

Diversion in the Criminal Justice System: Regression Discontinuity Evidence on Court Deferrals

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Abstract: The historically unprecedented size of the U.S. criminal justice system has necessitated the development of diversion programs to reduce caseloads as a cost containment strategy. Court deferrals, which allow felony defendants to avoid formal convictions through probation, are one example. Using two discontinuities in deferral rates in Harris County, Texas separated by 13 years, we find consistent evidence that diversion reduces reoffending and unemployment among first-time felony defendants. Similar benefits are not observed for repeat offenders suggesting felony record stigma as a key mechanism. Young, African American men drive the total effect, a pattern consistent with over-targeting by law enforcement.

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

JEL classification codes: J24, K14, K42

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Whether defined in terms of arrestees, defendants or detainees, the U.S. criminal justice caseload has expanded dramatically in the last half century. As of 2010, more than eight percent of the U.S. adult population, accounting for roughly 20 million individuals, had a prior felony conviction up from 3 percent in 1980 (Shannon et al. 2016). A number of legal and procedural strategies have emerged in this time that divert offenders away from the criminal justice system given that caseloads have outpaced system capacity. As an example, over 95 percent of state felony cases are settled with a plea deal (Durose and Langan 2007) in order to avoid the time and resources required for a court trial. Even starker, many jurisdictions now decline to arrest, prosecute or sanction certain classes of offenders through formal criminal justice diversion programs (Center for Health and Justice 2013). Such programs differ across the timing, eligibility, and requirements for successful completion, but each espouse the broad goals of easing capacity constraints in the criminal justice system and offering deserving offenders a second chance to avoid the potentially harmful consequences of a criminal conviction.

While often implemented as a cost management strategy, diversion may have a profound impact on the lives of offenders given recent findings that document a damaging effect of criminal punishment on employment, education, and reoffending (Mueller-Smith 2015, Aizer and Doyle 2015, Di Tella and Schargrodsky 2013, Raphael 2014, Lovenheim and Owens 2014, Finlay 2009, Pager 2008, 2003). And at the macro level, the practice may help reduce inequality in the United States in light of the disparate reach of the criminal justice system into disadvantaged communities. In particular, African American men — a group in which one-in-three have a felony conviction (Shannon et al. 2016) — stand the most to gain particularly if existing disparities in arrest and conviction rates reflect excess enforcement.¹ Clear and convincing evidence on the effects of these programs on affected individuals and communities is especially urgent given recent instructions by U.S. Attorney General Jeff Sessions for federal prosecutors to pursue convictions of the most serious provable charge for low-risk criminal defendants (Sessions 2017) reversing course on recent momentum for criminal justice reform.

¹As an example of racial disparities in arrest and convictions, survey data suggests that marijuana usage rates are similar among African American and white adults in the U.S., yet African Americans are nearly 4 times more likely to be arrested for marijuana possession (American Civil Liberties Union 2013). Importantly, racial inequalities in the access to diversion programs also need to be addressed (MacDonald et al. 2014).

In this paper, we study the effects of a diversion strategy commonly used by prosecutors and judges that allows defendants to avoid a felony conviction by completing a probationary period without incident. We colloquially refer to this diversion option as a *court deferral*.² In contrast to other diversionary strategies like the establishment of specialized drug courts or treatment programs, this approach requires minimal additional public resources since court deferrals typically rely on established community supervision programs (e.g. probation). As a consequence, this policy option can feasibly be implemented without serious funding or infrastructure hurdles.

Despite the increasing prevalence of court deferrals and other diversion programs in the criminal justice system, little is known about their causal impact on defendants' future behavior.³ To the best of our knowledge, this study provides the first quasi-experimental empirical evidence on the use of a diversion strategy on recidivism and labor market outcomes.⁴ While a growing literature identifies causal impacts of criminal justice sanctions on future outcomes (Mueller-Smith 2015, Aizer and Doyle 2015, Hansen 2015), our setting provides the first opportunity to isolate the stigma effects of a felony conviction from other ways in which sanctions impact future outcomes without relying on an audit or correspondence-based research design.

We exploit two natural experiments in Harris County, Texas (TX) that dramatically altered the provision of deferral agreements to offenders. The first follows a TX penal code reform in 1994 that dramatically decreased court deferrals for offenders charged with certain reclassified drug and property offenses. The second tracks the fallout from an unexpected failure of a 2007 ballot initiative intended to expand local county jails but instead resulted in an immediate increase in the deferral rate for low-risk defendants.

²In Texas the legal name for this diversion option is *deferred adjudication of guilt*. In other states, similar diversion options are labeled *deferred prosecution*, *probation before judgment*, *deferred disposition*, or *deferred sentence*. At the extreme end, some jurisdictions have explored court diversions that fully eliminate any form of prosecution or community supervision for specific low-risk offenses. Programs of this nature are outside the scope of this project.

³Chiricos, 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.

⁴Additionally, we provide the first causal evidence (to our knowledge) on the effect of a felony conviction record that does not rely on an audit or correspondence design, which we believe is a strong contribution to the criminal record literature as well.

What is particularly attractive about these policy shifts from a research perspective is that each was implemented quite rapidly such that defendants charged or disposed one day versus the next experienced distinctly different court verdicts and sanctions but do not differ across any observable predetermined characteristics. Additionally, through combining two events that together increase and decrease rates of deferrals for offenders, we can be more confident that our estimates are not simply capturing other unobserved changes contemporaneous with the discontinuities.

Because of the immediate nature of these changes, we use a regression discontinuity (RD) research design to demonstrate that court deferral (and consequently felony conviction) rates change sharply across the thresholds and present reduced form evidence on the impact of these changes on future criminal justice and labor market outcomes.⁵ Our results are documented graphically and through formal statistical tests. We find that an increase in court deferrals for first-time felony defendants substantially improves outcomes over a five-year follow-up period. We observe statistically significant and economically meaningful changes in recidivism, driven mainly by changes in drug and property offenses, as well as in employment and earnings. An evaluation of the joint determination of labor market and reoffending behavior indicates these outcomes are interdependent. Our effects are driven by young, African American men with a misdemeanor record, a group that exhibits the highest likelihood of future interaction with the criminal justice system during the five-year follow-up period.

We do not find evidence of any significant discontinuities in observable demographic characteristics, prior criminal histories, or the density of the criminal caseload. These results support our argument that these two natural experiments present a valid context for the use of a RD methodology. We conduct a number of robustness tests to verify that our results do not rely on any specific implementation design choices or functional form assumptions in addition to a variety of placebo exercises to validate these findings.

We contribute to a large literature investigating the impact of the severity of criminal sanctions imposed on an individual on his or her future outcomes. Due to opposing mechanisms and

⁵Appendix B presents fully replicated results using a research design rooted in time series econometrics that relaxes the assumption of independence between observations along our running variable.

heterogeneous effects, the expected impact is ambiguous. While several recent studies find that enhanced criminal sanctions can have long-lasting negative impacts (Mueller-Smith 2015, Aizer and Doyle 2015, Di Tella and Schargrodsky 2013, Shapiro and Chen 2007), others estimate declines in reoffending associated with more severe punishments (Hansen 2015, Owens 2009). We are able to rule out several of the opposing mechanisms (e.g. incapacitation, transmission of criminal capital) since our analysis focuses on changes in punishment that do not involve large differences in served incarceration.⁶

Our goal then is to sort among the few key mechanisms that remain in our setting. A number of channels would suggest that those with formal convictions would reoffend at a lower rate. First, individuals who receive a felony conviction rather than a court deferral may be deterred from reoffending since future punishments are often a function of prior convictions. Moreover, there could be a reductions in reoffending through a specific deterrence effect where harsh punishments alter preferences or information involved in future decisions. Hansen (2015) provides compelling evidence of this—stricter penalties reduced future reoffending among individuals arrested for drunk driving in the state of Washington.

On the other hand, increasing evidence suggests that a felony conviction carries a stigma that diminishes employment opportunities for former offenders (Raphael 2014, Finlay 2009, Pager 2008, 2003). A stigma effect may also exist within the criminal justice system wherein those with a felony record are treated differently by police, prosecutors, or judges in future criminal justice interactions.⁷ Both would suggest that instead those with deferrals would reoffend at a lower rate. Moreover, court deferrals may deter particularly lower reoffending while the probationary period is in effect as defendants will seek to avoid conviction and re-sentencing for their original crime, a dynamic we term *sentence overhang*.⁸

⁶Increases in the incidence or length of incarceration can decrease reoffending and employment through an incapacitation effect (Barbarino and Mastrobuoni 2014, Buonanno and Raphael 2013, Johnson and Raphael 2012, Owens 2009, Kuziemko and Levitt 2004, Levitt 1996). On the other hand, incarceration can increase reoffending through a transmission of criminal capital among peers or while incarcerated (Stevenson 2015, Ouss 2011, Bayer et al. 2009). Moreover, certain types of incarceration that involve job training and drug treatment program participation could also have a positive future impact (Bhuller et al. 2016).

⁷For instance, certain types of prior convictions can trigger aggravated (elevated) charges upon reoffense 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.

⁸We think the closest parallel to this mechanism in the literature is Drago et al. (2009).

On net, our results strongly indicate that the stigma and sentence overhang effects outweigh any decrease in reoffending through deterrence.⁹ Offenders receiving a court deferral, the more lenient and less stigmatizing court disposition, are less — not more — likely to recidivate. We believe that the stigma of a felony conviction record in particular plays a focal role in generating this finding. When looking at the impacts of court deferrals for offenders who already have a prior felony conviction—a group for whom the stigma effect would be greatly reduced yet the sentence overhang mechanism would remain — we fail to observe any significant impact on future behavior.

One may suspect that changes in criminal justice policy around the discontinuities would trigger a general deterrence effect and generate aggregate changes in crime before and after the policy changes. Such changes would imply a change in the number and composition of offenders around our threshold dates and would be a threat to our identification strategy. Several recent studies find evidence of general deterrence (Helland and Tabarrok 2007, Drago et al. 2009, Abrams 2012); however, we do not find any clear indication that general deterrence operates in the context of either of our natural experiments. There are no changes in the number of felony charges or in the characteristics of the defendants across the discontinuity. This is consistent with another body of evidence that fails to observe large general deterrence responses among young offenders reaching the age of maturity (Lee and McCrary 2016), and with results suggesting that criminal offenders are present-oriented (low discount factors and short time horizons) and, therefore, less responsive to changes in expected punishment (Mastrobuoni and Rivers 2016).¹⁰ Recent research finds general deterrence effects following the swift implementation of monitoring programs for probationers suggesting that increases in the certainty of punishment is more of a deterrent than increases in the severity of punishment (Kilmer et al. 2013, Hawken and Kleiman

⁹These mechanisms, which oppose the deterrence effect of harsher punishment, are not present in the natural experiment analyzed in Hansen (2015) since the increase in punishment severity is primarily through increases in fines, incarceration, and the time of license suspension. It is possible that a stigma effect exists with more serious convictions (aggravated DUI vs. DUI) to the extent that employers distinguish between different levels of charges. However, we are not aware of any evidence of this type of discrimination nor do we expect the stigma effects across different levels of the same type of behavior to be very large.

¹⁰It is possible that the two discontinuities we exploit were not salient to potential offenders which is why we do not observe discontinuous changes in caseload composition. This is particularly true for the 2007 experiment which did not arise due to any official change in policy. Because the 1994 changes were publicly enacted in early 1993 and could be fully anticipated by would-be criminal offenders, it is more surprising that no general deterrence response is observed for that time period.

2009).¹¹

Overall, our results suggest that greater use of court deferrals for first-time felony offenders will lead to long-term reductions in criminal offending and improvements in labor market outcomes. A potential concern with this conclusion is that individuals without any criminal involvement may suffer from statistical discrimination. For instance, an employer averse to hiring former offenders who no longer observes the prior criminal activity of job applicants may decrease his propensity to hire individuals who are part of high-offending demographic groups. [Agan and Starr \(2016\)](#) and [Doleac and Hansen \(2016\)](#) find decreases in employment outcomes for young black males following the implementation of “Ban-the-Box” policies, which restrict questions about felony convictions on employment applications. While we are not able to test for such effects given our research design, we believe this mechanism might be stunted in this context given evidence that employers are less averse to hiring former drug and property offenders relative to other types of felons ([Holzer et al. 2007](#)). In this case, court deferrals would permit these offenders to avoid being pooled with the more undesirable violent convicts without imposing much harm on the non-offending population.

The remainder of the paper is structured as follows: Section 2 describes the two discontinuities in deferral rates in Harris County, TX that form the basis of our research design and the administrative data sets used to perform the analysis; Section 3 outlines our regression discontinuity empirical strategy; Section 4 presents and discusses our results, provides evidence supporting our identification assumptions, shows several robustness and placebo exercises; and, Section 5 discusses the causal effect of diversion implied by our reduced form estimates, highlights mechanisms which can and cannot explain our pattern of empirical results, and provides concluding remarks. The Appendix shows further results from robustness exercises, alternative specifications, and detailed results for several subgroups.

¹¹Caution should be taken thought given recent work seeking to replicate the success of these models has failed to replicate the original impacts ([O’Connell et al. 2016](#), [Lattimore et al. 2016](#)).

2. Data Sources, Institutional Background, and Sample Construction

This section details the administrative data sources, institutional background of both the 1994 and 2007 natural experiments used in our RD analysis, and the sample construction strategy.

2.1. Administrative Data Sources

We rely on several county-specific and statewide sources of administrative data to evaluate the effect of court deferrals. From the criminal justice system, these include criminal court records from the Harris County District Clerk, jail booking and incarceration span data from 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 felony and misdemeanor charges and court outcomes for all adults between 1980 and 2013 regardless of the final verdict.¹² We use this data to construct the core analysis sample, measure our identifying source of variation (date of charge or disposition), document first-stage conviction/sentencing outcomes and track ongoing criminal activity. A benefit of using a county-specific measure of recidivism is that we are able to observe charges that do not result in a formal conviction (such as those resulting in a court deferral). A downside, however, is that we cannot observe criminal activity that is not prosecuted in Harris County, which could be problematic if deferrals induced out-of-county mobility. To this end, we also construct a statewide measure of recidivism using the CCH database.¹³ The CCH database only tracks convictions, and previous audits have found this data to have incomplete statewide coverage,¹⁴ particularly prior to the early 2000's, due to the voluntary nature of the reporting to the CCH. As a result, neither approach is ideal, but we view them both as complementary

¹²Cases sealed to the public by order of the court, which account for less than half of a percentage point of the overall caseload, and criminal appeals were not included in the data.

¹³There currently does not exist a dataset accessible to researchers to track criminal activity nationwide.

¹⁴This has been confirmed in our own analysis of the records.

strategies that help evaluate the robustness of our findings.¹⁵

We are able to observe time spent in the Harris County Jail and the Texas prison system. We cannot directly link either type of facility to a specific conviction, and as a consequence any impacts on actual time served may conflate the initial change in sentencing with the future incarceration sentences resulting from ongoing criminal behavior.

The two Harris County data sets are linked using a unique county identifier tied to an individual's fingerprint known as the SPN. We match this data to state-level data (CCH and the state prison records) using a defendant's full name and date of birth.¹⁶

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 2016 from the Texas Workforce Commission. Wage and employment records were matched to the criminal justice records using a social security number.¹⁷

2.2. Institutional Background

A major strength of this paper is our ability to leverage two distinct discontinuous changes in the use of court deferrals. These events are separated 13 years in time and stem from different circumstances. Through demonstrating the consistency of our findings over these two episodes, we argue that the change in court deferrals causes the change in defendants criminal and labor market outcomes. Below, we provide detailed information regarding each of the natural experiments. As is common when studying natural experiments, both have their drawbacks and limitations from a research design perspective, but, we view them together as a convincing source of exogenous variation for studying diversion in the criminal justice system.

¹⁵The Harris County Jail data provides a third method to measure recidivism through county bookings that may or may not have progressed to formal court charges.

¹⁶In 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.

¹⁷Approximately two-thirds of our sample contains a non-missing social security number. Individuals without a social security number 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 social security numbers.

2.2.1. The 1994 Penal Code Reform

In 1993 the Texas Legislature enacted its most sweeping sentencing reform in the history of the state.¹⁸ The legislature sought to “get smart” on incarceration through reducing requirements for low-risk offenders while increasing time served before parole eligibility for aggravated violent offenders.¹⁹ The need came as a consequence of prison overcrowding that started in the 1970s and the corresponding lawsuits that ensued. The new sentencing regime applied only to defendants who had committed their offenses on or after September 1, 1994,^{20,21,22} and since prosecutors were required to file charges within 48 hours of arrest there was limited ability to manipulate who was charged when.²³

While the new legislation was intended to ease sanctioning on low-risk offenders, the specific wording in the revision led prosecutors to abandon deferral agreements in lieu of formal convictions. As seen in Figure 1, across the entire caseload the conviction rate actually increased by 8 percentage points from 46 percent to 54 percent. This unintended outcome resulted in a sentencing regime that was arguably worse off for low-risk defendants.

The cause for this shift was an unanticipated consequence of the probated incarceration provision for first-time offenders included in the new penal code. At issue was that assistant district attorneys felt that the threat of incarceration provided crucial leverage for ensuring compliance with the terms of court deferrals. However, under the new regime a second round of effective probation would have to be offered to defendants who violated their deferral agreements

¹⁸Two pieces of legislation accomplished this overhaul: Senate Bill 1067 reclassified most non-violent felony crimes as “state jail felonies,” a newly created offense level below a 3rd degree felony; and, Senate Bill 532 created “state jails,” correctional institutions part of the statewide prison system set up specifically for individuals who had been convicted of state jail felonies.

¹⁹Low-risk individuals convicted of the newly created state jail felony offense would still be considered as having a felony record by the state but would be subject to more lenient sentencing guidelines. These new guidelines limited the maximum incarceration sentence to two years and required a probated (conditional) incarceration sentence for defendants without prior state jail felony convictions.

²⁰In practice, it appears that the courts used the charging date rather than the offending date as the key variable for determining which code applied.

²¹Offenses occurring prior to September 1, 1994, but not disposed until after this cutoff date were not grandfathered into the new policy regime.

²²Additional provisions in the penal reform related to truth-in-sentencing for violent offenders went into effect a year earlier on September 1, 1993 but are beyond the scope of this present study.

²³In our main empirical estimation, we drop individuals charged on three days before and after September 1, 1994 to avoid any issues with short-term sorting. Estimates provided in Table A5 show that our finding are robust to eliminating this donut approach.

since they had not yet been legally convicted or sentenced.²⁴ The fact that the language of this provision undermined deferral agreement credibility was raised in October 1993 during a simulated plea bargaining exercise between prosecutors and defense attorneys, yet no action was taken to amend the statutory language before the changes were implemented (Fabelo 1997). As a consequence, when the new penal code went into effect the majority of the would-be court deferrals instead were given formal felony convictions. For those charged just after the new regime went into effect, it was very unlikely they would be able to avoid a permanent felony conviction record.

Not all crimes were impacted by the probated incarceration requirement. The legislative changes targeted the lowest felony drug and property crimes, specifically possession of less than 1 gram of a controlled substance²⁵ and property offenses totaling less than \$20,000 in total damages. Later on we refer to these as *affected statutes* to highlight the types of offenses that were likely to exhibit a change in deferral rates.

2.2.2. The 2007 Failed Jail Expansion

Overcrowding in prisons and jails remained an important concern across Texas during the 2000s, especially in the Harris County Jail. This local jail—which houses inmates with shorter sentences and serves several other functions including pre-trial detention and holding for local inmates waiting to be transferred to the state prison system—had up to 1,900 inmates sleeping on mattresses on the floor by 2005 (Hughes 2005). To address overcrowding, the county sought to expand the jail 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 needed permission from local voters in the November 6, 2007 election.²⁶

²⁴In the eyes of the law, they were still considered first-time offenders and had to be given a probated incarceration sentence.

²⁵This mainly was comprised of 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.

²⁶The proposed jail expansion (Proposition 3) was part of a broader bond package being put to local voters in 2007 in response to the county's fast growing population. Together Harris County and the Port of Houston Authority added six local bond propositions to the November 6, 2007 election ballot at combined total of \$880 million in potential bonds. The projects included upgrading roads and parks, expanding capacity at the port, building a new forensic lab and constructing a new family law center.

A local campaign against the jail expansion and an unexpectedly 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 of 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. Some commentators explicitly placed the responsibility of the overcrowding problem on the courts in Harris County, suggesting that they depended too heavily on incarceration at the cost of taxpayer funds.²⁷

Most Harris County criminal courts showed an immediate response in their sentencing practices after the election suggesting they paid close attention to the election results. In the days, weeks and months following, guilty verdicts dropped 8 percentage points from around 65 percent of the total caseload to 57 percent (Figure 1). Marginal cases were typically converted into court deferrals although a fraction also appear to have had their charges dismissed altogether (Figure 4).²⁸ With the drop in guilty rates, incarceration sentences decreased and were replaced by community-supervised probation sentences (Figure 4).

These changes accomplished two goals for the courts. First, the courts were able to show that they were responding to their pre-election critics, an important task given that both the district attorney and the criminal court judges are publicly elected officials in Harris County. And second, it immediately reduced the inflow of inmates into the jail system which would help alleviate the overcrowding crisis.²⁹

The fact that the defeat of the jail expansion was unanticipated limits the likelihood of sorting across this threshold. In addition, disposition dates are typically scheduled well in advance also limiting the scope for endogenous sorting. As shown in Section 4, it does not appear that

²⁷See the following articles for discussions at the time of the election: Snyder (2007), Grits for Breakfast (2007b), and Grits for Breakfast (2007a).

²⁸While we do not find a statistically significant change in case dismissals following the 1994 discontinuity, we find a marginally insignificant 2.7 percentage point increase in case dismissals associated with the 2007 shift.

²⁹In spite of these actions, by the summer of 2008 the Harris County Jail had to transport an additional 1,130 inmates to Louisiana in order to stay in compliance with the Texas Commission on Jail Standards. With this addition, total annual costs for incarcerating inmates outside Harris County reached \$24 million (Peterson 2008).

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 when conviction rates dropped and deferral rates were elevated.

2.3. Sample Restrictions

While both caseloads exhibit clear overall conviction rate discontinuities (Figure 1), we employ several sample restrictions to focus the analysis sample and to distinguish potential mechanisms. First, as depicted in Figure 1 we restrict our sample to defendants charged two years before and after the September 1, 1994 threshold and defendants disposed two years before and after the November 6, 2007 threshold. When computing our point estimates though, it should be noted that the effective sample window will be narrower due to the data-driven optimal bandwidth procedure referenced in Section 3.

We further limit our main analysis sample to defendants who were likely to be impacted by the policies that lead to changes in diversion in the first place. Table 3 shows regression discontinuity estimates for the change in felony convictions and court deferrals for several subpopulations of the 1994 and 2007 caseloads.³⁰ The first three rows show separate discontinuity estimates from the 1994 natural experiment among: (1) all felony defendants, (2) felony defendants charged with statutes affected by the introduction of state jail felonies, and (3) felony defendants charged with unaffected statutes. The second three rows show a parallel exercise for 2007 where we have highlighted the discontinuities for low-risk and high-risk crimes.³¹

The statutes impacted by the 1994 penal code reform's probated incarceration provision that prosecutors thought would undermine deferral agreements are labelled *affected statutes*. Only defendants charged with these types of crimes should observe a change in their deferral rates, which is the pattern observed in Table 3. Similarly, only those defendants in 2007 at risk for incarceration in the county jail, which we label as the *low-risk* group, should have had

³⁰Please see Section 3 for a detailed description of the methodology for constructing these coefficients.

³¹The high-risk group is defined as individuals who are sentenced to a year or more of incarceration for their charge who have a very low likelihood of being sentenced to the county jail for their offense. The low-risk group is defined as individuals who are sentenced to less than a year of incarceration, no incarceration or not convicted at all.

their likelihood of conviction impacted by the 2007 election result. This is also observed in Table 3.^{32,33}

In addition, it is also clear that first-time felony defendants exhibit larger discontinuities in deferral and conviction rates compared to repeat felony defendants. This pattern reflects the fact that first-time felony defendants received deferrals at roughly five times the rate of repeat defendants (33 versus 7 percent), and so the repeat defendant caseload had a smaller response margin after the 1994 penal code or the 2007 election. However, the significant first-stage effect on diversion for repeat offenders in our focal groups provides a valuable opportunity to assess whether avoiding a felony conviction record is a key component through which court deferrals impact defendants' future outcomes. First-time offenders can maintain a clean record through a court deferral whereas repeat offenders can only avoid potentially harsher sanctions. If similar future outcomes are observed for both groups, it would suggest some other mechanism besides stigma is causing the changes in future behavior.

Our focal analysis sample throughout the paper will be first-time felony defendants in the 1994 affected statute group and the 2007 low-risk group (we refer to the combined group as first-time low-risk felony defendants). This restriction serves several purposes. First, through avoiding the unaffected statutes and high-risk groups, we can be more confident that other potential changes occurring at the threshold are not contaminating our results. Second, if the policy changes impact criminal recidivism then the study sample could be differentially impacted by endogenous sample entry across the threshold. Through imposing the first-time offender restriction, we ensure that each defendant will only appear once in our estimation sample. Finally, individuals without a felony record are generally the target of diversion programs in the U.S. and so providing evidence for this group is particularly relevant and important from a policy perspective.

³²There 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.

³³The motivation behind isolating the low-risk group from the high-risk group also comes from the fact that the passage of the previously mentioned \$1 billion state-wide bond to expand state prison capacity increased both the sentenced length of incarceration as well as time served for those sentenced to the state prison system (99.8 percent of which fall into the high-risk group). As such, failing to distinguish between these two group creates a challenging pattern as one group experienced more lenient sanctions across the threshold while the other experienced more severe sanctions across the threshold (and had no change in deferral rates). To avoid this issue altogether, we simply focus our analysis on the low-risk group that should not have been impacted by the state-wide prison bond.

Although not part of our focal sample, we also estimate effects for repeat offenders and discuss these results in Section 4.4 to help distinguish whether avoiding a felony record is an important channel through which a court deferral influences future defendant outcomes.

3. Research Design

We estimate the reduced form effect of the 1994 and 2007 deferral policy changes for first-time felony defendants using a sharp regression discontinuity (RD) design. We present both graphical evidence as well as statistical tests to confirm the reliability of our results. For our statistical tests, we follow the approach of Calonico et al. (2014, 2016a) 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:

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

where,

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

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). In our primary specification, we pool defendants from both the 1994 and 2007 experiments. To facilitate a combined analysis, we unify the running variable across the two natural experiments such that there is a discontinuous increase in court deferrals at $X_i = 0$ for both samples:

$$X_i = \begin{cases} 09/01/94 - DateCharge_i & \text{if } Sample_{1994} == 1 \\ DateDisp_i - 11/07/07 & \text{if } Sample_{2007} == 1 \end{cases}$$

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 polynomial function:

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

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

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,³⁴ 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. Three days before and after each discontinuity (one week total) are dropped from the analysis to protect against any potential contamination from transitory sorting across the threshold. Our primary specification also adjusts for baseline covariates of age, gender, race/ethnicity, and prior number of misdemeanor convictions.³⁵ These parameterization decisions can be modified without impacting the findings of this study as documented in Appendix Tables A3, A4, and A5.

We first measure the effect of the discontinuity on case dispositions (felony conviction, court deferral, case dismissal) to measure our “first-stage”—a discontinuous relationship between the running variable and the outcome of the felony charge. We then measure the reduced form effect

³⁴We use the option *msecomb2* within the STATA *rdrobust* command described by Calonico et al. (2016a) 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.

³⁵See Calonico et al. (2016b) for notation of this methodology including baseline covariates.

of the transition between the low and high deferral regime on future offending behavior. We focus on reduced form estimates rather than treatment effects from a fuzzy RD design because of the stronger identification assumptions required for such a design. For instance, our setting may violate the exclusion restriction if exogenous shifts also occur between court deferrals and case dismissals on top of shifts from felony convictions to deferrals or dismissals. Violation of this assumption could lead to an underestimate of how many defendants have their court dispositions affected by the discontinuities which could lead to overestimates of the causal effects. We do not, however, see changes in the sentencing of defendants (e.g., drug rehabilitation, probation, or incarceration) as violating the exclusion restriction as these changes would be derivative of the impact to court dispositions. Instead, we view these as additional potential mechanisms that can explain changes in defendant outcomes across the discontinuity. Because evaluating the magnitude of the court deferral's treatment effect is fundamental for policy analysis, fuzzy RD estimates are still presented in Section 5 with the caveat that stronger assumptions are necessary than needed elsewhere in the paper.

To attribute a causal interpretation to our reduced form RD estimates, we must assume that defendants are effectively randomly allocated before and after the two threshold: individuals charged immediately before September 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 after November 6, 2007 should be observationally equivalent to those disposed before and we should not see a discontinuity in the total number of dispositions.³⁶

A general deterrence response to the change in deferral rates leading to a discontinuous change in the likelihood of committing crimes would threaten this assumption. Additional threats to our empirical strategy include changes in policing practices or sorting of offenders by prosecutors/judges across the threshold dates in order to guarantee they face one punishment regime versus the other. This is particularly concerning in the context of the 1994 reform when all relevant actors could fully anticipate the adoption of the new penal code. Each of these threats would have the testable prediction of discontinuous changes in either the size or composition of the criminal caseload across the discontinuity, which will be empirically evaluated in the next

³⁶Dispositions include case dismissals, guilty verdicts and court deferrals.

section.

4. Empirical Results

Tables 1 and 2 show the summary statistics for the main analysis sample (first-time felony defendants), specific analysis subgroups and each court disposition group individually.

Our focal sample is predominately male (73%), around 30 years of age, and fairly evenly split between three mutually exclusive race and ethnicity categories (Non-Hispanic African American, Non-Hispanic White, and Hispanic). The majority of our first-time low-risk felony defendant sample are charged with drug (45%) or property (37%) offenses. Because violent crimes were not affected by the probated incarceration provision in 1994, they only comprise 6% of our estimation sample. With respect to pre-charge criminal histories and labor market outcomes, our focal sample has on average 0.58 misdemeanor convictions, 20 days in jail or prison, a 27 percent quarterly employment rate, and \$1,380 of average quarterly earnings.

From the 1994 to 2007 samples, there is a discernible shift away from African American to Hispanic defendants. The incarceration rate is higher in 2007 (45 percent compared to 26 percent), yet the average (unconditional) incarceration length drops to 1.5 months from over 5 months in 1994. The unconditional average duration of probation similarly contracts to 1.1 years from 3.1 years.

The repeat felony defendant sample is comprised of defendants who have been previously convicted or deferred on a single felony charge.³⁷ By limiting the repeat defendant sample, we aim to: (1) narrow the sample to a group for whom there is a clear first-stage relationship across the threshold, and (2) make the descriptive characteristics of the sample more closely resemble that of the first-time defendants.³⁸

³⁷We include previously deferred individuals since being charged with a new offense will likely convert their prior deferral to an official conviction thus creating a prior felony record.

³⁸With regard to this second aim, the less similar the first-time and repeat defendant groups are the less confident we can be that we are identifying the relative importance of a felony conviction record since both criminal history as well as other demographic characteristics are changing between the two samples. While imposing the one prior conviction restriction does improve similarity in the caseloads, clear differences still remain: the repeat defendants are older, more likely to be African American, experience significantly higher conviction and incarceration rates, have greater prior exposure to the criminal justice system and have significantly worse

Among the various court dispositions, there is a clear pattern of selection between those receiving deferrals versus convictions (Table 2). The deferral group is on average less likely to be male, roughly 2.3 years younger, has fewer prior misdemeanor offenses, and exhibits better labor market outcomes. All individuals who receive a deferral are placed on probation with an average length of 3.5 years.³⁹ One limitation of the data is that probated incarceration sentences from after the implementation of the new penal code in 1994 may have been recorded in the electronic administrative records as an incarceration sentence omitting any indication of probation. As such, the 74 percentage point differential in probation between the deferral and conviction group must be taken as an upper bound since probations in the conviction group are likely underestimated.

4.1. Caseload Density and Baseline Characteristics

In support of causal identification, we do not observe discontinuities in caseload densities or in baseline defendant demographic characteristics including gender, age, race, and ethnicity. Balance across these factors is presented graphically in Figures 2 and 3 and estimated discontinuities using our primary RD methodology are presented in Table 4.⁴⁰ We also do not find evidence of a discontinuity in the prior number of misdemeanor convictions for our estimation sample. As a further test, we calculate a predicted recidivism risk score for each defendant using the baseline characteristics and prior number of misdemeanor convictions.⁴¹ 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. This prediction is confirmed in Figure 3 as well as the coefficient reported in the first column of the second row of Table 4.

pre-charge labor market outcomes.

³⁹In 1994 (2007), the average deferral probation length is 5.2 (2.5) years.

⁴⁰Balance tables that separately consider the 1994 and 2007 experiments and confirm balance in the subsamples can be found in Appendix Tables C1 and C2.

⁴¹We calculate our measure of recidivism risk from the predicted dependent variables for each individual from the following OLS regression: $\text{Total Charges}_i^{\text{5 Years}} = \alpha + \mathbf{X}_i' \beta + \varepsilon_i$, 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 five years based on their 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.

Continuity in the caseload density, demographic composition, and predicted recidivism risk strongly support our identification assumption of continuity in unobserved determinants of our outcomes across the threshold. In Table 4, we test whether any discontinuities exist across prior incarceration and labor market outcomes. We do not detect statistically significant differences in prior outcomes corresponding to the sharp change in court deferrals. We do, however, estimate a significant difference in the ability to match defendants to the labor market data. Upon further investigation, this difference is driven by an inability to match some defendants whose felony charges are initially dismissed since a social security number that is necessary for our match is often omitted in the electronic court records for this group. As reported in Appendix Table C2, an increase in the proportion of case dismissals following the 2007 shift creates the lack of continuity detected in the probability of a match between the court records and administrative labor market data. While any imbalance in observable characteristics across the threshold is a concern, we expect the exclusion of individuals for whom a case is dismissed to bias our estimates of the labor market impact downwards given the benefits we find from diversion away from conviction.

4.2. Court Verdicts and Sentencing Outcomes

While the defendants appear observationally equivalent across the threshold, the court outcomes differ quite dramatically (Figure 4 and Table 5). The share of felony conviction verdicts decreases from nearly 55 percent to 35 percent for our estimation sample. At the same time, the rate of diversion through a court deferral increases from around 32 to 48 percent, nearly a one-for-one tradeoff with the shift in felony convictions.

The differences in deferral rates has downstream consequences for criminal sentencing. In our setting, a court deferral always includes a sentence to probationary supervision and rarely includes an incarceration sentence. For drug offenders, the deferral agreement also can include a sentence to a drug treatment program.⁴² In Figure 4 and Table 5, we observe a large drop in the

⁴²15.6 percent of the drug felony defendants in our estimation sample whose case is disposed with a deferral also receive a drug treatment sentence.

fraction of felony defendants sentenced to incarceration caused by the increase in deferrals.⁴³ These notable differences in sentences, however, translate only into small differences in actual experiences incarceration during the period immediately following case disposition as seen in Panel A of Table 6. Furthermore, we do not find large differences in the total days in prison or jail for defendants in the high- vs. low-deferral regimes.⁴⁴ We do not estimate a statistically significant increase in drug treatment sentences in Table 5, but Figure 4 provides some visual evidence that suggests a small increase.⁴⁵

4.3. Reoffending and Labor Market Outcomes

To assess the impact of these policy shifts on future criminal justice outcomes, we present evidence for four separate measures of future offending over a five-year period following the focal charge: bookings in the Harris County Jail, charges in the Harris County Criminal Courts, convictions in the Harris County Criminal Courts, and convictions in the TX statewide Criminal Conviction History database. We report estimated discontinuities along both extensive and intensive measures of these four outcomes in Table 7. Overall, we find that an increase in diversion is associated with a large decrease in reoffending. These effects are substantial in magnitude and are supported by the visual evidence presented in Figure 5. We observe a 5.6 percentage point decrease in reconviction in Harris County, reflecting a 9 percent increase in desistance over the five year follow-up period. Even larger impacts are observed on the intensive margin: 0.24 fewer total criminal convictions in Harris County representing a 26 percent decline.

Comparing estimated effects across different recidivism measures, the estimated impact is larger for charges and convictions than Harris County jail bookings (a proxy for arrests). A possible explanation of this pattern is that criminal convictions may have an impact on the likelihood of escalation from one stage to the next in the criminal justice system independent of

⁴³In 1994, the increase in sentenced incarceration following the reform was subject to a mandated community supervised release requirement in the new penal code and so the change in sentenced incarceration does not necessarily represent higher rates of actual experience in jail or prison.

⁴⁴The timing of the incarceration patterns over the five year follow-up period is discussed in the following subsection.

⁴⁵In a subgroup analysis focusing on first-time drug felony defendants, we do find a statistically significant 3.1 percentage point increase in drug treatment sentences. Results are available upon request.

any impacts to the underlying illegal activity itself.⁴⁶

We also present estimates based on the statewide conviction database to address concerns about out-of-county migration. These results are generally consistent with the Harris County conviction estimates although slightly smaller in absolute magnitude.⁴⁷ As discussed in Section 2, the statewide database does not capture all convictions due to the voluntary nature of reporting to the program,⁴⁸ but does capture information on convictions outside of Harris County. In Appendix Table A1, we split the statewide conviction outcomes into Harris County and non-Harris County convictions and find that total effects are largely concentrated in Harris County but we also observe a statistically significant decrease in non-Harris County convictions as well. This indicates that the smaller statewide magnitudes are not a consequence of geographic displacement of criminal activity missed in the county-specific estimates but instead result from measurement error due to incomplete coverage in the statewide database.

To assess heterogeneous impacts across different types of reoffending outcomes, we estimate effects for outcomes by crime type in Table 8. The impact on reconvictions is roughly evenly split between felony and misdemeanor offenses. Additionally, we see that the largest impacts are concentrated in property crimes — typically larceny, burglary, or fraud — with smaller but still significant impacts on both future drug and violent convictions. The large impact on property crimes is suggestive of potential effects on employment or self-sufficiency which is the next focus of this section.

We begin our analysis of labor market outcomes by examining the five-year total impact on employment, earnings, and tenure in Table 9.⁴⁹ The second row of plots in Figure 5 provides the corresponding graphical evidence. Overall, the high deferral regimes exhibit higher likelihood of employment and improved earnings in the covered sectors of the TX unemployment insurance

⁴⁶An extreme reading of Table 7 could suggest that deferrals had minimal impact on underlying illegal behavior but instead only changed the way that police, prosecutors and judges treated and coded criminal episodes. This labeling effect hypothesis, however, could not account for the labor market impacts which, in our opinion, limits its explanatory power and relevance.

⁴⁷In terms of percent changes, the estimates are very similar. Relative to the low-deferral group means, we estimate a decrease of 25.8 percent using Harris County convictions and a decrease of 26.6 percent using the statewide database.

⁴⁸This is evident from the large decrease in the low-deferral mean conviction outcomes observed in the statewide database relative to the Harris County conviction records.

⁴⁹All wages are inflation adjusted using the Houston MSA consumer price index with the year 2000 as the baseline.

administrative data. The 16 percentage point increase in the deferral rate is associated with a 4.3 percentage point increase in the quarterly probability of employment during the five year follow-up period, of which 2.7 percentage points are for individuals whose earnings exceed the federal poverty level for a single adult (Panel A). This trend is reflected in the statistically significant 0.3 log point impact on log average quarterly earnings (Panel B).

We further explore the nature of the labor market impacts through evaluating the effects on employment and earnings tenure during the follow-up period. Panel C of Table 9 shows the impacts on the maximum continuous period of employment with a specific employer for each defendant as well as the maximum continuous period of earnings across all employers (both measured in total quarters). Consistent with the prior results, we find an increase in diversion generates longer durations of continuous within-firm employment (0.56 quarters) and continuous earnings (0.73 quarters). Given that the effective first-stage of the experiment ranges between 16 and 19 percentage points depending on whether we use the change in felony convictions or court deferrals, the magnitude of these results imply that deferrals caused substantive changes in the lives of the affected criminal defendants.

The timing of the impacts over the five year follow-up period is traced out in Figure 6.⁵⁰ Court deferrals appear to have a fairly stable impact on both reconvictions and employment over the five year follow-up period. In the case of recidivism, impacts do not emerge until a few quarters after the focal felony episode and for both outcomes, the impacts weaken somewhat in the last year of the followup. The timing of effects on incarceration suggest that the reoffending and labor market impacts are not heavily influenced by an incapacitation mechanism.⁵¹ We further discuss the key mechanisms implied by our results in Section 5.

Tables 7, 8, and 9 consider criminal and labor market outcomes independently. Theoretically the incidence of these impacts could fall on mutually exclusive subgroups, a common population or some combination thereof. Differentiating between these possibilities strengthens our understanding of the mechanisms at work as well as the potential joint determination of these

⁵⁰These figures plot out estimates using our primary RD specification described in Section 3 for specific quarter-by-quarter time windows.

⁵¹We cannot tie a given incarceration spell observed to a specific conviction due to data limitations. The sentence for the focal offense could explain differences in incarceration during the first year, but would not explain the observed differences in later years depicted.

plausibly interdependent outcome variables.⁵²

To distinguish these possibilities, we create four mutually-exclusive outcomes for the five-year follow-up period to test these possibilities: *stable* individuals who work for at least half of the five year follow-up period and have no recorded convictions during the five-year follow-up period;⁵³ *unstable* offenders who work for less than half the follow-up period and have at least one conviction; a *reoffending* group who works at least 50% of the time but also experiences at least one future conviction; and an *out of work* group of offenders who work for less than 50% of the time but do not reoffend. These categories are illustrated in Figure 7.

The results presented in Table 10 suggest that an increase in diversion leads to a substantial increase in the fraction of *stable* individuals who jointly experience higher levels of employment and lower levels of future convictions. These estimated effects imply a striking difference in outcomes for these two observationally-equivalent groups of first-time felony defendants. Given a 16 to 19 percentage point change in court dispositions, we observe a 7.2 to 7.5 percentage point shift from the *unstable* to *stable* outcome. While discussion of treatment effects is generally held for Section 5, the results from this exercise would imply that court deferrals have a meaningful and substantive impact on 1 in every 2 to 2.5 marginal defendants.

4.4. Heterogeneity Analysis

We turn towards an examination of effects across different subpopulations to first show how effects differ across the two natural experiments we, to this point, combined into a unified analysis and to provide insight into the groups that benefit most from court deferrals (Table 11). For our analysis of heterogeneous effects, we use our primary RD methodology and present the estimated impact of an increase in the use of diversion on the initial case disposition (felony conviction

⁵²As an example, an offender who faces diminished labor market opportunities due to a felony conviction may be more likely to engage in property crime for economic stability. Similarly, an offender who receives a court deferral and then obtains a full-time job will have less time to devote towards criminal activity. These are clear departures from a world wherein one group suffers employment loss but no change in criminal behavior while another group exhibits the opposite pattern.

⁵³We selected 2.5 years of employment as the cutoff to satisfy several goals. First, we aimed to use a definition that represented a non-trivial amount of time, while recognizing that incarceration spells may make it impossible to work for all 5 years in the follow-up period. We also wanted to use a cutoff that would have defendants represented in each of the four mutually exclusive outcomes.

and court deferral) and for two outcomes capturing reoffending (Harris County convictions) and labor market effects (average quarterly employment) during the five year follow-up period.⁵⁴

Panels A and B present estimates from subsets of the main analysis sample: the 1994 and 2007 natural experiments separately,⁵⁵ and by type of original criminal offense (e.g. drug, property, or violent). We find statistically significant impacts on both reoffending and employment in both experiments. While the magnitude of the point estimates are similar, the 2007 shift exhibits a smaller shift in initial court dispositions (14 percentage points compared to 26 in 1994) and implicitly a greater change with respect to case dismissals given the gap between the felony conviction rate and the deferral rate.⁵⁶ It is difficult to know precisely why 2007 shows a higher ratio of reduced form to first-stage coefficients. It is possible that the increase in outright dismissals produced a stronger treatment effect in the 2007 experiment. It is also possible that the growing prevalence and coverage of criminal background checks over time made the impact of avoiding a felony conviction record more consequential. Unfortunately, given the limitations of the research design, we can only speculate as to why we observe these differences.

As described in Table 1 and Section 2, our estimation sample is primarily comprised of first-time drug and property offenders and a small fraction of violent offenders. Our overall results appear to be driven by drug and property offenders. The magnitude of the point estimates of the impact on reoffending and labor market outcomes between drug and property offenders is similar, but the “first-stage” effects on court dispositions are smaller for property offenders implying larger benefits for this group. Given the small sample of violent offenders, there is not as much precision in estimating their reduced form impacts. The magnitude of the employment coefficient is in line with that observed for other types of offenders, but the effect on recidivism is a clear departure.

Finally, Panel C reports estimates from a separate sample altogether: individuals with one prior conviction or deferral.⁵⁷ For this group of repeat felony defendants, there is a statistically significant shift of roughly 6.3 percentage points in felony convictions and court deferrals but no

⁵⁴Appendix C provides further results and RD graphs for the subgroups.

⁵⁵Note that the running variables used in the heterogeneity analysis are consistent with the unified analysis.

⁵⁶We observe marginally insignificant 2.7 percentage point increase in case dismissals for the 2007 subsample.

⁵⁷See Footnotes 37 and 38 for explanations of this restriction.

clear evidence of changes in reoffending or labor market outcomes.⁵⁸ These results suggest that the strong gains for the first-time felony defendant caseload are likely related to the stigma effect of a felony conviction.

We further explore heterogeneous effects across our main estimation sample of first-time felony defendants split by age, race/ethnicity, and gender in Table 12. All groups exhibit large changes in case disposition, but an interesting pattern of results emerge upon examination of the future outcomes. While we do not find large differences between male and female defendants in (Panel A), when splitting our sample by age (Panel B) we find substantial decreases in reoffending and increases in employment for defendants younger than 30 years old. However, despite a strong first-stage for older offenders, we do not find significant changes in future convictions or employment. These effects are consistent with a greater response among younger offenders as they have more to gain from a clean record. Among subsamples split by race and ethnicity (Panel C of Table 12), we find the largest reductions in reoffending associated with the increase in diversion for African American defendants and similar effects on labor market outcomes for White and African American.

Given these results, we next focus on measuring whether effects differ across the spectrum of the predicted recidivism risk score.⁵⁹ We prefer this approach to the typical sub-group analysis for several reasons. First, our sample sizes can be quite limited for some subgroups which undermines the precision of the estimates and could be misunderstood to indicate a lack of an impact. Second, it is difficult to isolate effects specific to certain groups since there is a high degree of correlation between certain demographic traits. For example, female offenders are twice as likely to be white than male offenders, so heterogeneous effects across gender categories may also reflect differences across race.

This is accomplished by separately running our local polynomial RD estimates for each

⁵⁸The first-stage relationship for this group is significantly smaller compared to our main estimation sample. As a result, there is less identifying variation, which consequently could lead the reduced form estimates regarding recidivism and future employment to lack precision and statistical significance. For this reason, the results are suggestive that felony convictions records are a central factor in the main estimation sample but are not conclusive.

⁵⁹See Section 4.1 for a description of the construction of this index. To account for the bias discussed in [Abadie et al. \(2016\)](#), the estimation of the risk score employs a leave-one-out or jackknife estimation procedure for the purpose of these exercises.

percentile in the risk score quantile function. Because we are constrained by our sample size, we utilize a thirty 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, asymmetric bandwidths occur above the 85th and below the 15th percentiles.⁶⁰ Figure 8 provides information on the shape 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 more prior misdemeanor convictions.

The first row of Figure 9 displays the results of this exercise for court outcomes and future behavior. Surprisingly, the discontinuous change in conviction status and court deferrals remains fairly constant throughout the distribution, indicating that prosecutors and judges almost uniformly increased the use of diversion across the predicted risk distribution rather than offering diversion on the margin only to low-risk offenders. This pattern allows us to rule out sizable interactions between the underlying risk score distribution and the experimental variation which would make interpreting the reduced form effects on long-term outcomes more difficult.

The second row of Figure 9 shows the results for total convictions and average employment rate during the five year follow-up period. These graphs clearly demonstrate that the strongest response to the increase in diversion across both reoffending (total convictions) and labor market outcomes (probability of employment) exists among those with the highest predicted risk of recidivism.

These results are striking in the fact that observable characteristics would predict that the marginal high-risk defendants would have worse future outcomes. Yet, we find that a court deferral all but closes the employment gap between the 90th and median risk percentiles and nearly halves the reconviction gap between these groups. Two related mechanisms could generate these results. First, young, African American men may be overtargeted by law enforcement leading to a disproportionate treatment response in the marginal subpopulation. Second, consistent with existing evidence from audit studies (Pager 2003), the subpopulation may suffer more

⁶⁰This exercise requires the stronger assumption that defendants before and after the discontinuities exhibit similar risk score distributions. Figure C9 shows a series of local polynomial RD estimates using whether an offender was at or below a specific percentile in the risk score distribution as an outcome variable. This provides a consolidated way to demonstrate balance throughout the distribution (as opposed being balanced on average as was previously documented in Figure 3).

on average from a felony conviction record due to statistical discrimination or animus.

4.5. Robustness Checks and Placebo Exercises

One shortcoming of measuring reoffending outcomes using Harris County records is that differential mobility could bias the results, particularly if defendants with deferrals have greater ability to travel out of Harris County. To assess this, we compare Harris County and non-Harris County reoffending outcomes in Texas using the state-wide Computerized Criminal History (CCH) Database (Table A1). If differential mobility were biasing the results, we should expect to see a negative impact in Harris County and a positive impact elsewhere in the state. In contrast, the results show a decline in reoffending both in and outside of 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 true gains from deferrals.⁶¹

We conduct several additional robustness checks to confirm the reliability of our results. Table A2 presents estimated effects of a placebo experiment where we shift the cutoff dates to one year earlier (September 1, 1993 and November 7, 2006) or one year later (September 1, 1996 and November 7, 2008) than our actual cutoffs. The goal in these exercises is to investigate whether our findings could be picking up latent seasonal breaks.⁶² Overall, we do not find any meaningful discontinuities in initial court outcomes, future reoffending, or future labor market outcomes in these placebo exercises.

We also provide a number of other robustness checks including specifications using several alternative bandwidth selection methods (Table A3) and alternative variance estimation strategies (Table A4). Table A5 reports results for several other modifications including models without the donut restriction, without including covariates, and without using the robust standard errors and bias correction suggested by Calonico et al. (2014). We also provide estimates from models

⁶¹While 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, this could bias our estimated effects. However, we expect this bias to go in the opposite direction of our estimated effects as those receiving a felony conviction compared with a court deferral would be more likely to be deported.

⁶²For example, the beginning of the school year or the timing of elections may impact criminal behavior or unobserved sample characteristics.

based on outcomes aggregated to the week level. Among these tables, there is no evidence to suggest our findings are driven by arbitrary implementation decisions.

Because the running variable in both of our discontinuities rely on time variation, another strategy to quantify the magnitude of these effects is to estimate a structural break in a time series framework. We re-evaluate our key results using a standard time series framework in Appendix B. Overall, results are closely aligned with our preferred RD approach.

5. Conclusion

This paper studies two sharp discontinuities in court deferrals — a cost-saving diversion strategy that shields defendants from further interaction with the criminal justice system — among first-time felony defendants in Harris County, TX. While these two changes occurred 13 years apart and originate from different contexts, we find large and consistent impacts from both experiments on future offending and labor market outcomes. Through combining two natural experiments that together exhibit both increasing and decreasing deferral rates towards drug offenders in the same location, we can be more confident that our estimates represent a causal impact of deferrals and are not simply capturing latent trends in recidivism or other unobserved shocks contemporaneous with the experiments.

Through a set of data-driven regression discontinuity plots and statistical tests, we demonstrate that first-time felony defendants who become 19 percentage points less likely to receive a felony conviction through either a court deferral or outright dismissal are significantly less likely to recidivate and significantly more likely to work within a 5 year follow-up period. Moreover, these impacts on criminal activity and labor market behavior are jointly determined—our results indicate a substantial shift towards desistance and self-sufficiency when court deferrals are more prominent.

The main analysis in this paper presents reduced form estimates of the aggregate impact of the discontinuities on future outcomes. While requiring stronger assumptions,⁶³ it is also

⁶³See discussion in Section 3.

important to report treatment effect estimates based on a fuzzy RD design to better understand the magnitude of the effect of deferrals on individual defendants.⁶⁴ The estimated treatment effects are presented in Table 13, showing just how consequential a deferral could be for first-time defendants: a decline of any re-convictions by 32 percentage points, 1.3 fewer crimes, a 19 percentage point higher likelihood of employment in a quarter and 1.4 more log points of average earnings in a quarter. With regard to the extensive margin changes, we observe an 72 percent decline in recidivism and a 57 percent increase in employment relative to average outcomes for convicted defendants during the low-deferral period.

While our empirical strategy does not provide direct tests of specific mechanisms, some patterns in the results help us better understand why we observe such large improvements from an increase in diversion. First, the net impact and timing of outcomes allows us to rule out specific deterrence and incapacitation as dominant mechanisms. Those receiving more lenient dispositions show better future outcomes — the precise opposite of what would be expect from a strong specific deterrence channel. Additionally, there is not a strong negative relationship between the incarceration and convictions either overall or in the timeline figures as predicted by incapacitation.

A general deterrence effect stemming from the change in sentencing regime also appears quite limited in this context. While a decrease in the expected cost of crime among defendants could have encouraged new individuals to start committing crimes, we do not observe any significant changes in either the caseload size or composition across the threshold. This is true in the overall estimation sample as well as in the 1994 and 2007 specific sub-samples.

We cannot, however, fully rule out another related deterrence mechanism which we term *sentence overhang*. If a defendant violates their court deferral agreement, which on average last 3.5 years in the sample, they can be convicted and sentenced at that time for their original offense. This potential mechanism would predict declines in recidivism while the agreements are active with the strongest effect on minor crimes.⁶⁵ The high deferral regimes do exhibit declines

⁶⁴Because the 2007 experiment shows a small but significant impact on case dismissals we collapse court deferrals and case dismissals into a single outcome for this part of the analysis so as to not underestimate the first-stage relationship and overstate the treatment effects.

⁶⁵A 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 deferral resentencing, whereas a new serious felony

in misdemeanor convictions and lower recidivism during the first 3.5 years. But, there are also declines in felony convictions as well as impacts in the remainder of the follow-up period. As a consequence, we do not believe that the sentence overhang mechanism can fully explain our results.

We believe the stigma of a felony conviction record remains a strong and viable mechanism that contributes to our overall results. This is supported by two key pieces of evidence. First, the impacts of a deferral remain fairly constant throughout the followup period. Second, similar impacts failed to materialize for repeat defendants who already have a felony conviction record.

A felony conviction record could affect offender outcomes because employers avoid hiring felony convicts or because police, prosecutors or judges view and treat felony convicts differently in the criminal justice system. Either type of discrimination could have spillover effects from one domain to another (lack of employment leads to criminality; further sanctioning in the criminal justice system worsens job prospects). Moreover, defendants may change their own behavior in response to receiving a conviction if they anticipate a stigma associated with the conviction. Pursuing further criminal activity will become relatively more attractive since they already suffer from the potential stigma of having a felony conviction on their record and they might decrease their job search effort if they believe (correctly or incorrectly) that employers will not hire them. A better understanding of these potential dynamic relationships is an important area for future work.

A striking pattern emerges that those who have the highest rate of predicted recidivism based on their observed covariates stand the most to gain from a second chance in the form of a court deferral. These individuals are typically young, African American men with one or more misdemeanor convictions already on their record, a group discussed as potentially over-targeted by law enforcement in the United States. The empirical pattern is consistent with previous evidence from correspondence and audit studies indicating an interactive treatment effect between race and conviction status on labor market outcomes (Pager 2003), but we believe this paper is the first causal evidence of this pattern based on a true rather than experimentally

conviction (e.g., aggravated assault) would trigger a felony conviction and harsh sanctioning regardless of whether a deferral was active at the time.

generated population. Importantly, our results indicate that intervening for this hard-to-serve population at this critical moment (i.e., when they are being charged with their first felony offense) could significantly alter their life course in a positive direction.

Substantial changes to laws and sanctions associated with certain types of criminal activity have been made at the state and federal level over the past several years. The overarching trend is towards more leniency, especially for first-time and low-risk defendants. Our results suggest that these changes may lead to lower rates of reoffending and higher rates of rehabilitation over the coming years. Our results also suggest that improving defendant outcomes may be achievable through modifying sanctions in ways that do not require significant investments or changes to the existing criminal justice infrastructure. In summary, diversion options (such as court deferrals) for low-risk offenders should be viewed as an attractive and feasible option for jurisdictions seeking to reduce the fiscal cost and community impact of their criminal justice system.

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6. Figures

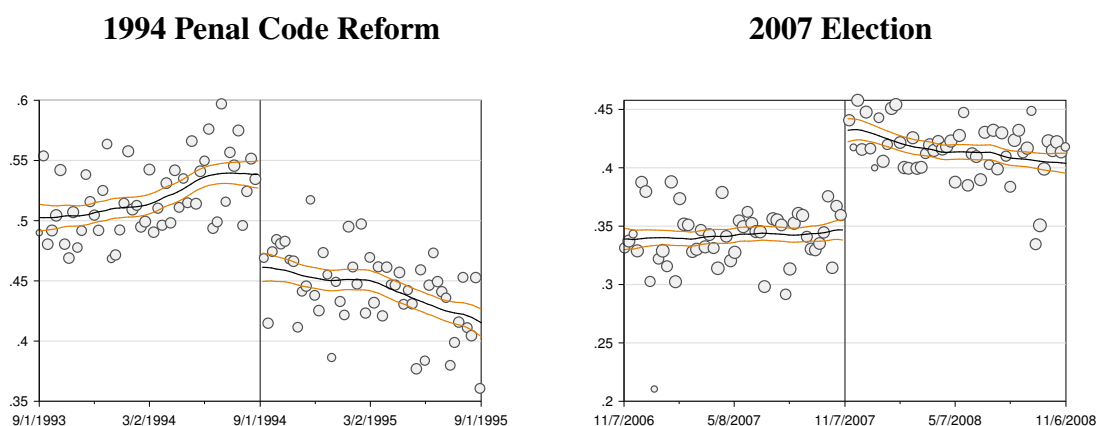


Figure 1: Discontinuities in non-conviction court outcome (i.e., court deferral or dismissal) in the entire felony caseload

Figure Notes: This figure shows the relationship between the probability of a felony conviction court outcome by date of filing (1994) or disposal (2007) for the entire felony caseload. Discontinuities in specific court outcomes for our primary estimation sample are presented in Figure 4.

General Graphing Notes: The scatter plots are generally pooled at the week (or day) level and weighted according to the total number of cases. The local-polynomial lines are based on data at the exact date level. All plots and local-polynomial lines omit observations during the week of the cutoff. Unless otherwise noted, subsequent figures combine data from the two discontinuities using a unified running variable measuring the distance in days from the threshold date and are based on our primary estimation sample described in Section 2.

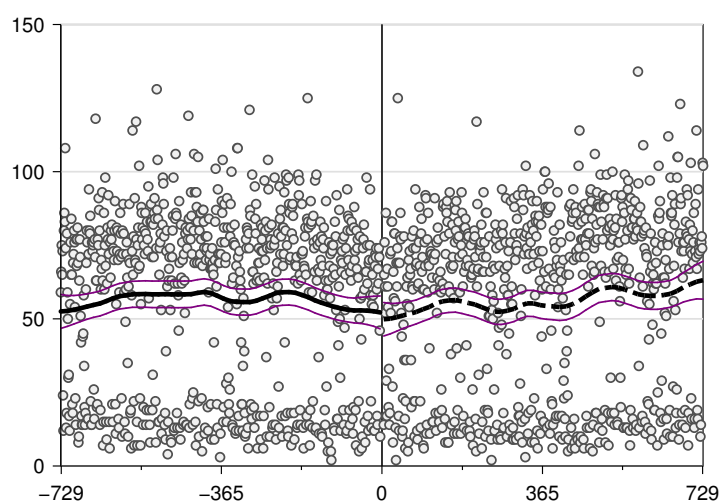


Figure 2: Discontinuities in caseload density

Figure Notes: This figure shows the relationship between the number of cases by the focal date. Weekend days and holidays comprise the points above with lower average caseloads than weekdays. Table 4 reports the corresponding estimated discontinuity using our regression discontinuity methodology. Formal manipulation testing procedures for local polynomial density estimators as proposed in Cattaneo et al. (2016) via the *rddensity* procedure in STATA fail to reject the null of no manipulation with a corresponding p-value of 0.38. General graphing notes from Figure 1 apply.

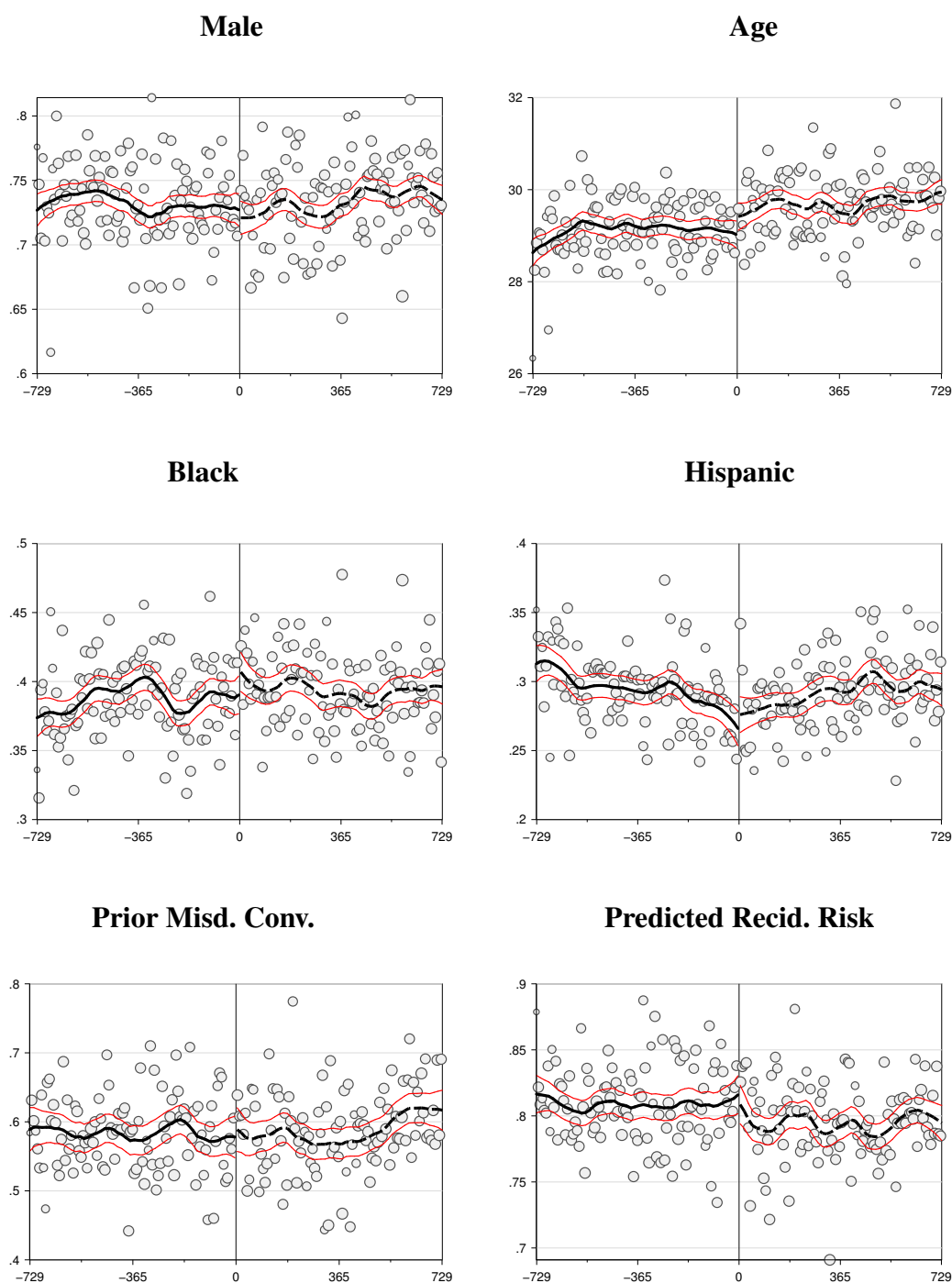


Figure 3: Discontinuities in baseline characteristics and predicted risk of recidivism

Figure Notes: This first two rows of this figure show the relationship between the focal date and the proportion of each week's cases by demographic characteristics. The third row plots the number of prior misdemeanor convictions (left column) and the predicted risk of recidivism (right column) based on all of the baseline characteristics. Table 4 reports the corresponding estimated discontinuities using our regression discontinuity methodology. General graphing notes from Figure 1 apply.

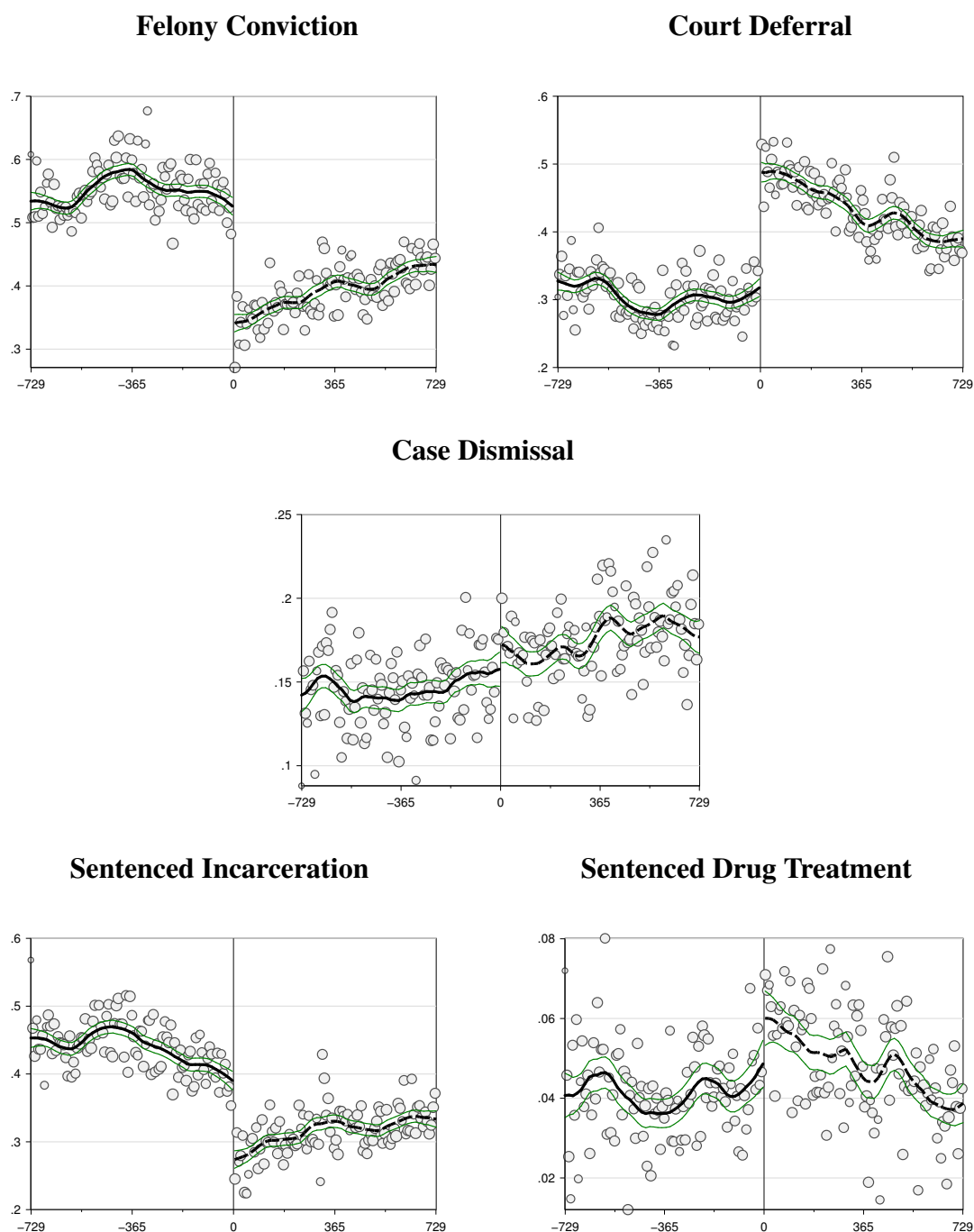


Figure 4: Discontinuities in court outcomes

Figure Notes: This figure shows the relationship between the focal date and court outcomes for our primary estimation sample. The first three plots graph the relationship between the running variable and the court verdict. The final two plots depict the relationship between the running variable and the immediate court imposed sanctions after considering the focal charge. Table 5 reports the corresponding estimated discontinuities using our regression discontinuity methodology. General graphing notes from Figure 1 apply.

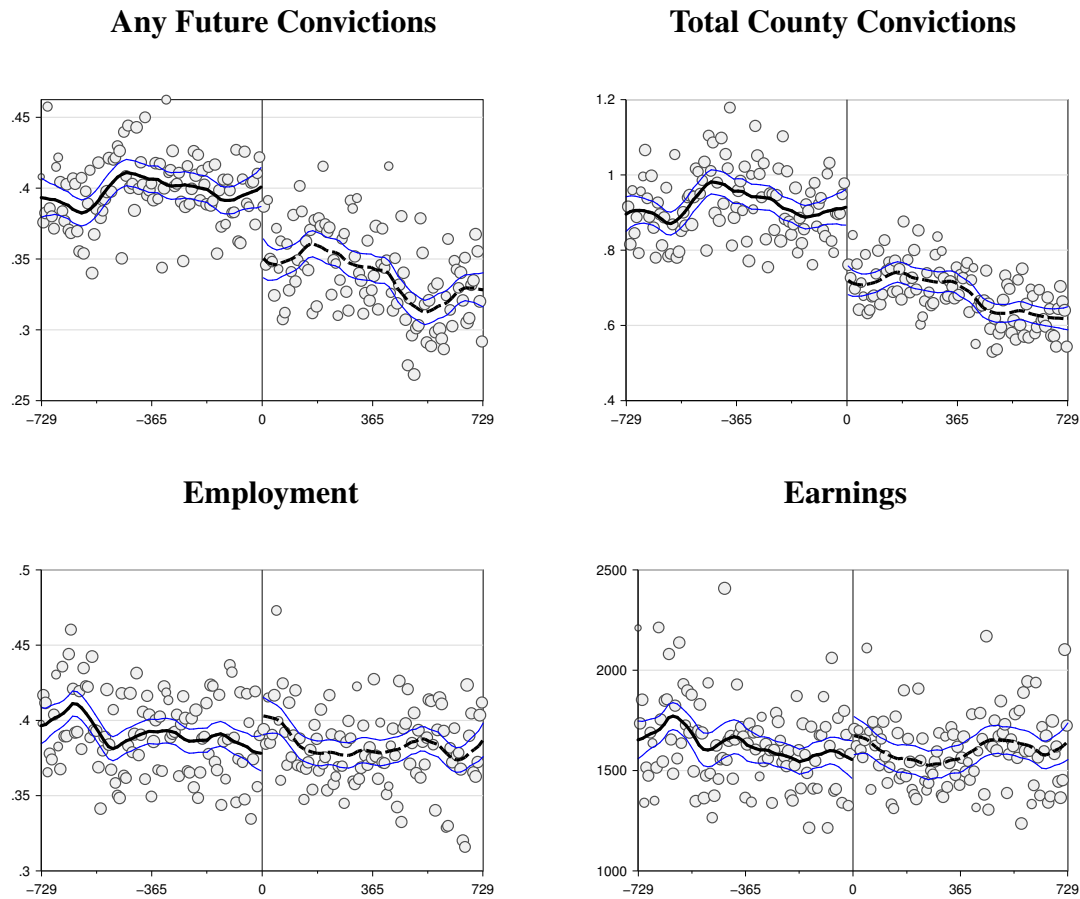


Figure 5: Discontinuities in five-year criminal justice and labor market outcomes

Figure Notes: This figure shows the relationship between the focal date and outcomes over a five-year follow-up period for our primary estimation sample. The first row depicts the relationship between the running variable and reoffending outcomes: whether the defendant had any future convictions in Harris County within the five year follow-up period (left column); and, the total number of convictions in the Harris County criminal courts during the follow-up period (right column). The second row presents the relationship between the running variable and labor market outcomes: the probability of employment each quarter (left column); and, average quarterly earnings (right column). Tables 7 and 9 report the corresponding estimated discontinuities using our regression discontinuity methodology. General graphing notes from Figure 1 apply.

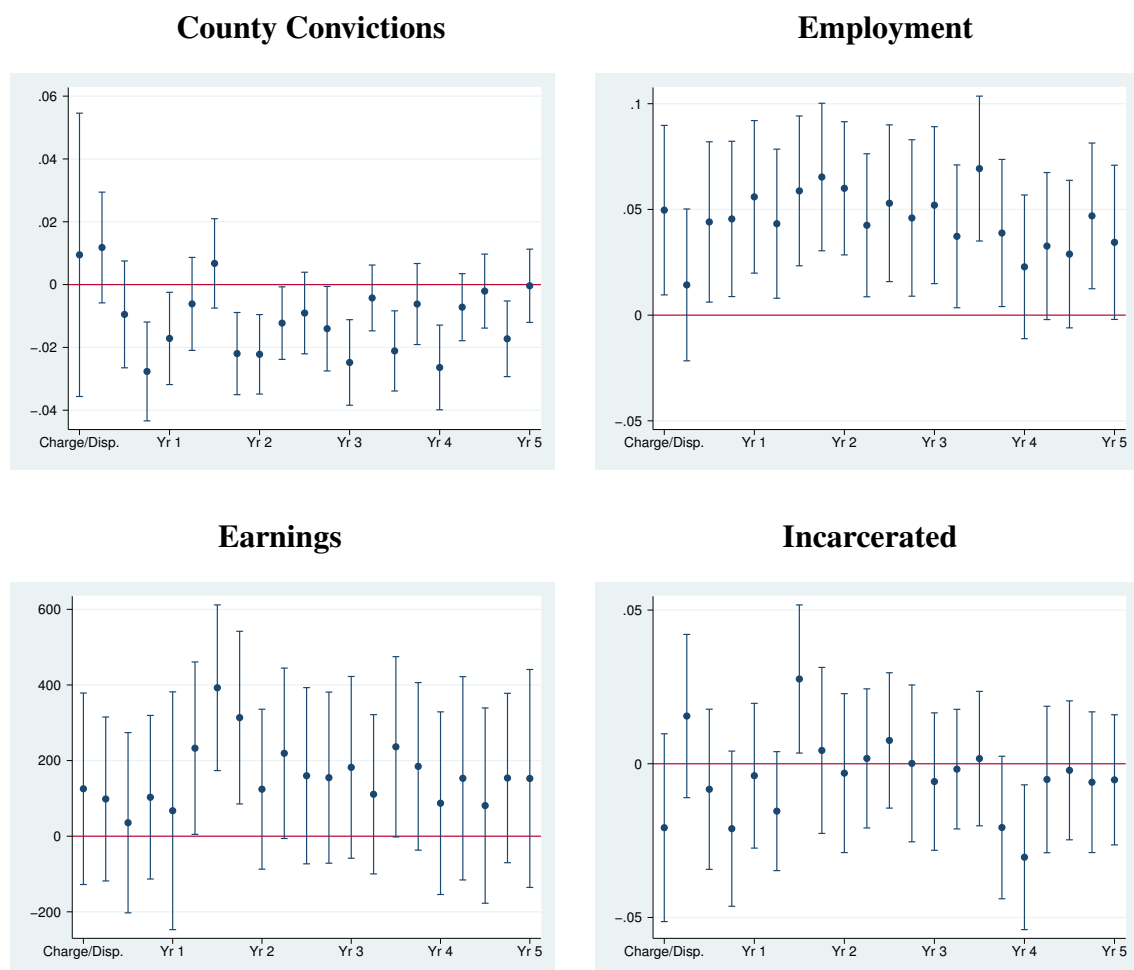


Figure 6: Timeline of outcomes

Figure Notes: This figure depicts estimated discontinuities in the number of Harris County convictions during the quarter (first row, left column); the quarterly probability of employment (first row, right column); average quarterly earnings (second row, left column); and whether the defendant spent any time incarcerated in the Harris County Jail or a TX state prison during a follow-up quarter (second row, right column). Each point represents an estimate during a quarter of the follow-up period. General regression discontinuity estimation notes from Table 4 apply.

		Criminal Behavior	
		No Convictions	1+ Convictions
Labor Market	Employed 2.5+ Years	Stable	Reoffending
	Employed < 2.5 Year	Out of Work	Unstable

Figure 7: Definition of joint crime-labor outcomes

Figure Notes: This figure illustrates the mutually exclusive categories created to examine effects across combinations of crime and labor market outcomes. We create indicators for each category for our estimation sample and estimate the effects of each discontinuity on the proportion of offenders in each of these categories (presented in Table 10).

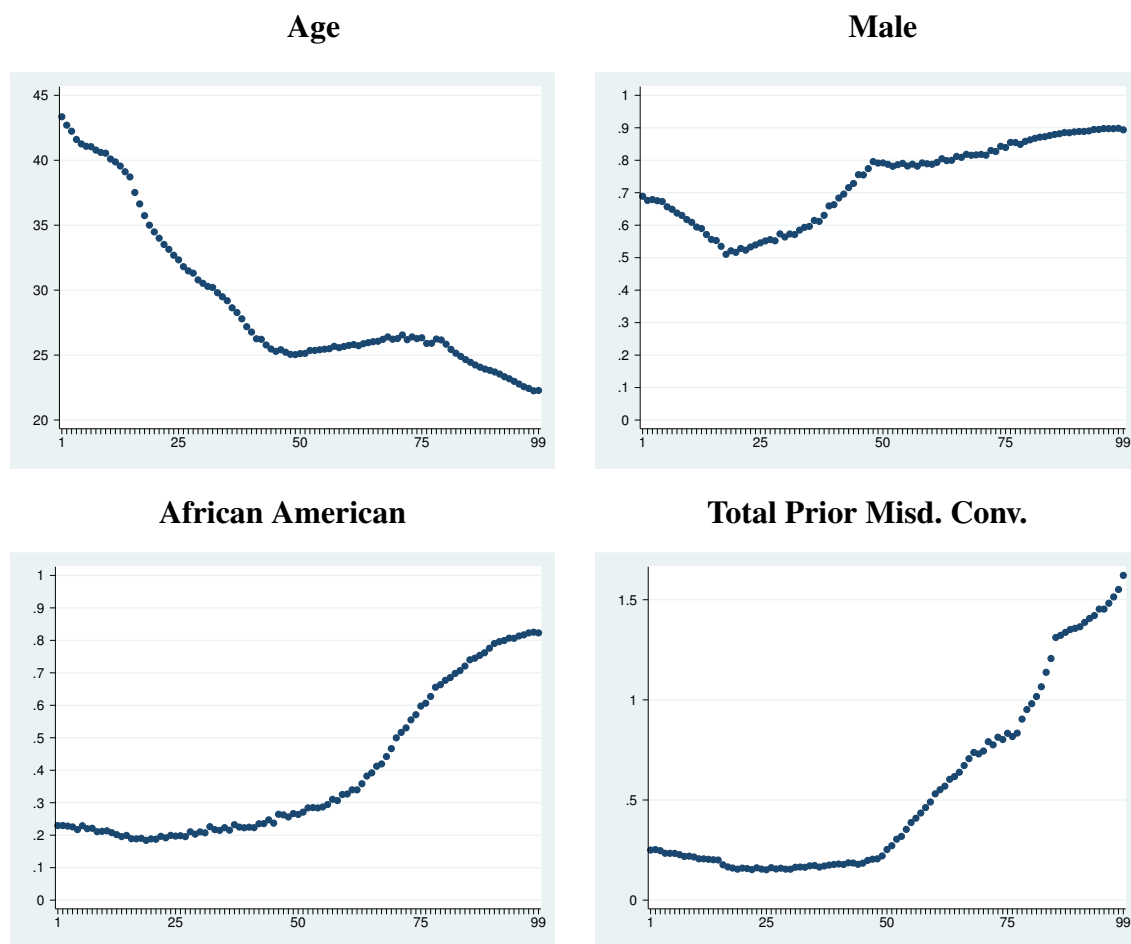


Figure 8: Background characteristics over the risk score distribution

Figure Notes: This figure displays the proportion of cases by the indicated baseline characteristics over the quantile function of the predicted recidivism risk score described in Section 3.

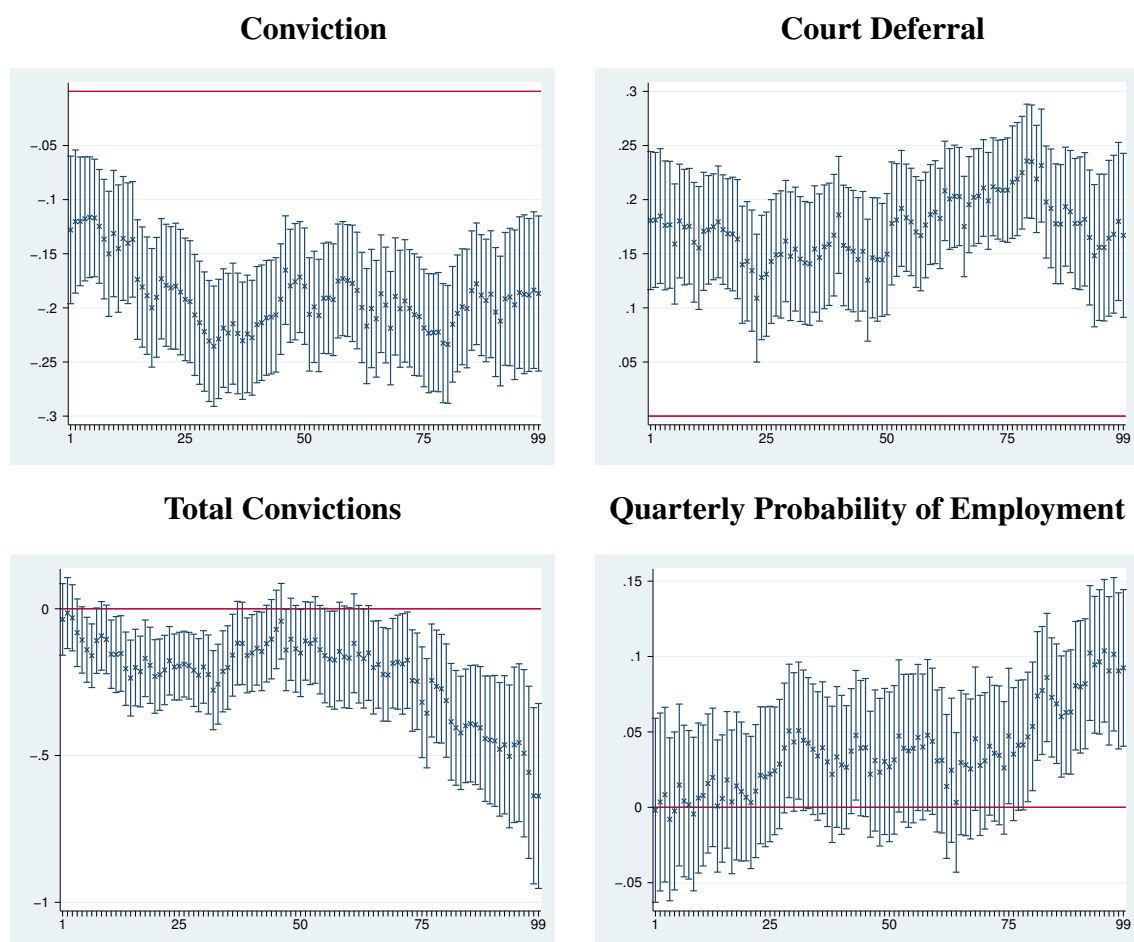


Figure 9: Impacts on first stage and final outcomes over recidivism risk score distribution

Figure Notes: This figure displays the estimated impact of the discontinuities on court verdicts and outcomes over the quantile function of the predicted recidivism risk score described in Section 3. Each coefficient reflects a distinct local polynomial RD estimate from regression centered at the focal percentile using a uniform kernel with a 30 percentile bandwidth. Estimates below the 15th and above the 85th percentiles will reflect narrower, asymmetric bandwidths.

7. Tables

Table 1: Summary statistics, overall and by analysis subgroup

	First-Time Felony Def.	First-Time Subgroup Analysis Samples					Repeat Felony Def.
		1994	2007	Drug	Property	Violent	
<i>Demographic Characteristics</i>							
Male	0.73	0.71	0.74	0.77	0.66	0.73	0.78
Age	29.42	28.51	29.96	29.90	28.46	30.31	31.79
Race/Ethnicity							
Black, not Hisp.	0.39	0.45	0.35	0.42	0.40	0.33	0.53
White, not Hisp.	0.30	0.31	0.30	0.30	0.30	0.31	0.26
Hispanic	0.29	0.23	0.33	0.28	0.28	0.35	0.20
1994 Sample	0.38	1.00	0.00	0.39	0.53	0.00	0.39
<i>Current Charge Type</i>							
Drug	0.45	0.47	0.44	1.00	0.00	0.00	0.56
Property	0.37	0.53	0.28	0.00	1.00	0.00	0.30
Violent	0.06	0.00	0.10	0.00	0.00	1.00	0.06
<i>Court Sentence</i>							
Conviction	0.47	0.49	0.46	0.53	0.42	0.33	0.72
Court Deferral	0.37	0.36	0.38	0.36	0.39	0.39	0.12
Incarceration	0.38	0.26	0.45	0.43	0.30	0.33	0.67
Incarceration Length (Years)	0.24	0.43	0.13	0.32	0.20	0.10	1.05
Probation	0.49	0.62	0.41	0.48	0.52	0.42	0.20
Probation Length (Years)	1.85	3.10	1.09	1.87	2.07	1.24	0.88
Fined	0.36	0.47	0.30	0.39	0.34	0.30	0.12
Fine Amount (\$1,000)	0.18	0.31	0.10	0.21	0.17	0.11	0.07
Drug Treatment	0.04	0.03	0.06	0.06	0.02	0.06	0.03
<i>Pre-Charge Criminal History</i>							
Prior Misd. Convictions	0.58	0.52	0.61	0.61	0.46	0.68	1.41
Prior Fel. Convictions	0.01	0.00	0.01	0.00	0.01	0.01	0.65
Total Pre-Charge Incar. (Days)	16.86	10.91	20.46	19.71	12.69	12.44	130.28
<i>Pre-Charge Labor Market</i>							
SSN Not Recorded	0.32	0.28	0.35	0.32	0.31	0.34	0.18
Qtrly Pr. Empl.	0.27	0.27	0.41	0.23	0.24	0.45	0.23
Avg. Qtrly Earn. (\$1,000)	1.38	0.96	2.12	1.04	1.11	2.75	0.96
N	82,190	30,966	51,224	37,186	30,678	5,266	29,757

Table Notes: This table presents summary statistics for our estimation sample in the first column; for subgroups of the estimation sample split by either the year of the reform or the type of charge in the second through seventh columns; and for a group of repeat felony defendants in the final column (who are not part of our estimation sample). Each characteristic is observed in the court records from the Harris County District Court except for the number of incarceration days prior to the focal charge and the *Pre-Charge Labor Market* characteristics. Jail and prison data is obtained from the Harris County Sheriff's Department and the Texas Department of Criminal Justice, and labor market data from the TX unemployment insurance wage records from the Texas Workforce Commission.

Table 2: Summary statistics, by court disposition

	Dismissal	Deferral	Conviction
<i>Demographic Characteristics</i>			
Male	0.72	0.68	0.78
Age	30.32	27.97	30.23
Race/Ethnicity			
Black, not Hisp.	0.36	0.41	0.39
White, not Hisp.	0.36	0.31	0.27
Hispanic	0.26	0.26	0.33
1994 Sample	0.35	0.36	0.39
<i>Current Charge Type</i>			
Drug	0.32	0.44	0.51
Property	0.43	0.40	0.33
Violent	0.11	0.07	0.05
<i>Court Sentence</i>			
Conviction	0.00	0.01	1.00
Court Deferral	0.00	1.00	0.00
Incarceration	0.00	0.03	0.78
Incarceration Length (Years)	0.00	0.01	0.51
Probation	0.00	1.00	0.26
Probation Length (Years)	0.00	3.48	1.21
Fined	0.00	0.66	0.25
Fine Amount (\$1,000)	0.00	0.29	0.15
Drug Treatment	0.00	0.10	0.02
<i>Pre-Charge Criminal History</i>			
Prior Misd. Convictions	0.41	0.43	0.76
Prior Fel. Convictions	0.00	0.00	0.01
Total Pre-Charge Incar. (Days)	13.57	4.04	27.95
<i>Pre-Charge Labor Market</i>			
SSN Not Recorded	0.36	0.25	0.36
Qtrly Pr. Empl.	0.31	0.31	0.23
Avg. Qtrly Earn. (\$1,000)	1.46	1.21	0.88
N	13220	30311	38804

Table Notes: This table presents summary statistics of our primary estimation sample by the type of case disposition. Court sentence variables were parsed from electronic case notes. As a consequence some measurement error enters into these variables, which can be seen most clearly in the 0.01 share of the Deferral caseload that is also recorded as receiving a conviction.

Table 3: Change in court verdicts by year and type of charge

	Conviction		Court Deferral	
All Felony Defendants, 1994	0.138*** (0.023)	-0.009 (0.027)	-0.179*** (0.020)	-0.024* (0.013)
Affected Statutes:	0.255*** (0.031)	0.015 (0.030)	-0.274*** (0.030)	-0.059*** (0.019)
Unaffected Statutes:	-0.014 (0.035)	-0.064 (0.041)	-0.011 (0.032)	0.034* (0.020)
All Felony Defendants, 2007	-0.105*** (0.020)	-0.070*** (0.013)	0.053** (0.021)	0.033*** (0.009)
Low Risk Crimes:	-0.146*** (0.025)	-0.123*** (0.017)	0.090*** (0.026)	0.064*** (0.012)
High Risk Crimes:	-0.029 (0.032)	-0.000 (0.019)	-0.005 (0.029)	0.002 (0.014)
Criminal Background	No Prior Felony Charges	Has Prior Felony Charges	No Prior Felony Charges	Has Prior Felony Charges

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

Table Notes: This table presents estimates of discontinuities of the change in felony convictions and court deferrals for several subpopulations of the 1994 and 2007 caseloads. The first three rows show separate discontinuity estimates from the 1994 natural experiment among: (1) all felony defendants, (2) felony defendants charged with statutes affected by the introduction of state jail felonies, and (3) felony defendants charged with unaffected statutes. The second three rows show a parallel exercise for 2007 where we have highlighted the discontinuities for low-risk and high-risk crimes. See Section 2.3 for a detailed description of sample restrictions for each row.

General RD Estimation Notes: Local-polynomial regression-discontinuity point estimates are bias-corrected and rely on a Uniform kernel. For bandwidth selection, we use a method which selects the median bandwidth from the set of MSE-optimal selection procedures. We use the HC₂ heteroskedasticity-robust variance estimation. With the exception of tests for discontinuities in baseline characteristics, our preferred RD model also adjusts for baseline covariates (age, gender, race/ethnicity, and prior number of convictions). These procedures were implemented using the *rdrobust* package available in STATA (Calonico et al. 2016a).

Table 4: Discontinuities in baseline characteristics

	Caseload Density	Prior Misd.	Age	Male	Black	Hisp
High Deferral Regime	0.776 (2.810)	0.021 (0.038)	0.288 (0.399)	-0.010 (0.017)	0.016 (0.018)	0.011 (0.015)
Mean (Low Deferral)	33.33	0.58	29.13	0.73	0.39	0.29
Observations	2,456	81,769	81,513	81,769	81,769	81,769
	Recid. Risk Score	Total Pre-Charge Inc. Days	Match to Earnings Records	Pre-Charge Qrtly Prob. Employed	Total Pre-Charge Earnings	
High Deferral Regime	-0.012 (0.018)	-3.489 (3.624)	-0.045** (0.021)	-0.011 (0.015)	-0.048 (0.104)	
Mean (Low Deferral)	0.81	16.34	0.68	0.29	1.12	
Observations	81,513	81,769	81,769	55,201	55,201	

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

Table Notes: This table presents estimates of discontinuities in pre-determined characteristics as recorded in the criminal court records from the Harris County District Court and the TX unemployment insurance wage records from the Texas Workforce Commission. The calculation of the recidivism risk score is described in Section 4.1. General RD estimation notes from Table 3 apply.

Table 5: Discontinuities in court verdict and immediate sanctions

<i>Panel A: Case Disposition</i>	Felony Conviction	Court Deferral	Charge Dismissed
High Deferral Regime	-0.192*** (0.020)	0.160*** (0.020)	0.019 (0.013)
Mean (Low Deferral)	0.55	0.30	0.15
Observations	81,513	81,513	81,513
<i>Panel B: Court Sanctions</i>	Sentenced to Incarceration	Drug Treatment	
High Deferral Regime	-0.118*** (0.018)	0.014 (0.008)	
Mean (Low Deferral)	0.44	0.04	
Observations	81,513	81,513	

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

Table Notes: This table presents estimates of discontinuities in court outcomes as recorded in the criminal court records from the Harris County District Court. General RD estimation notes from Table 3 apply.

Table 6: Discontinuities in served incarceration

<i>Panel A: Ever incarcerated during follow-up period:</i>				
	1 month	6 months	1 Year	5 Years
High Deferral Regime	−0.032** (0.015)	−0.047*** (0.016)	−0.041** (0.017)	−0.025 (0.017)
Mean (Low Deferral)	0.36	0.41	0.55	0.69
Observations	81,513	81,513	81,513	81,513

<i>Panel B: Time served over 5 years:</i>				
	≤ 7 Days	≤ 30 Days	≤ 90 Days	> 90 Days
High Deferral Regime	0.023 (0.019)	0.001 (0.017)	−0.002 (0.017)	0.002 (0.017)
Mean (Low Deferral)	0.37	0.47	0.59	0.41
Observations	81,513	81,513	81,513	81,513

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

Table Notes: This table presents estimates of discontinuities in actual time spent incarcerated as recorded by the Harris County Sheriff's Office (time spent in a Harris County Jail) and the Texas Department of Criminal Justice (time spent in a state prison). Panel A presents results from a specification where the dependent variable is an indicator for any incarceration within the time window listed as the column title. Panel B displays estimates from models in which the dependent variable is a count of the number of days within the time window specified. General RD estimation notes from Table 3 apply.

Table 7: Discontinuities in reoffending outcomes over 5 years

	Jail Bookings 5 years	County Charges 5 years	County Convictions 5 years	Statewide Convictions 5 years
<i>Panel A: Extensive Margin</i>				
High Deferral Regime	−0.020 (0.020)	−0.045** (0.017)	−0.056*** (0.018)	−0.034** (0.018)
Mean (Low Deferral)	0.58	0.44	0.40	0.30
Observations	81,513	81,513	81,513	81,513
<i>Panel B: Intensive Margin</i>				
High Deferral Regime	−0.120* (0.071)	−0.252*** (0.065)	−0.237*** (0.059)	−0.181*** (0.052)
Mean (Low Deferral)	1.56	1.12	0.92	0.68
Observations	81,513	81,513	81,513	81,513

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

Table Notes: This table presents estimates of discontinuities in criminal offending over a five-year follow-up period. The first row presents estimated discontinuities in models where the dependent variables in these models are binary indicators for the outcome listed in the column title. The second row presents results for outcomes that represent counts of the reoffending outcomes over the five-year follow-up period. The first through fourth columns present estimates of the discontinuity for the following outcomes: the total number of jail bookings in Harris County, the total number of charges filed with the Harris County, and the total number of convictions in Harris County, and the total number of convictions recorded in the TX statewide conviction database during the five-year follow-up period. General RD estimation notes from Table 3 apply.

Table 8: Discontinuities in reoffending over 5 years by type of offense

	County Convictions 5 years FELONY	County Convictions 5 years MISD	County Convictions 5 years DRUG	County Convictions 5 years PROPERTY	County Convictions 5 years VIOLENT
High Deferral Regime	−0.110*** (0.035)	−0.113*** (0.037)	−0.051* (0.028)	−0.103*** (0.028)	−0.038** (0.015)
Mean (Low Deferral)	0.44	0.48	0.30	0.24	0.11
Observations	81,513	81,513	81,513	81,513	81,513

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

Table Notes: This table presents estimates of discontinuities in the total number of convictions in Harris County for different degrees of offense (felony and misdemeanor) and types of crime (property, drug, and violent). General RD estimation notes from Table 3 apply.

Table 9: Discontinuities in labor market outcomes over 5 years

	Qrtly Prob. Employed 5 years	Qrtly Prob. Earn \geq 100% Fed. Pov. Level 5 years
<i>Panel A: Employment</i>		
High Deferral Regime	0.043*** (0.016)	0.027* (0.014)
Mean (Low Deferral)	0.39	0.25
Observations	55,151	55,151
<i>Panel B: Earnings (\$1,000s)</i>	Log Avg. Qrtly Earnings 5 years	Avg. Qrtly Earnings 5 years
High Deferral Regime	0.303** (0.139)	145.678 (115.457)
Mean (Low Deferral)	5.07	1,629.98
Observations	55,151	55,151
<i>Panel C: Empl. Tenure</i>	Length of Longest Empl. Spell 5 years	Length of Longest Earn. Spell 5 years
High Deferral Regime	0.560** (0.219)	0.728*** (0.281)
Mean (Low Deferral)	4.41	6.10
Observations	55,151	55,151

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

Table Notes: This table presents estimates of discontinuities in labor market outcomes (from the quarterly unemployment insurance wage records) over a five-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 column and quarterly employment at or above the federal poverty level in the second column. All wage outcomes are adjusted to 2000 US dollars using the Houston MSA CPI index and the 2000 Federal Poverty Level for a single adult is (\$8,350). Panel B presents results on a average quarterly earnings dependent variable in log form (first column) and the raw level (second column). Finally, Panel C 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 column) or consecutive earnings (second column) during the five-year follow-up period. General RD estimation notes from Table 3 apply.

Table 10: Discontinuities in intersection outcomes

	Stable	Reoffending	Out of Work	Unstable
High Deferral Regime	0.072*** (0.020)	−0.005 (0.012)	−0.005 (0.020)	−0.075*** (0.020)
Mean (Low Deferral)	0.26	0.12	0.29	0.32
Observations	55,151	55,151	55,151	55,151

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

Table Notes: This table presents estimates of discontinuities for four mutually-exclusive categories considering labor market outcomes and criminal offending outcomes jointly over the five-year follow-up period. These categories are illustrated in Figure 7. General RD estimation notes from Table 3 apply.

Table 11: Heterogeneous effects by year or type of defendant

	Case Disposition		County	Qrtly Prob.
	Felony Conviction	Court Deferral	Convictions 5 years	Employed 5 years
<i>Panel A: Experimental Episode</i>				
1994 Penal Code Reform	−0.257*** (0.031)	0.274*** (0.030)	−0.229*** (0.088)	0.045* (0.024)
2007 Election	−0.142*** (0.025)	0.090*** (0.026)	−0.215*** (0.074)	0.044** (0.022)
<i>Panel B: Offense Category</i>				
Drug	−0.295*** (0.031)	0.243*** (0.031)	−0.235*** (0.090)	0.052** (0.024)
Property	−0.131*** (0.027)	0.123*** (0.027)	−0.284*** (0.087)	0.038 (0.026)
Violent	−0.184*** (0.057)	0.088 (0.067)	0.038 (0.154)	0.056 (0.062)
<i>Panel C: Repeat Offenders</i>				
One Prior Felony Conviction or Deferral	−0.064*** (0.024)	0.063*** (0.019)	−0.045 (0.111)	0.026 (0.022)

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

Table Notes: This table presents estimates of discontinuities for different types of defendants. Effects are reported for our main case disposition outcomes (conviction and court deferral), total Harris County convictions, and the quarterly employment probability. Panel A splits our estimation sample by the reform period (1994 or 2007) and separately estimates effects for each reform. Panel B splits our estimation sample by the type of the focal charge. All subgroups in Panels A and B are included in our primary estimation sample of first-time felony defendants. Panel C presents results for the group of defendants charged with an affected (1994) or low risk statute (2007) who already have a prior felony conviction or prior court deferral and are thus not included in our primary estimation sample. We include previously deferred individuals since being charged with a new offense will likely convert their prior deferral to an official conviction thus creating a prior felony record. General RD estimation notes from Table 3 apply.

Table 12: Heterogeneous effects by demographic characteristics

	Case Disposition		County	Qrtly Prob.
	Felony	Court	Convictions	Employed
	Conviction	Deferral	5 years	5 years
<i>Panel A: Sex</i>				
Male	−0.198*** (0.022)	0.176*** (0.022)	−0.223*** (0.069)	0.041** (0.018)
Female	−0.173*** (0.035)	0.121*** (0.037)	−0.239** (0.101)	0.048* (0.027)
<i>Panel B: Age</i>				
Under 30 years old	−0.152*** (0.026)	0.145*** (0.026)	−0.240*** (0.079)	0.056*** (0.020)
30+ years old	−0.203*** (0.028)	0.194*** (0.025)	−0.108 (0.078)	0.023 (0.025)
<i>Panel C: Race/Ethnicity</i>				
White	−0.176*** (0.030)	0.154*** (0.027)	−0.206** (0.088)	0.056* (0.031)
African American	−0.219*** (0.032)	0.218*** (0.030)	−0.316*** (0.101)	0.051** (0.020)
Hispanic	−0.137*** (0.034)	0.102*** (0.034)	−0.157* (0.083)	0.010 (0.030)

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

Table Notes: This table presents estimates of discontinuities for different subgroups of our primary estimation sample by demographic characteristics of the defendant (as recorded in the Harris County court records). Effects are reported for our main case disposition outcomes (conviction and court deferral), total Harris County convictions, and the quarterly employment probability. Panel A reports separate estimates by the gender of the defendant. Panel B separately estimates effects for defendants under 30 years of age at the time of the focal charge and for those 30 years or older. Panel C splits our estimation sample by the race/ethnicity of defendants into three mutually exclusive categories: White (non-Hispanic), African American (non-Hispanic), and Hispanic. All subgroups in Panels A, B, and C are included in our primary estimation sample of first-time felony defendants. General RD estimation notes from Table 3 apply.

Table 13: Fuzzy regression discontinuity estimates

	Any County Convictions 5 years	Total County Convictions 5 years	Qrtly Prob. Employed 5 Years	Log Avg. Qrtly Earnings 5 years
Court Deferral	−0.320*** (0.102)	−1.255*** (0.320)	0.187*** (0.064)	1.385** (0.584)
Mean (Low Deferral)	0.40	0.91	0.39	5.05
Observations	81,513	81,513	55,151	55,151

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

Table Notes: This table presents estimates from a fuzzy RD design where we instrument for a court deferral with the discontinuities for our our main case disposition outcomes (conviction and court deferral), total Harris County convictions, and the quarterly employment probability. Because the 2007 experiment shows a small impact on case dismissals, for this part of the analysis we redefine court deferrals to also include case dismissals as a combined outcome so as to avoid underestimating the first stage and by construction overestimating the treatment effects. As a result, in this exercise if $CourtDeferral = 0$ then by construction $Conviction = 1$. General RD estimation notes from Table 3 apply.

8. Appendix

A. Robustness Exercises

Table A1: Discontinuities in criminal activity in state-wide computerized criminal history (CCH) file

	TX CCH Conviction 5 years	TX CCH Conviction Harris 5 years	TX CCH Conviction Not Harris 5 years
High Deferral Regime	−0.181*** (0.052)	−0.124*** (0.046)	−0.048*** (0.018)
Mean (Low Deferral)	0.68	0.55	0.13
Observations	81,513	81,513	81,513

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

Table Notes: This table presents estimates of discontinuities in future criminal convictions using the Texas statewide conviction database. General RD estimation notes from Table 3 apply.

Table A2: RD results using placebo samples

	Guilty	Court Deferral	County Convictions 5 years	Qrtly Prob. Employed 5 years
Placebo 1993	−0.030 (0.032)	0.000 (0.043)	−0.002 (0.091)	0.011 (0.036)
Mean (Low Deferral)	0.33	0.52	0.73	0.41
Observations	15,071	15,071	15,071	10,903
Placebo 1995	0.013 (0.037)	0.013 (0.033)	0.119 (0.128)	−0.027 (0.032)
Mean (Low Deferral)	0.61	0.25	0.93	0.43
Observations	15,413	15,413	15,413	11,100
Placebo 2006	−0.035 (0.033)	0.048 (0.033)	−0.060 (0.111)	0.003 (0.023)
Mean (Low Deferral)	0.51	0.34	0.92	0.38
Observations	25,261	25,261	25,261	16,726
Placebo 2008	0.024 (0.028)	−0.033 (0.029)	−0.032 (0.090)	−0.006 (0.027)
Mean (Low Deferral)	0.40	0.43	0.72	0.37
Observations	25,401	25,401	25,401	16,165

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

Table Notes: This table presents estimated discontinuities for samples one year prior and one year post our discontinuities as placebo tests. General RD estimation notes from Table 3 apply.

Table A3: RD results using alternative bandwidth selectors

	Guilty	Court Deferral	County Convictions 5 years	Qrtly Prob. Employed 5 years
MSE1	−0.188*** (0.019)	0.159*** (0.020)	−0.226*** (0.059)	0.039*** (0.013)
Mean (Low Deferral)	0.55	0.30	0.92	0.39
Observations	81,513	81,513	81,513	55,151
MSE2	−0.163*** (0.020)	0.164*** (0.019)	−0.236*** (0.057)	0.044*** (0.016)
Mean (Low Deferral)	0.55	0.30	0.92	0.39
Observations	81,513	81,513	81,513	55,151
MSE3	−0.193*** (0.020)	0.157*** (0.019)	−0.234*** (0.059)	0.043*** (0.016)
Mean (Low Deferral)	0.55	0.30	0.92	0.39
Observations	81,513	81,513	81,513	55,151
CER1	−0.172*** (0.024)	0.150*** (0.025)	−0.186** (0.076)	0.045*** (0.017)
Mean (Low Deferral)	0.55	0.30	0.92	0.39
Observations	81,513	81,513	81,513	55,151
CER2	−0.153*** (0.025)	0.153*** (0.023)	−0.224*** (0.075)	0.032* (0.019)
Mean (Low Deferral)	0.55	0.30	0.92	0.39
Observations	81,513	81,513	81,513	55,151
CER3	−0.169*** (0.025)	0.156*** (0.023)	−0.192*** (0.073)	0.030 (0.019)
Mean (Low Deferral)	0.55	0.30	0.92	0.39
Observations	81,513	81,513	81,513	55,151

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

Table Notes: This table presents results from local-polynomial RDs varying the bandwidth selection for the September 1, 1994 natural experiment. 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 General RD estimation notes from Table 3 apply.

Table A4: RD results using alternative variance estimators

	Guilty	Court Deferral	County Convictions 5 years	Qrtly Prob. Employed 5 years
Day Cluster	−0.188*** (0.025)	0.161*** (0.022)	−0.235*** (0.057)	0.044** (0.018)
Mean (Low Deferral)	0.55	0.30	0.92	0.39
Observations	81,513	81,513	81,513	55,151
Week Cluster	−0.190*** (0.029)	0.156*** (0.025)	−0.187*** (0.039)	0.044*** (0.016)
Mean (Low Deferral)	0.55	0.30	0.92	0.39
Observations	81,513	81,513	81,513	55,151
Nearest Neighbor	−0.192*** (0.019)	0.161*** (0.020)	−0.237*** (0.059)	0.043*** (0.016)
Mean (Low Deferral)	0.55	0.30	0.92	0.39
Observations	81,513	81,513	81,513	55,151

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

Table Notes: This table presents estimates using alternative variance estimators as indicated. All other General RD estimation notes from Table 3 apply.

Table A5: RD results relaxing various specification choices

	Guilty	Court Deferral	County Convictions 5 years	Qrtly Prob. Employed 5 years
No Donut	−0.173*** (0.018)	0.156*** (0.018)	−0.203*** (0.053)	0.045*** (0.015)
Mean (Low Deferral)	0.55	0.30	0.92	0.39
Observations	81,933	81,933	81,933	55,455
No Covariates	−0.190*** (0.020)	0.158*** (0.020)	−0.240*** (0.062)	0.041*** (0.016)
Mean (Low Deferral)	0.55	0.30	0.92	0.39
Observations	81,769	81,769	81,769	55,201
Non-Robust	−0.192*** (0.017)	0.160*** (0.017)	−0.237*** (0.051)	0.043*** (0.013)
Mean (Low Deferral)	0.55	0.30	0.92	0.39
Observations	81,513	81,513	81,513	55,151
Non-Bias Corrected	−0.193*** (0.017)	0.168*** (0.017)	−0.215*** (0.051)	0.038*** (0.013)
Mean (Low Deferral)	0.55	0.30	0.92	0.39
Observations	81,513	81,513	81,513	55,151
Week Binned Running Variable	−0.180*** (0.019)	0.158*** (0.019)	−0.235*** (0.056)	0.039** (0.015)
Mean (Low Deferral)	0.55	0.30	0.92	0.39
Observations	81,513	81,513	81,513	55,151

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

Table Notes: This table presents estimates that relax the implementation decisions as described. All other General RD estimation notes from Table 3 apply.

B. A Time Series Approach to Estimating the Causal Effect of Court Deferrals

The previously discussed results in this paper are identified off of a regression discontinuity (RD) framework wherein we have conceptualized the date of charge or the date of disposition for a given defendant as an individual characteristic. We examine the discrete change in court outcomes at two distinct discontinuities in these “characteristics” to identify the causal effect of the changes in punishment. This approach should alleviate any concerns about a selection bias or an omitted variable bias influencing our results.

Our application of the sharp RD methodology in this setting may be considered different than the standard application since our forcing variables are time-based. This appendix applies a time series econometric approach to measuring the effects of the two sharp changes in 1994 and 2007. While we prefer a local RD approach since we believe it requires less restrictive assumptions, a time series approach can better capture the potential interdependence of observations over the forcing variables which could be important for both identification and inference.

In this section, we re-evaluate our key results using a time series framework. We collapse the data to a weekly time series and use pre-reform data to model the time series process for our key outcomes of interest. These models are then applied to the full data to evaluate whether there are structural breaks at the two discontinuity dates. Once we establish the structural breaks, we perform two complementary exercises to estimate the causal effect of the discontinuities. First, we use an interrupted time series analysis and, second, we use a pre-reform calibrated model to measure the post-reform (out-of-sample) forecast error. Both exercises yield results that closely mirror our RD results.

B.1. Modeling the Time Series

In this exercise, we first determine the degree of serial correlation in our weekly observations. For each of the key dependent variables we estimate a series of $AR(p)$ models with successively more autoregressive lags of the dependent variable. We fit each model to a year of pre-reform

data collapsed to the weekly level, and include a week linear, quartic and cubic time trends as well as average defendant characteristics (age, race, sex, and misdemeanor record). We rely on the Akaike Information Criteria (AIC) to evaluate which specification best fits the observed patterns, a method which penalizes models with more parameters. The AIC for each estimated $AR(p)$ specification is presented in Table B1.

Our estimates imply limited-to-moderate degrees of serial correlation in our dependent variables (after including our set of controls). In both samples, we select models with no lagged dependent variables for employment outcomes and just one lag for reoffending behavior in 2007 indicating limited interdependence in these variables over time. Court deferral and felony conviction rates in 1994 are best fit with two and four lagged dependent variables respectively while court disposition in 2007 show no lag structure using the AIC selection criteria. The estimated models using the selected $AR(p)$ lag length are presented in Tables B2 and B3.

B.2. Testing for Structural Breaks

To evaluate whether the discontinuities we study can be explained as a function of serial dependence, trends and/or seasonality, we test for whether there is a structural break at the two threshold dates using the full sample of data. We perform a Wald test to evaluate the null hypothesis of no structural breaks. These test statistics and associated p-values are provided at the bottom of Tables B2 and B3.

We find strong evidence of structural breaks occurring in both the 1994 and 2007 series for our measures of court outcomes (court deferrals and conviction rates) as well as defendant outcomes (total future convictions over five years and total quarters employed over five years).^{66,67} Consistent with previous analysis of caseload densities around the thresholds, we fail to reject the null of no structural break in caseload size. This is consistent with evidence discussed in the RD framework and supports our identification assumption that the density and background characteristics are smooth across the discontinuity dates.

⁶⁶Structural breaks in our outcome variables in 1994 are less precise compared to the other tests and are not statistically significant.

⁶⁷Additionally, Sup Wald tests (not shown) over the same sample generally correctly identify the structural breaks as occurring during the weeks of our discontinuities.

B.3. Estimating the Impact of the Discontinuities

To evaluate the impact of the discontinuities on defendant outcomes, we employ two complementary strategies. First, we analyze the data using a simple interrupted time series model and present results in Table B4. This basic design uses the time series models selected with the AIC criteria and introduces two additional parameters: a variable indicating when the reform is active (*Post*) and an interaction between this indicator variable and the running variable to allow a change in trend at the threshold date (*Week x Post*). In practice we observe this as similar to estimating a linear regression discontinuity design while allowing for some pre-specified degrees of autocorrelation across the running variables. Consistent with our evaluation of the structural breaks, we estimate clear impact of the reform as captured by the estimated coefficient on the *Post* indicator variable. In both 1994 and 2007, there is clear evidence of a shift in court outcomes at the threshold dates as well as a corresponding shift in outcome variables. While the direction of the labor market effects are consistent with our RD estimates, the estimates become insignificant and imply more modest magnitudes. Figure B1 presents the raw time series data and the fitted interrupted time series models.

Second, we present results using an alternative strategy in which we calibrate a time series model using pre-reform data and evaluate how well it predicts the series immediately after the discontinuity. In contrast to the interrupted time series model, this approach does not assume a permanent level shift after the threshold. The effect of the discontinuity can then be measured as the average forecast error in the post-threshold period. Since it is difficult to extrapolate trends far out into the future with an out-of-sample prediction, we use one year of pre-reform data to forecast one quarter following each reform.

To calculate standard errors, we employ a multi-step forecast simulation procedure. This approach first generates a set of realized in-sample forecast errors and then builds out a series of simulated iterative out-of-sample forecast predictions that randomly draw (with replacement) from the set of in-sample forecast errors. The empirical quantiles for each successive out-of-sample period are used to obtain the confidence intervals over the period. We also take the average quarterly out-of-sample forecast error for each of the 5,000 simulations per series to

generate statistical significance thresholds.

The results of this forecast error exercise are presented in Figure B2 and Table B5. The plots illustrate a pattern in which the realized observations in both 1994 and 2007 are systematically above or below the predicted path. The magnitude of the deviations for the court and future outcomes often puts the realized observations at or beyond the simulated confidence interval bands. The average forecast error over the first out-of-sample quarter, which we interpret as the impact of the discontinuity, yield results that are closely aligned with the estimates from the main RD and supplementary interrupted time series analysis.

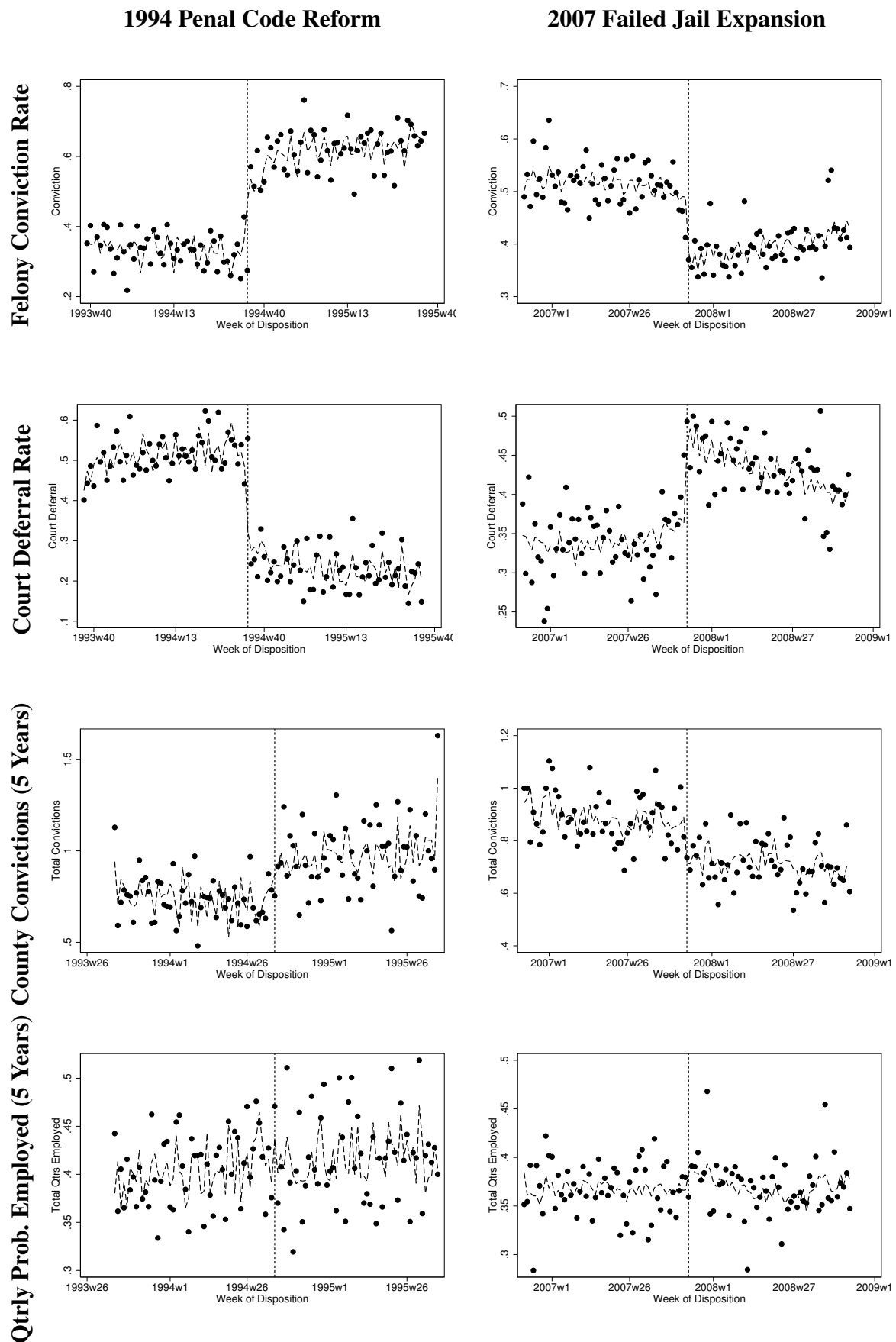


Figure B1: Interrupted time series plots

Figure Notes: This figure presents the raw time series data and the fitted interrupted time series model. The interrupted times series model selects a lags according to the AIC criteria and introduces a variable indicating the post discontinuity period and an interaction between the post threshold period and the week. The lags selected for each outcome and sample are indicated in Table B1 and estimates in Table B4.

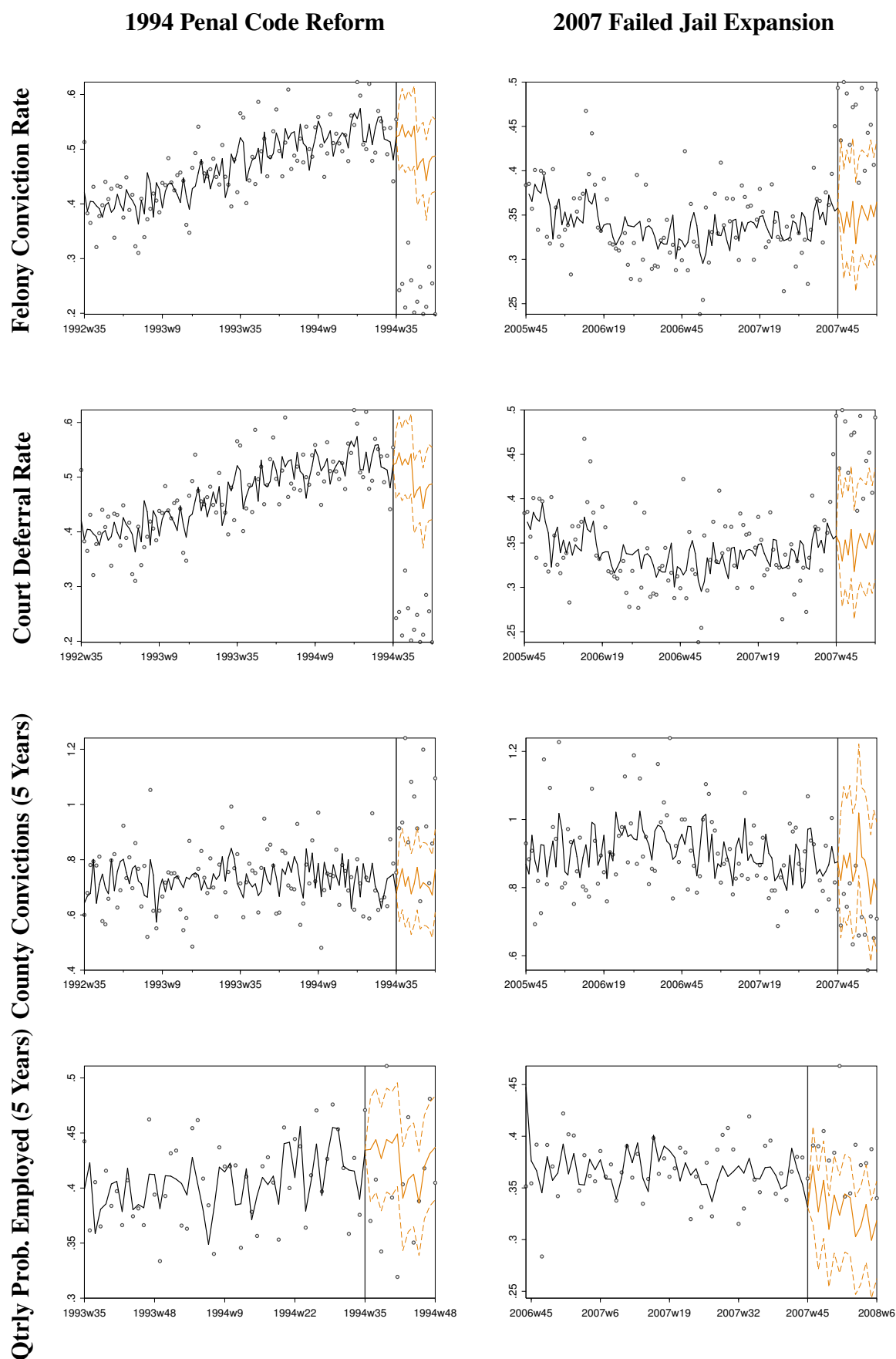


Figure B2: Comparing observed trends and forecasted out-of-sample predictions

Figure Notes: This figure presents the raw time series data and a model which is calibrated with pre-reform data and then we compare actual post-reform outcomes to predicted outcomes extending the model through the discontinuity. Since it is difficult to extrapolate trends far into the future, we use two years of pre-reform data to forecast one quarter post-reform. Results from this analysis are presented in Table B5.

Table B1: Time series model selection

	Felony Conviction	Court Deferral	Total Convictions	Qrtly Prob. Employed
AR(p) Lag Length, 1994				
0	-161.4	-153.8	-71.3	-192.6
1	-159.5	-153.7	-69.6	-190.6
2	-162	-154.2	-68.3	-191.8
3	-160.6	-154.6	-68.4	-191.6
4	-159.7	-155.5	-69.8	-189.6
Selected Lag Length	2	4	0	0
AR(p) Lag Length, 2007				
0	-185.9	-167.6	-86.4	-210
1	-184.3	-165.6	-87.6	-208.8
2	-183.5	-163.7	-87	-209.1
3	-181.7	-162.6	-85.1	-207.9
4	-180	-163	-83.3	-206.9
Selected Lag Length	0	0	1	0

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

Table Notes: This table presents the Akaike Information Criteria (AIC) for time series models which successively more autoregressive lags of the dependent variable. We fit each model to two years of pre-reform data, and include indicators for each calendar month, a monthly linear time trend and average defendant characteristics (age, race, sex, and misdemeanor record). The models which minimizes the AIC for each outcome are presented in Tables B2 and B3 and in Figure B1.

Table B2: Time series estimated model, 1994

	Court Deferral	Felony Conviction	Total County Convictions	Qrtly Prob. Employed
Lag Dep. Var.	−0.041 (0.136)	−0.348** (0.160)		
Lag2 Dep. Var.	−0.265* (0.142)	−0.344* (0.171)		
Lag3 Dep. Var.		−0.294 (0.174)		
Lag4 Dep. Var.		−0.220 (0.150)		
Week	−0.003 (0.006)	−0.010 (0.007)	−0.016 (0.016)	0.002 (0.005)
Week ²	0.000 (0.000)	0.000 (0.000)	0.000 (0.001)	−0.000 (0.000)
Week ³	−0.000 (0.000)	−0.000 (0.000)	−0.000 (0.000)	0.000 (0.000)
Observations	48	48	48	48
Wald Test for Structural Break	82.5	44.6	14.9	12.2
P-Value	0	0	.14	.27

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

Table Notes: This table presents the results from the time series models with lags selected through the AIC criteria to evaluate the September 1, 1994 shift for each of the dependent variables indicated in the column titles. We fit each model to two years of pre-reform data, and include indicators for each calendar month, a monthly linear time trend and average defendant characteristics (age, race, sex, and misdemeanor record). The predictions from the model are presented in Figure B1.

Table B3: Time series estimated model, 2007

	Court Deferral	Felony Conviction	Total County Convictions	Qrtly Prob. Employed
Lag Dep. Var.			0.234 (0.146)	
Week	0.024*** (0.005)	-0.014** (0.006)	-0.006 (0.014)	-0.003 (0.004)
Week ²	-0.001*** (0.000)	0.001** (0.000)	0.000 (0.001)	0.000 (0.000)
Week ³	0.000*** (0.000)	-0.000** (0.000)	-0.000 (0.000)	-0.000 (0.000)
Observations	48	48	48	48
Wald Test for Structural Break	58.6	47.3	17.9	17.4
P-Value	0	0	.08	.07

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

Table Notes: This table presents the results from the time series models with lags selected through the AIC criteria to evaluate the November 7, 2007 shift for each of the dependent variables indicated in the column titles. We fit each model to two years of pre-reform data, and include indicators for each calendar month, a monthly linear time trend and average defendant characteristics (age, race, sex, and misdemeanor record). The predictions from the model are presented in Figure B1.

Table B4: Interrupted time series estimates

	Felony Conviction	Court Deferral	Total Convictions	Qrtly Prob. Employed
Sample = 1994				
Post	0.176** (0.073)	-0.231*** (0.086)	0.154** (0.076)	-0.003 (0.021)
Week of Charge	-0.003 (0.003)	0.004** (0.002)	-0.009* (0.005)	0.000 (0.002)
Week x Post	-0.000 (0.003)	-0.001 (0.002)	-0.007 (0.007)	0.001 (0.002)
Observations	102	104	106	106
Sample = 2007				
Post	-0.130*** (0.020)	0.119*** (0.022)	-0.151*** (0.046)	0.019 (0.014)
Week of Disposition	0.001 (0.002)	-0.001 (0.002)	-0.002 (0.004)	0.001 (0.001)
Week x Post	0.002 (0.002)	-0.002 (0.002)	0.003 (0.005)	-0.002 (0.001)
Observations	106	106	105	106

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

Table Notes: This table adds an indicator for post-reform and an interaction between the post indicator and the weekly running variable to the $AR(p)$ models presented in Tables B2 and B3 to test whether there is a significant break. These models are similar to our RD specifications but include lagged dependent variables.

Table B5: Average forecast error and bootstrapped significance thresholds

	Out of Sample Average Forecast Error	0.005	0.025	0.05	0.95	0.975	0.995
Sample = 1994							
Felony Conviction Rate	0.304***	-0.043	-0.016	-0.012	0.008	0.011	0.025
Court Deferral Rate	-0.247***	-0.029	-0.018	-0.014	0.014	0.018	0.03
County Convictions (5 Years)	0.194***	-0.068	-0.054	-0.045	0.043	0.051	0.067
Qrtly Prob. Employed (5 Years)	-0.017**	-0.021	-0.016	-0.014	0.013	0.015	0.02
Sample = 2007							
Felony Conviction Rate	-0.125***	-0.022	-0.018	-0.015	0.016	0.02	0.026
Court Deferral Rate	0.028***	-0.022	-0.017	-0.014	0.018	0.022	0.029
County Convictions (5 Years)	-0.091**	-0.134	-0.088	-0.07	0.05	0.063	0.099
Qrtly Pr. Employed (5 Years)	0.051***	-0.023	-0.018	-0.015	0.01	0.013	0.017

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

Table Notes: This table presents results using an empirical methodology in which we calibrate a time series model using two years of pre-reform data and evaluate how well it predicts the series immediately after the discontinuity (in the first quarter following the shift). The effect of the discontinuity is measured as the average forecast error in the post-threshold period. To calculate standard errors, we employ a multi-step forecast simulation procedure. This approach first generates a set of realized in-sample forecast errors and then builds out a series of simulated iterative out-of-sample forecast predictions that randomly draw (with replacement) from the set of in-sample forecast errors. The empirical quantiles for each successive out-of-sample period are used to obtain the confidence intervals over the period. We take the average quarterly out-of-sample forecast error for each of the 5,000 simulations per series to generate statistical significance thresholds.

C. Supplemental Heterogeneity Analysis Results

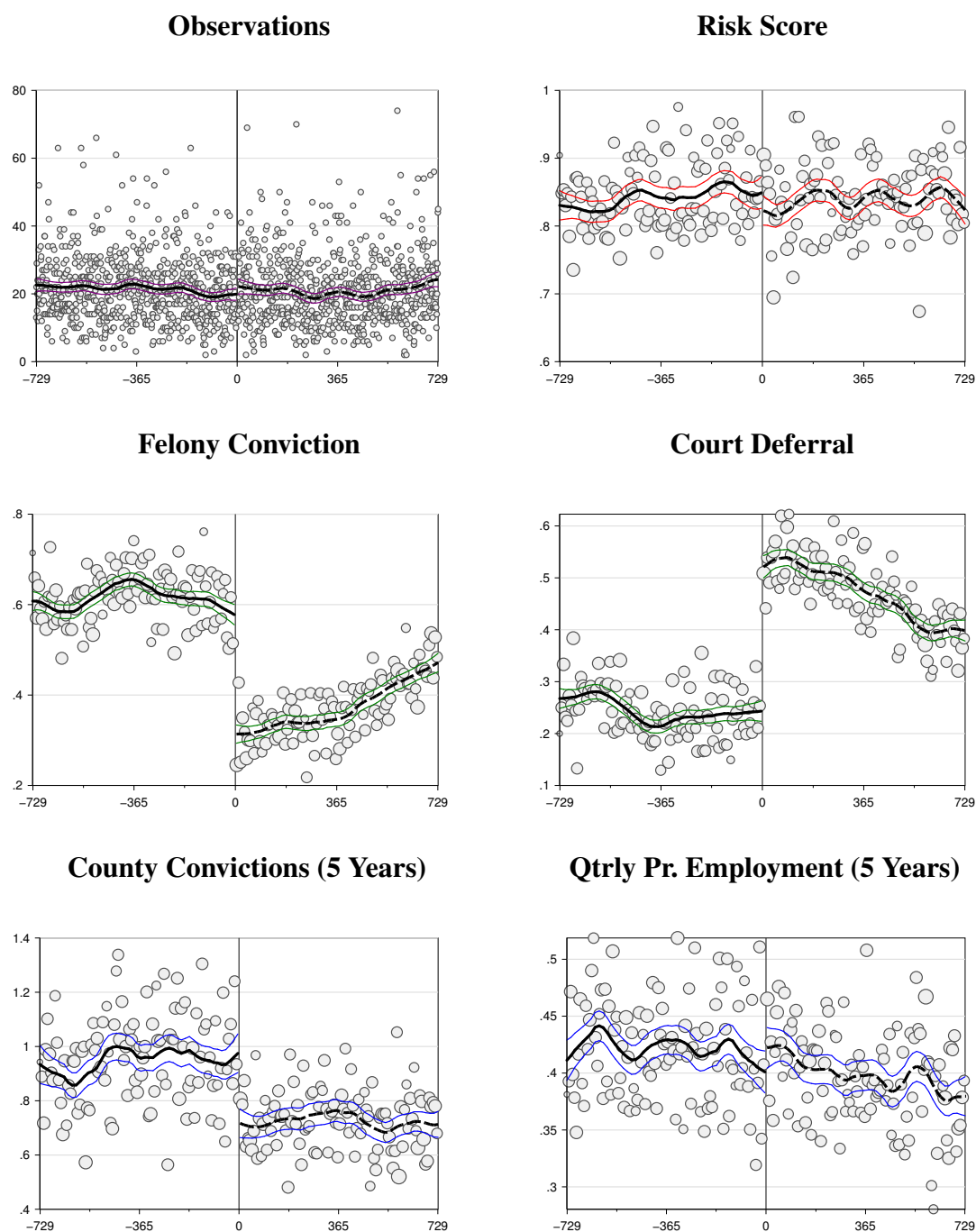


Figure C1: Visual Summary of Evidence, 1994 Sample

Figure Notes: This figure summarizes the graphical evidence from the 1994 sample specifically. The first two plots graph the relationship between the forcing variable and the caseload size as well as recidivism risk score. The next two plots depict the relationship between the running variable and court dispositions. The final two plots show the relationship with reoffending an employment outcomes. Table 11 reports the corresponding estimated discontinuities using our regression discontinuity methodology. General graphing notes from Figure 1 apply.

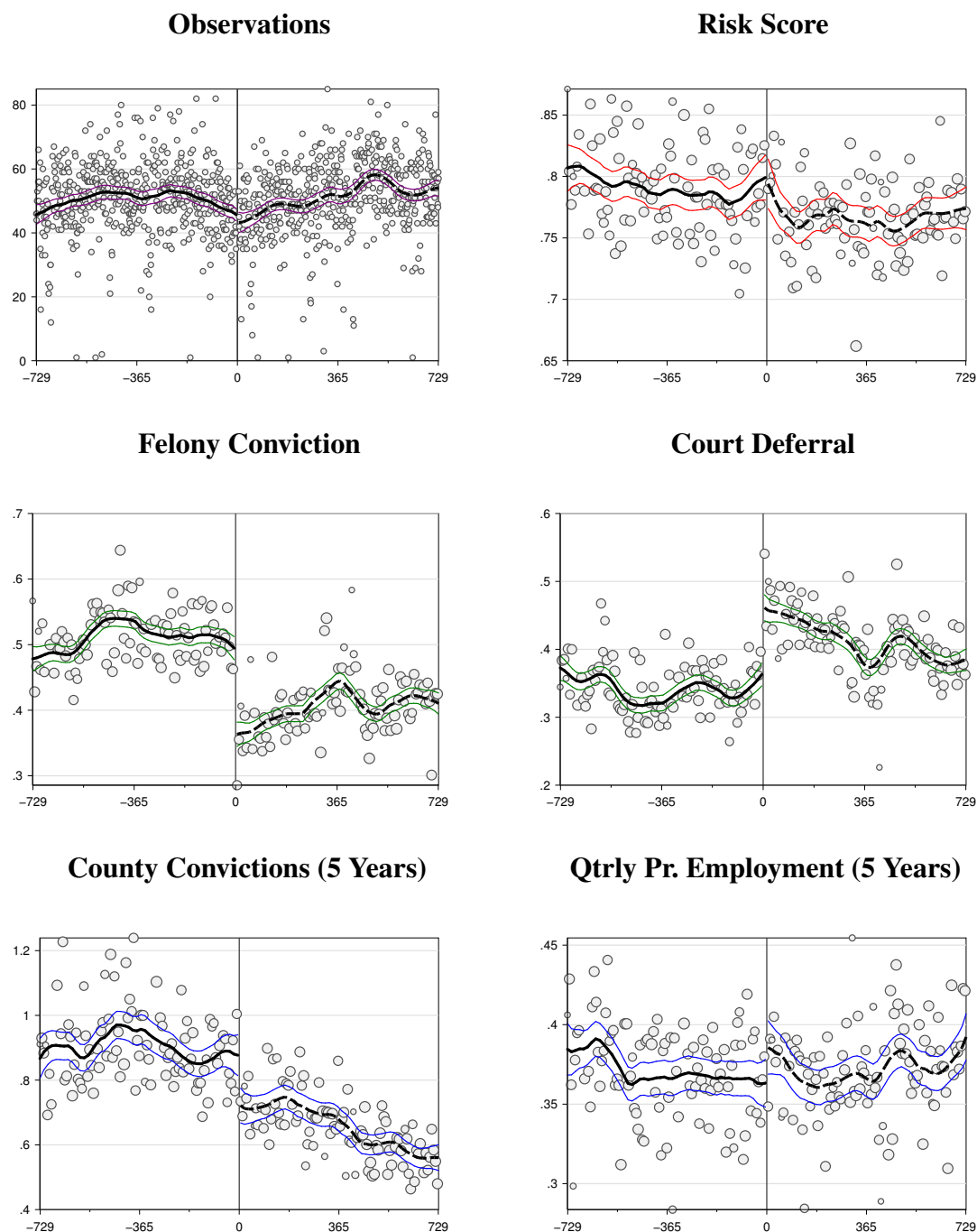


Figure C2: Visual Summary of Evidence, 2007 Sample

Figure Notes: This figure summarizes the graphical evidence from the 2007 sample specifically. The first two plots graph the relationship between the forcing variable and the caseload size as well as recidivism risk score. The next two plots depict the relationship between the running variable and court dispositions. The final two plots show the relationship with reoffending and employment outcomes. Table 11 reports the corresponding estimated discontinuities using our regression discontinuity methodology. General graphing notes from Figure 1 apply.

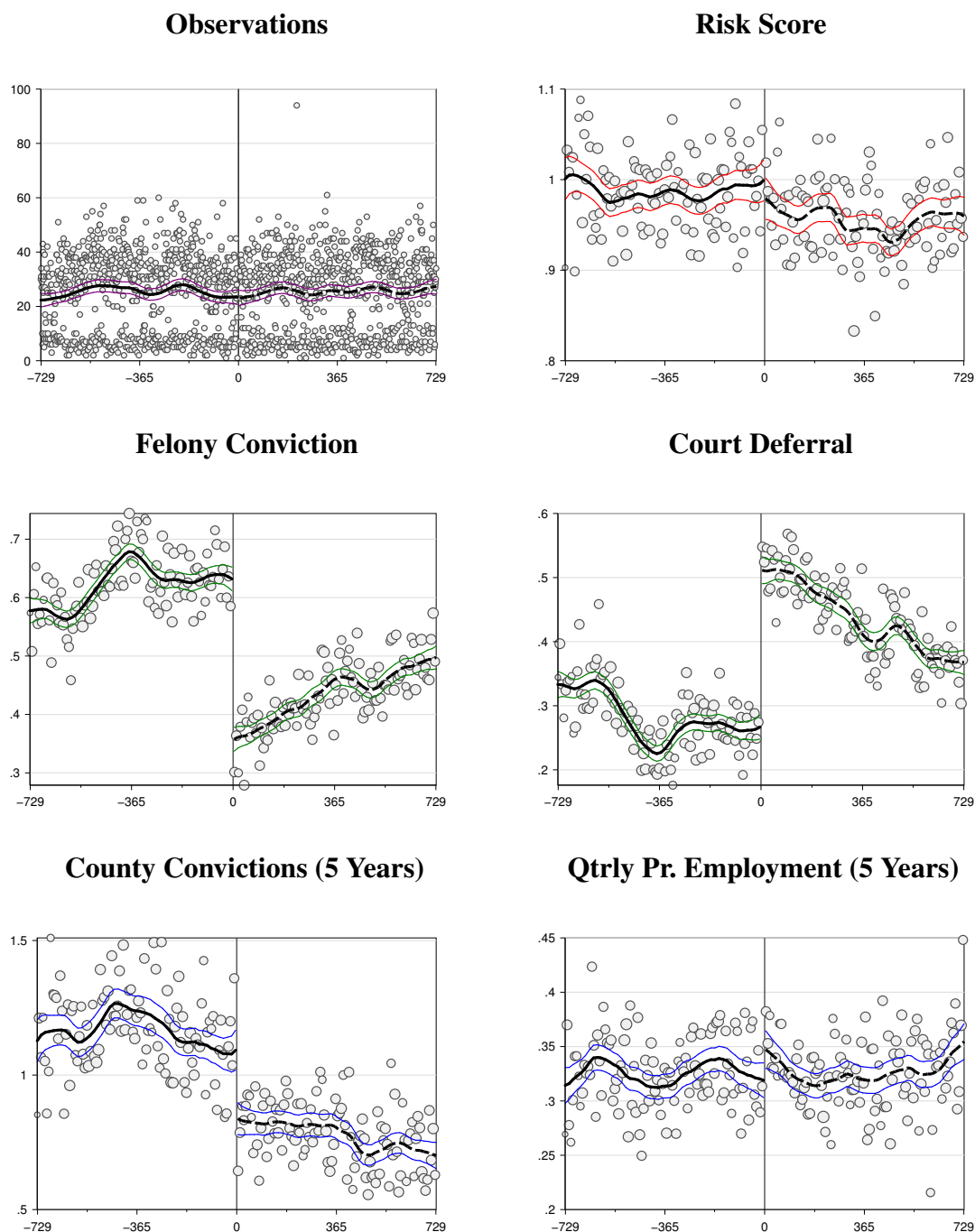


Figure C3: Visual Summary of Evidence, First-Time Drug Offenders

Figure Notes: This figure summarizes the graphical evidence from the drug offender sample specifically. The first two plots graph the relationship between the forcing variable and the caseload size as well as recidivism risk score. The next two plots depict the relationship between the running variable and court dispositions. The final two plots show the relationship with reoffending an employment outcomes. Table 11 reports the corresponding estimated discontinuities using our regression discontinuity methodology. General graphing notes from Figure 1 apply.

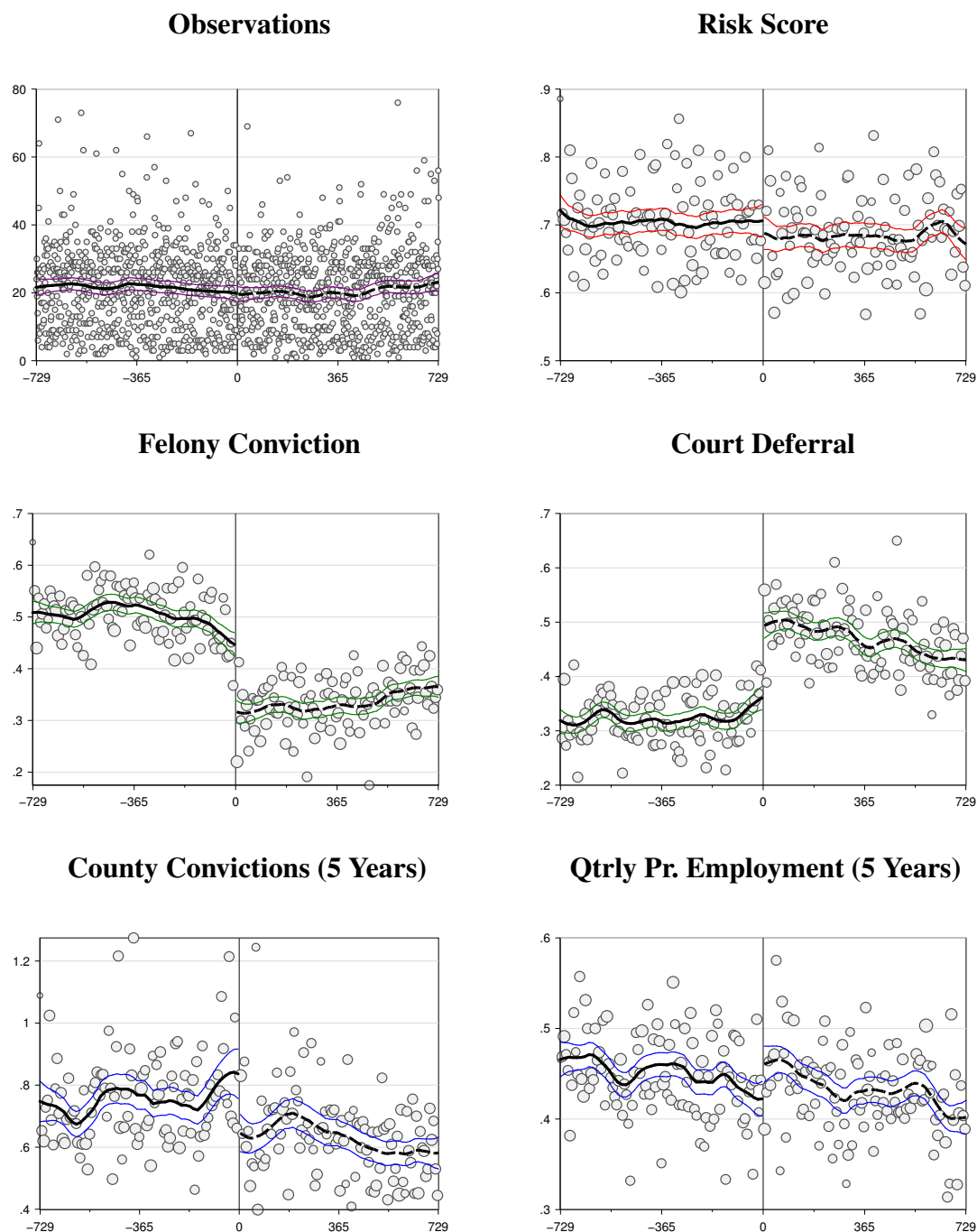


Figure C4: Visual Summary of Evidence, First-Time Property Offenders

Figure Notes: This figure summarizes the graphical evidence from the property offender sample specifically. The first two plots graph the relationship between the forcing variable and the caseload size as well as recidivism risk score. The next two plots depict the relationship between the running variable and court dispositions. The final two plots show the relationship with reoffending an employment outcomes. Table 11 reports the corresponding estimated discontinuities using our regression discontinuity methodology. General graphing notes from Figure 1 apply.

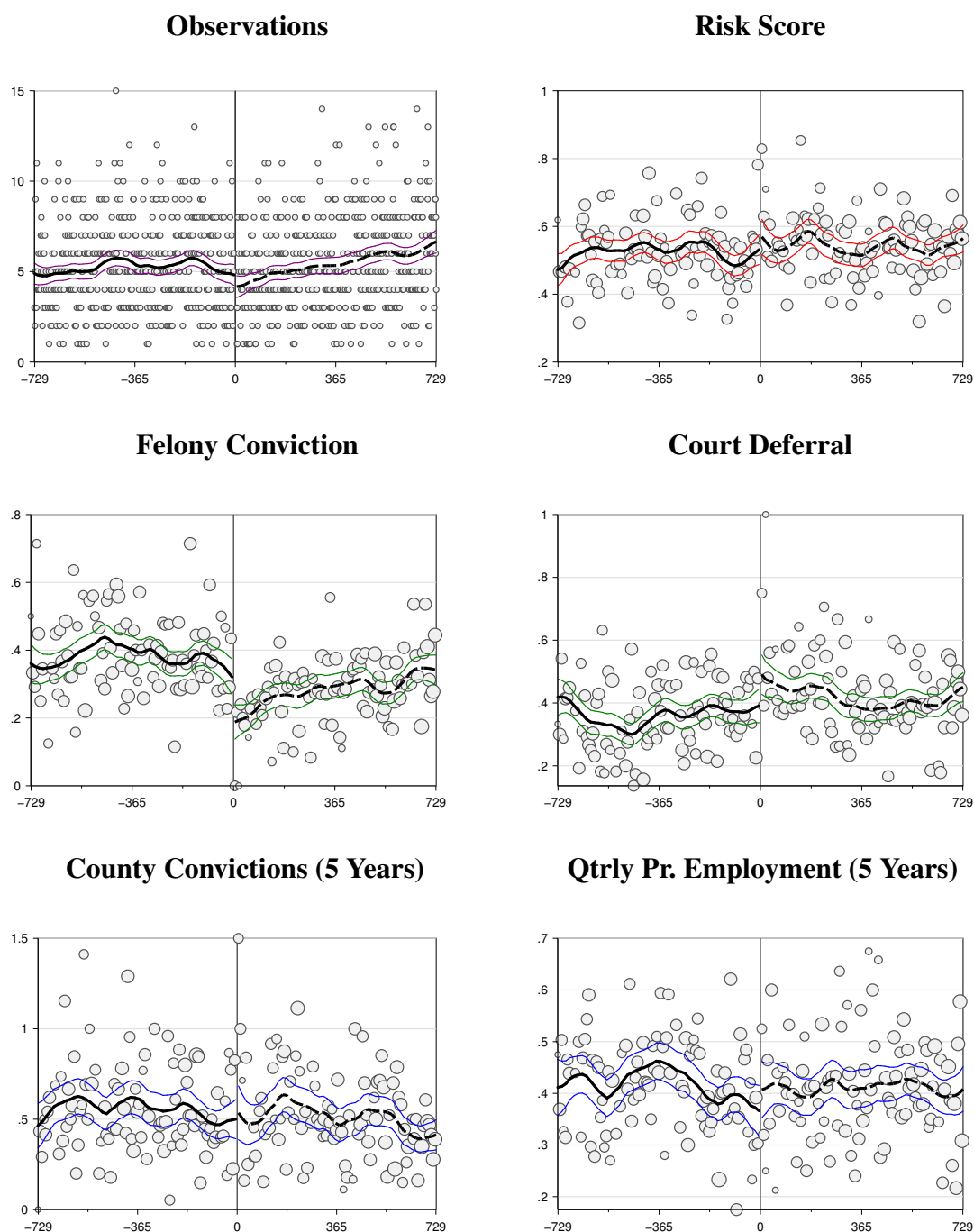


Figure C5: Visual Summary of Evidence, First-Time Violent Offenders (2007 only)

Figure Notes: This figure summarizes the graphical evidence from the violent offender sample specifically. The first two plots graph the relationship between the forcing variable and the caseload size as well as recidivism risk score. The next two plots depict the relationship between the running variable and court dispositions. The final two plots show the relationship with reoffending an employment outcomes. Table 11 reports the corresponding estimated discontinuities using our regression discontinuity methodology. General graphing notes from Figure 1 apply.

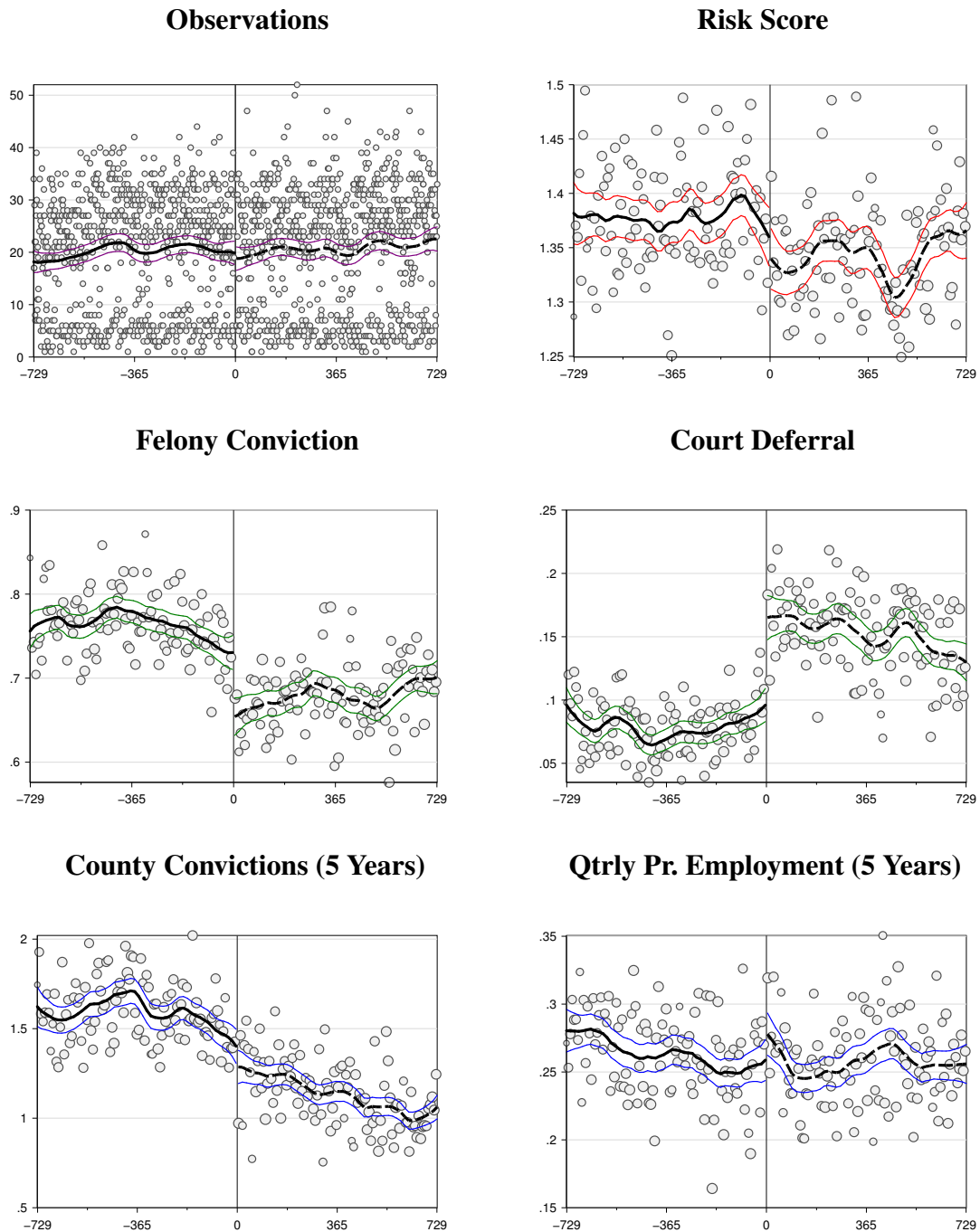


Figure C6: Visual Summary of Evidence, Offenders with one prior conviction or deferral

Figure Notes: This figure summarizes the graphical evidence from the repeat offender sample specifically. The first two plots graph the relationship between the forcing variable and the caseload size as well as recidivism risk score. The next two plots depict the relationship between the running variable and court dispositions. The final two plots show the relationship with reoffending an employment outcomes. Table 11 reports the corresponding estimated discontinuities using our regression discontinuity methodology. General graphing notes from Figure 1 apply.

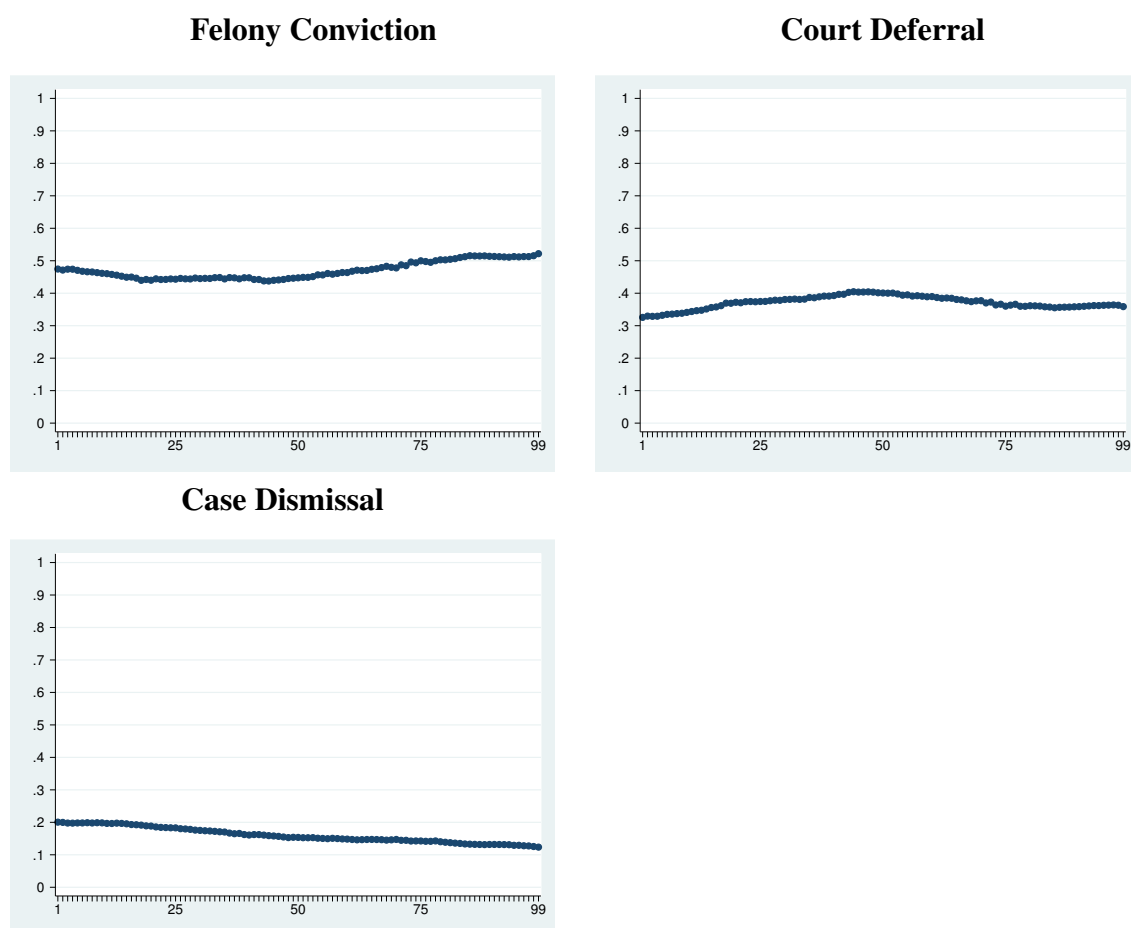


Figure C7: Court outcomes over risk score quantile function

Figure Notes: This figure shows the relationship between the focal outcome variables and the quantile function of the predicted recidivism risk score. Each point estimate documents the average outcome in a 30 percentile bandwidth centered at the focal percentile using a uniform kernel. Estimates below the 15th and above the 85th percentiles will reflect narrower, asymmetric bandwidths.

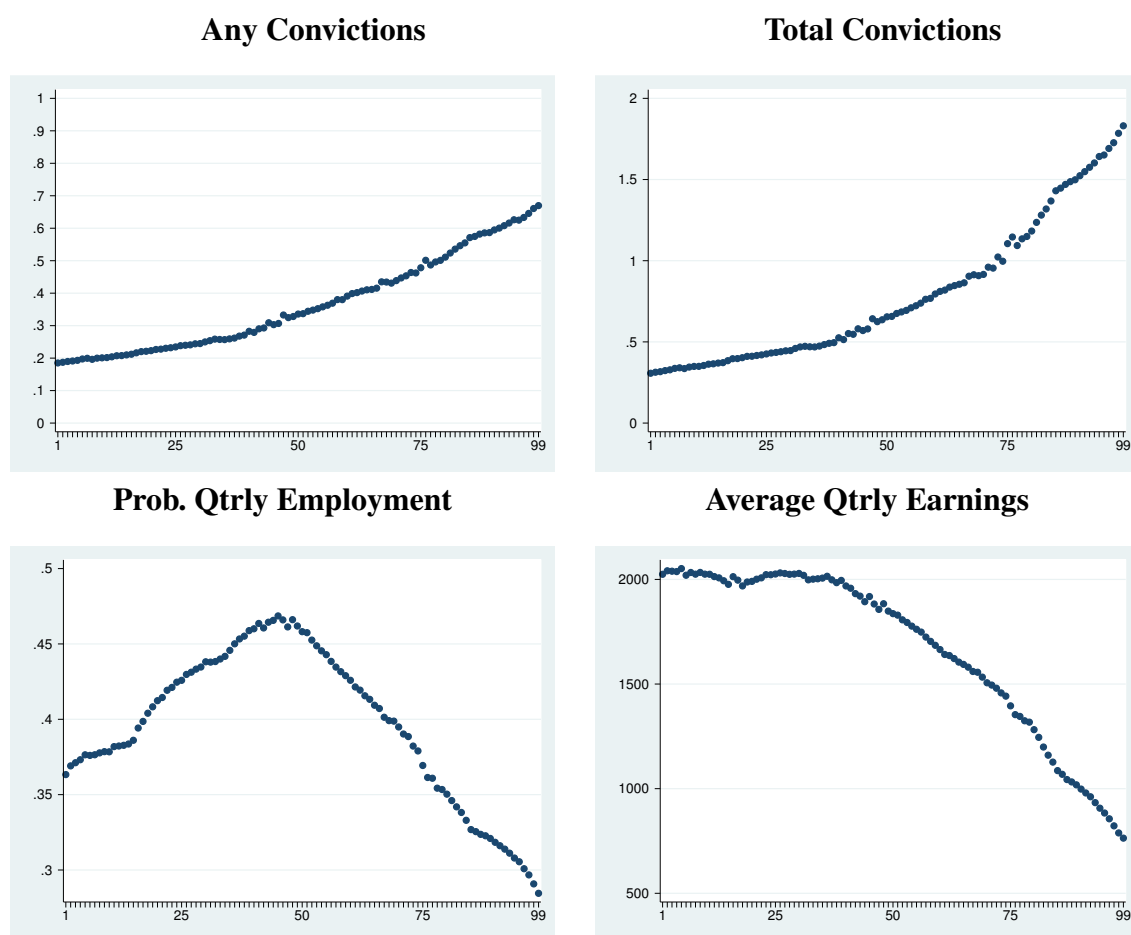


Figure C8: Average five-year outcomes over risk score quantile function

Figure Notes: This figure shows the relationship between the focal outcome variables and the quantile function of the predicted recidivism risk score. Each point estimate documents the average outcome in a 30 percentile bandwidth centered at the focal percentile using a uniform kernel. Estimates below the 15th and above the 85th percentiles will reflect narrower, asymmetric bandwidths.

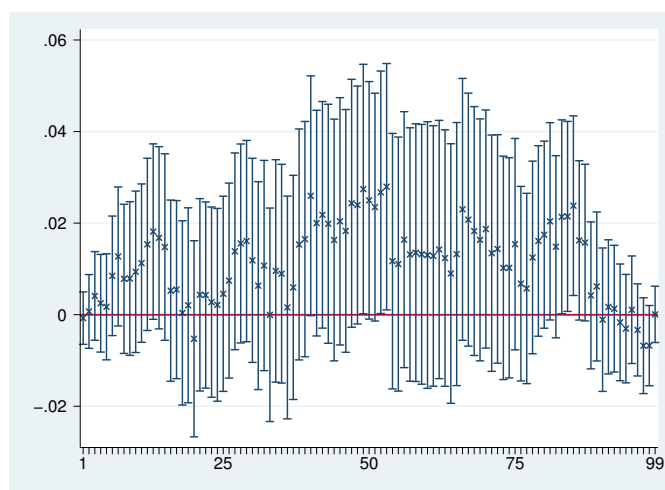


Figure C9: RD balance across risk score quantile function

Figure Notes: This figure shows a series of separate regressions over the quantile function of the predicted recidivism risk score. The percentile-specific estimates document the relationship between the focal date and the CDF of the risk score at that percentile. For uniformity, relevant bandwidths were estimated at the median using specification choices from Figure 1 and applied to the other regressions. Covariates, however, were excluded from the estimation. 90% confidence intervals are included.

Table C1: Discontinuities in Baseline Characteristics, 1994

	Caseload Density	Prior Misd.	Age	Male	Black	Hispanic
High Deferral Regime	2.813 (2.501)	0.017 (0.061)	0.564 (0.513)	-0.034 (0.029)	0.040 (0.029)	0.006 (0.024)
Mean (Low Deferral)	21.43	0.52	28.28	0.72	0.44	0.25
Observations	1,454	30,768	30,567	30,768	30,768	30,768
	Recid. Risk Score	Total Pre-Charge Incarceration Days	Possible to Match to Earnings Records	Pre-Charge Qrtly Prob. Employed	Total Pre-Charge Earnings	
High Deferral Regime	-0.031 (0.029)	3.261 (3.893)	0.000 (0.027)	-0.006 (0.047)	0.294*	
Mean (Low Deferral)	0.84	10.19	0.72	0.44	1.25	
Observations	30,567	30,768	30,768	22,133	22,133	

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

Table Notes: This table presents estimates of discontinuities in pre-determined characteristics as recorded in the criminal court records from the Harris County District Court and the TX unemployment insurance wage records from the Texas Workforce Commission specifically for the 1994 sample. The calculation of the recidivism risk score is described in Section 4.1. General RD estimation notes from Table 3 apply.

Table C2: Discontinuities in Baseline Characteristics, 2007

	Caseload Density	Prior Misd.	Age	Male	Black	Hispanic
High Deferral Regime	-1.747 (3.374)	0.001 (0.050)	0.396 (0.556)	0.024 (0.020)	-0.009 (0.022)	0.027 (0.022)
Mean (Low Deferral)	50.53	0.62	29.65	0.74	0.36	0.32
Observations	1,002	51,001	50,946	51,001	51,001	51,001
	Recid. Risk Score	Total Pre-Charge Incarceration Days	Possible to Match to Earnings Records	Pre-Charge Qrtly Prob. Employed	Total Pre-Charge Earnings	
High Deferral Regime	0.005 (0.025)	-7.169 (5.536)	-0.071** (0.028)	0.017 (0.019)	-0.002 (0.163)	
Mean (Low Deferral)	0.79	20.11	0.66	0.39	1.60	
Observations	50,946	51,001	51,001	33,068	33,068	

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

Table Notes: This table presents estimates of discontinuities in pre-determined characteristics as recorded in the criminal court records from the Harris County District Court and the TX unemployment insurance wage records from the Texas Workforce Commission specifically for the 2007 sample. The calculation of the recidivism risk score is described in Section 4.1. General RD estimation notes from Table 3 apply.