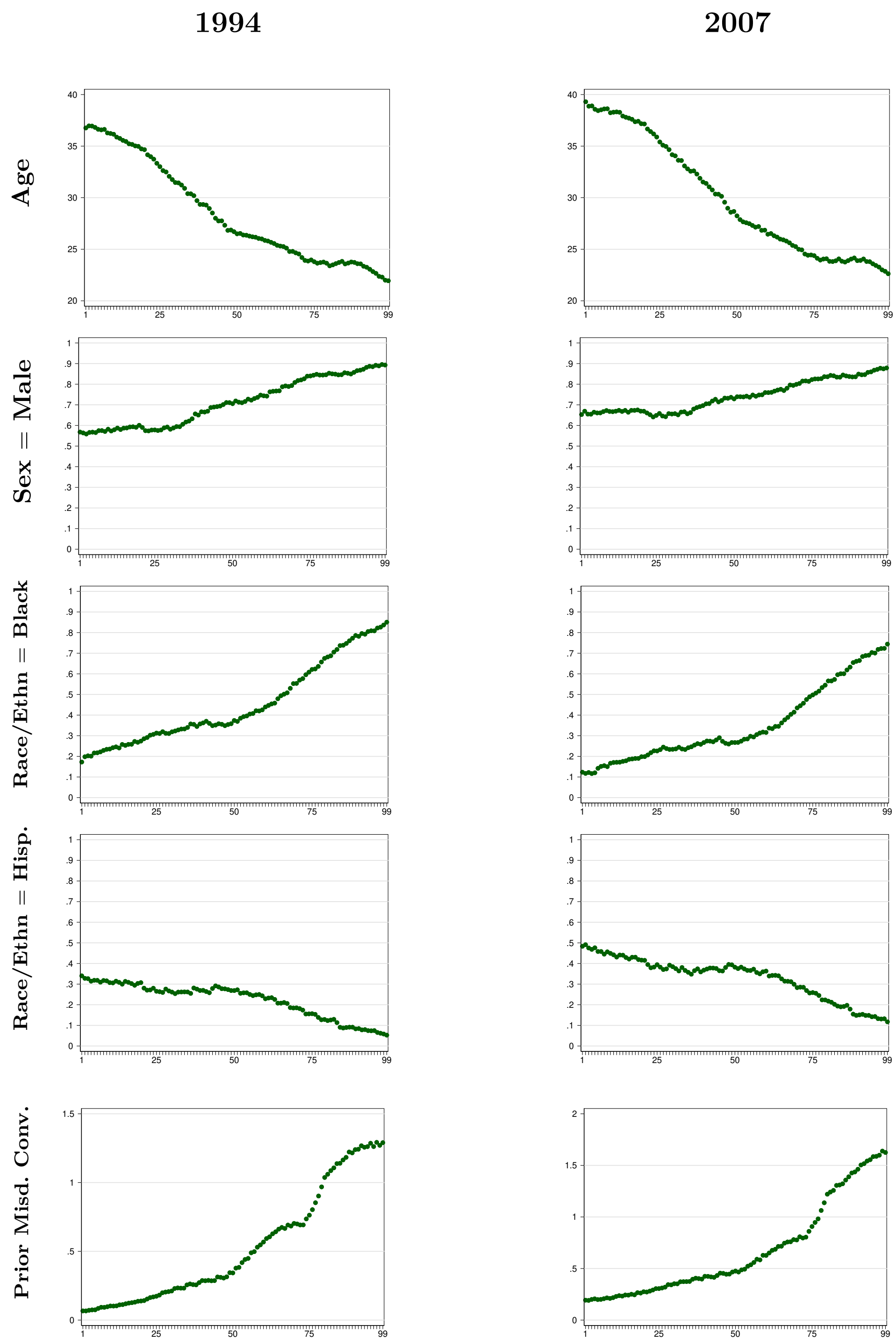


Figure A.3: Smoothed Defendant Characteristics over the Recidivism Risk Profile

This figure plots mean defendant demographic and criminal history characteristics over the quantile function of the predicted recidivism risk score described in Section 5.4. Estimated impacts of diversion across this distribution are presented in Figure 5.

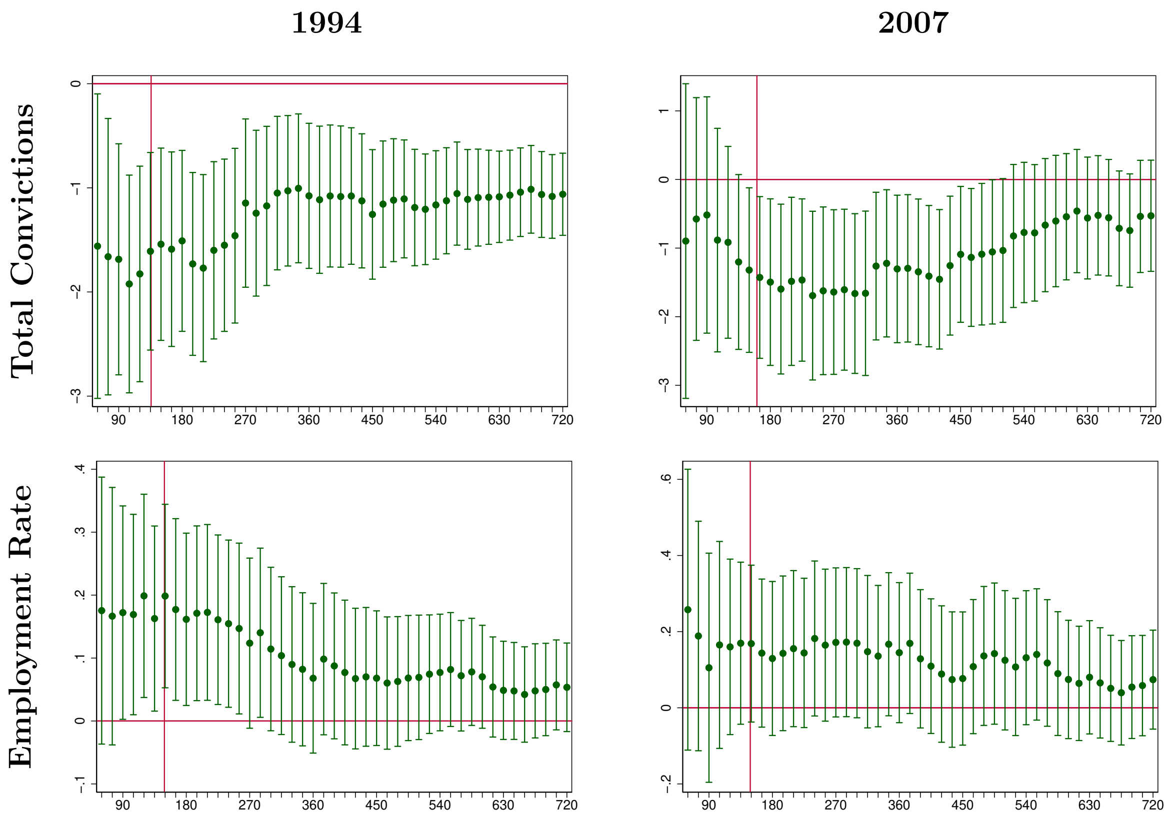


Figure A.4: Estimated effects by RD bandwidth

This figure displays fuzzy RD estimates and associated 95% confidence bands for future conviction and employment outcomes varying a fixed bandwidth in 15 day intervals from 60 days on each side of the threshold date to 720 days. The vertical line in each plot indicates the average data-driven bandwidth used in our primary regression results in Tables 3 and 4.

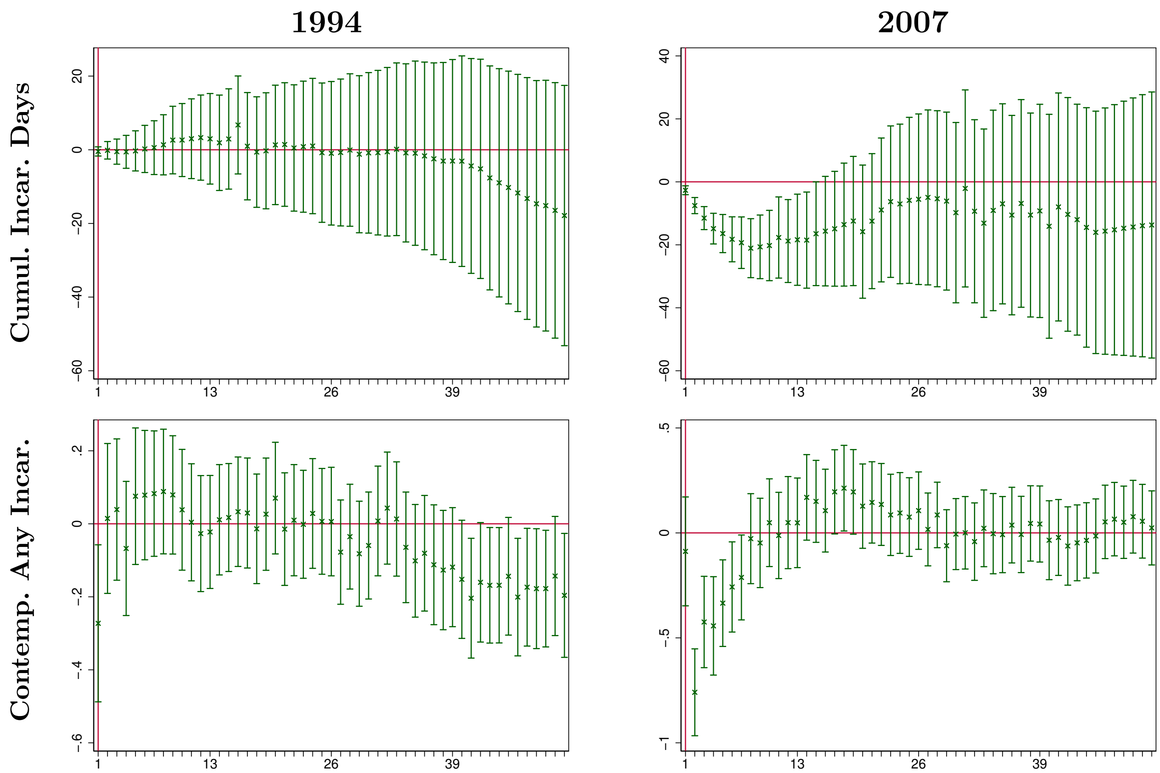


Figure A.5: Measures of Exposure to Incarceration by Week for First Year

This figure displays fuzzy RD estimates and associated 95% confidence bands for incarceration outcomes that measure week-by-week impacts of diversion for the first year following the case disposition for the 1994 sample (left column) and the 2007 sample (right column). The first row depicts the cumulative days incarcerated each week since the disposition date. The second row presents fuzzy RD estimates on the effect on any incarceration for the week indicated on the horizontal access. RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2.

Table A.1: State-level general population diversion programs

State	*Diversion Programs*	*Filing Stage*	*Charge Type*	*Recent Caseload Sizes*
AK	Supspended Entry of Judgment (Statewide, §12.55.078)	Courts	Misd.	129 (2016-2017)
AL	Pretrial Diversion (Statewide, §12-17-226 et seq.); Municipal Pretrial Diversion (Statewide, §12-14-90 et seq.)	Prosecutors (Pretrial Diversion); Courts (Municipal Pretrial Diversion)	Misd./ Felony	Not Reported
AR	Pre-Adjudication Probation (Statewide, §5-4-901 et. seq.)	Courts	Felony	694 (2017)
AZ	Deferred Prosecution and Pretrial Diversion (Statewide, §11-361 et seq., §9-500.22; Coconino County, §11-361 et seq.; Maricopa County, §11-361 et seq.; Pima County, §11-361 et seq.)	Prosecutors	Misd./ Felony	17800 (2017, Statewide); 619 (2015-2016, Coconino County); 3662 (2016-2017, Maricopa County); 1233 (2016-2017, Pima County)
CA	Misdemeanor Diversion (Statewide, Penal Code §1001.1 et seq.); Pretrial Diversion (Statewide, Penal Code §1001.50 et seq.)	Prosecutors	Misd.	Not Reported
CO	Deferred Sentencing of Defendant (Statewide, §18-1.3-102); Pretrial Diversion (Statewide, §18-1.3-101)	Courts (Deferred Sentencing of Defendant); Prosecutors (Pretrial Diversion)	Misd./ Felony	5254 (2018, Deferred Sentencing); 502 (2016, Pretrial Diversion)
CT	Accelerated Pretrial Rehabilitation (Statewide, §54-56e)	Courts	Misd./ Felony	5225 (2017-2018)
DC	Pretrial Diversion (Statewide, §12-17-226 et seq.)	Prosecutors	Misd./ Felony	5375 (2018)
DE	Probation Before Judgment (Statewide, Title 11 §4218)	Courts	Misd./ Felony	550 (2016)
FL	Pretrial Intervention (Statewide, §948.08)	Courts	Misd./ Felony	Not Reported
GA	Pretrial Release and Diversion (Statewide, §42-3-70 et. seq.); Pretrial Intervention and Diversion (Statewide, §15-18-80 et. seq.)	Courts (Pretrial Release and Diversion); Prosecutors (Pretrial Intervention and Diversion)	Felony (Pretrial Release and Diversion); Misd./Felony (Pretrial Intervention and Diversion)	Not Reported
HI	Deferred Acceptance of Plea (Statewide, §853-1 et seq.)	Courts	Misd./ Felony	Not Reported
IA	Deferred Judgment (Statewide, §907.3)	Courts	Misd./ Felony	Not Reported
ID	Supspension of Judgment/Withheld Judgment (Statewide, §19-2601)	Courts	Misd./ Felony	Not Reported
IN	Deferred/Diverted (Statewide, §33-39-8-5); Withholding Prosecution (Statewide, Withholding Prosecution)	Prosecutors	All (Deferred/Diverted); Misd./ Felony (Withholding Prosecution)	98946 (2017, Deferred/Diverted); 503 (2017, Withholding Prosecution)
KS	Diversion Agreement (Statewide, §22-2907 et seq.)	Prosecutors (sometimes courts)	Misd./ Felony	3814 (2018)
KY	Pretrial Diversion (Statewide, §533.250)	Courts	Felony	7346 (2016)
MD	Probation Before Judgment (Statewide, [Crim. Proc.] §6-220)	Courts	Misd./ Felony	10798 (2017)
MN	Pretrial Diversion (Rice County, §401.065; Sherburn County, §401.066; Washington County, §401.067; St. Louis County, §401.068; Anoka County, §401.069)	Prosecutors	Misd./ Felony	472 (2008, Anoka County); 14 (2018, Rice County); 13 (2017, Sherburn County); 4 (2016, St. Louis County); 55 (2014, Washington County)

See table notes on following page.

Table A.1. (continued): State-level general population diversion programs

State	*Diversion Programs*	*Filing Stage*	*Charge Type*	*Recent Caseload Sizes*
MO	Diversionary Programs (Statewide, §217.777)	Dept. of Correction	Misd./ Felony	Not Reported
MS	Pretrial Intervention (Statewide, §99-15-105)	Prosecutors (w/approval from court judge)	Misd./ Felony	Not Reported
MT	Pretrial Diversion (Statewide, §46-16-130)	Prosecutors	Misd./ Felony	Not Reported
NC	Deferred Prosecution (Statewide, §15A-1341)	Prosecutors (w/approval from court judge)	Misd./ Felony	14656 (2017-2018)
NE	Pretrial Diversion (Statewide, §29-3601 et. seq.)	Prosecutors	Misd./ Felony	3800 (2018)
NJ	Pretrial Intervention Program: Supervisory Treatment (Statewide, §2C:43-12 et. seq.); Conditional Dismissal Program (Statewide, §2C:43-13.1 et seq.)	Courts	Misd./ Felony	486 (2013, Pretrial Intervention Program); Not Reported (Conditional Dismissal Program)
NM	Preprosecution Diversion (Statewide, §31-16A-1 et seq.)	Prosecutors	Misd./ Felony	3662 (2018)
NV	Preprosecution Diversion (Statewide, AB 470 (2017))	Courts	Misd.	Not Reported
OH	Pretrial Diversion (Statewide, §2935.36)	Prosecutors (w/approval from court judge)	Misd./ Felony	5973 (2017)
OK	Deferred Prosecution (Statewide, 22 §305.1); Deferred Sentence (Statewide, 22 §991c)	Prosecutors (Deferred Prosecution); Courts (Deferred Sentence)	Misd./ Felony	Not Reported
OR	Diversion Agreement (Statewide, §135.881 et. seq.)	Prosecutors	Misd./ Felony	Not Reported
SC	Pretrial Intervention (Adult, Juvenile) (Statewide, §17-22-10 et seq.)	Prosecutors	Misd./ Felony	8127 (2014-2016, Adult); 261 (2016-2017, Juvenile)
TN	Pretrial Diversion (Statewide, §40-15-101 et. seq.); Judicial Diversion (Statewide, §40-15-313 et seq.)	Prosecutors (Pretrial Diversion); Courts (Judicial Diversion)	Misd./ Felony	4522 (2017-2018, Pretrial Diversion and Judicial Diversion)
TX	Deferred Adjudication Community Supervision (Statewide, [Crim. Proc.] Code §42A.101)	Courts	Misd./ Felony	237,463 (2017)
UT	Diversion Agreement (Statewide, §77-2-5 et. seq.)	Prosecutors (w/approval from court judge)	Misd./ Felony	Not Reported
VT	Adult Court Diversion (Statewide, 3 §164)	Prosecutors	Misd./ Felony	2199 (2015)
WI	Deferred Prosecution (Statewide, §971.39); Volunteers in Probation Program (Statewide, §971.40, §973.11)	Prosecutors (Deferred Prosecution); Courts (Volunteers in Probation Program)	Misd./ Felony	Not Reported
WV	Pretrial Diversion (Statewide, §61-11-22); Deferred Adjudication (Statewide, §61-11-22a)	Prosecutors (Pretrial Diversion); Courts (Deferred Adjudication)	Misd./ Felony	Not Reported
WY	Deferred Prosecution (Statewide, §7-13-301)	Courts	Misd./ Felony	Not Reported

This table describes state general population diversion programs from documentation provided by the National Conference of State Legislatures at `http://www.ncsl.org/research/civil-and-criminal-justice/pretrial-diversion.aspx` [accessed August 5, 2019]. Where available, recent caseload statistics were obtained from state criminal justice annual reports.

Table A.2: Effect of felony convictions and felony deferred adjudications on civil liberties and program eligiblity

Eligibility	*Felony conviction*	*Felony deferred adjudication*	*Source*
Occupational licenses	Can revoke, suspend or deny a license for certain convictions	Cannot consider a def. adj. a conviction[A]	Texas Occupations Code, §53.0211, See Texas Department of Labor Relations guidelines[B]
Right to vote	Not eligible to vote until successful completion of punishment, including any term of incarceration, parole, supervision, period of probation, or has been pardoned	Eligible to vote	Section 11.002 of the Texas Election CodeTX, See discussion on Secretary of State webpage[C]
Possession of a firearm	Prohibited until 5th anniversary of release from confinement (community supervision, parole, or prison)	Not subject to state restrictions	Tex. Penal Code §46.04
TANF (welfare) and Food Stamp (SNAP)	Ineligible due to drug felony convictions (post April 1, 2002 for TANF, post Aug. 22, 1996 for SNAP), SNAP ban relaxed effective Sept 2015	Not subject to restrictions	Texas Admin. Code §372.501
Immigration status	Subject to deportation for certain types of offenses under U.S. immigration statute (I.N.A. §237)	Def. Adj. considered a conviction for immigration purposes (Moosa v. INS, 171 F.3d 994, 1005-06 (5th Cir. 1999))	I.N.A. §237; Moosa v. INS, 171 F.3d 994, 1005-06 (5th Cir. 1999)
Pell	Ineligible from certain drug felony convictions in Texas	Not subject to restrictions	20 USC 1091(r), 2002
Federally subsidized public housing	Can be excluded	Can be excluded	42 USC 1437(1)(b) (2002)

This table summarizes the impact of felony convictions and felony deferred adjudication in Texas on selected civil liberties and eligibility for assistance programs. This is not an exhaustive list.

[A]For occupational licenses, a deferred adjudication does not provide authority to revoke, suspend, or deny occupational licenses on the basis of a criminal conviction but a few legislated exceptions for specific occupations exist including: licensed breeders licenses require no conviction or deferred adjudication for animal cruelty or neglect; message therapy licenses require no conviction or deferred adjudication for offenses involving trafficking of persons, prostitution, or another sexual offense. [B] `https://www.tdlr.texas.gov/crimconvict.htm` [Accessed Sept. 11, 2019] [C] `https://www.sos.state.tx.us/elections/laws/effects.shtml` [Accessed Sept. 11, 2019]

Table A.3: Employment penetration by industry

Industry Sector (NAICS code)	1994	2007	Average	Penetration classification
Ag., Forestry, Fishing and Hunting (11)	1.0	0.7	0.9	High
Mining (21)	2.1	1.5	1.8	Low
Utilities (22)	1.4	2.5	2.0	Low
Construction (23)	1.1	1.3	1.2	High
Manufacturing (31-33)	1.5	1.5	1.5	High
Wholesale Trade (42)	1.7	2.0	1.9	Low
Retail Trade (44-45)	1.7	1.9	1.8	Low
Transportation & Warehousing (48-49)	1.7	1.8	1.8	Low
Information (51)	2.1	3.1	2.6	Low
Finance & Insurance (52)	2.3	1.9	2.1	Low
Real Estate Rental & Leasing (53)	2.0	1.6	1.8	Low
Prof., Scientific, & Tech. Services (54)	1.8	1.9	1.8	Low
Management (55)	2.5	2.2	2.4	Low
Admin & Waste Management Services (56)	1.1	1.3	1.2	High
Educational Services (61)	2.5	2.1	2.3	Low
Health Care & Social Assistance (62)	1.7	1.9	1.8	Low
Arts, Entertainment, & Recreation (71)	1.5	1.5	1.5	High
Accomodation & Food Services (72)	1.2	1.2	1.2	High
Other Services (except Pub. Admin) (81)	1.5	1.5	1.5	High
Public Administration (92)	2.5	2.3	2.4	Low

This table presents the ratio of employment in a particular industry for individuals without a felony conviction relative to those with a felony conviction without adjusting for the baseline observable characteristics (gender, age, race). Equal employment penetration among these two groups is expressed as a ratio equal to 1. We clasify industries as low penetration if the ratio exceeds 1.5 or if individuals without felony records have more than 50% representation in the industry relative to those with a felony conviction. Table 4 reports estimated effects of diversion on employment in low or high penetration industries using the classifications reported above.

Table A.4: Classic Heterogeneity Analysis

Panel A: 1994 Sample	Black	White	Hispanic	Crime = Property	Crime = Drug	Crime = Violent	Male	Female	< 30 Yrs Old	≥ 30 Yrs Old	No Misd. Conv.	1+ Misd. Conv.	Empl. at Charge	Not Empl. at Charge
Sharp RD: Diversion	-0.31***	-0.25***	-0.16***	-0.15***	-0.37***		-0.26***	-0.25***	-0.23***	-0.29***	-0.33***	-0.23***	-0.27***	-0.25***
	(0.044)	(0.045)	(0.055)	(0.047)	(0.043)		(0.034)	(0.052)	(0.034)	(0.053)	(0.042)	(0.038)	(0.052)	(0.070)
Fuzzy RD: Total Convictions	-1.27**	-0.93	-1.07	-1.99**	-0.63		-1.04*	-1.00	-1.72**	-0.70*	-0.49	-1.81***	-0.88	-2.75**
	(0.63)	(0.64)	(1.19)	(0.82)	(0.48)		(0.61)	(0.76)	(0.75)	(0.41)	(0.59)	(0.57)	(1.08)	(1.13)
Fuzzy RD: Qtrly Employment Rate	0.22*	0.13	-0.20	0.35**	-0.0069		0.050	0.25	0.12	0.031	-0.012	0.25**	0.15	0.11
	(0.12)	(0.11)	(0.25)	(0.16)	(0.068)		(0.074)	(0.18)	(0.087)	(0.12)	(0.11)	(0.11)	(0.14)	(0.16)
Observations	12,480	7,047	4,322	12,455	11,720		16,882	7,206	14,347	9,741	7,434	16,654	8,056	8,442
Panel B: 2007 Sample	Black	White	Hispanic	Crime = Property	Crime = Drug	Crime = Violent	Male	Female	< 30 Yrs Old	≥ 30 Yrs Old	No Misd. Conv.	1+ Misd. Conv.	Empl. at Charge	Not Empl. at Charge
Sharp RD: Diversion	0.19***	0.17***	0.13***	0.088**	0.25***	0.18***	0.17***	0.16***	0.16***	0.20***	0.19***	0.15***	0.14***	0.22***
	(0.039)	(0.046)	(0.047)	(0.042)	(0.044)	(0.060)	(0.033)	(0.053)	(0.035)	(0.038)	(0.040)	(0.038)	(0.036)	(0.038)
Fuzzy RD: Total Convictions	-2.24**	-0.47	-1.68**	-2.33	-0.40	-1.18	-1.67**	-0.69	-2.61***	-0.19	-1.50	-1.09	-1.73	-1.22
	(0.89)	(1.03)	(0.82)	(1.87)	(0.63)	(1.30)	(0.69)	(1.24)	(0.85)	(0.70)	(0.99)	(0.79)	(1.13)	(0.81)
Fuzzy RD: Qtrly Employment Rate	0.23*	0.100	0.17	0.051	0.15*	0.044	0.14	0.087	0.081	0.11	0.084	0.13	0.24	-0.060
	(0.13)	(0.18)	(0.20)	(0.35)	(0.089)	(0.49)	(0.10)	(0.18)	(0.12)	(0.14)	(0.11)	(0.16)	(0.16)	(0.075)
Observations	16,848	11,778	10,707	11,313	17,309	4,263	28,199	11,566	23,321	16,444	14,979	24,786	20,402	19,363

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

This table presents estimates of the impact of diversion for subgroups defined based on demographic characteristics or prior criminal histories as indicated in the title to each column. Panel A presents estimates across our key first stage (diversion) and second stage (total convictions and quarterly employment rate) for 11 subsamples. Panel B repeats this exercise for the 2007 experiment. *General RD Table Notes* from Table 1 apply.

Table A.5: Placebos Exercises using Alternate Year Discontinuities

	τ^{93}	τ^{95}	τ^{06}	τ^{08}
Sharp RD: Diversion	-0.031	-0.015	0.034	-0.028
	(0.031)	(0.034)	(0.026)	(0.026)
	[0.65]	[0.35]	[0.44]	[0.56]
Sharp RD: Total Convictions	-0.053	-0.012	-0.069	-0.051
	(0.12)	(0.15)	(0.15)	(0.11)
	[1.24]	[1.48]	[1.43]	[1.12]
Sharp RD: Qtrly Employment Rate	-0.070**	-0.026	0.0096	-0.022
	(0.030)	(0.028)	(0.020)	(0.023)
	[0.41]	[0.41]	[0.36]	[0.39]
Observations	15,312	15,857	26,226	26,566

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

This table presents sharp RD estimates of the impact of the discontinuity at four placebo threshold dates on diversion, total convictions and the average quarterly employment rate. We shift the true discontinuity dates back one year in the first and third column to 9/1/1993 and 11/7/2006, respectively; and forward one year in the second and fourth column to 9/1/1995 and 11/7/2008, respectively. We do not report fuzzy RD estimates since there is zero first-stage relationship. *General RD Table Notes* from Table 1 apply.

Table A.6: Alternative Measures of Recidivism

	τ^{94}	τ^{07}	$H_0 : \tau^{94} = \tau^{07}$
Total Bookings	-1.38**	-1.44*	0.063
	(0.65)	(0.80)	(1.03)
	[2.56]	[2.87]	
Total Charges	-1.64***	-2.00***	0.35
	(0.53)	(0.72)	(0.89)
	[2.30]	[2.67]	
Total Convictions	-1.61***	-1.70***	0.090
	(0.48)	(0.63)	(0.80)
	[2.08]	[2.27]	
Total Convictions and Revocations[a]	-1.81***	-1.58**	-0.23
	(0.51)	(0.64)	(0.82)
	[2.47]	[2.25]	
Total TDPS CCH Conv.	-1.09**	-1.38***	0.29
	(0.48)	(0.40)	(0.62)
	[1.63]	[1.48]	
TDPS CCH Conv. (Harris County)	-1.07***	-1.02***	-0.056
	(0.41)	(0.36)	(0.54)
	[1.40]	[1.22]	
TDPS CCH Conv. (Non-Harris Counties)	-0.055	-0.49***	0.44*
	(0.19)	(0.17)	(0.26)
	[0.23]	[0.25]	
Observations	31,131	52,792	

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

This table presents fuzzy RD estimates of the effect of diversion on various ways of quantifying recidivism. The first four rows report estimates for various measures of recidivism available from Harris County: jail bookings, charges filed, convictions (baseline result), and convictions and non-technical (criminal) probation revocations. The remaining three row report the impact of diversion on the total number of convictions recorded in the statewide CCH conviction database and then separates outcomes into Harris County convictions and non-Harris County convictions to evaluate spatial spillovers. *General RD Table Notes* from Table 1 apply.

[a] We use reports from the Texas Legislative Board to obtain proxies for the fraction of criminal revocations for each estimation sample. TX Legislative Budget Board (2008) reports that 36.8 % of the 2007 revocations for felony community supervision arrangements were for non-technical (criminal violations) which we use for the 2007 sample. We were unable to find a similar statistic for Harris County in 1994 or 1995 and therefore use the state-wide average non-techical revocation rate of 45% between 1999 and 2005 as reported by TX Legislative Budget Board (2005). This exercise allows us to quantify the potential role of prosecutors deciding not to pursue new convictions for offenses that violate a diversion agreement. This is a conservative approach given that many non-techincal revocations were actually separately prosecuted in practice and as a result would be double counted as both revocations and new convictions in this exercise.

Table A.7: Quantifying potential role of missing SSNs in employment findings

	Missing Dropped	Missing imputed
Panel A: 1994 Sample		
Fuzzy RD: Qtrly Employment Rate	0.18**	0.16**
	(0.079)	(0.073)
Fuzzy RD: Total Earnings	85,365**	72,376**
	(37,033)	(34,677)
Fuzzy RD: Log Total Earnings	1.95**	1.36
	(0.96)	(0.89)
Panel B: 2007 Sample		
Fuzzy RD: Qtrly Employment Rate	0.15	0.085
	(0.10)	(0.072)
Fuzzy RD: Total Earnings	41,438	13,216
	(52,626)	(45,241)
Fuzzy RD: Log Total Earnings	1.96	2.25*
	(1.30)	(1.16)

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

This table repeats the fuzzy RD estimates of the impact of diversion on employment and earnings labor market outcomes from Table 4 in the first column in order to compare these baseline estimates to those reported in the second column where we impute earnings for defendants that we are unable to match to earnings records due to missing SSNs in the Harris County court records. The second column reports estimates where we impute earnings each quarter for defendants with missing SSNs based on a model that includes demographic characteristics, and prior criminal histories. We impute quarterly earnings separately for pre- and post-discontinuity observations. *General RD Table Notes* from Table 1 apply.

Table A.8: Robustness Exercises

	1994 Sample			*2007 Sample*		
	Sharp RD: Diversion	Fuzzy RD: Total Conv.	Fuzzy RD: Qtrly Empl.	Sharp RD: Diversion	Fuzzy RD: Total Conv.	Fuzzy RD: Qtrly Empl.
Panel A: Bandwidth Selection Criteria						
MSE1	-0.26***	-1.70***	0.16**	0.18***	-1.39**	0.14
	(0.027)	(0.49)	(0.080)	(0.025)	(0.58)	(0.10)
MSE2	-0.24***	-1.49***	0.18**	0.18***	-1.61**	0.15
	(0.028)	(0.50)	(0.079)	(0.025)	(0.64)	(0.10)
MSE3	-0.23***	-1.61***	0.073	0.17***	-1.33**	0.16
	(0.029)	(0.48)	(0.057)	(0.019)	(0.62)	(0.11)
CER1	-0.24***	-1.61***	0.17	0.16***	-0.85	0.14
	(0.031)	(0.59)	(0.11)	(0.031)	(0.82)	(0.16)
CER2	-0.22***	-1.79***	0.19*	0.17***	-1.00	0.10
	(0.033)	(0.59)	(0.11)	(0.031)	(0.91)	(0.15)
CER3	-0.21***	-1.63***	0.13*	0.18***	-0.66	0.10
	(0.034)	(0.58)	(0.069)	(0.024)	(0.93)	(0.15)
Panel B: Variance Estimators						
Day Cluster	-0.24***	-1.34**	0.21**	0.17***	-1.42**	0.17*
	(0.033)	(0.53)	(0.094)	(0.032)	(0.57)	(0.098)
Week Cluster	-0.24***	-1.52***	0.20	0.17***	-0.91*	0.19**
	(0.032)	(0.50)	(0.12)	(0.028)	(0.54)	(0.088)
Nearest Neighbor	-0.24***	-1.61***	0.19**	0.18***	-1.71***	0.15
	(0.028)	(0.48)	(0.085)	(0.024)	(0.63)	(0.10)
Panel C: Alternative Specification Choices						
No Donut	-0.24***	-1.61***	0.18**	0.13***	-1.28*	0.10
	(0.028)	(0.48)	(0.079)	(0.023)	(0.67)	(0.11)
No Cov.	-0.24***	-1.45***	0.17**	0.17***	-1.78**	0.13
	(0.029)	(0.54)	(0.076)	(0.025)	(0.69)	(0.11)
Non-Robust	-0.24***	-1.61***	0.18***	0.18***	-1.70***	0.15*
	(0.025)	(0.42)	(0.071)	(0.021)	(0.53)	(0.089)
Non-Bias Corrected	-0.25***	-1.35***	0.16**	0.18***	-1.46***	0.13
	(0.025)	(0.42)	(0.071)	(0.021)	(0.53)	(0.089)
Week Bins	-0.19***	-1.88***	0.043	0.17***	-1.40**	0.15
	(0.028)	(0.60)	(0.087)	(0.024)	(0.61)	(0.10)
Epa. Kernel	-0.26***	-1.21***	0.11*	0.17***	-1.26**	0.14
	(0.025)	(0.39)	(0.064)	(0.025)	(0.59)	(0.10)
Tri. Kernel	-0.25***	-1.35***	0.15**	0.17***	-1.20*	0.15
	(0.025)	(0.42)	(0.069)	(0.024)	(0.64)	(0.10)

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

This table presents sharp RD (diversion) and fuzzy RD (total convictions and employment rate) estimates that relax other baseline specification choices that are described with row titles. 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). All other RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2.

Table A.9: Quantifying potential role of incapacitation in employment findings

	Baseline	Incar. Quarters Recoded to Earnings > 0	Incar. Quarters Recoded to Average Employ. Rate from 1 Year prior to Incar.
Diversion, 1994 Sample	0.18**	0.16**	0.20**
	(0.079)	(0.076)	(0.080)
Diversion, 2007 Sample	0.15	0.091	0.18*
	(0.10)	(0.10)	(0.11)

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

This table repeats the fuzzy RD estimates of the impact of diversion on our quarterly employment outcome from Table 4 in the first column in order to compare these baseline estimates to those reported in the second and third columns where our outcome variable recodes labor market results during quarters of incarceration to explore how employment might have evolved in the absence of incapacitation. The second column replaces employment status with the average employment rate in the year prior to their current incarceration spell. The third column recodes employment status to equal 1 for all quarters when we observe an individual incarcerated. This last exercise assumes that all individuals would have found employment in the formal sector if not for incapacitation, a scenario which is unlikely to occur given the observed pre-incarceration employment rates. *General RD Table Notes* from Table 1 apply.