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# **Does Changing Employers' Access to Criminal Histories Affect Ex-Offenders' Recidivism? Evidence from the 2010–2012 Massachusetts CORI Reform**

**Osborne Jackson and Bo Zhao**

## **Abstract**

This paper examines how changes in employers' access to job applicants' criminal histories affect ex-offender recidivism. We use extensive state administrative data on individual criminal histories spanning the 2010–2012 Massachusetts Criminal Offender Record Information (CORI) Reform, widely regarded as landmark legislation governing access to individuals' criminal information. The CORI Reform: i) banned inquiring about criminal history on initial job applications, and ii) broadened the list of groups eligible to use the state's criminal records repository while simultaneously restricting the scope of record access. Using survival analysis and panel regressions, we generally find small reductions in recidivism resulting from each component of the CORI Reform.

**JEL Classifications:** K14, K40, K42

**Keywords:** recidivism, ex-offenders, Massachusetts, CORI

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This paper presents preliminary analysis and results intended to stimulate discussion and critical comment. The views expressed herein are those of the authors and do not indicate concurrence by the Federal Reserve Bank of Boston, or by the principals of the Board of Governors, or the Federal Reserve System.

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## **1. Introduction**

According to a 2006 report from the United States Department of Justice, approximately 30 percent of the U.S. adult population has a state criminal record (U.S. Department of Justice 2006). Within this large segment of the population, many ex-offenders eventually commit new crimes, resulting in a high national recidivism rate. A 2014 report from the U.S. Bureau of Justice Statistics notes that 67.8 percent of prisoners released from state prisons in 2005 were arrested for a new criminal offense within three years, and 76.6 percent were arrested within five years (Durose, Cooper, and Snyder 2014). Even in Massachusetts, where the incarceration rate is relatively low, about 60 percent of all individuals released from state prison or county jails are convicted of new charges within six years (Jonas 2015).

One possible reason for such high recidivism rates is that ex-offenders may face significant barriers when seeking legal employment. When making interview and hiring decisions, many employers conduct criminal background checks and might reject ex-offenders during the job application process. The resulting lack of viable alternatives for legal employment may prompt some ex-offenders to return to criminal activity. As a result, changes to state policies governing employer access to criminal history information could affect recidivism rates and play a role in helping to reintegrate the large ex-offender population into civil society.

This paper examines how changing the laws governing employers' access to criminal histories affects ex-offender recidivism rates by studying the 2010–2012 Massachusetts Criminal Offender Record Information (CORI) Reform. Widely regarded as landmark legislation controlling access to criminal history information, the CORI Reform had two major components. The first component, enacted in 2010 and known as the “ban the box” legislation, made it illegal to inquire about an individual’s criminal history on initial job applications. The second

component of CORI Reform, heretofore referred to as the “record-access” legislation, was enacted in 2012 and broadened the list of employers and other entities eligible to use the state’s criminal records repository while simultaneously restricting the scope of the information that could be accessed. The CORI Reform thus creates a unique opportunity for us to examine the effect of altering employer access to applicants’ criminal histories on recidivism rates. The findings from this analysis have important implications for how legislation should be structured that pertains to criminal history access, which is particularly relevant given the ongoing discussion in Massachusetts and the nation regarding criminal justice reform.

Using an extensive dataset obtained from the Massachusetts Department of Criminal Justice Information Services (DCJIS) on individual criminal histories that spans the periods before and after the CORI Reform was implemented, we employ survival analysis and panel regressions to estimate the reform’s impact on criminal behavior. In general, the estimated hazard rates taken from the survival analysis suggest that the probability an ex-offender will recidivate decreases the longer he/she goes without committing a new convicted offense following an initial conviction. Such negative duration dependence is consistent with some previous recidivism studies and reinforces the importance of what occurs in the initial periods following a criminal conviction in determining re-offense probabilities. Regarding the CORI Reform, we generally find small reductions in recidivism resulting from both components of the reform. Those ex-offenders whose criminal history information is less accessible to employers due to the CORI Reform display lower estimated hazard rates of reoffending following a criminal conviction than they would have experienced in the absence of the reform — with the largest probability of reductions occurring in the initial and crucial post-offense periods. Similarly, panel regressions show that the CORI Reform reduces the probability of reoffending

on the extensive margin, lowers the average number of convicted criminal offenses on the intensive margin, and increases the time that elapses until an ex-offender commits a new criminal offense that results in an additional conviction.

Previous research on the impact that access to criminal histories has on recidivism is relatively limited, and the existing evidence is mixed. Lee (2011) uses a state-level panel to show that with the introduction of online criminal databases, increasing public access to criminal records leads to higher recidivism rates. Using individual-level data from court processing records, D'Alessio, Stolzenberg, and Flexon (2014) similarly find that Hawaii's 1998 ban the box law, which prohibited the practice of inquiring about criminal histories on initial job applications, reduced recidivism rates. Other work has also explored the link between how ex-offenders fare in the labor market and recidivism rates, as this interaction is a likely mechanism for describing the relationship between employer access to applicants' criminal records and repeat offenses. These studies find that increased employment reduces ex-offenders' recidivism rates, though these effects can differ by the ages of the ex-offenders or the sector that employs them (Uggen 2000; Yang 2017; Schnepel forthcoming).

This paper contributes to the existing literature by making use of extensive individual-level state repository data to investigate the multiple policy treatments generated by the Massachusetts CORI Reform. Our study is the first to focus on Massachusetts and this nationally prominent reform governing access to criminal information, and presents results that are potentially generalizable to other low-incarceration states. The remainder of the paper is organized as follows: section 2 provides an overview of the CORI Reform, while section 3 explains the methodology and data. Section 4 reports the results from survival analysis, while section 5 discusses the results from panel regressions. Finally, section 6 concludes.

## **2. Massachusetts CORI Reform**

On August 6, 2010, then-Governor Deval Patrick signed into Massachusetts law Chapter 256 of the Acts of 2010, formally titled “An Act Reforming the Administrative Procedures Relative to Criminal Offender Record Information and Pre- and Post-Trial Supervised Release.” This legislation, commonly known as the CORI Reform, made significant changes to how and to what extent employers, landlords, and licensing boards gain access to an applicant’s criminal history.<sup>1</sup> The reform has two key elements related to employer access to individual criminal histories, which took effect at different times.

Ban the box, the first key element of the CORI Reform, was implemented on November 4, 2010. Before the ban the box policy, employers were allowed to ask individuals about their criminal histories on initial job applications, though some public-sector employers, such as certain state agencies and the Boston City Hall, imposed ban the box in their own recruitment process. After implementing the ban the box policy, both public and private employers were prohibited from inquiring about job applicants’ criminal histories on initial applications, except when federal or state law requires employers to make early inquiries about an applicant’s criminal background, such as for positions involving work with vulnerable populations like children or the elderly. However, the ban the box policy does not prohibit employers from inquiring about an applicant’s criminal history, but just defers this step to a later stage in the process.

By prohibiting the criminal background inquiry from the initial job application, ban the box aims to give ex-offenders an opportunity to be evaluated for a position based on their professional qualifications first and a chance to explain their criminal histories later instead of

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<sup>1</sup> See American Civil Liberties Union of Massachusetts (2010), Massachusetts Department of Criminal Justice Information Services (2012), and Critsley and Koulouris (2012) for more details of the reform.

running the high risk of being automatically rejected by some employers during the initial screening process. In a ban the box advocate's own words, "This [policy] is going to change things enormously, because now people get a chance to get their foot in the door and prove who they are and be considered for their merits before their demerits are counted against them".<sup>2</sup> Reform proponents in Massachusetts and elsewhere argue that ban the box will increase interview and employment opportunities for ex-offenders.

Effective on May 4, 2012, the second key element of the CORI Reform changed who has access to the state's CORI database and how much CORI information employers can obtain (for brevity, we call this the "record-access reform"). Before the record-access reform was implemented, only 3–5 percent of Massachusetts employers were required or certified by the state to access its CORI database.<sup>3</sup> These statutorily required or certified employers often work with vulnerable populations (for example, schools and long-term care facilities) or operate in high-security industries (for instance, banks and security guard companies). The majority of Massachusetts employers had to rely on consumer reporting agencies (CRAs) to conduct criminal background checks on job applicants. CRAs gather criminal history information by using criminal court files, daily police arrest logs, newspaper articles, and so on. Many ex-offenders and advocacy groups are concerned that CRA reports are prone to error or incomplete information—for instance, that the case was eventually dismissed or closed without a finding of guilt—or may contain information that it is illegal to disseminate (Yu and Dietrich 2012). For example, two major employee background check firms were fined \$13 million by the Consumer Financial Protection Bureau in 2015 for selling inaccurate information about the background of job applicants to employers (DiGangi 2015).

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<sup>2</sup> See Michael Levenson, "Criminal Records Bill Gets House OK: Would Limit Access to Job Seekers' Past," *Boston Globe*, May 27, 2010.

<sup>3</sup> See Jack Nicas, "CORI Changes Become the Law," *Boston Globe*, August 7, 2010.

After the record-access reform was implemented, all employers in Massachusetts gained access to the CORI database. The state offered incentives for employers to switch from using CRAs to using the CORI database to conduct criminal background checks. First, the new law provides that if employers solely rely on the CORI database and do not perform additional criminal history background checks through other sources, they will not be held liable for negligent or discriminatory hiring practices within 90 days of obtaining a CORI report.<sup>4</sup> There is no such legal protection for Massachusetts employers who use criminal history information obtained from outside the CORI database. Second, state-maintained CORI data are presumably more accurate than CRA reports.<sup>5</sup> However, from the employer's perspective, one drawback is that the Massachusetts CORI database does not include information about federal crimes or crimes committed in other states. This shortcoming may make national employers operating in Massachusetts or more cautious employers wishing to conduct more comprehensive background checks less motivated to switch to using the CORI system.

The record-access reform also imposed content and time limits on the CORI records available for access when requested by standard employers.<sup>6</sup> Before the reform, there were no

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<sup>4</sup> Before employers can question job applicants about their criminal histories and when employers make an adverse hiring decision on the basis of applicants' criminal histories, employers are required by the new law to provide job applicants with a copy of their criminal history records in the employers' possession, which may be obtained from the state CORI system or from other sources.

<sup>5</sup> In State Fiscal Year 2014 the Criminal Record Review Board, which was created as part of the CORI reform, received only 89 complaints alleging that data provided by the CORI were incorrect (Massachusetts Department of Criminal Justice Information Services 2014).

<sup>6</sup> In addition to standard access, there are three other levels of CORI access with different restrictions. Personal access (for self-audit) and required access (for statutorily required requestors) have fewer limitations on record dissemination than standard access. Individuals may obtain their self-audit CORI reports without cost every 90 days. Using self-audits, individuals are able to see which employers and other non-law enforcement entities have requested their CORI and then determine whether the CORI checks were conducted before being rejected for a job interview or offer. Therefore, self-audits help to ensure employers are following the proper policies and the procedures. In addition, the law prohibits an individual or entity from requesting or requiring another individual to provide a copy of his or her self-audit CORI report. Violators are subject to fines and imprisonment. In contrast, open access CORI requests (for the general public, mainly the media) have more limitations imposed than standard access CORI requests. See the "Summary of Levels of CORI Access with Requestor Types" provided by the Massachusetts Department of Criminal Justice Information Services at <http://www.mass.gov/eopss/agencies/dcjis/summary-of-levels-of-cori-access-with-requestor-types.html>.



legal limitations on the dissemination of unsealed conviction and non-conviction records by the state or CRAs. After the reform, the CORI records for non-convictions and non-incarcerable offenses, just like the records related to sealed, juvenile, and civil cases, are not available for standard employer access. However, the CORI system is required to supply information on standard employer requests regarding convictions for manslaughter, murder, and sex offenses, as well as pending cases for any criminal charges. In addition, the record-access reform shortened the “look-back period,” meaning how long misdemeanor and felony convictions will appear on standard CORI reports. Standard employers have no access to any CORI records for individuals whose misdemeanor convictions are all beyond a five-year limit and whose felony convictions are all beyond a ten-year limit. When standard employers make an inquiry about such individuals, the CORI system reports that no CORI records were found. But if an individual has a misdemeanor conviction that is less than five years old or a felony conviction that is less than ten years old, this conviction and all previous convictions are available for standard employer access.<sup>7</sup> The time used to determine the age of a conviction record is its disposition date (that is, the date when the outcome of a criminal case was decided) or the incarceration release date, whichever is later. However, there is no look-back restriction on convictions for manslaughter, murder, and sexual offenses and pending cases for any crime charges.

### **3. Methodology and Data**

#### **3.1 Conceptual Framework**

Before turning to our analysis, we first consider how the CORI Reform might affect recidivism by affecting the experiences of ex-offenders in the labor market. Within the economics literature,

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<sup>7</sup> See <http://www.mass.gov/eopss/crime-prev-personal-sfty/bkgd-check/cori/reading-rec/sample-cori-response.pdf> for a sample CORI report. Employers are required to submit the name, the date of birth, and the last six digits of the social security number of the persons whom they inquire about.

it has been established that unlawful activity can be modeled as rational and optimizing behavior on the part of criminals (for example, Becker 1968; Ehrlich 1973). Given such a framework, the CORI Reform and its impact on employer access to criminal histories could prompt a change in criminal behavior by altering the net benefit to reoffending via changes in labor market outcomes for ex-offenders (for example, employment and wages).

Conceptually, it is possible for the CORI Reform to cause either increases or decreases in recidivism rates. For instance, the benefit of reoffending could be depicted as earning a short-run wage in the criminal sector. Meanwhile, the costs of reoffending could be framed both as an initial, short-run opportunity cost from forgoing wages in the legal sector during the period of criminal activity, as well as a later, long-run opportunity cost from subsequent forgone wages in the legal sector due to the barriers imposed by having a criminal history. Framing the choice in the context of earning wages legally or illegally, the CORI Reform could thus decrease recidivism by raising the short-run costs of reoffending due to improving ex-offenders' employment and wage prospects in the legal sector. However, the CORI Reform could also conceivably increase recidivism by lowering the long-run costs of reoffending due to reducing the access to ex-offenders' criminal records. Answering the question thus requires conducting an empirical analysis to determine the CORI Reform's actual impact on subsequent criminal behavior.

Additionally, the results from our recidivism analysis may help shed further light on related research we have conducted that examines the impact of the CORI legislation on the labor market outcomes of ex-offenders (Jackson and Zhao 2016). A fall in recidivism rates due to the two components of the reform would be consistent with a labor supply-side driven response to the policy change in our labor study that likely reflects ex-offenders perceiving an improvement in their labor market opportunities. In contrast, finding a rise in recidivism rates

due to the CORI Reform would align with a labor demand-side driven response to the policy change that likely indicates a worsening of labor outcomes for ex-offenders.

### **3.2 Description of Data**

To examine the CORI Reform's effect on ex-offenders' recidivism rates, we utilize records obtained from the Massachusetts Department of Criminal Justice Information Services (DCJIS). The CORI data capture individual criminal histories and reflect the universe of unsealed records available through 2015:Q3. Each record contains information on the individual, such as name, date of birth, Social Security number (SSN), gender, and an address, as well as information on the offense, such as the arraignment date, indication of a civil, misdemeanor, or felony charge, a description of the crime, and the disposition (that is, the court's final determination of the case, including whether the person was convicted of the charge). Missing values are nonexistent for some variables such as the arraignment date, but remain prevalent for others such as the incarceration release date of applicable ex-offenders.

We created an anonymized version of this record-level dataset, dropping some individuals in order to reduce uncertainty about how the DCJIS governs records and to focus on those individuals who are likely to reside in Massachusetts labor markets.<sup>8</sup> We restrict our analysis to records from 1982:Q1 to 2015:Q3 because the DCJIS data we accessed terminate in 2015:Q3, while the records prior to 1982:Q1 are from handwritten and microfilm documents, so

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<sup>8</sup> Specifically, we dropped people if they had one or more of the following issues: a) an invalid SSN; b) the same SSN with different names, birthdates, genders, or races/ethnicities; c) the same name (own and parental) and birthdate with different SSNs; d) the county could not be uniquely identified; e) a non-Massachusetts home address; f) any missing value for the home address, race/ethnicity, gender, date of birth, offense type, disposition type, or arraignment date; g) deported from United States; h) the disposition type is "civil;" i) the arraignment date occurred before age 7 (the minimum age at which someone can be charged with a juvenile offense in Massachusetts, known as the age of criminal responsibility), after September 2015, or after the disposition date when the court reaches its final determination; j) the disposition date occurred after incarceration release date. These restrictions are fewer in number than those imposed in Jackson and Zhao (2016) since the data were not being matched to unemployment insurance wage records for this recidivism study.

the data quality is less certain. This 1982–2015 date range is consistent with examining ex-offender birth cohorts born between 1965 and 1998 since, in our primary analyses, we focus on individuals who were 17 years of age or older when they committed their first crime.<sup>9</sup>

This record-level dataset is transformed into individual-level panel data for our longitudinal analyses and data on “spells” of non-offending for our duration analyses. We restrict our consideration to conviction records only. The unbalanced panel sample corresponds to all conviction records from 1982:Q1 to 2015:Q3, a period spanning the ban the box and the record-access reforms that became effective in 2010:Q4 and 2012:Q2, respectively. This estimation sample consists of 113,558 individuals appearing in the CORI data beginning in the quarter after committing their first crime. The duration sample corresponds to the same records and individuals as the panel sample, and captures the spells (length of time) an ex-offender goes without committing a new crime.

### **3.3 Estimation Strategy**

One potential and basic approach to estimating the CORI Reform’s impact on ex-offenders’ recidivism rates might be to utilize some form of cross-sectional analysis, either at the record level or at the individual level. Such an approach would most closely align with the method used in the limited literature that examines the effect on recidivism of restricting employer access to a job applicant’s criminal history (for example, D’Alessio, Stolzenberg, and Flexon 2014). However, despite the appeal of such a simple approach, we have prohibitive reservations about using this method. The most crucial concern is the fact that recidivism, given its examination of

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<sup>9</sup> For nearly all of the estimation period, this restriction corresponds to a focus on adult criminal records. It was not until September 18, 2013 that the “Raise the Age” bill was signed into law by Governor Deval Patrick, raising the age limit for juvenile offenses from 16 years to 17 years (that is, the maximum age at which someone can be charged with a juvenile offense in Massachusetts).

how long it takes an ex-offender to be convicted of a new offense, is intrinsically tied to the time dimension. Cross-sectional analysis effectively reduces the time element associated with a criminal act to only detecting the presence of a conviction record relative to the implementation of the ban the box and record-access reforms, as well as some base period definition for the recidivism outcome (for example, recidivism within one year, five years, etc.). As a result, the periods of criminal inactivity that are relevant to measuring recidivism may not be fully considered in cross-section estimation.

To avoid this technical limitation, we employ two alternative approaches to estimation. One method is spell-level survival analysis, while the other approach is individual-level longitudinal analysis. We describe the details of each approach in the following two subsections.

### **3.3.1 Spell-Level Survival Analysis**

Reformulating the record-level criminal history dataset as an individual-level duration dataset of spells of criminal inactivity allows us to address the time dimension limitations imposed by the use of cross-section estimation. Besides this advantage, survival analysis also offers other appealing features. For instance, some individuals may not be shown as reoffending solely due to the data sample ending in 2015:Q3. Survival analysis can take such censoring explicitly into account via the likelihood function.<sup>10</sup> Additionally, we may prefer to impose specific distributional assumptions in estimation that are not assumed when employing ordinary least squares (OLS) estimation in cross-sectional analysis. Lastly, with cross-sectional analysis, the exit intervals for the dependent variable need to be pre-specified (for example, recidivism occurs within three years, five years, and so on), and we might want to use an approach where such

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<sup>10</sup> Although alternatively, similar to Yang (2017), one could estimate a censored normal regression for the duration of time without a conviction record. We explore this approach in our panel data analysis.

interval predetermination is not necessary. Rather, we may wish to continuously measure the probability of recidivism at some time  $t$  conditional on having survived up until time  $t$ , and then examine how that probability varies with time  $t$  (for example, do we observe negative duration dependence, such that recidivism becomes less likely the longer one has gone without reoffending?).

We avoid the shortcomings noted above by reconstructing the record-level criminal history dataset as a spell-level duration dataset (a spell is defined as a period without reoffending), in which individuals commence a given spell once they have committed an offense that results in a criminal conviction. Using the survival analysis approach for individual  $i$  and quarter  $t$ , we estimate the hazard rates of recidivism in a Cox proportional hazard model of the following basic form:

$$h(t, x) = h_0(t)\exp(\beta_1 PostBTB_t + \beta_2 TreatRA_{it}), \quad (1)$$

where  $h$  is the hazard function,  $h_0$  is the baseline hazard,  $PostBTB$  is a ban the box post-period dummy, and  $TreatRA$  is a dummy for whether an individual is treated by the record-access reform as of the start of the current year-quarter (that is, the individual is not searchable in the CORI database). This treatment variable always equals zero before 2012:Q2. If both components of the CORI Reform reduced recidivism on average for ex-offenders, and if these effects are detectable in the short run, we expect  $\beta_1 < 0$  and  $\beta_2 < 0$ . However, it is possible that reform effects may not be visible in the short run or, alternatively, that the reform actually increased recidivism, as theorized earlier in section 3.1. Additional covariates, such as demographic

characteristics or county unemployment rates, can also be added to this model in order to examine various mechanisms driving the results or heterogeneous effects.

### 3.3.2 Individual-Level Panel Analysis

While spell-level survival analysis addresses the shortcomings of cross-sectional analysis and has many other appealing benefits, there also are some reasons to prefer an alternative estimation approach. For instance, we may wish to relax distributional assumptions on the estimation errors. Alternatively, we may wish to examine both binary and continuous outcomes in order to separately examine whether the CORI Reform affected the extensive margin of whether or not a person reoffends and compare this result to the intensive margin of how frequently they reoffend (that is, the number of additional crimes).<sup>11</sup>

Reconstructing the record-level criminal history dataset as an individual-level panel dataset allows us to address the limitations imposed by utilizing survival analysis and to also use the panel regressions as a robustness check on our duration data results. As in the survival dataset, individuals enter the (unbalanced) panel once they have committed the crime that earns their first conviction. Using the panel approach for individual  $i$  and quarter  $t$ , we estimate specifications of the following form:

$$Y_{it} = \beta_0 + \beta_1 PostBTB_{it} + \beta_2 TreatRA_{it} + \varepsilon_{it}, \quad (2)$$

where  $Y$  is either a dummy variable for the existence of a conviction record within some base period given a previous conviction (for example, five years), the number of records within some

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<sup>11</sup> We do not examine fixed effects specifications because such models, by utilizing variation within each individual over time, estimate the impact of CORI Reform on recidivism solely from individuals who reoffend at least once. This approach thereby excludes those who never reoffend from contributing to the parameter estimates and is thus not our parameter of interest.

base period of time since a previous conviction, or the number of quarters since the previous conviction was incurred; *PostBTB* is a ban the box post-period dummy; and *TreatRA* is a dummy variable for whether an individual is treated by the record-access reform as of the start of the current year-quarter. We utilize OLS for estimation of the models where the dependent variable is the existence of a CORI record or the number of CORI records, and use a Tobit model (that is, a censored normal regression) when the dependent variable is the number of quarters since the previous conviction record.<sup>12</sup> As was the case in the spell-level survival analysis, our sign predictions are  $\beta_1 < 0$  and  $\beta_2 < 0$  if both components of the CORI Reform reduced recidivism on average for ex-offenders, although the magnitudes and interpretation will now differ. Additional covariates, such as demographic characteristics or county unemployment rates, may again be added to this model.

#### **4. Survival Analysis Results**

Since we observe when each CORI record began, we can use the data to conduct a survival analysis in order to examine whether and how the CORI Reform affects the recidivism rates. This approach has several advantages over other regression methods such as OLS, probit, and logit, since survival analysis better accounts for the sequential nature of the data and thereby makes fuller use of the information. Survival analysis also handles the censoring and structural modeling issues and incorporates time-varying covariates more appropriately.<sup>13</sup>

We start with a hazard model without covariates in which the only explanatory variable is time. The hazard rate is defined as the probability of having a new conviction record (i.e., new “failure”) at time  $t$  conditional on no reconviction up to time  $t-1$ . The temporal unit of analysis is

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<sup>12</sup> We also estimate the censored normal regressions by OLS, and the results are quantitatively very similar.

<sup>13</sup> See Jenkins (2005) for more details about the comparison of the survival time model with other research approaches.



defined as a three-month quarter so that we could incorporate the quarterly county unemployment rate in the later regression analysis.<sup>14</sup> The origin, time  $t = 0$ , is defined as when the most recent conviction record began. In other words, an ex-offender enters the data and is at risk of recidivism. He or she exits the data when a new conviction appears. The exit would be permanent if we use the single-failure data that are restricted to the duration between each ex-offender's first conviction and the second conviction.

However, many individuals are repeat offenders and have multiple conviction records. Thus, we use the multiple-failure data that include repeat durations for each individual. Having a new conviction means a person would simply exit one duration spell and immediately enter the next spell. An ex-convict would never exit the multiple-failure data because theoretically this person is always at risk of recidivism. If the person does not have a new conviction record by 2015:Q3, when the data period ends, his or her data are right censored.

Using the multiple-failure data of all ex-offenders born in 1965 or after, Figure 1 shows the kernel-smoothed hazard rate for a new conviction over time.<sup>15</sup> Overall, the recidivism hazard declines over time, meaning that generally, the longer an ex-offender avoids incurring a new conviction record, the less likely he or she will recidivate. In particular, the hazard rate falls substantially between about the 20th quarter (year 5) and the 40th quarter (year 10) since the last conviction. The hazard rate approaches an almost zero probability at the right tail of the analysis time. This result is consistent with the findings of Blumstein and Nakamura (2009), Bushway,

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<sup>14</sup> The drawback of using a quarterly interval is that we lose some information and do not account for the variation within quarters. If an individual has more than one conviction record within a quarter, we treat these multiple entries as one record. If one of these records is a felony conviction, we code the person to have a felony conviction in that quarter in the survival analysis.

<sup>15</sup> We use the Stata command, "sts graph, hazard," to plot the kernel-smoothed hazard function. Because of the so-called boundary-bias, kernel estimates of the hazard function are less reliable near the boundaries of the data range than in the middle of the data range. Therefore, the default graphing range in Stata is restricted to be between the minimum analysis plus a bandwidth and the maximum analysis time minus a bandwidth. This is why there is a gap near each data endpoint in Figure 1. When we add the Stata command option "no-boundary" to remove this range restriction, we observe that the hazard increases in the first several quarters and then declines.

Nieuwbeerta, and Blokland (2011), and Kurlycheck, Bushway, and Brame (2012), all of whom suggest that 7 to 10 years after being released from prison, the arrest and conviction rates of ex-prisoners fall to near zero. In addition, Sampson and Laub (2003) conduct a 63-year longitudinal study on 500 youth who were committed to reform school and conclude that all offenders eventually desist from further criminal behavior. We observe a similar declining pattern of the recidivism hazard rate for ex-offenders when using the single-failure data.

The relationship plotted in Figure 1 between the recidivism hazard and the analysis time does not consider covariates. Therefore, this figure masks the heterogeneity across ex-offenders, which may affect the risk of recidivism. For example, people with different demographic characteristics may have different recidivism rates. For example, male ex-offenders are probably more likely to reoffend at a given analysis time than female ex-offenders. More important for this study, the ex-offenders who are directly treated by the CORI Reform may have different recidivism hazards at a given analysis time compared to the ex-offenders who are not affected by the CORI Reform.

Figures 2 and 3 present a preliminary visual assessment of the potential impact that the CORI Reform may have on recidivism, using data stratified by each reform treatment variable. We define the ban the box variable to be 1 if time  $t$  is 2010:Q4 or later and to be 0 (zero) otherwise, since the ban the box reform took effect in early November 2010 and affected all ex-offenders. We define the record-access reform variable to be 1 if an ex-offender's criminal history information is inaccessible under standard employer access at the very beginning of the current quarter and to be zero (0) otherwise. This treatment variable is always zero in the pre-record-access-reform period since criminal history information was accessible by employers through CRAs. It may still be zero for many ex-offenders in the post-record-access-reform

period because not every ex-offender is treated by the record-access reform. After its implementation, the CORI system still must report manslaughter, murder, and sex offense convictions and pending cases for any criminal charges on standard employer requests, as well as the CORI records of individuals who incurred at least one misdemeanor conviction less than five years ago or one felony conviction less than ten years ago. In addition, even within the post-record-access-reform period, the value of the record-access reform variable can change for a given individual. After the record-access reform, when an ex-offender receives a new conviction, which falls under the five-year and ten-year limits, the new law requires that the CORI system must show this conviction and all previous convicted offenses if standard employers make a request. Therefore, the value of the record-access reform variable could change from 1 to 0 in the post-record-access-reform period. These variations help to isolate the effect of the record-access reform from the possible time trend associated with recidivism.

In both Figures 2 and 3, the hazard rate shifts somewhat downward with the treatment. This downward shift implies that both elements of the CORI Reform may have led to a small reduction in recidivism. Both figures also suggest that it is likely appropriate to use a proportional hazard model to estimate the treatment effects, since the hazard rate is shown to make a largely parallel shift after the reform. We use a Cox proportional hazard model—a semi-parametric approach—so that we do not need to impose a restrictive assumption on the distribution of the baseline hazard that other parametric models require.

First, we define a standard Cox proportional hazard model with covariates using the single-failure data as follows:

$$h_i(t) = h_0(t) * e^{(\alpha BT_{it} + \beta RA_{it} + \gamma X_{it} + \varepsilon_{it})}. \quad (3)$$

Here  $h_i(t)$  is the hazard at time  $t$  for individual  $i$ ,  $h_0(t)$  is the baseline hazard,  $BTB$  and  $RA$  are the ban the box and record-access treatment variables, and  $X_{it}$  represents the covariates reflecting individual demographic characteristics, criminal background history, and local labor market conditions. Some of the covariates do not vary with time, including gender, race and ethnicity, birth cohort, the age category at the time of the previous conviction, whether the previous conviction was a felony, and home-county fixed effects. We group ex-offenders into six birth cohorts: those born in 1965–1969, those born in 1970–1974, those born in 1975–1979, those born in 1980–1984, those born in 1985–1989, and those born in 1990 or later. An individual who was born during the 1960s or 1970s may be more likely to be exposed to violence and crime, as the rates for both were higher when they were young, meaning these cohorts may have a higher tendency for recidivism than someone who was born in the latter decades. Including these birth cohort variables helps us to control for the possible trends in recidivism that may be coincident with the CORI Reform. We define seven age categories at the time of the previous conviction: younger than 20 years, 20–24 years, 25–29 years, 30–34 years, 35–39 years, 40–44 year, and 45–50 years. In general, ex-offenders in older age categories are expected to have a lower probability of recidivism than ex-offenders in younger age categories. Therefore, including these age category variables helps capture the changes in recidivism rates that occur during an individual’s life cycle. Other covariates are time-varying, such as the quarterly county unemployment rate and quarter fixed effects, which capture local labor market conditions and the seasonality of crime activities. Other studies, such as Crutchfield and Pitchford (1997), Uggen (2000), Raphael and Weiman (2007), Makarios, Steiner, and Travis (2010), Monnery (2015), Schnepel (forthcoming), and Yang (2017), show that being employed reduces recidivism.

Finally, we cluster standard errors at the individual level to account for heteroskedasticity and arbitrary correlations over time within individuals.<sup>16</sup>

Following Cleves (1999), we use an extension of the Cox proportional hazard model—the conditional risk set model—to analyze the multiple-failure data.<sup>17</sup> This approach essentially runs a stratified Cox proportional hazard regression with data stratified by the order of convictions after the first conviction (meaning the first instance of recidivism, the second instance, and so on). Each stratum is assumed to have a different baseline hazard; for example, because an ex-offender with two previous convictions may be more likely to recidivate than an ex-offender who only has one previous conviction, holding everything else equal.

Therefore, we define a stratified Cox proportional hazard model using the multiple-failure data as follows:

$$h_i^j(t) = h_0^j(t) * e^{(\alpha BT_{it} + \beta RA_{it} + \gamma X_{it} + \varepsilon_{it})}, \quad (4)$$

where  $h_i^j(t)$  is the hazard at time  $t$  for an ex-offender  $i$  who already has  $j$  number of previous conviction records, while  $h_0^j(t)$  is the baseline hazard specific to stratum  $j$  and varies by the recidivism order. The coefficients  $(\alpha, \beta, \gamma)$  are assumed to remain the same across all strata. We cluster standard errors at the individual level to account for correlations across spells within the same individuals.

Table 1 shows the estimated coefficients,  $\alpha$ ,  $\beta$ , and  $\gamma$ , obtained from the survival analysis.

The first two columns are based on the single-failure data, while the last two columns are based

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<sup>16</sup> We do not cluster standard errors at the county level because we have only 14 counties. Cameron and Miller (2015) show that the standard errors can be significantly underestimated if there are too few clusters. They recommend a rule of thumb that if the number of clusters is less than 50, this can be considered too few. Nevertheless, we did try clustering standard errors at the county level and find that the results remain statistically significant.

<sup>17</sup> It is also known as conditional model B or the Prentice-Williams-Peterson gap time model (Prentice, Williams, and Peterson 1981).

on the multiple-failure data. When using each dataset, we start with a baseline specification that omits the covariates and then add the covariates in the next, fuller specification to check the robustness of the results. In most cases, adding covariates reduces the size of the coefficients on the ban the box and the record-access reforms, but does not qualitatively affect their statistical significance.

We find that the ban the box variable is consistently negative and significant across the various specifications and across both datasets. The coefficient magnitude is also similar for the same specifications, regardless of the dataset used. In comparison, the record-access treatment variable is less robust. When we use the single-failure data, the coefficient for the record-access reform is positive, close to zero, and not significant. It becomes negative and highly significant when we use the multiple-failure data. There are two possible explanations for these different results in regards to the record-access treatment. First, the multiple-failure data use a larger sample and since it contains all conviction records, this dataset includes more variation than the single-failure data. Therefore, using the multiple-failure data gives us a stronger testing power and allows us to better identify the record-access treatment effect. Second, the record-access reform might simply be a more effective policy tool for reducing the recidivism rates of repeat offenders than reducing the recidivism rates of first-time offenders. This could be because ex-offenders with multiple conviction records may face more significant employment barriers and therefore may benefit more from the reform than do first-time offenders.

Next, we convert the estimated coefficients into hazard ratios and then use these ratios to calculate the percent change in the recidivism hazard that is attributable to each of the reform treatments. Based on the results from the multiple-failure data with full controls, we find that

holding everything else equal, the ban the box and the record-access reforms each result in reducing the recidivism hazard by 11 and 10 percent, respectively.

Most of the covariates are statistically significant and have the expected sign. For example, females; Asians; individuals from more recent birth cohorts than the 1965–1969 birth cohorts; and individuals who were older at the time of their previous conviction are less likely to recidivate. Blacks, Hispanics, Native Americans, and individuals whose previous conviction was for a felony have a higher propensity to recidivate. In addition, the recidivism hazard increases with the quarterly county unemployment rate, a result that suggests ex-offenders are more likely to revert to criminal activities when job opportunities are scarcer.

We conduct additional checks and find that the results are fairly robust. First, we alter the data by adding individuals who have juvenile criminal records. While the analysis is still based on each individual's adult conviction records, we add a covariate indicating whether the person had at least one prior juvenile conviction. The results based on the new expanded data, shown in Appendix Table 1, are almost identical to the previous results. As expected, having at least one juvenile conviction increases the adult recidivism hazard.

Second, we redefine the dependent variable by only using felony convictions or misdemeanor convictions. In other words, we construct new duration data to conduct separate examinations of the hazard of felony recidivism and the hazard of misdemeanor recidivism. The results of this exercise, illustrated in Appendix Table 2, show that the ban the box and record-access treatment variables remain negative and significant. The effect of the reforms on felony recidivism is larger than the effect on misdemeanor recidivism. This could be because ex-felons face more significant employment barriers and therefore may benefit more from the CORI Reforms than ex-misdemeanants.

## 5. Panel Analysis Results

We now use our panel dataset to examine the CORI Reform's impact on recidivism. Table 2 presents the regression results from our estimation of equation (2). The first column shows that when no controls are included, both the ban the box and the record-access reforms decrease the probability, by 1.0 and 1.3 percentage points, respectively, that an ex-offender will incur a new conviction record within the five years following his or her most recent conviction. Before the 2010 passage of the ban the box reform, the unconditional mean of this outcome variable is 2.4 percent. Thus, the estimated coefficient implies that the reform engenders a large reduction in recidivism. Once controls are included to account for observable differences that might affect an individual's probability of reoffending, the magnitude of the ban the box effect more than doubles, going from a 1.0 to a 2.6 percentage point reduction in five-year recidivism rates. However, the coefficient on the record-access reform is no longer statistically distinguishable from zero. This result offers evidence that the CORI Reform reduces recidivism on the extensive margin.

When considering the CORI Reform's impact on the number of conviction records in the five-year period following the most recent conviction, the results are qualitatively similar. The third column shows that without controls that can account for other factors, both the ban the box and the record-access reforms decrease the average number of conviction records by 0.02 and 0.03 records, respectively, in the five years following a previous conviction. Once again, these are substantively large effects, as before the 2010 implementation of the ban the box reform, the unconditional mean of this outcome variable is 0.05 records. With controls added, once again, the size of the ban the box treatment effect grows but remains negative, resulting in a reduction



of 0.05 conviction records on average during this same five-year period. However, we now observe that the coefficient on the record-access reform, an increase of 0.003 conviction records on average during the past five years, is actually significantly positive statistically, albeit with very small economic significance. Overall, while the evidence regarding the ban the box component of the CORI Reform suggests that the policy change leads to a reduction in recidivism on the intensive margin, the results examining the reform's record-access component imply that the evidence on this intensive margin is somewhat more mixed than what is found regarding the extensive margin.

Finally, the last two columns of Table 2 display the results from a censored normal regression model estimating the CORI Reform's impact on the number of quarters that elapse without an individual committing a new crime since the last conviction record. The fifth column shows that without controls, both the ban the box and the record-access reforms increase the number of quarters since an individual's last convicted record by 8.5 and 32.1 quarters, respectively. Prior to enacting ban the box, 21.8 is the mean number of quarters since an individual's prior conviction record. Once controls are added, these effects remain large and positive for both components of the CORI Reform. However, the relative magnitudes of the effects now change, consistent with the prior panel analysis and the survival analysis. The ban the box reform now increases by 27.0 the number of quarters since an individual's last conviction record, while the record-access reform increases by 17.1 the number of quarters since the last conviction record was incurred. Thus, the CORI Reform appears to significantly delay the time that elapses until an ex-offender commits a new crime, due in part to some individuals never reoffending. Additionally, the magnitude of this delay that is attributable to each component of the CORI Reform is consistent with the results on how the reform affects the

probability of having a conviction record in the past five years. On average, compared to the five-year window, the ban the box reform delays the occurrence of a new conviction record for nearly seven years, while the record-access reform delays incurring a new conviction record by just over four years (that is, it falls within the five-year window).

## **6. Conclusion**

This paper examines how changes in employers' access to job applicants' criminal histories affect ex-offender recidivism using the 2010–2012 Massachusetts CORI Reform as the source of variation in criminal history access. Across both survival analysis and panel regressions, albeit with some differences in the main results, we find evidence that both the ban the box and the record-access components of the reform may have reduced ex-offender recidivism. Duration data analysis reveals the existence of negative duration dependence in the relationship between the time that elapses without a incurring a new offense and the probability that an ex-offender will commit a new offense. Meanwhile, panel data analysis shows that the CORI Reform had both extensive and intensive margin effects on recidivism rates.

One could interpret these results as potential evidence that the CORI Reform may have led to a small improvement in labor market outcomes for ex-offenders, thus causing a reduction in incentives to participate in the criminal sector. Such an interpretation might also align with related research that directly examines the labor market question (Jackson and Zhao 2016). Additionally, because new crimes by ex-offenders may be committed against non-offenders or other ex-offenders, an even larger population may have experienced small but similar social welfare improvements as a result of the CORI Reform legislation.

Before such positive interpretations of these results can be definitively accepted, however, additional research is needed on the intermediate mechanisms in markets (for example,

labor markets and housing markets) that might drive such recidivism reductions. For instance, it might be of interest to determine if those individuals experiencing the largest reductions in recidivism, either due to the CORI Reform or another source of variation in labor market access, are the same individuals experiencing the largest improvements in labor market outcomes. Such findings would help to further substantiate a link between ex-offenders gaining greater access to the labor market and reduced recidivism rates.

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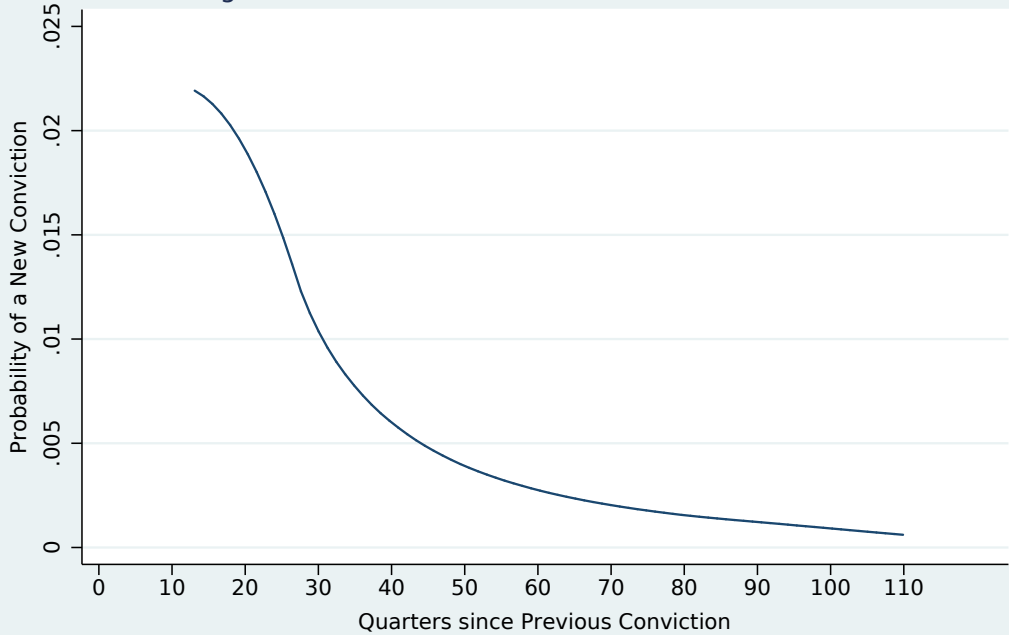
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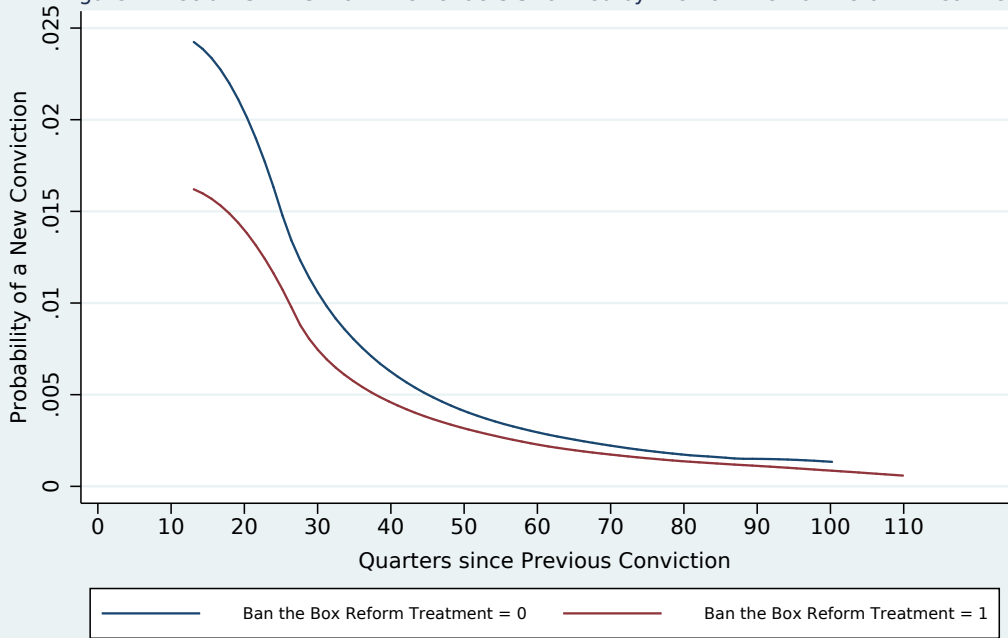
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Figure 1. Recidivism Risk for Ex-Offenders Over Time



Note: The analysis is based on the Kernel-smoothed hazard rate using multiple-failure data.  
Source: Authors' calculations based on the Massachusetts CORI database.

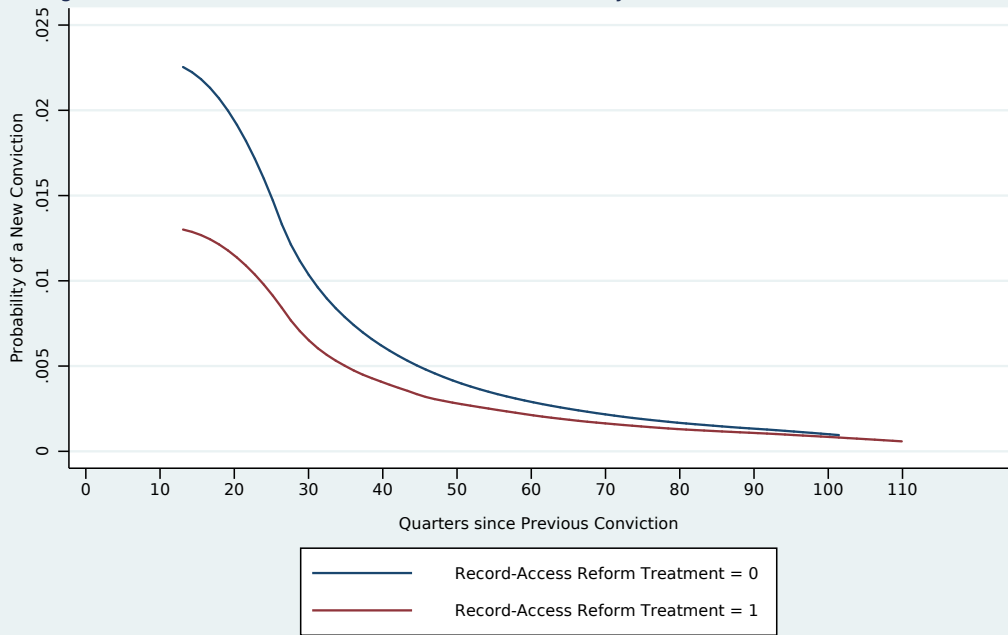
Figure 2. Recidivism Risk for Ex-Offenders Stratified by the Ban the Box Reform Treatment



Note: The analysis is based on the Kernel-smoothed hazard rate using multiple-failure data.  
Source: Authors' calculations based on the Massachusetts CORI database.



Figure 3. Recidivism Risk for Ex-Offenders Stratified by the Record-Access Reform Treatment



Note: The analysis is based on the Kernel-smoothed hazard rate using multiple-failure data.  
Source: Authors' calculations based on the Massachusetts CORI database.

Table 1. Results from Cox Proportional Hazard Models

	Single-Failure Data		Multiple-Failure Data	
Ban the Box Reform Treatment	-0.462*** (0.0160)	-0.184*** (0.0194)	-0.385*** (0.00836)	-0.119*** (0.0101)
Record-Access Reform Treatment	0.0118 (0.0318)	0.0261 (0.0321)	-0.121*** (0.0218)	-0.102*** (0.0221)
Female		-0.265*** (0.0115)		-0.0217*** (0.00819)
Black		0.294*** (0.0137)		0.184*** (0.00757)
Hispanic		0.113*** (0.0126)		0.107*** (0.00727)
Asian		-0.289*** (0.0386)		-0.215*** (0.0295)
Native American		0.417*** (0.111)		0.187*** (0.0643)
Previous Conviction was a Felony		0.191*** (0.0100)		0.0864*** (0.00537)
Quarterly County Unemployment Rate		0.00445** (0.00226)		0.00668*** (0.00128)
Birth Cohort 1970–1974		-0.131*** (0.0109)		-0.102*** (0.00642)
Birth Cohort 1975–1979		-0.149*** (0.0127)		-0.116*** (0.00778)
Birth Cohort 1980–1984		-0.156*** (0.0144)		-0.128*** (0.00922)
Birth Cohort 1985–1989		-0.232*** (0.0190)		-0.197*** (0.0132)
Birth Cohort Since 1990		-0.279*** (0.0322)		-0.245*** (0.0228)
20–24 Years-Old at Previous Conviction		-0.330*** (0.00948)		-0.327*** (0.00680)
25–29 Years-Old at Previous Conviction		-0.583*** (0.0136)		-0.587*** (0.00828)
30–34 Years-Old at Previous Conviction		-0.661*** (0.0197)		-0.749*** (0.0102)
35–39 Years-Old at Previous Conviction		-0.772*** (0.0292)		-0.917*** (0.0132)
40–44 Years-Old at Previous Conviction		-1.013*** (0.0486)		-1.113*** (0.0198)
45–50 Years-Old at Previous Conviction		-1.076*** (0.106)		-1.304*** (0.0397)
Quarter 2 Dummy		0.0819*** (0.0120)		0.0641*** (0.00677)
Quarter 3 Dummy		0.101*** (0.0119)		0.0842*** (0.00664)
Quarter 4 Dummy		0.0458*** (0.0122)		0.0214*** (0.00687)
County Dummies	No	Yes	No	Yes
Observations	4,079,091	4,079,091	7,743,006	7,743,006

Note: Standard errors are in parentheses and are clustered at the individual level.

\* p<0.10, \*\* p<0.050, \*\*\* p<0.010

Table 2 – The Impact of CORI Reform on Recidivism (Panel Analysis)

Dependent Variable	(1) Probability of Conviction Record in Past Five Years	(2) Probability of Conviction Record in Past Five Years	(3) Number of Conviction Records in Past Five Years	(4) Number of Conviction Records in Past Five Years	(5) Quarters Since Last Conviction Record	(6) Quarters Since Last Conviction Record
Ban the Box Reform Treatment	-0.0100*** (0.0002)	-0.0258*** (0.0002)	-0.0186*** (0.0005)	-0.0530*** (0.0006)	8.48*** (0.08)	26.96*** (0.06)
Record-Access Reform Treatment	-0.0127*** (0.0002)	0.0002 (0.0002)	-0.0259*** (0.0005)	0.0027*** (0.0004)	32.09*** (0.10)	17.15*** (0.05)
Controls	No	Yes	No	Yes	No	Yes
Observations	7,743,006	7,743,006	7,743,006	7,743,006	7,743,006	7,743,006

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Note: The controls are a female dummy, black dummy, Hispanic dummy, Asian dummy, Native American dummy, birth cohort dummies, age at previous conviction dummies, a dummy for the previous conviction being a felony, quarter dummies, county dummies, and the quarterly county unemployment rate. All specifications have standard errors clustered at the individual level.

Appendix Table 1. Cox Proportional Hazard Model Results Using Data Incorporating Individuals with Prior Juvenile Conviction(s)

	Single-Failure Data		Multiple-Failure Data	
Ban the Box Reform Treatment	-0.464*** (0.0158)	-0.182*** (0.0192)	-0.388*** (0.00806)	-0.126*** (0.00978)
Record-Access Reform Treatment	0.0108 (0.0316)	0.0247 (0.0318)	-0.123*** (0.0215)	-0.104*** (0.0218)
Female		-0.265*** (0.0114)		-0.0180** (0.00809)
Black		0.289*** (0.0134)		0.173*** (0.00744)
Hispanic		0.115*** (0.0124)		0.104*** (0.00713)
Asian		-0.293*** (0.0386)		-0.216*** (0.0297)
Native American		0.406*** (0.109)		0.186*** (0.0635)
Previous Conviction was a Felony		0.185*** (0.00987)		0.0736*** (0.00523)
Had Prior Juvenile Conviction(s)		0.686*** (0.0262)		0.0964*** (0.0121)
Quarterly County Unemployment Rate		0.00451** (0.00224)		0.00662*** (0.00125)
Birth Cohort 1970–1974		-0.131*** (0.0108)		-0.0974*** (0.00632)
Birth Cohort 1975–1979		-0.146*** (0.0125)		-0.115*** (0.0076)
Birth Cohort 1980–1984		-0.157*** (0.0142)		-0.125*** (0.00891)
Birth Cohort 1985–1989		-0.228*** (0.0185)		-0.192*** (0.0126)
Birth Cohort Since 1990		-0.292*** (0.0315)		-0.241*** (0.0218)
20–24 Years-Old at Previous Conviction		-0.330*** (0.0094)		-0.331*** (0.00655)
25–29 Years-Old at Previous Conviction		-0.583*** (0.0135)		-0.588*** (0.00801)
30–34 Years-Old at Previous Conviction		-0.662*** (0.0196)		-0.748*** (0.00997)
35–39 Years-Old at Previous Conviction		-0.773*** (0.0292)		-0.916*** (0.0129)
40–44 Years-Old at Previous Conviction		-1.012*** (0.0485)		-1.110*** (0.0194)
45–50 Years-Old at Previous Conviction		-1.079*** (0.106)		-1.292*** (0.039)
Quarter 2 Dummy		0.0798*** (0.0118)		0.0616*** (0.00659)
Quarter 3 Dummy		0.101*** (0.0117)		0.0834*** (0.00647)
Quarter 4 Dummy		0.0430*** (0.012)		0.0179*** (0.00669)
County Dummies	No	Yes	No	Yes
Observations	4,100,190	4,100,190	7,845,789	7,845,789

Note: Standard errors are in parentheses and are clustered at the individual level.

\* p<0.10, \*\* p<0.050, \*\*\* p<0.010

Appendix Table 2. Cox Proportional Hazard Model Results for Felony Recidivism or Misdemeanor Recidivism Only

	Felony Recidivism Only	Misdemeanor Recidivism Only
Ban the Box Reform Treatment	-0.158*** (0.0188)	-0.109*** (0.0115)
Record-Access Reform Treatment	-0.482*** (0.065)	-0.190*** (0.0247)
Female	-0.125*** (0.0168)	-0.0116 (0.00902)
Black	0.118*** (0.0144)	0.219*** (0.00852)
Hispanic	0.0231 (0.0142)	0.125*** (0.00817)
Asian	-0.428*** (0.0684)	-0.191*** (0.032)
Native American	0.177 (0.129)	0.188** (0.0773)
Quarterly County Unemployment Rate	0.0180*** (0.00306)	0.00379*** (0.00143)
Birth Cohort 1970–1974	-0.0825*** (0.0148)	-0.112*** (0.00707)
Birth Cohort 1975–1979	-0.112*** (0.0168)	-0.127*** (0.00868)
Birth Cohort 1980–1984	-0.0816*** (0.0192)	-0.142*** (0.0103)
Birth Cohort 1985–1989	-0.105*** (0.0262)	-0.226*** (0.015)
Birth Cohort Since 1990	-0.0196 (0.0417)	-0.313*** (0.027)
20–24 Years-Old at Previous Conviction	-0.224*** (0.0154)	-0.322*** (0.00758)
25–29 Years-Old at Previous Conviction	-0.347*** (0.0177)	-0.587*** (0.00922)
30–34 Years-Old at Previous Conviction	-0.469*** (0.0214)	-0.758*** (0.0114)
35–39 Years-Old at Previous Conviction	-0.602*** (0.0272)	-0.928*** (0.0149)
40–44 Years-Old at Previous Conviction	-0.736*** (0.0391)	-1.133*** (0.0226)
45–50 Years-Old at Previous Conviction	-0.847*** (0.0786)	-1.373*** (0.0481)
Quarter 2 Dummy	0.0692*** (0.0153)	0.0514*** (0.00753)
Quarter 3 Dummy	0.0836*** (0.0151)	0.0765*** (0.00743)
Quarter 4 Dummy	0.0289* (0.0155)	0.0154** (0.00766)
County Dummies	Yes	Yes
Observations	2,633,378	7,087,329

Note: Standard errors are in parentheses and are clustered at the individual level. The analysis is based on multiple-failure data.

\* p<0.10, \*\* p<0.050, \*\*\* p<0.010