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The Effect of Changing Employers' Access to Criminal Histories on Ex-Offenders' Labor Market Outcomes: Evidence from the 2010–2012 Massachusetts CORI Reform

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Abstract

Many regard the 2010–2012 Massachusetts Criminal Offender Record Information (CORI) Reform as a national model to improve ex-offenders' labor market outcomes. This reform prohibits most employers from inquiring about an individual's criminal history on the initial job application (the "ban the box" reform), and reduces employers' access to an applicant's criminal record (the record-access reform). Using the CORI Reform as a natural experiment and a unique large confidential dataset linking individuals' CORI records with their unemployment insurance quarterly wage records, we examine the impact of changing employers' access to applicants' criminal histories on ex-offenders' labor market outcomes. We find that contrary to the intended goal, the CORI Reform has a small negative effect on ex-offenders' employment that grows over time, with mixed effects on earnings and industry composition. Suggestive evidence shows that the negative employment effect is more likely to result from a labor supply response rather than a labor demand response to the policy changes.

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1. Introduction

How to reintegrate the large number of ex-offenders into civil society is an important and challenging policy question. U.S. Department of Justice (2006) estimates that more than 30 percent of the U.S. adult population has some kind of criminal record, with even higher percentages among some minority populations. These ex-offenders face serious barriers when seeking legal employment, as employers often inquire about and check job applicants' criminal histories when making interview and hiring decisions. A 2010 survey by the Society of Human Resource Management finds that around 93 percent of employers check at least some job applicants' criminal background information, and 73 percent of employers conduct checks on all job applicants (Yu and Dietrich 2012). Facing these employment barriers, ex-offenders tend to experience high rates of unemployment and recidivism. For example, ex-inmates have an average unemployment rate of 50 percent or even higher in the nine to twelve months after being released from prison.[1] About 77 percent of ex-inmates were arrested for a new crime within five years after being released (Durose, Cooper, and Snyder 2014). These employment barriers are costly not only for the individual ex-offenders but also for society as a whole. The Center for Economic and Policy Research recently estimated that the United States loses as much as $87 billion in annual GDP because of the reduction in the overall employment rate due to the barriers faced by ex-inmates and ex-felons (Bucknor and Barber 2016).

States and localities across the country have enacted various legal and regulatory changes to reduce the employment barriers faced by ex-offenders in order to improve their labor market outcomes. For example, 24 states and more than 100 cities and counties have adopted the so-called ban the box policy, which typically prohibits employers (public employers in most cases) from inquiring about a job applicant's criminal history on the initial job application (Rodriguez

[1] See Steven Greenhouse, "States Help Ex-Inmates Find Jobs," *New York Times*, January 25, 2011.

and Avery 2016). While the ban the box movement is gaining more momentum across the country, there is limited and sometimes conflicting information about its actual impact on the labor market. A new working paper by Shoag and Veuger (2016) suggests that ban the box increases the employment of those individuals residing in high-crime areas by up to 4 percent. However, two other new working papers, Agan and Starr (2016) and Doleac and Hansen (2016), both show that ban the box leads to employers practicing more statistical discrimination based on race and ethnicity, which results in lower employment of minorities. More research is clearly needed in order to catch up with the fast-changing political movement.

In 2010–2012 Massachusetts implemented the groundbreaking Criminal Offender Record Information (CORI) Reform, with the aim of reducing the employment barriers facing ex-offenders. The CORI Reform, which has significantly changed employer access to criminal history information, is widely regarded as the most comprehensive such reform in the nation. It applies the ban the box provision on both public and private employers, making Massachusetts the second state after Hawaii to do so.[2] During the formal background check, the reform also limits employers' access to a job applicant's criminal record in the state's CORI database. Certain CORI records (for instance, non-convictions, misdemeanor convictions that are older than five years, and felony convictions that are older than 10 years) are not reported on a standard employer search request. According to the Massachusetts Executive Office of Public Safety, the new law prevented the dissemination of CORI information in 57,029 access requests, or 17 percent of total access requests, between May 2012 and November 2014.[3]

[2] Only eight other states have adopted ban the box for private employers (Rodriguez and Avery 2016). They are Connecticut, Hawaii, Illinois, Minnesota, New Jersey, Oregon, Rhode Island, and Vermont. Both Connecticut and Vermont passed the law in 2016, which will take effect in 2017.

[3] See Gintautas Dumcius, "Patrick Touts Impact of Criminal Record Reforms," *State House News Service*, December 18, 2014.

Advocates and officials expected the CORI Reform to improve ex-offenders' job prospects.[4] Reform proponents argued that the ban the box policy would help ex-offenders to more easily pass the application screening and therefore to secure more job interviews, instead of being automatically rejected by some employers. By limiting employers' access to job applicants' criminal records in the state's CORI database repository, reform proponents expected that ex-offenders would find it easier to pass criminal background checks and therefore would be more likely to be hired and to receive higher wages than in the absence of the reform. However, as we will discuss in the section outlining the paper's conceptual framework, economic theory does not necessarily predict that policy changes like the Massachusetts CORI Reform will have positive effects on ex-offenders' labor market outcomes. In fact, the predictions are ambiguous, since the effects depend upon whether and how labor demand (employers) and labor supply (ex-offenders) strategically respond to the policy changes. Therefore, empirical work is needed to test the effectiveness of such reform efforts.

To examine the effect of changing employers' access to job applicants' criminal histories on ex-offenders' labor market outcomes, we treat the Massachusetts CORI Reform as a natural experiment. Specifically, we ask whether the CORI Reform affected ex-offenders in terms of their employment, earnings, and the industries that hire them. We also attempt to understand the underlying mechanism that drives the reform's effect, if any.

This paper makes several important contributions to the literature and policy debate. One, it is the first study to use large data and rigorous econometric techniques to examine the effect that the Massachusetts CORI Reform has had on ex-offenders' labor market outcomes. To our best knowledge, only a 2012 report by the Boston Foundation evaluates the implementation of

[4] See Michael Levenson, "Criminal Records Bill Gets House OK: Would Limit Access to Job Seekers' Past," *Boston Globe*, May 27, 2010.

ban the box in Massachusetts, but this report uses focus group interviews with reform advocates, employers, and other stakeholders (Priest, Finn, and Engel 2012). Three other recent related studies exclusively examine the impact of implementing a ban the box policy. Agan and Starr (2016) use data from New Jersey and New York City, while Doleac and Hansen (2016) and Shoag and Veuger (2016) use nationwide data. However, the Massachusetts CORI Reform goes beyond just having a ban the box policy, as it also reduces employers' access to ex-offenders' criminal record information in the state's CORI database. As the Massachusetts CORI Reform sets a national example, our findings may provide important lessons for other states, especially the ones considering adopting similar policy changes.

Second, we use a unique confidential dataset linking individuals' criminal records with their unemployment insurance quarterly wage records through individual social security numbers over the 2010–2015 period. To our best knowledge, this is the first time that these two large restricted administrative datasets have been linked together. The matched dataset has rich information on individuals' criminal histories, payroll employment status, total earnings, and job sector at the three-digit NACIS code level, as well as some demographic information. This unique dataset allows for a detailed analysis of the impact that the policy reforms had on ex-offenders' employment, earnings, and the composition of industries employing these individuals.

This dataset is significantly different from the ones used in the few related studies. Agan and Starr (2016) use data collected from a field experiment, in which they sent fictitious online job applications to employers in New Jersey and New York City in 2015–2016 and calculated employer callback rates before and after ban the box was implemented. Doleac and Hansen (2016) and Shoag and Veuger (2016) rely on publically available national survey data on employment, matched with state and local ban the box information from the National Employment Law Project. Doleac and Hansen (2016) use monthly Current Population Survey data for 2004–2014. Shoag and Veuger (2016) use annual 2005–2014 American Community Survey data and the Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics. Unlike this paper, neither Doleac and Hansen (2016) nor Shoag and Veuger (2016) observe individual criminal histories. Rather, both these studies use other variables such as race and ethnicity, educational attainment, and local area crime rates as proxies to identify the likely ex-offenders. In contrast, our data allow us to directly observe who the ex-offenders are at a given point of time.

Finally, this paper is the first to provide suggestive evidence of how ex-offenders (labor supply) may respond to the ban the box legislation. Agan and Starr (2016), Doleac and Hansen (2016), and Shoag and Veuger (2016) focus on employers' (labor demand) response to ban the box policies.

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 as to how and to what extent employers, landlords, and licensing boards gain access to an applicant's criminal history.[5] 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

[5] See American Civil Liberties Union of Massachusetts (2010), Massachusetts Department of Criminal Justice Information Services (2012), and Critsley and Koulouris (2012) for more details about the reform.

criminal histories on initial job applications, though some public-sector employers, such as some state agencies and the Boston City Hall, imposed ban the box in their own recruitment process. Pager (2003) and Agan and Starr (2016) find that when employers ask about criminal records on job applications, the probability decreases that an applicant will be asked to contact employers or to interview. 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 for employers required by federal or state law to make early inquiries, such as those who 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 rather defers this step to a later stage in the process.

By prohibiting the criminal background inquiry from being made on 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 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.'"[6] 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 imple-

[6] See Michael Levenson, "Criminal Records Bill Gets House OK: Would Limit Access to Job Seekers' Past," *Boston Globe*, May 27, 2010.

mented, only 3–5 percent of Massachusetts employers were required or certified by the state to access its CORI database.[7] 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, may not include complete information that the case was eventually dismissed or closed without finding guilty, 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.[8]

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 use 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.[9] There is no such legal protection for Massachusetts employers who use criminal history information obtained

[7] See Jack Nicas, "CORI Changes Become the Law," *Boston Globe*, August 7, 2010.
[8] See Christine DiGangi, "Major Employee Background-Check Firms to Pay $13M over Inaccurate Reports," *Credit.com*, October 30, 2015. Available at http://blog.credit.com/2015/10/major-employee-background-check-firms-to-pay-13m-for-selling-inaccurate-reports-128682.
[9] 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.

from outside the CORI database. Second, state-maintained CORI data are presumably more accurate than CRA reports.[10] 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 that are available for access when requested by standard employers.[11] Before the reform, there were no 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

[10] 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).

[11] 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 that employers will properly follow the 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.

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.[12] The time used to determine the age of a conviction record is its disposition date (that is, the date on which the outcome of a criminal case occurred) 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 labor market outcomes. Depending on whether firms change their beliefs about ex-offenders due to the policy, one might expect that the reform would lead ex-offenders to experience employment and/or earnings outcomes that are either positive or negligible. Positive effects would occur if, before the reform, firms use criminal history information to screen out ex-offenders during the hiring process and firms become less willing to engage in such background screening after the reform.

For instance, although the ban the box component of the CORI Reform prohibits most Massachusetts employers from inquiring about applicants' criminal histories on the initial job application, employers are free to make such inquiries later in the application process. Thus, if the ban the box reform is to increase employment or earnings for ex-offenders, such conditions

[12] 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 person whom they inquire about.

would likely require that employers initiate or act upon criminal history inquiries less often after gaining some initial exposure to applicants who are ex-offenders.[13] Absent such changes in employer beliefs about the employability of ex-offenders, we might expect the ban the box reform only to affect the timing of criminal history inquiries rather than the extent of such inquiries or how firms utilize the information from such inquiries. In other words, under these circumstances, the ban the box policy would likely have negligible effects on ex-offenders' labor market outcomes.

Meanwhile, for the record-access reform to improve labor market outcomes for ex-offenders, absent responses by workers or firms, after the reform there must be at least some applicants whose criminal records are less accessible to employers than was the case before implementing the reform. If there are no such applicants or only a very small number, then the record-access reform would likely have a negligible impact on ex-offenders' labor market outcomes.[14]

However, in addition to the potentially positive or nonexistent reform effects discussed above, we might observe responses to the reform on either side of the labor market that consequently could result in negative employment and/or earnings effects for ex-offenders. For instance, on the supply side, there could be a decrease in the willingness of ex-offenders to work at any given wage. Such a decrease might be driven by higher reservation wages for this population due to an expanded set of employment opportunities, resulting in increased quit rates

[13] An additional increase in labor supply due to the reform (for example, resulting from higher labor force participation among ex-offenders) might further amplify this positive effect.

[14] Additionally, if the CORI Reform induces some employers to switch from not checking job applicants' criminal histories to checking these histories, such switching would likewise cause the reform to have a negligible effect on the labor market outcomes of ex-offenders whose histories become inaccessible due to the reform or whose histories are inaccessible before and after the reform. Meanwhile, if ex-offenders' criminal histories are accessible both before and after the reform, an increase in employer inquiries may cause the reform to have a negative effect on labor market outcomes for ex-offenders. However, because the motivation for the CORI Reform was the existence of many employers checking applicants' criminal histories, we suspect that there may be relatively few if any employers who switch from not checking criminal histories to checking these histories.

when employed or prolonged job searches when unemployed.[15] In some surveys, focus groups, and interviews, ex-offenders who have had their criminal records cleared express feelings of accomplishment, hope, and increased control over their lives, any of which might contribute to increasing their reservation wages.[16]

Alternatively, on the demand side of the labor market, employers might reduce their willingness to hire ex-offenders at any given wage. This reduction could occur through a change in hiring criteria, with employers adjusting how they utilize other characteristics like education or experience in order to continue attempting to screen out ex-offenders.

Furthermore, the magnitude of any potential supply-side response could vary by ex-offender traits like race/ethnicity, while the size of a possible demand-side response might differ by employer characteristics such as the industry in which the firm operates. These attributes, in addition to affecting the size of a labor supply or labor demand shift driven by the CORI Reform, could likewise alter supply and demand elasticities that would also affect the reform's impact on ex-offenders' labor market outcomes. As an example, for a labor supply-side response coupled with relatively inelastic labor demand, we would anticipate that the CORI Reform would have smaller employment effects and larger earnings effects on ex-offenders.

3.2 Data Description

To examine the CORI Reform's effect on ex-offenders' labor market outcomes, we combine Massachusetts CORI records obtained from the state's Department of Criminal Justice Information Services (DCJIS) with Massachusetts unemployment insurance (UI) wage records obtained from the Executive Office of Labor and Workforce Development (EOLWD) and the

[15] Although we characterize this mechanism as a labor supply response for simplicity, we acknowledge that an "expanded set of employment opportunities" for ex-offenders also involves a perceived or actual change in labor demand, stemming from employers changing their beliefs about ex-offenders and being more willing to hire them.

[16] See Keramet Reiter, Jeffrey Selbin, and Eliza Hersh, "Should Shoplifting Conviction Be an Indelible Scarlet Letter? Not in California," *Los Angeles Times*, December 28, 2014.

Department of Unemployment Assistance (DUA). The CORI data capture individuals' criminal histories and reflect the universe of available unsealed records 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, and a description of the crime. There are no missing values for some variables, such as the arraignment date, but there are a number of missing values for other variables, such as the incarceration release date for some of the ex-offenders who were sentenced to serve time in jail or prison.

The UI wage data capture individuals' labor market outcomes and reflect employer-provided quarterly earnings records for employees covered by the Federal Unemployment Tax Act of 1939 (FUTA). While these data represent a large share of employees in Massachusetts, the coverage is not comprehensive because it excludes, for instance, some agricultural and self-employed workers. The data also do not include hours worked and contain no occupational information.

Due to confidentiality considerations from the EOLWD/DUA that prevent access to individual-level earnings records, we created a merged criminal history and labor panel dataset that averages the data into cells of no fewer than 20 individuals. This cell-level dataset combines individuals with similar characteristics into each cell. We use these cell-level aggregates of comparable individuals to examine the CORI Reform's impact on labor market outcomes.

To create the cell-level data, we began with the individual-level data on each person's criminal history from the DCJIS. We first determined each person's treatment versus control group assignment, separately for the ban the box and the record-access reforms, based on the relevant characteristics of their criminal histories (the details are discussed further in sections 4 and 5). We then assigned each person to a county based on the address information available from their criminal records. We combined Nantucket, Dukes, Barnstable, and Plymouth counties into a Southeastern MA "super-county," and grouped Berkshire, Franklin, and Hampshire counties into a Western MA "super-county," in order to ensure that the EOLWD/DUA's minimum cell size of 20 persons was satisfied.[17] Within each treatment/control and super-county grouping, individuals were ranked by date of birth. Then this ranked distribution was used to determine the cell assignments of 20–39 people.[18] As a result, each cell represents people who are comparable in age, residential location, and treatment assignment, although the individuals within a particular cell may be heterogeneous along other dimensions like race/ethnicity or gender.

We sent these individual-level cell assignments, along with the SSNs, to the EOLWD/DUA, dropping some individuals in order to reduce uncertainty about how the DCJIS governs records and also to facilitate the matching of earnings records.[19] The EOLWD/DUA then matched these individuals by SSN to their UI wage records before anonymizing and averaging the labor outcome data for each cell. We received these cell-level, earnings record data for the 2010:Q1 to 2015:Q3 period from the EOLWD/DUA. The 2015:Q3 end date corresponds to when the DCJIS data we accessed terminate, while the 2010:Q1 start date corresponds to the

[17] Besides the seven counties that make up the two super-counties, the remaining seven super-counties represent actual counties in Massachusetts.
[18] There are exactly 20 people in 97 to 99 percent of the cells.
[19] Specifically, we dropped people if their information 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 of residence could not be uniquely identified; e) a non-Massachusetts home address; f) addresses in different counties over time; g) any missing values for the home address, race/ethnicity, gender, birthdate, offense type, disposition type, or arraignment date; h) age under 13 years or above 67 years in the 2010:Q1 to 2015:Q3 estimation period; i) deported from United States; j) in prison for at least part of the 2010:Q1 to 2015:Q3 estimation period; k) the disposition type is "civil"; l) the arraignment date occurred before the individual was 7 years old (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; m) the disposition date occurred after the incarceration release date; or n) the individual does not belong to any treatment or control group.

earliest available date that the EOLWD/DUA could provide us with earnings record data. We then linked the cell-level EOLWD/DUA data to the corresponding cell-averaged CORI data (for example, the share of each cell that is female, the share of each cell that is Asian, etc.) and the quarterly super-county unemployment rates. Because we determined each person's treatment and control group assignment separately for each component of the CORI Reform, we constructed two distinct estimation samples. The ban the box reform sample has 32,941 cells averaged across 659,183 individuals, while the record-access reform sample has 28,958 cells averaged across 580,020 individuals.

3.3 Estimation Strategy

We employ various difference-in-differences approaches to estimate by ordinary least squares (OLS) the CORI Reform's impact on ex-offenders' labor market outcomes. For cell *i*, aggregated across approximately 20 individuals, and year-quarter *t*, our baseline estimating equation is:

$$Y_{\mathrm{it}} = \beta_0 + \beta_1 Post_{\mathrm{t}} + \beta_2 Treat_{\mathrm{i}} + \beta_3(Post_{\mathrm{t}} \times Treat_{\mathrm{i}}) + \varepsilon_{\mathrm{it}}, \tag{1}$$

where *Y* is the share of individuals in the cell who are employed (total or across 3-digit North American Industry Classification System (NAICS) industries) or the average earnings of individuals in the cell, *Post* is a post-period dummy, and *Treat* is a treatment group dummy that is identical for all individuals in the cell. Both *Post* and *Treat* vary with each component of the CORI Reform and will be discussed further in sections 4 and 5. We also examine alternatives to our baseline specification that add covariates such as cell demographics or super-county unemployment rates, estimate the dynamics of the effects over time, and estimate heterogeneous effects.[20]

Our coefficient of primary interest is β_3, the differential effect on some labor market outcome of being treated by the CORI Reform in the post-period. As discussed earlier, the sign of β_3 is ambiguous. The sign could be zero if employers do not change their beliefs about ex-offender productivity, might be positive if employers acquire more favorable beliefs about ex-offender productivity, or alternatively, could be negative if labor demand or supply decreases in response to the reform. To interpret β_3 as the causal effect of the CORI Reform on ex-offenders' labor market outcomes depends on unobserved factor(s) related to those outcomes in ε being uncorrelated with $Post \times Treat$. This identification assumption is consistent with the absence of pre-period trend differences between those who are treated by the CORI Reform and those who are not, so that any post-period trend differences may be attributed to the reform's impact on ex-offenders. We examine this parallel trends assumption and its implications for estimation more closely in sections 4 and 5.

4. Impact of Ban the Box on Ex-Offenders' Labor Market Outcomes

This section examines the impact of the first element of the CORI Reform—ban the box—on ex-offenders' labor market outcomes. As discussed in Section 3, we use a difference-in-differences approach as the empirical framework.

4.1 Framework Setup

Given limited availability of older data, the ban the box pre-period consists of the first three quarters of 2010.[21] The ban the box post-period starts from 2010:Q4 (when the ban the

[20] In the record-access reform analysis, covariates additionally include the average labor market outcomes preceding the pre-period as an attempt to capture unobserved productivity (for example, due to education, previous experience, motivation, luck, and so on).

box policy took effect in November) and ends in 2012:Q1 (before the implementation of the record-access reform in May). We do not include data from 2012:Q2 and onward because any effect observed in that later period cannot be solely attributable to ban the box, since the record-access reform was also in place.

We create both the treatment and control groups using individuals from the CORI database. The treatment group includes individuals having at least one CORI record before 2010. Thus, these people keep their ex-offender status in both the pre- and post-periods and are exposed to the ban the box treatment in the post-period (that is, intent-to-treat). Because most of these individuals received their first CORI record long before 2010, their treatment-group status is exogenous to the CORI Reform.

We create the control group by exploiting the fact that some individuals did not commit an offense and therefore did not enter the CORI database until 2012:Q2 or later (meaning after our defined ban the box post-period). In other words, these individuals were still considered non-offenders in both the pre- and post-periods, since they did not have any CORI records throughout this study period. Therefore, they are not exposed to the ban the box treatment in the post-period.[22]

In this difference-in-differences framework, the *Treat* dummy variable captures unobserved permanent differences between the treatment and control groups—such as differences in educational attainment, skills, and work experience—that already existed before

[21] We did not receive access to the pre-2010 unemployment insurance quarterly wage records. The state maintained that the pre-2010 data are not comparable with the post-2010 data because of changes in reporting standards.

[22] There is a possibility that some control group individuals had criminal records in other states before May 2010, which are not covered by the Massachusetts CORI database. Therefore, they were actually treated by the Massachusetts ban the box. However, due to data limitations, we cannot identify these individuals. This would bias our results toward zero.

the ban the box policy went into effect.[23] The *Post* dummy variable captures trends and other unobserved aggregate factors that may cause changes in all individuals' labor market outcomes even in the absence of the ban the box policy. With the *Treat* and *Post* dummy variables, the coefficient on *Post* × *Treat* is identified as the average change between the pre- and post-period in the treatment group, subtracting the average change between the pre- and post-period in the control group.

4.2 Unbalanced Covariates

When using the conventional difference-in-differences estimator, the identifying assumption is that in the absence of the treatment, the average outcomes for the treatment and control groups would have followed parallel paths over time. However, we find that this assumption does not hold in our raw data. As shown in Figures 1 and 2, the average employment rate and quarterly earnings of the control group display a more upward and steeper trend than those of the treatment group before the ban the box policy was enacted. This divergence is mostly because the two groups have significantly different age distributions, which are associated with the dynamics of the outcome variables. As shown in Figure 3, the average age of the control and treatment groups as of 2010:Q1 is 27 years and 41 years, respectively; these age differences have a statistical significance level of less than 1 percent.[24] Therefore, the fact that the average employment rate and quarterly earnings of the control group are on a more upward and steeper trend than those of the treatment group before the ban the box policy could largely

[23] Schanzenbach et al. (2016) report that male high-school dropouts aged 28 to 33 years are much more likely to have a criminal record than men in the same age group who have at least a four-year college degree.

[24] The control and treatment groups also show measurable differences in other observables such as gender or race and ethnicity, although the imbalance in other observables is much less pronounced than the age differential that exists between the two groups.

reflect the likelihood that younger people experienced different employment rates and earnings growth than older people experienced during this period.[25]

The reason why individuals in the control group tend to be much younger than those in the treatment group is due to the way we construct the two groups from the CORI database. Recall that we define the treatment group as those individuals whose first CORI record was incurred before 2010. In reality, most of them received their first CORI record long before 2010. We define the control group as those individuals whose first CORI record was incurred after 2012:Q1. Given that ex-offenders typically committed their first offense in their early 20s, it is not surprising to see the large differences in the age distribution across these two groups.

Because age is an important factor affecting the dynamics of the labor market outcomes, the average outcomes of the treatment and control groups, which have different age distributions, therefore do not follow parallel paths in the ban the box pre-period. Without correcting this covariate imbalance, the conventional difference-in-differences estimator would underestimate the treatment effect in the post-period.

4.3 Balancing Covariates

We use two approaches, as suggested by Linden and Adams (2010), to address the issue of the unbalanced covariates. The first approach runs weighted regressions using the so-called inverse-probability-of-treatment weights. These weights are generated based on the estimated propensity score for a subject receiving the treatment. The second approach runs unweighted regressions on stratified samples. The stratification is based on either the propensity score quintiles or the age quintiles.

[25] Despite having a lower average employment rate, Figure 2 shows that the treatment group has higher average quarterly earnings than the control group. Again, this difference could be largely because the treatment group is significantly older and therefore, if employed, tends to receive higher wages than the control group.

4.3.1 Inverse-Probability Weighted Regressions

The intuition for creating the weights is to give a subject in one group (for example, the control group) a higher weight if he or she "looks" more likely to belong in the other group (for instance, the treatment group) based on observable characteristics. By doing so, the weighted control group sample will have similar distributions of observable characteristics as the weighted treatment group sample, and therefore the covariates will be balanced between the two weighted group samples. Appendix A describes how we create the inverse-probability weights.

After applying the inverse-probability weights, the treatment and control groups display a similar age distribution, as shown in Figure 4. The weighted-average age as of 2010:Q1 is 34.3 years for the control group and 34.9 years for the treatment group. The difference between the two groups is not statistically significant at the 10 percent level. The difference in the weighted average of other observable characteristics for the treatment and control groups is also negligible.

With balanced covariates across the treatment and control groups, now the average employment rate and the average quarterly earnings of the two groups are largely on parallel paths in the period before ban the box was implemented, as depicted in Figures 5 and 6. The fact that the control group is above the treatment group in both figures is also consistent with a common perception that given similar observable characteristics**,** individuals who have CORI records are less likely to be employed and to have lower earnings than individuals without CORI records.[26] After ban the box goes into effect, the gap between the control and treatment groups in both figures appears to slightly widen, which implies a potentially small negative treatment effect.

[26] Unlike in Figure 2, the control group in Figure 6 is now on top of the treatment group in terms of the average quarterly earnings.

Table 1 presents the results from the inverse-probability weighted regressions of the employment rate. The dependent variable is the percentage of individuals in each cell who are employed in each year-quarter. To check the robustness of the results, we use three model specifications. The first column includes no controls, which provides a baseline difference-in-differences estimate. To reduce potential omitted variables bias, the second column adds controls for observed cell-level demographics (such as age, gender, race and ethnicity) and local labor market conditions (meaning the quarterly county/super-county unemployment rate) as well as quarter and county/super-county fixed effects. Demographic variables are defined as the percentage of females, the percentage of blacks/Hispanics/Asians/Native Americans, and dummy variables for the average age (rounded to the closest integer) in each cell. Both the first and second columns use the default standard errors. To account for the potential heteroscedasticity and correlations within cells, the third column uses standard errors clustered at the cell level.[27]

We find that the coefficient on *Post* × *Treat* is robust across all three specifications of the model.[28] Adding various controls in the second column only slightly reduces the magnitude of the coefficient. Clustering standard errors in the third column does not affect the statistical significance of the estimate. In addition, most of the control variables have the expected sign on their coefficients. For example, the average employment rate of each cell is lower when there is a higher share of Hispanics in the cell or when the quarterly county/super-county unemployment rate increases.

[27] We have also tried clustering standard errors at the county/super-county level, but this exercise does not change the statistical significance of the results. We prefer to cluster standard errors at the cell level, rather than at the county/super-county level. This is because we have only nine counties/super-counties. Cameron and Miller (2015) show that the standard errors could be significantly underestimated when there are too few clusters. The number of clusters is often considered too small when there are less than 50 clusters.

[28] We tried running unweighted difference-in-differences estimations on the full sample, without using the inverse-probability weights. The coefficients on *Post* ×*Treat* from those regressions are still negative and highly statistically significant.

Economically speaking, the effect of ban the box on ex-offenders' employment is negative but small. Holding everything else equal, the employment gap between ex-offenders and non-offenders grows 2.36 percentage points after implementing ban the box. This change is relatively small, considering that, on average, more than 46 percent of ex-offenders were already employed in the pre-period.

Similarly, Table 2 shows that ban the box has a negative, but small, impact on the quarterly earnings of ex-offenders. The gap in quarterly earnings between ex-offenders and non-offenders increases by $300 after ban the box goes into effect. The negative effect on earnings is partially due to the growing employment disparity between ex-offenders and non-offenders, since the earning measure is an average among all individuals in each cell, which includes the zero earnings of the unemployed.

We cannot pinpoint ban the box's pure effect on earnings because of the limitations of our data, which are aggregated to the cell level. Given the aggregate, anonymous nature of the data, we cannot identify who in each cell was employed in each year-quarter and therefore cannot directly compare the earnings of the same individuals who were employed in both the pre- and post-periods.[29]

4.3.2 Stratification

As an alternative to the inverse-probability weighted regressions, we run unweighted regressions on each of the to-be-defined stratum of the data. The rationale for using stratifications to address unbalanced covariates is that the treatment and control groups should have more similar observable characteristics within each stratum than in the whole sample.

[29] Appendix B describes an alternative, albeit imperfect, approach to explore the earnings effect of ban the box. It uses the average earnings of only the employed individuals in each cell in each year-quarter as the dependent variable in the regressions.

Previous research indicates that stratification is highly effective in balancing covariates. For example, Cochran (1968) and Rosenbaum and Rubin (1984) show that stratifying data into quintiles of propensity scores can eliminate over 90 percent of the initial estimation bias due to the unbalanced covariates.

We use two stratification strategies, one based on the quintiles of individual ages as of 2010:Q1 and the other based on the quintiles of treatment propensity scores. Age as of 2010:Q1 is chosen as one of our stratification criterion because it is the main unbalanced covariate between the treatment and control groups. Once we deploy each stratification approach, we find that the observable characteristics are indeed fairly similar between the treatment and control groups within each stratum. More importantly, the average employment rate and the average quarterly earnings of the two groups within each stratum are largely on parallel paths in the pre-period, validating using the difference-in-differences estimation on the stratified data.

The unweighted regression results on the stratified data are similar to those from the weighted regressions conducted on the whole unstratified data. These unweighted regressions include all the control variables that we use in Table 1 and cluster standard errors at the cell level. In the employment regressions, shown in Table3, the estimated coefficient on *Post* × *Treat* is negative, statistically significant, and within a narrow range of –1.6 to –3 percentage points for each stratum except for the fifth age quartile. Similarly, Table 4 shows that the coefficient on *Post* × *Treat* is consistently negative and small in each stratum regression of average quarterly earnings, although the results from three stratum regressions are not statistically significant.

4.4 Dynamics of the Effects

The standard difference-in-differences model that we use has one drawback: it assumes a constant treatment effect during the entire post-period. This assumption may not hold, since it could take time for employers and/or ex-offenders to learn and adapt to the policy changes. As a result, ban the box could have dynamic effects on ex-offenders' labor market outcomes.

To correct for this drawback, we estimate the following more flexible form of the difference-in-differences model, which allows the ban the box treatment effect to change over time:

$$Y_{it} = \beta_0 + \sum_{j=1}^{6} \beta_{1j} Post_{jt} + \beta_2 Treat_i + \sum_{j=1}^{6} \beta_{3j} \left(Post_{jt} \times Treat_i\right) + \beta_4 X_{it} + \varepsilon_{it}, \qquad (2)$$

where j is an index for each of the six year-quarters in the ban the box post-period. By construction, the average of β_{1j} and β_{3j} is equal to β_1 and β_3 estimated in the original model, respectively.

In Table 5, the first column shows that the ban the box policy indeed does have dynamic effects on ex-offenders' employment outcomes. The effect remains negative throughout the post-period year-quarters, but grows steadily over time. The effect increases from –1 percentage point in the first quarter following the policy implementation to –3.8 percentage points one year and a half (six quarters) later. On average, there is about a –0.5 percentage point change per each successive quarter. A statistical test strongly rejects the null hypothesis of equal coefficients for $post_{jt} \times treat_i$, thereby supporting the hypothesis of dynamic effects. In addition, each coefficient for $post_{jt} \times treat_i$ is by itself highly significant, and they are also jointly significant.

We do not find strong evidence that ban the box has dynamic effects on ex-offenders' earnings. In Table 5, the second column shows that there are no clear patterns in the estimated earnings effects over time. While the coefficients for $post_{jt} \times treat_i$ are jointly significant, not all are statistically significant. More importantly, even at the 10 percent significance level we cannot reject the null hypothesis of equal coefficients.

4.5 Heterogeneity of the Effects

Previous research suggests that ban the box policies might affect certain subgroups of ex-offenders differently than other subgroups. For example, Agan and Starr (2016) find that ban the box increases the gap between white and black applicants in terms of employer callback rates of job interviews. Doleac and Hansen (2016) show that ban the box decreases the likelihood of being employed for young black or Hispanic men who are low-skilled. Both papers infer that a ban the box policy encourages employers to practice statistical discrimination based on the applicant's race and ethnicity. These conclusions are consistent with Holzer, Raphael, and Stoll (2006), who suggest that some employers without access to criminal history information use information regarding an applicant's race to infer who has a criminal history. However, Shoag and Veuger (2016) suggest that ban the box increases the employment of black men.

We use a difference-in-difference-in-differences approach to explore whether the ban the box effect varies by ex-offender characteristics and by the labor market conditions. First, we interact *Post* × *Treat* with observed demographic variables, including the average age, the percentage of females, and the percentage of blacks/Hispanics/Asians/Native Americans in each cell. Second, we interact *Post* × *Treat* with several criminal history variables to test whether the effect of ban the box is stronger or weaker for ex-offenders with more lengthy criminal histories, with more serious criminal convictions, or with more recent convictions. One working hypothesis being tested is the idea that some employers may care less about information pertaining to minor crimes or crimes committed in the far past and about non-convictions than about information on newer major crimes (such as some felonies) and more recent conviction records.

The criminal history variables that we construct in each cell using the information before ban the box was implemented include the average number of CORI records, the average number of felony records, the average number of convictions, the percentage of individuals having a felony record, the percentage of individuals having a conviction, and the average number of year-quarters since the last CORI record. Third, we interact *Post* × *Treat* with the quarterly county/super-county unemployment rate, which is a proxy for the local labor market conditions. One working hypothesis is that the presumably positive effect of ban the box is stronger in a tighter labor market because employers lower their requirements and hire more ex-offenders. However, we do not find consistent patterns among these new interaction terms.

4.6 Effects on Ex-Offenders' Employment in Specific Industries

So far we have found that ban the box has a small but negative effect on ex-offenders' employment. However, this effect may not be evenly distributed across industries. This might be because employers in some industries, such as restaurants and construction companies, already tended to hire more ex-offenders than employers in other industries, such as financial firms and security firms before the ban the box went into effect.[30] To examine in what industries ex-offenders lose or gain jobs, relative to non-offenders, after ban the box was implemented, we turn to employment data detailed at the three-digit NAICS code levels, and run a difference-in-differences regression for each three-digit NAICS code. The dependent variable in each regression is defined as the percentage of individuals in each cell employed within each three-digit NAICS industry in each year-quarter. The model specification with full controls and clustered standard errors is the same as in Table 1, column 3.

[30] Many web sites, such as http://jobsthathirefelons.org/, http://www.jailtojob.com/companies-hire-felons.html, and https://exoffenders.net/employment-jobs-for-felons/, list the industries and employers that accept ex-offenders. They often mention the restaurant, hotel, and construction sectors.

For simplicity, Table 6 shows only the significant coefficients on *Post* × *Treat* from the industry-level regressions. These are ranked in an ascending order. First, ban the box has a negative effect on ex-offenders' employment in many more industries than it has a positive effect in other industries. Relative to non-offenders, ex-offenders experience job losses in 15 industries, but hiring increases in only three industries. The three positive coefficients are only weakly significant and are essentially negligible in terms of magnitude.

Second, the two industries that experience the largest number of job losses among ex-offenders relative to non-offenders—Administrative and Support Services (NAICS 561) and Food Services and Drinking Places (NAICS 722)—happen to be the ones that hired the most ex-offenders in the period before ban the box went into effect. These industries are also at the lower end of the wage distribution. In particular, the average weekly wages in the Food Services and Drinking Places category are the second lowest among all three-digit NAICS codes in Massachusetts in 2010, according to the data from the Massachusetts State Department of Labor. In other words, after implementing ban the box, the employment gap between non-offenders and ex-offenders increased the most in industries that are commonly known as the most accepting of ex-offender employees and which are also among the lowest-paying industries. One possible interpretation for this finding is that some ex-offenders, encouraged by ban the box, left or avoided low-paying sectors that traditionally hired those with criminal records, but did not make a successful transition to jobs in higher-paying industries.

4.7 Why Does Ban the Box Have a Negative Effect on Ex-Offenders' Employment Outcomes?

In theory, ban the box's negative effect on ex-offenders' employment outcomes could result from a labor demand response, a labor supply response, or both. On the labor demand side, employers may change their hiring practices after the ban the box policy goes into effect in order to screen out potential ex-offenders before conducting interviews. For example, employers may raise application requirements for educational attainment or work experience, even if higher educational attainment and longer work experience are not needed to fulfill the job task (Shoag and Veuger 2016). Compared to other applicants, higher requirements could exact more harm on ex-offenders because they tend to have lower education and spottier employment histories.

While Agan and Starr (2016) and Doleac and Hansen (2016) suggest that employers may also practice more statistical discrimination based on a job applicant's race and ethnicity after the implementation of ban the box, this type of labor demand response is unlikely to explain our results. First, after applying the inverse-probability-of-treatment weights, we obtain balanced covariates, including race and ethnicity, across the control and treatment groups. In other words, the two weighted groups have almost the same racial and ethnic distributions. Therefore, one cannot claim that employers can discriminate more against our treatment group for the reason that it has a higher percentage of blacks and Hispanics than our control group—the covariates are balanced. Second, we control for the percentage of blacks/Hispanics/Asians/Native Americans in the full regression specification. Therefore, we use the variation within race and ethnicity to identify the effect of ban the box on ex-offenders' employment outcomes.

The labor supply hypothesis that explains ban the box's negative effect on ex-offenders' employment outcomes is that ex-offenders might become somewhat more selective about what jobs they apply for or raise their reservation wages after the implementation of the ban the box policy. This could occur because they might expect lower employment barriers and more job opportunities especially if, through increased interactions with job candidates who are ex-offenders, employers start to develop more positive beliefs about this group of individuals. If this is the case, some ex-offenders might not apply for jobs with low pay and poor working conditions that they might have sought before ban the box was implemented. Likewise, ex-offenders might not accept a job offer that they would have taken before the policy change.

Unfortunately, we do not have the ideal data to test these hypotheses. To directly test the labor demand hypothesis, we would need data on each job posting's application requirements, data on each applicant's educational attainment and work experience, and data on the outcome of each job application before and after the implementation of ban the box. To directly test the labor supply hypothesis, we would need data on the jobs that each individual applied for, data on the job offers that he or she accepted or rejected, and data on his or her work hours, hourly wages, and employed industry before and after the policy change. In addition, we would need data from a longer post-period in order to detect the long-term effect of ban the box. However, we have none of these data, which restricts our ability to offer a definitive answer as to which hypothesis explains ban the box's negative effect on ex-offenders' employment outcomes.

Instead, we employ two less ideal approaches when attempting to disentangle the labor demand and supply hypotheses. The first approach uses the findings of other studies on the Massachusetts ban the box law and checks which hypothesis the results from these studies are more likely to support. To assess the implementation of ban the box, the Boston Foundation and the Crime and Justice Institute at Community Resources for Justice conducted a series of focus groups and interviews with employers, advocacy groups for ex-offenders' rights, CORI system officials, and state legislators in 2012 (Priest, Finn, and Engel 2012). Their report indicates that none of the employers that they interviewed made significant changes in the application or interview process in response to the implementation of ban the box, except for removing the criminal history check box on job applications. Practically speaking, this is likely because making further changes in hiring practices is costly for employers, but also may be viewed as unnecessary since the ban the box policy allows employers to inquire about job applicants' criminal histories at a later stage in the hiring process. In addition, changing the hiring process could unintentionally result in screening out many non-offender applicants who are mistakenly assumed to be ex-offenders based on problematic proxies such as educational attainment and work experience. On the other hand, the interviews with advocacy groups for ex-offenders' rights revealed that the ban the box policy has indeed resulted in more job interviews for ex-offenders in Massachusetts. This finding suggests that ban the box has the potential to raise ex-offenders' expectations regarding their job prospects.

We also rely on the findings from Jackson and Zhao (2016), another study of the Massachusetts ban the box reform, to test different implications of the labor demand and supply hypotheses for changes in the recidivism rate after ban the box was implemented. On the one hand, the labor demand hypothesis implies that ban the box would increase the recidivism rate. This is because if employers change their hiring practices in order to screen out applicants who are ex-offenders, it would become more difficult for ex-offenders to find jobs, and therefore more ex-offenders would return to criminal activities, thus driving up the recidivism rate. On the other hand, the labor supply hypothesis implies that ban the box would decrease the recidivism rate. This is because if ex-offenders become more optimistic about their potential job opportunities, wages, and working conditions, they would become more engaged in the legal labor market and therefore be less likely to recidivate. Using the Massachusetts individual-level CORI data, Jackson and Zhao (2016) find that ban the box reduces the recidivism rate in Massachusetts. This result is more consistent with the labor supply hypothesis, not with the labor demand hypothesis.

The second but still less than ideal approach that we take to disentangle the labor demand and supply hypotheses involves using our data to conduct correlation analyses and checking whether the correlation results are consistent with the predictions from the labor demand or supply hypothesis. First, the labor demand hypothesis predicts that those industries that are more accepting of ex-offenders may be less likely to change their hiring practices after the implementation of ban the box than those industries that are less accepting of ex-offenders. Therefore, ban the box's negative employment effect is expected to be smaller in industries that are more accepting of ex-offenders. To test this prediction, we create an index of industry acceptance of ex-offenders by calculating a ratio of the average percentage of the treatment-group individuals (ex-offenders) employed in each industry to the average percentage of the control-group individuals (non-offenders) employed in the same industry during the entire ban the box pre-period. The percentage of the control-group individuals employed in each industry is the denominator of the ratio used to account for the overall employment size of each industry. The higher the ratio, the more accepting the industry is of ex-offenders. To make the estimated employment effect of ban the box more comparable across industries, we scale the coefficient on *Post* × *Treat* in each industry regression (which is a percentage point change) by the average percentage of the treatment-group individuals employed in the same industry before ban the box was implemented. In doing so, the rescaled coefficient represents a percent change of ex-offenders' employment relative to their employment in that industry during the pre-period. The labor demand hypothesis predicts that this rescaled coefficient should be positively correlated with the index of industry acceptance of ex-offenders. However, we find that the correlation between the two measures across industries is –0.18 and is significant at the 10 percent level.

Second, the labor supply hypothesis predicts that after ban the box was implemented, ex-offenders are more likely to leave lower-paying industries than to leave higher-paying industries. Therefore, the negative employment effect of the ban the box policy is expected to be larger in industries paying lower wages. To test this prediction, we correlate the rescaled coefficient on *Post* × *Treat* with the 2010 average weekly wages, as reported by the Massachusetts State Department of Labor, across industries. The correlation is 0.17 with slightly greater than a 10 percent significance level. This result is largely aligned with the prediction of the labor supply hypothesis.

Overall, we find some suggestive evidence that ban the box's negative effect on ex-offenders' employment outcomes is more likely to result from the labor supply response. However, given the limitations of our data, we cannot rule out the possibility that the labor demand response may also contribute to the negative employment effect resulting from the ban the box policy.

5. Impact of the Record-Access Reform on Ex-Offenders' Labor Market Outcomes

5.1 Framework Setup

When considering the record-access component of the CORI Reform, which affected the persons or groups eligible to obtain criminal records from the state database and also affected the scope of access to those records, we now shift to using a new data sample and introduce different definitions of both the post-period as well as the treatment and control groups. We now define the post-period as 2012:Q2 through 2015:Q3, with the pre-period defined as 2010:Q4 through 2012:Q1 (that is, the ban the box post-period).

The treatment group is comprised of cells populated with ex-offenders whose criminal records are searchable in the pre-period and unsearchable (under DCJIS "standard" employer access rules) in the post-period. Some treated individuals are unsearchable in the post-period due to the timing of their offense(s) and the record(s) no longer being sufficiently recent to be accessible, while other treated individuals are unsearchable due to the type of crime(s) they committed and such crimes not being accessible via the state's database following the CORI Reform regarding record access. Lastly, the control group contains cells populated with ex-offenders whose records are either searchable in both the pre-period and the post-period (for example, those with convictions for manslaughter, murder, and/or sexual offenses), or else whose records are unsearchable in both the pre-period and the post-period (for example, those who were juveniles when their convictions were incurred).

5.2 Addressing Unbalanced Covariates

Again, our goal is to estimate the effect of the record-access reform on the labor market outcomes of ex-offenders. Thus, we employ the same difference-in-differences estimation approach with the inverse-probability-of-treatment weights that we used when examining the impact of the ban the box component of the CORI Reform.

As with our ban the box analysis, we begin by assessing and then attempting to address any covariate imbalances in order to ensure the validity of our identification assumptions for the difference-in-differences estimation. Figure 7 displays the raw employment trends in percentage points for our treatment and control groups. While in terms of levels, average employment is nearly twice as high for the treatment group as for the control group, unlike in our ban the box sample, we observe no evidence of disparate pre-period trends across the groups. The results shown in Figure 8 further confirm this lack of disparity between the treatment and control groups, as the age distributions in 2010:Q1 look fairly similar for both groups. When we examine average quarterly earnings in addition to average employment, once again we observe no difference in pre-period trends across the treatment and control groups.

Nevertheless, our analytical approach proceeds in a manner similar to the methodology we used for the ban the box analysis. In order to address any covariate imbalances that do exist, we once again take two approaches: 1) using inverse-probability weighted regressions, and 2) running unweighted regressions on data stratified by either propensity scores or age in 2010:Q1. Similar to what we observe in Figures 7 and 8, when we examine the descriptive statistics for several variables, the treatment and control groups are more similar in the record-access sample than in the ban the box sample (albeit not identical). Perhaps this similarity exists because everyone in the record-access analysis is an ex-offender at the time of observation, or perhaps because the period under analysis occurs after the ban the box reform was implemented.

Figure 9 shows that, upon imposing the common support criteria and re-weighting the treatment and control group observations using inverse probability weights, the age distributions in 2010:Q1 for the two groups now appear more similar than in Figure 8. Likewise, when we examine the descriptive statistics for numerous variables, we find that the covariates are more balanced across the inverse-probability-weighted treatment and control groups compared to the unweighted covariates. Lastly, by examining Figure 10, we observe that the employment levels of the treatment and control groups are more similar after weighting, and the similarity remains in the pre-period trends.[31] The same similarity holds when assessing the average quarterly earnings trends (shown in thousands of 2015 U.S. dollars) in Figure 11. Having addressed these potential covariate imbalance issues, we proceed to the estimation.

[31] In this case, employment for the treatment group still lies above employment for the control group because the treatment group contains individuals with non-convictions and non-incarcerable offenses whose records are not searchable in the post-period and whose employment rates tend to be high. Meanwhile, the control group contains individuals with manslaughter, murder, and/or sexual offenses whose records are always searchable in the post-period and whose employment rates tend to be low.

5.3 Main Effects on Ex-Offenders' Employment and Earnings

We now discuss our main estimation results regarding the employment and earnings effects of the record-access component of the CORI Reform, starting first with the estimation of employment effects using inverse-probability-weighted regressions. Table 7 shows that across specifications, with or without controls or standard errors clustered at the individual level, our results remain quite stable. The record-access reform, like the ban the box reform, lowers employment among affected ex-offenders, albeit by a smaller magnitude of 0.43 to 0.46 percentage points, compared to the ban the box effect of 2.36 to 2.57 percentage points. Additionally, we can examine these employment effects across a more disaggregated treatment group that distinguishes the reason why an individual's record is unsearchable following the enactment of the record-access reform. Using this disaggregated approach, we observe a larger average employment decline of 0.81 percentage points for ex-offenders treated due to the timing of their offense, and a smaller average employment decline of 0.33 percentage points for ex-offenders treated due to the type of offense committed.[32]

When, alternatively, we employ unweighted regressions and stratify by propensity score quintiles or age quintiles to examine employment effects as in Table 8, we obtain qualitatively similar results. The average employment reduction across propensity score quintiles is approximately 0.41 percentage points and across age quintiles is 0.43 percentage points, similar in both cases to our weighted regression results. However, the stratification also reveals some underlying heterogeneity in the estimates. The employment effect is largest and significant only for the highest propensity score quintile and the two highest age quintiles. Thus, older ex-offenders and

[32] Likewise, we observe broadly similar results when utilizing the subset of the control group whose records are always searchable before and after the reform, the subset whose records are always unsearchable before and after the reform, or when controlling for additional characteristics related to ex-offenders' criminal histories such as distinguishing between felony and misdemeanor convictions.

ex-offenders who, based on observables, are the most likely to have records that become unsearchable due to the reform, appear to experience somewhat larger negative employment effects.[33]

Examining the earnings effects shown in Table 9, we once again see that across the various specifications, with or without controls or standard errors clustered at the individual level, our results are very stable. However, unlike the ban the box reform, the record-access reform actually increases average quarterly earnings, albeit by a very small amount, 90 dollars per quarter, or 360 dollars per year. Given the average pre-period earnings of the control group in our sample, this roughly corresponds to a 3 percent increase in annual earnings for ex-offenders.

5.4 Effects on Ex-Offenders' Employment in Specific Industries and Dynamic Effects

Delving further into the record-access reform's effects on the employment outcomes of ex-offenders, we can disaggregate effects across three-digit NAICS industries. Upon doing so, we observe that there is some heterogeneity across sectors in the employment effects, similar to what we observe when analyzing the ban the box reform. Table 10 shows that after the record-access reform was implemented, food services and drinking places experienced the largest reduction in the employment of affected ex-offenders, perhaps due to a labor supply-side effect and the fact that this sector is among the lowest-paying industries in terms of average weekly wages. Meanwhile, in some other industries—such as government support, ambulatory health care services, and transit and ground passenger transportation—the employment of ex-offenders increased. If the employment effects are indeed affected by supply-side responses in the labor

[33] If the employment effect is largely driven by a supply-side response from ex-offenders (discussed in section 5.5), perhaps this observed pattern could be due to differences in ex-offenders' knowledge regarding their criminal records becoming unsearchable due to the reform. Additionally, because we constrained our sample to those individuals who, based on limited observables, seemed least likely to be incarcerated during our 2010–2015 estimation period, we do not suspect that incarceration is playing a substantial role, if any, in generating the heterogeneity across quintiles in employment effects.

market, then perhaps these could be industries that ex-offenders transitioned to given a perceived expansion in employment opportunities following the record-access component of the CORI Reform.

We can also examine dynamics and how the employment and earnings effects evolve over time following the implementation of the record-access reform. Table 11 displays the coefficients for both outcomes in each of the fourteen post-period year-quarters from 2012:Q2 to 2015:Q3. Once again, this disaggregation reveals some heterogeneity over time. Initially, the employment effects are significantly positive in the first year-quarter that the reform was in effect, but then become increasingly negative and significant about one year after the reform, a result that is similar to the one-year effect following the ban the box reform. This could be consistent with the slow spread of information about the record-access reform and a subsequent delayed supply-side response on the part of ex-offenders affected by the reform. Earnings effects, on the other hand, are a bit more mixed. These effects start with an average earnings reduction in the first year-quarter of the reform, but then subsequently bounce around in sign and magnitude. However, when these earnings effects are significant in later year-quarters, they are always positive.[34]

5.5 Why Does the Record-Access Reform Have a Negative Effect on Ex-Offenders' Employment Outcomes?

As explored in our analysis of the ban the box reform, given the negative employment results we observe again for the record-access reform, we want to assess whether the results for this reform are most likely stemming from a labor supply response or a labor demand response.

[34] We also explore whether there is any heterogeneity of the effects from record-access reform on employment and earnings based on differences in cell-level characteristics (for example, demographics, super-county unemployment rates, etc.). However, we are unable to detect any consistent patterns, similar to our results analyzing such heterogeneous effects from the ban the box reform.

However, unlike the ban the box reform, a labor demand response driving our observed results for the record-access reform seems, a priori, less plausible.

Recall that for a labor demand response to be the primary mechanism responsible for our results, it would need to be the case that employers respond to the record-access reform by changing their screening practices in order to continue to eliminate ex-offenders whose criminal records became unsearchable due to the reform. However, if employers had a strong enough desire to eliminate such ex-offenders, they could simply continue to rely on criminal history information obtained from private consumer reporting agencies in order to identify ex-offenders, rather than switch to using information from the state's CORI repository.[35] Moreover, even if some employers did decide to switch to using the state repository but also decided to alter their screening practices in order to eliminate ex-offenders whose records became unsearchable due to CORI Reform, it is unclear what screening tools employers could use in order to filter out such ex-offenders disproportionately more than those ex-offenders whose record access did not change due to the reform. As discussed earlier, the treatment and control groups for the record-access reform appear very similar based on observable characteristics. Thus, it would seem fairly challenging for employers to find the necessary observable trait(s) to successfully screen in this manner, although we acknowledge that such traits may exist and be observable to employers but not observable in our data. Therefore, these considerations suggest that a labor supply response, rather than or in addition to a labor demand response, is likely to be contributing to our observed findings regarding the record-access reform. This conclusion is further supported by the suggestive evidence of several tests similar to those we used to explore the likely mechanism for our ban the box results, as well as our findings in Jackson and Zhao (2016) that the record-access

[35] Such desire by employers would need to be sufficiently strong in order to overcome the economic and legal incentives to switch to using information from the CORI database, as discussed in section 2.

component of the CORI Reform generally appears, like the ban the box component, to have led to a small decline in recidivism.

6. Conclusion

Using a difference-in-differences model and a unique dataset linking individuals' criminal histories with their wage records, this paper investigates the impact of the ground-breaking Massachusetts CORI Reform on ex-offenders' labor market outcomes. We find that contrary to the intended goal, the reform has a small negative effect on ex-offenders' employment outcomes. On average, the ban the box and record-access reforms lower ex-offenders' employment by 2.4 and 0.4 percentage points, respectively. The negative employment effect is not constant, but instead grows gradually over time. It is also not evenly distributed across industries. Those sectors that hired a larger number of ex-offenders before the reform, such as food and drinking places and administrative and support services, experience larger declines in ex-offender employment once the reform is in place.

We find mixed evidence regarding the CORI Reform's impact on ex-offenders' earnings. On the one hand, ban the box lowers ex-offenders' quarterly earnings on average by about $300. On the other hand, the record-access reform increases ex-offenders' quarterly earnings on average by just $90. However, our earnings data have limitations. Because of the required confidentiality constraints, the earnings are averaged over both employed and unemployed individuals within a given cell. As a result, we cannot directly compare the same employed individuals over time. We also have no data on hourly wages because the unemployment insurance system in Massachusetts does not collect information on work hours. Therefore, our ability to make a definitive conclusion on the earnings effect of the CORI Reform is limited.

In theory, either a labor demand response or a labor supply response to the policy changes could help to explain the CORI Reform's negative effect on ex-offenders' employment outcomes. We find some suggestive evidence that the effect is more likely to result from the labor supply response, with ex-offenders seeking better working conditions and/or raising their wage expectations after the reform. However, given the limitations of our data, we cannot rule out the possibility that the labor demand response may also contribute to the CORI Reform's negative effect on ex-offenders' employment.

Future research can take several directions, depending upon data availability. First, if individual-level labor data, including individual hourly wages, become available, we would be able to isolate the CORI Reform's effect on ex-offenders' earnings from the employment effect. The individual-level data could also enable us to develop more powerful tests to examine whether the labor demand response or the labor supply response is the main driver behind the negative employment effect. Second, we wish to add data from other time periods to examine whether effects of the CORI Reform are sensitive to business cycle conditions and whether the CORI Reform can produce a positive effect on ex-offenders' labor market outcomes in the long run. Third, it would be desirable to conduct a similar study using other states' data and to see whether similar patterns emerge. Above all, new research is needed to understand the consequences of the criminal justice reform across the country and ideally guide the reform moving forward.

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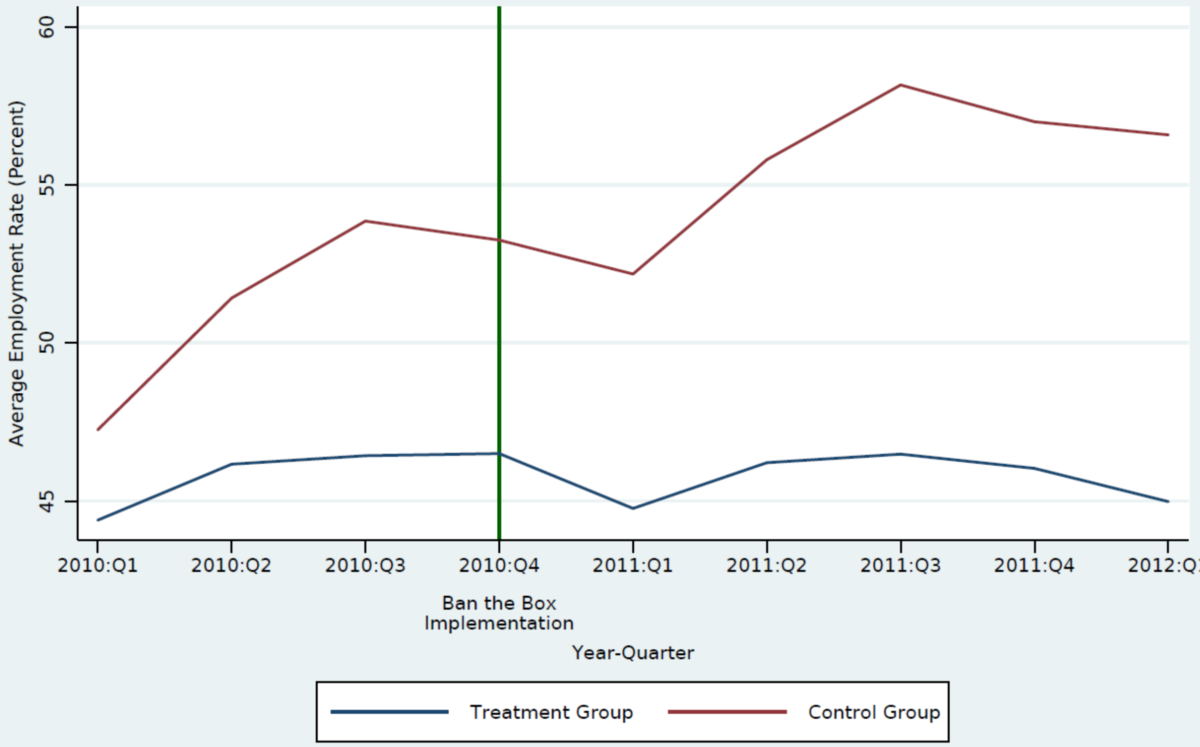
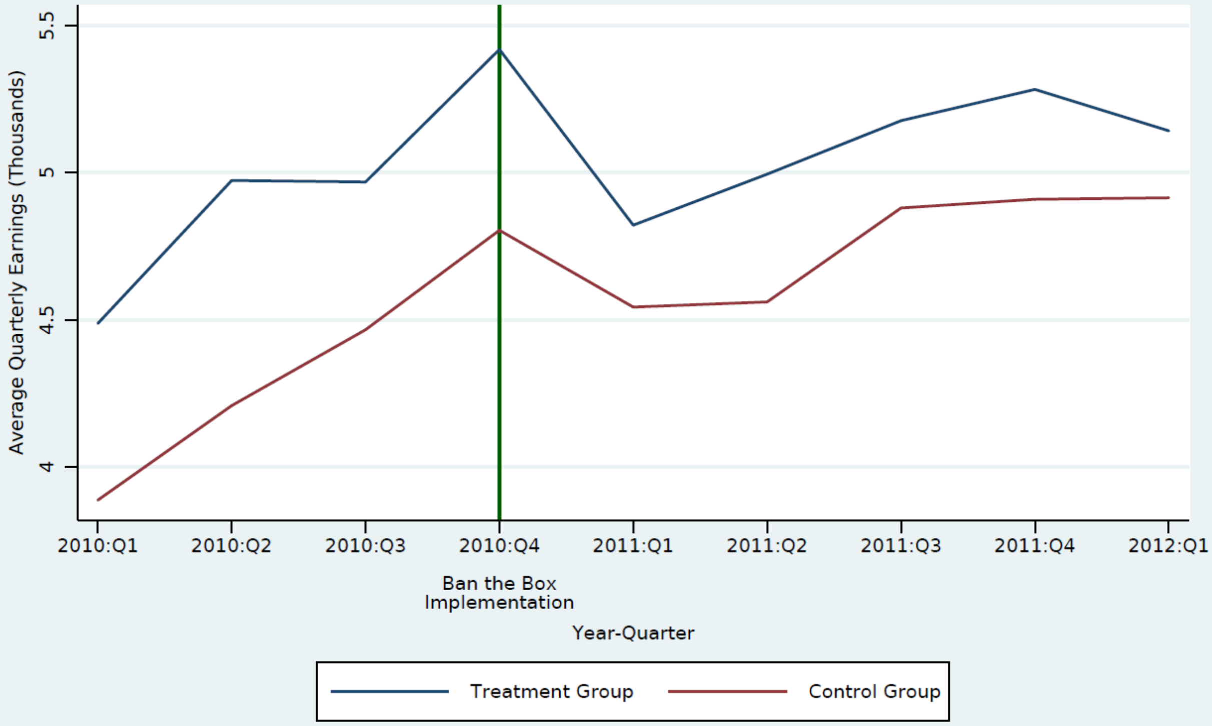
Figure 1. Unweighted Average Employment Rate
Before and After Implementing the Ban the Box Reform



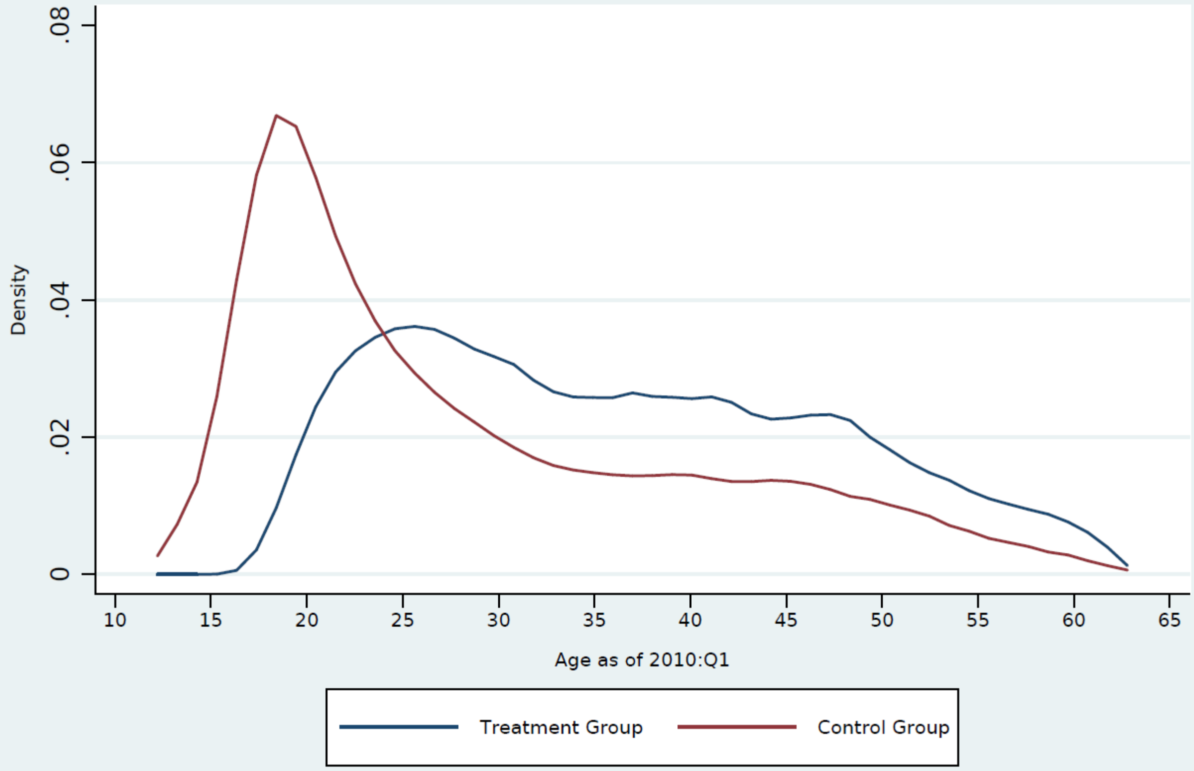
Source: Authors' calculations.

Figure 2. Unweighted Average Quarterly Earnings
Before and After Implementing the Ban the Box Reform



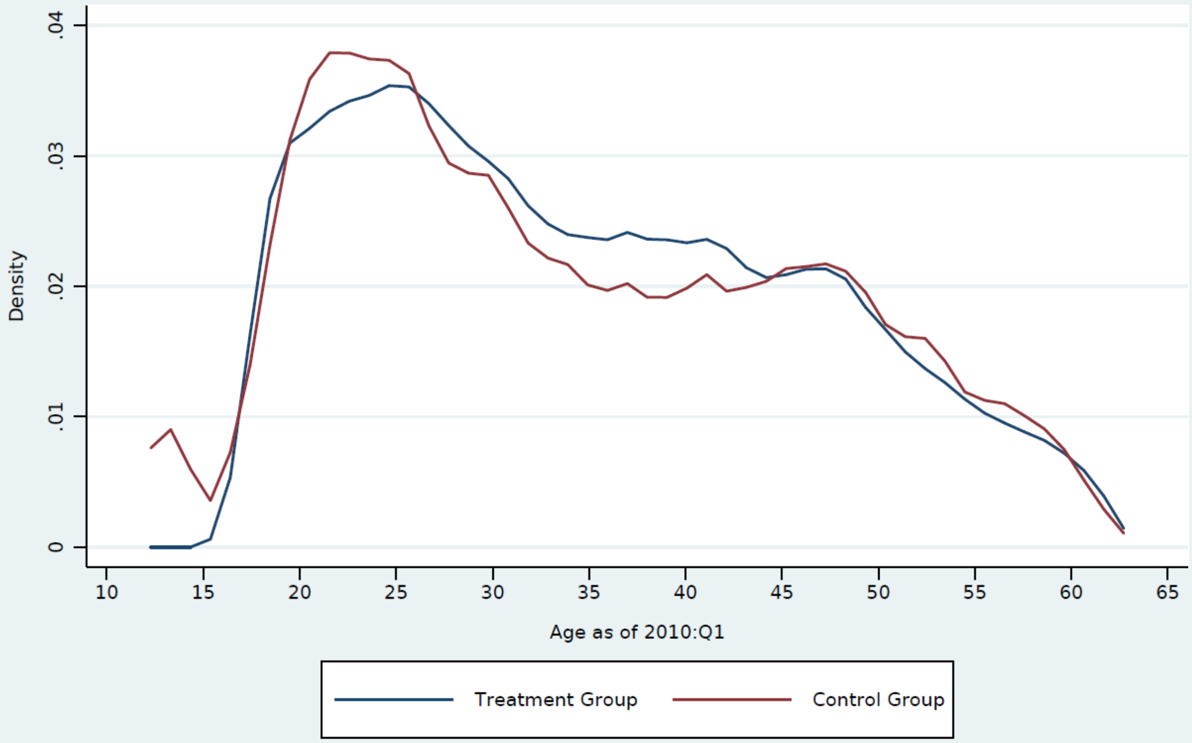
Source: Authors' calculations.
Note: Earnings are in 2015 dollars.

Figure 3. Unweighted Age Distribution for the Ban the Box Sample



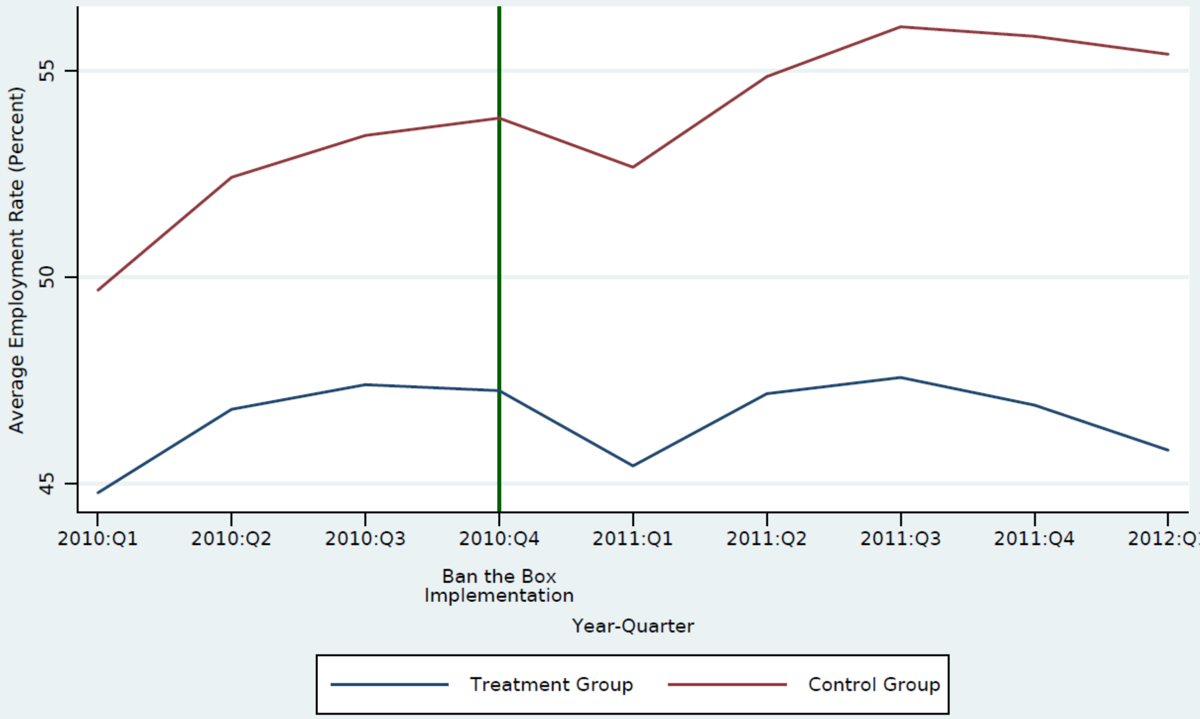
Source: Authors' calculations.

Figure 4. Weighted Age Distribution for the Ban the Box Sample



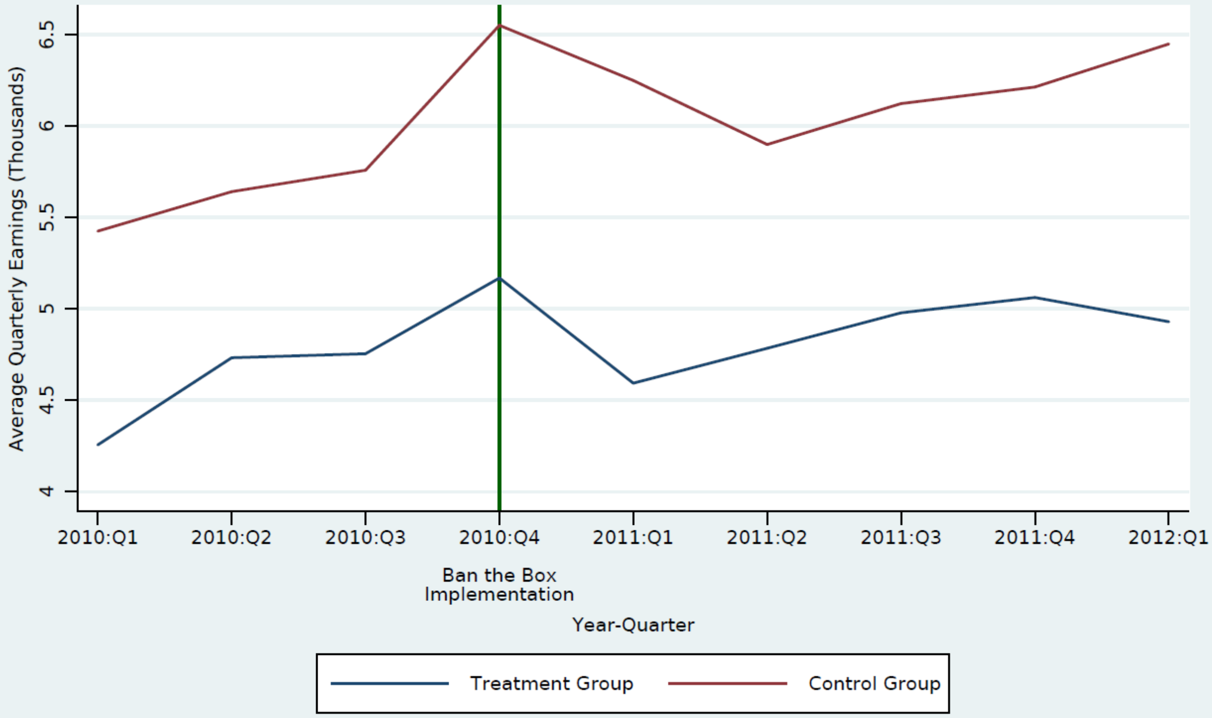
Source: Authors' calculations.
Note: We use the inverse-probability weights.

Figure 5. Weighted Average Employment Rate
Before and After Implementing the Ban the Box Reform



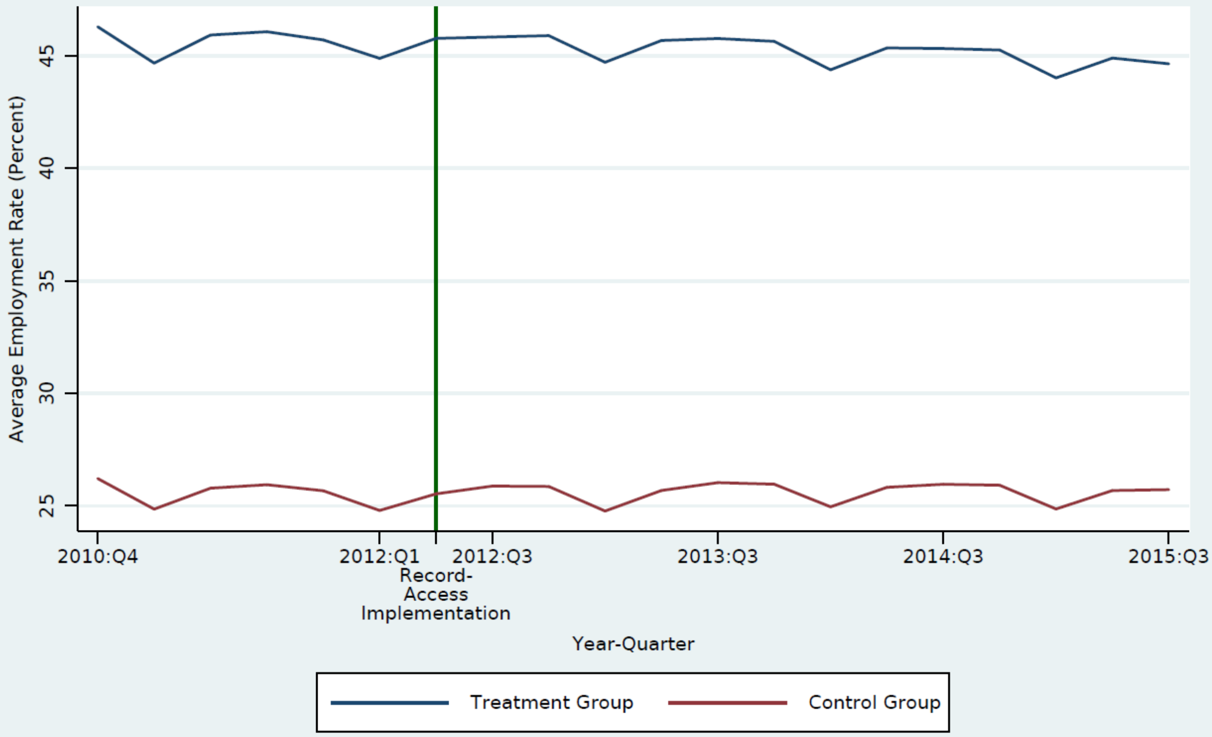
Source: Authors' calculations.
Note: We use the inverse-probability weights.

Figure 6. Weighted Average Quarterly Earnings
Before and After Implementing the Ban the Box Reform



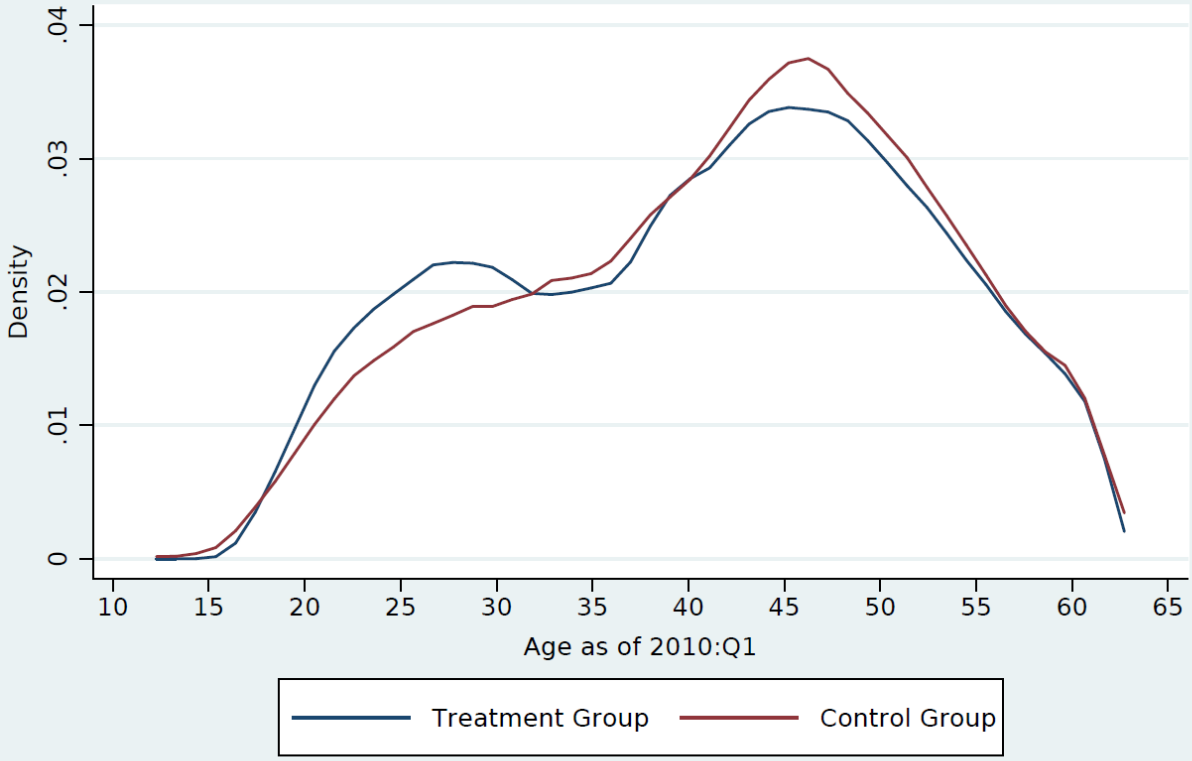
Source: Authors' calculations.
Note: Earnings are in 2015 dollars. We use the inverse-probability weights.

Figure 7. Unweighted Average Employment Rate
Before and After Implementing the Record-Access Reform



Source: Authors' calculations.

Figure 8. Unweighted Age Distribution for the Record-Access Sample



Source: Authors' calculations.

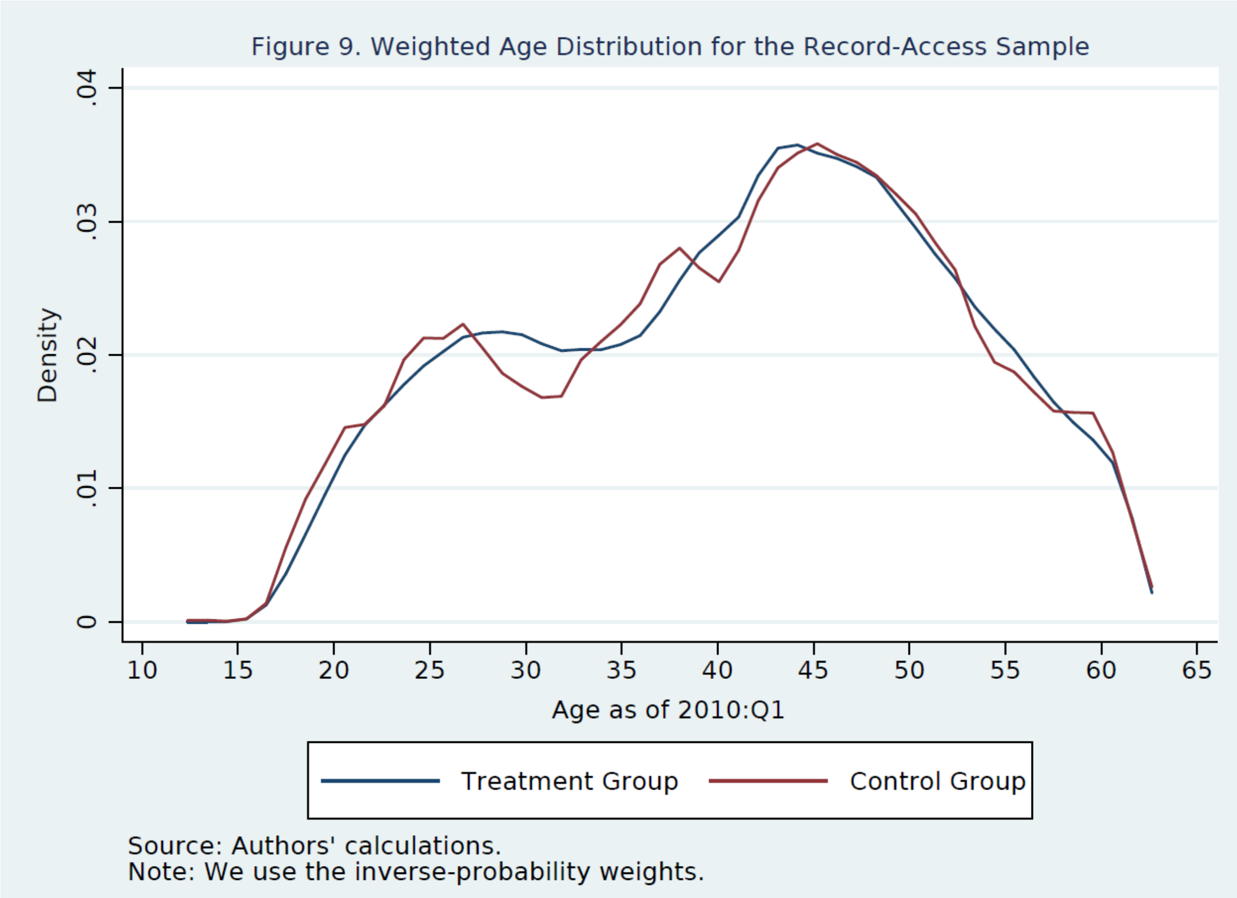
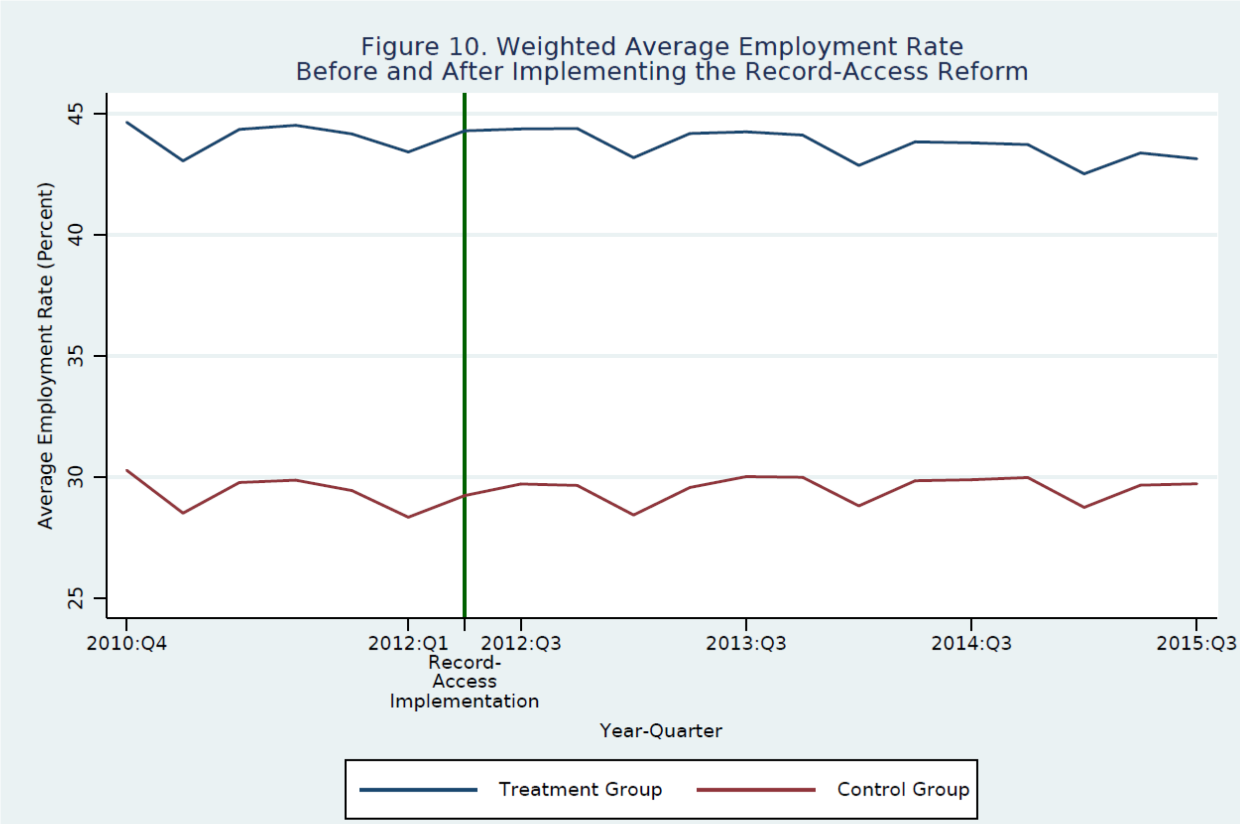
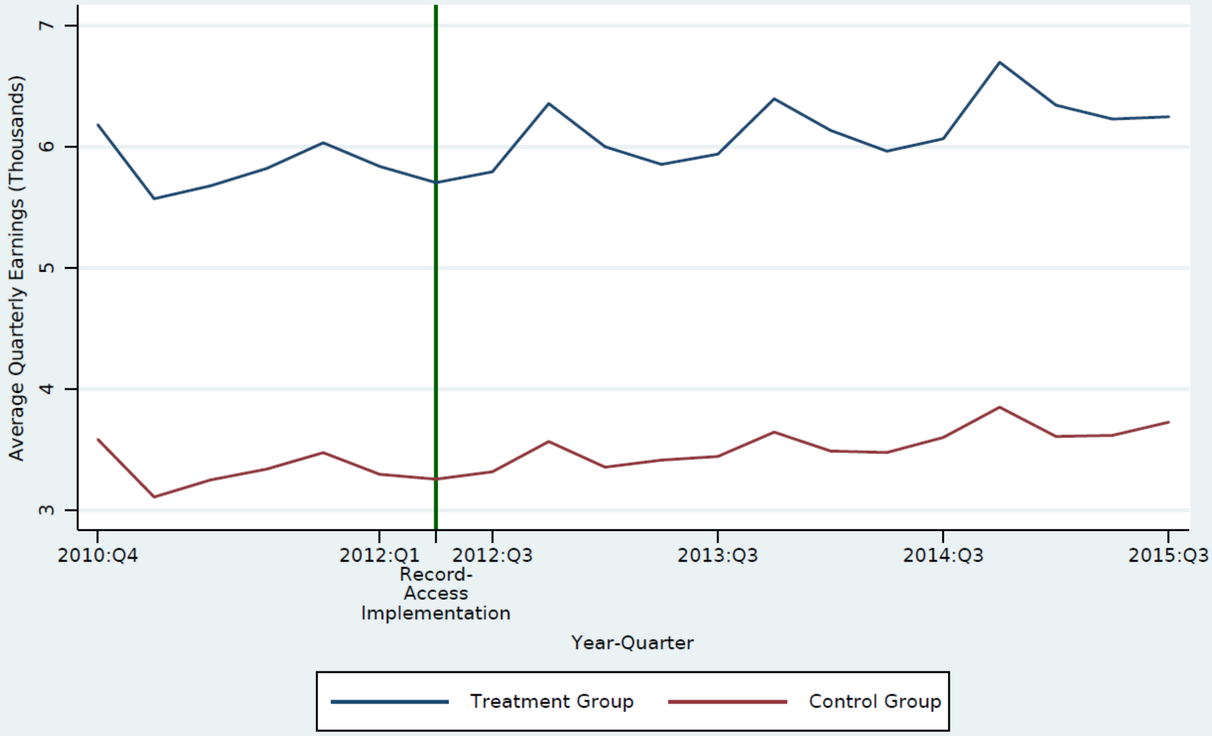


Figure 9. Weighted Age Distribution for the Record-Access Sample
Density
0
.01
.02
.03
.04
10
15
20
25
30
35
40
45
50
55
60
65
Age as of 2010:Q1
Treatment Group
Control Group
Source: Authors' calculations.
Note: We use the inverse-probability weights.



Source: Authors' calculations.
Note: We use the inverse-probability weights.

Figure 11. Weighted Average Quarterly Earnings
Before and After Implementing the Record-Access Reform



Source: Authors' calculations.
Note: Earnings are in thousands of 2015 dollars. We use the inverse-probability weights.

Table 1. Effects of the Ban the Box Reform on Ex-Offenders' Employment
Difference-in-Differences Estimates, Using Inverse-Probability Weighted Regressions

	Dependent Variable = Average Employment Rate (Percent)		
	(1)	(2)	(3)
Post	2.935***	1.682***	1.682***
	(0.110)	(0.119)	(0.215)
Treat	-5.522***	-7.080***	-7.080***
	(0.127)	(0.102)	(0.340)
Post x Treat	-2.569***	-2.362***	-2.362***
	(0.156)	(0.124)	(0.221)
Percentage of Individuals who are Female		7.194***	7.194***
		(0.314)	(1.588)
Percentage of Individuals who are Black		0.398	0.398
		(0.407)	(2.245)
Percentage of Individuals who are Hispanic		-16.15***	-16.15***
		(0.417)	(2.494)
Percentage of Individuals who are Asian		-5.467***	-5.467
		(0.795)	(4.123)
Percentage of Individuals who are Native American		-52.13***	-52.13**
		(4.622)	(21.23)
Quarterly County/Super-County Unemployment Rate (Percent)		-1.219***	-1.219***
		(0.0604)	(0.0818)
Quarter 2 Dummy		0.771***	0.771***
		(0.0908)	(0.0945)
Quarter 3 Dummy		1.365***	1.365***
		(0.0969)	(0.114)
Quarter 4 Dummy		0.387***	0.387***
		(0.0951)	(0.104)
Constant	51.85***	12.64***	12.64***
	(0.0898)	(0.921)	(1.432)
Age Dummies	No	Yes	Yes
County/Super-County Dummies	No	Yes	Yes
Clustered Standard Errors	No	No	Yes
Observations	148,662	148,662	148,662
R-Squared	0.0654	0.409	0.409

Note: Standard errors are in parenthesis. In the third column, standard errors are clustered at the cell level.
* p<0.10, ** p<0.050, *** p<0.010

Table 2. Effects of the Ban the Box Reform on Ex-Offenders' Quarterly Earnings
Difference-in-Differences Estimates, Using Inverse-Probability Weighted Regressions

	Dependent Variable = Average Quarterly Earnings (Thousands)		
	(1)	(2)	(3)
Post	0.639***	0.213***	0.213**
	(0.0449)	(0.0570)	(0.0897)
Treat	-1.027***	-1.278***	-1.278***
	(0.0520)	(0.0487)	(0.109)
Post x Treat	-0.301***	-0.302***	-0.302***
	(0.0637)	(0.0594)	(0.0783)
Percentage of Individuals who are Female		-0.612***	-0.612
		(0.150)	(0.544)
Percentage of Individuals who are Black		-0.366*	-0.366
		(0.194)	(0.810)
Percentage of Individuals who are Hispanic		-2.535***	-2.535***
		(0.199)	(0.681)
Percentage of Individuals who are Asian		-2.143***	-2.143
		(0.380)	(1.646)
Percentage of Individuals who are Native American		-14.58***	-14.58***
		(2.208)	(5.006)
Quarterly County/Super-County Unemployment Rate (Percent)		-0.134***	-0.134**
		(0.0289)	(0.0545)
Quarter 2 Dummy		-0.0711	-0.0711
		(0.0434)	(0.0952)
Quarter 3 Dummy		0.00284	0.00284
		(0.0463)	(0.101)
Quarter 4 Dummy		0.209***	0.209
		(0.0454)	(0.134)
Constant	5.609***	1.934***	1.934***
	(0.0367)	(0.440)	(0.643)
Age Dummies	No	Yes	Yes
County/Super-County Dummies	No	Yes	Yes
Clustered Standard Errors	No	No	Yes
Observations	148,662	148,662	148,662
R-Squared	0.0128	0.145	0.145

Note: Standard errors are in parentheses. In the third column, standard errors are clustered at the cell level. Earnings are in 2015 dollars.
* p<0.10, ** p<0.050, *** p<0.010

Table 3. Effects of the Ban the Box Reform on Ex-Offenders' Employment, by Quintiles
Estimated Post x Treat from Unweighted Difference-in-Differences Regressions

	Stratified by Propensity Score	Stratified by Age as of 2010:Q1
Quintile 1	-1.872***	-2.053***
	(0.276)	(0.300)
Quintile 2	-2.817***	-2.962***
	(0.358)	(0.415)
Quintile 3	-2.645***	-2.334***
	(0.512)	(0.408)
Quintile 4	-1.815***	-1.660***
	(0.547)	(0.388)
Quintile 5	-1.863***	-0.353
	(0.720)	(0.413)

Note: Standard errors are in parenthesis and are clustered at the cell level. Regressions include all control variables in the third column in Table 1.
*p<0.10, **p<0.050, ***p<0.010

Table 4. Effects of the Ban the Box Reform on Ex-Offenders' Quarterly Earnings, by Quintiles
Estimated Post x Treat from Unweighted Difference-in-Differences Regressions

	Stratified by Propsensity Score	Stratified by Age as of 2010:Q1
Quintile 1	-0.0633*	-0.0820***
	(0.0325)	(0.0204)
Quintile 2	-0.500**	-0.209***
	(0.231)	(0.0462)
Quintile 3	-0.220*	-0.205***
	(0.133)	(0.0702)
Quintile 4	-0.327*	-0.275
	(0.168)	(0.219)
Quintile 5	-0.355	-0.266
	(0.229)	(0.274)

Note: Standard errors are in parenthesis and are clustered at the cell level. Regressions include all control variables in the third column in Table 2. Earnings are in thousands of 2015 dollars.
*p<0.10, **p<0.050, ***p<0.010

Table 5. Dynamic Effects of the Ban the Box Reform on Ex-Offenders' Employment and Quarterly Earnings

	Dependent Variables	
	Average Employment Rate (Percent)	Average Quarterly Earnings (Thousands)
Post Period 1 x Treat	-1.005***	-0.365
	(0.211)	(0.252)
Post Period 2 x Treat	-1.589***	-0.635***
	(0.245)	(0.204)
Post Period 3 x Treat	-1.989***	-0.0902
	(0.278)	(0.0740)
Post Period 4 x Treat	-2.747***	-0.114
	(0.299)	(0.0870)
Post Period 5 x Treat	-3.115***	-0.115
	(0.302)	(0.0979)
Post Period 6 x Treat	-3.751***	-0.493***
	(0.318)	(0.152)
Observations	148,662	148,662
R-Squared	0.410	0.146
P-Value for Joint Significance Test	0.000	0.007
P-Value for Equal Coefficients Test	0.000	0.103

Note: Standard errors are in parenthesis and are clustered at the cell level. Regressions include all control variables in the third column in Tables 1 and 2.
Earnings are in 2015 dollars.
* p<0.10, ** p<0.050, *** p<0.010

Table 6. Effects of the Ban the Box Reform on Ex-Offenders' Employment by Industry
Using Weighted Difference-in-Differences Regressions

	Post x Treat
561 Administrative and Support Services	-0.661***
	(0.101)
722 Food Services and Drinking Places	-0.358***
	(0.103)
621 Ambulatory Health Care Services	-0.173***
	(0.0624)
448 Clothing and Clothing Accessories Stores	-0.153***
	(0.0430)
721 Accomodation	-0.105***
	(0.0390)
814 Private Households	-0.0787*
	(0.0402)
311 Food Manufacturing	-0.0644*
	(0.0336)
517 Telecommunications	-0.0596**
	(0.0240)
454 Nonstore Retailers	-0.0494***
	(0.0190)
562 Waste Management and Remediation Services	-0.0338*
	(0.0183)
488 Support Activities for Transportation	-0.0336**
	(0.0136)
487 Scenic and Sightseeing Transportation	-0.0187*
	(0.00980)
316 Leather and Allied Product Manufacturing	-0.0164**
	(0.00704)
313 Textile Mills	-0.0152**
	(0.00763)
112 Animal Production	-0.00921*
	(0.00539)
923 Administration of Human Resource Programs	0.00119*
	(0.000638)
221 Utilities	0.0192*
	(0.00987)
322 Paper Manufacturing	0.0274*
	(0.0156)

Note: The dependent variable in each regression is the percentage of individuals in each cell employed in each industry. Each regression includes all control variables in the third column of Table 1. This table shows only the significant coefficients on Post x Treat from the regressions. Standard errors are in parenthesis and are clustered at the cell level.

Table 7. Effects of the Record-Access Reform on Ex-Offenders' Employment
Difference-in-Differences Estimates, Using Inverse-Probability Weighted Regressions

	Dependent Variable = Average Employment Rate (Percent)		
	(1)	(2)	(3)
Post	0.149**	0.594***	0.594***
	(0.070)	(0.041)	(0.137)
Treat	14.653***	3.453***	3.453***
	(0.082)	(0.045)	(0.174)
Post x Treat	-0.458***	-0.435***	-0.435***
	(0.098)	(0.050)	(0.150)
Percentage of Individuals who are Female		1.287***	1.287**
		(0.104)	(0.625)
Percentage of Individuals who are Black		-0.408**	-0.408
		(0.172)	(1.127)
Percentage of Individuals who are Hispanic		-6.214***	-6.214***
		(0.142)	(0.951)
Percentage of Individuals who are Asian		-7.349***	-7.349***
		(0.318)	(2.269)
Percentage of Individuals who are Native American		-7.817***	-7.817
		(1.704)	(8.031)
Quarterly County/Super-County Unemployment Rate (Percent)		-0.239***	-0.239***
		(0.015)	(0.035)
Pre-period Average Employment Rate (Percent)		73.194***	73.194***
		(0.128)	(0.889)
Pre-period Average Quarterly Earnings (Thousands)		0.000***	0.000***
		(0.000)	(0.000)
Quarter 2 Dummy		0.849***	0.849***
		(0.034)	(0.040)
Quarter 3 Dummy		1.069***	1.069***
		(0.034)	(0.041)
Quarter 4 Dummy		1.012***	1.012***
		(0.034)	(0.043)
Constant	29.360***	-3.600***	-3.600***
	(0.059)	(0.322)	(0.851)
Age Dummies	No	Yes	Yes
County/Super-County Dummies	No	Yes	Yes
Clustered Standard Errors	No	No	Yes
Observations	463,620	463,620	463,620
R-Squared	0.181	0.786	0.786

Note: Standard errors are in parenthesis. In the third column, standard errors are clustered at the cell level.
* p<0.10, ** p<0.050, *** p<0.010

Table 8. Effects of the Record-Access Reform on Ex-Offenders' Employment, by Quintiles
Estimated Post x Treat from Unweighted Difference-in-Differences Regressions

	Stratified by Propensity Score	Stratified by Age as of 2010:Q1
Quintile 1	0.165	-0.226
	(0.161)	(0.329)
Quintile 2	-0.309	-0.249
	(0.227)	(0.207)
Quintile 3	-0.243	0.156
	(0.317)	(0.204)
Quintile 4	-0.600	-0.305*
	(0.406)	(0.178)
Quintile 5	-1.179**	-1.416***
	(0.473)	(0.177)

Note: Standard errors are in parenthesis and are clustered at the cell level. Regressions include all control variables in the third column in Table 7.
*p<0.10, **p<0.050, ***p<0.010

Table 9. Effects of the Record-Access Reform on Ex-Offenders' Quarterly Earnings
Difference-in-Differences Estimates, Using Inverse-Probability Weighted Regressions

	Dependent Variable = Average Quarterly Earnings (Thousands)		
	(1)	(2)	(3)
Post	0.184***	0.082***	0.082***
	(0.024)	(0.023)	(0.028)
Treat	2.511***	0.351***	0.351***
	(0.028)	(0.025)	(0.050)
Post x Treat	0.086***	0.090***	0.090***
	(0.033)	(0.028)	(0.033)
Percentage of Individuals who are Female		0.169***	0.169
		(0.058)	(0.142)
Percentage of Individuals who are Black		-0.984***	-0.984***
		(0.097)	(0.244)
Percentage of Individuals who are Hispanic		-0.484***	-0.484
		(0.080)	(0.307)
Percentage of Individuals who are Asian		-0.886***	-0.886**
		(0.179)	(0.439)
Percentage of Individuals who are Native American		-2.170**	-2.170
		(0.959)	(1.649)
Quarterly County/Super-County Unemployment Rate (Percent)		-0.180***	-0.180***
		(0.008)	(0.011)
Pre-period Average Employment Rate (Percent)		0.996***	0.996
		(0.072)	(0.825)
Pre-period Average Quarterly Earnings (Thousands)		0.001***	0.001***
		(0.000)	(0.000)
Quarter 2 Dummy		-0.191***	-0.191***
		(0.019)	(0.043)
Quarter 3 Dummy		-0.087***	-0.087**
		(0.019)	(0.042)
Quarter 4 Dummy		0.187***	0.187***
		(0.019)	(0.046)
Constant	3.340***	-0.055	-0.055
	(0.020)	(0.181)	(0.243)
Age Dummies	No	Yes	Yes
County/Super-County Dummies	No	Yes	Yes
Clustered Standard Errors	No	No	Yes
Observations	463,620	463,620	463,620
R-Squared	0.058	0.325	0.325

Note: Standard errors are in parentheses. In the third column, standard errors are clustered at the cell level.
* p<0.10, ** p<0.050, *** p<0.010

Table 10. Effects of the Record-Access Reform on Ex-Offenders' Employment by Industry
Using Weighted Difference-in-Differences Regressions

	Post x Treat
722 Food Services and Drinking Places	-0.256***
	(0.066)
445 Food and Beverage Stores	-0.077**
	(0.037)
624 Social Assistance	-0.060*
	(0.036)
813 Religious, Grantmaking, Civic, Professional, and Similar Organizations	-0.040*
	(0.023)
323 Printing and Related Support Activities	-0.037***
	(0.014)
721 Accommodation	-0.034*
	(0.018)
532 Rental and Leasing Services	-0.032*
	(0.018)
492 Couriers and Messengers	-0.027*
	(0.016)
515 Broadcasting (except Internet)	-0.014***
	(0.005)
312 Beverage and Tobacco Product Manufacturing	-0.008*
	(0.004)
113 Forestry and Logging	0.002***
	(0.001)
811 Repair and Maintenance	0.051*
	(0.028)
621 Ambulatory Health Care Services	0.082**
	(0.039)
485 Transit and Ground Passenger Transportation	0.082***
	(0.025)
921 Executive, Legislative, and Other General Government Support	0.321***
	(0.043)

Note: The dependent variable in each regression is the percentage of individuals in each cell employed in each industry. Each regression includes all control variables in the third column of Table 7. This table shows only the significant coefficients on Post x Treat from the regressions. Standard errors are in parenthesis and are clustered at the cell level.
* p<0.10, ** p<0.050, *** p<0.010

Table 11. Dynamic Effects of the Record-Access Reform on Ex-Offenders' Employment and Quarterly Earnings

	Dependent Variables	
	Average Employment Rate (Percent)	Average Quarterly Earnings (Thousands)
Post Period 1 x Treat	0.422***	-0.061*
	(0.154)	(0.036)
Post Period 2 x Treat	0.021	-0.032
	(0.171)	(0.043)
Post Period 3 x Treat	0.106	0.282***
	(0.163)	(0.057)
Post Period 4 x Treat	0.126	0.138*
	(0.178)	(0.075)
Post Period 5 x Treat	-0.013	-0.067
	(0.180)	(0.047)
Post Period 6 x Treat	-0.406**	-0.013
	(0.185)	(0.044)
Post Period 7 x Treat	-0.504***	0.244***
	(0.192)	(0.049)
Post Period 8 x Treat	-0.577***	0.140
	(0.194)	(0.096)
Post Period 9 x Treat	-0.637***	-0.019
	(0.204)	(0.048)
Post Period 10 x Treat	-0.733***	-0.041
	(0.206)	(0.055)
Post Period 11 x Treat	-0.885***	0.342***
	(0.212)	(0.060)
Post Period 12 x Treat	-0.866***	0.231**
	(0.210)	(0.105)
Post Period 13 x Treat	-0.918***	0.107**
	(0.219)	(0.053)
Post Period 14 x Treat	-1.228***	0.016
	(0.227)	(0.055)
Observations	463,620	463,620
R-Squared	0.786	0.326
P-Value for Joint Significance Test	0.000	0.000
P-Value for Equal Coefficients Test	0.000	0.000

Note: Standard errors are in parenthesis and are clustered at the cell level. Regressions include all control variables in the third column in Tables 7 and 9.
Earnings are in 2015 dollars.
* p<0.10, ** p<0.050, *** p<0.010

Appendix A: Creating the Inverse-Probability-of-Treatment Weights

Three steps are needed to generate the inverse-probability-of-treatment weights. First, using a logistic regression, we estimate the treatment group assignment status of each cell as a function of its observable characteristics including age, gender, race and ethnicity, as well as geographic location. As expected, the cells populated with younger individuals are more likely to be in the control group than be in the treatment group. Second, we predict the conditional probability of a cell being in the treatment group—that is, the propensity score—based on the logistic regression results. Third, we give each cell in the treatment group a weight equal to 1(its propensity score), and give each cell in the control group a weight equal to 1/(1 – its propensity score). If a cell in the treatment group has similar observable characteristics as typical cells in the control group, the cell in the treatment group would likely receive a lower propensity score and thereby a higher inverse-probability weight. Similarly, if a cell in the control group has similar observable characteristics as typical cells in the treatment group, the cell in the control group would likely receive a higher propensity score and thereby a higher inverse-probability weight.

When the propensity score is extremely large and very close to one (1) for some control group cells or is extremely small and very close to zero (0) for some treatment group cells, the inverse-probability weight would become unusually large, causing unstable and unreliable estimation results in the weighted regressions. Therefore, it is typical practice to impose a "common support" assumption to restrict the propensity score to within a range where the control and treatment groups are more likely to overlap with each other. Therefore, we keep the propensity score between 0.025 and 0.975 so that the weights would be smaller than 40.[36]

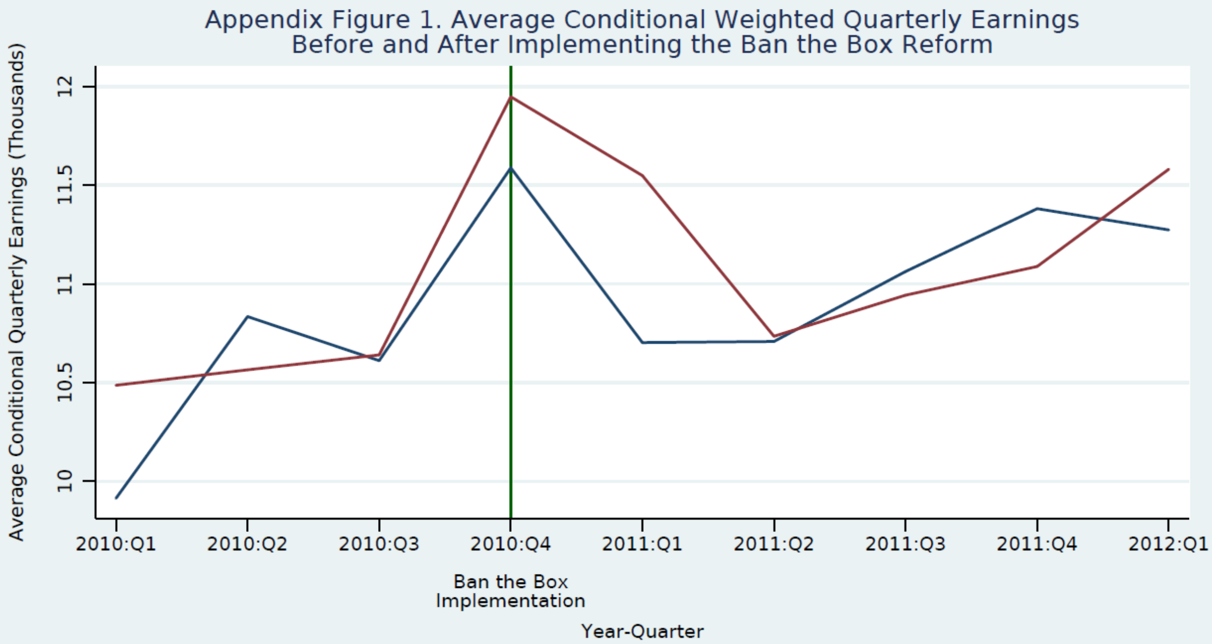
[36] To test the robustness of our results to the weight restrictions, we tried running the weighted regressions without restricting the propensity scores and weights. These regressions produced similar results as our regressions using restricted propensity scores and weights produced.

Appendix B: An Alternative Approach to Explore the Earnings Effect of the Ban the Box Policy

We explore the effect that ban the box has on ex-offenders' earnings by only using the average earnings of the employed in each cell in each year-quarter. We first calculate the average quarterly earnings among just the employed by dividing the unconditional average earnings by the employment rate in each cell in each year-quarter. In doing so, we ignore the zero earnings of the unemployed. However, this conditional earnings measure is not entirely comparable over time because the employed individuals within each cell may change from time to time. Therefore, we may not follow the earnings of the same employed individuals over time. For this reason, caution is needed when interpreting the results based on this conditional earning measure.

Appendix Figure 1 shows that even after applying the inverse-probability weights, the conditional earnings of the treatment and control groups do not follow parallel paths in the ban-the-box pre-period. The figure suggests that in the pre-period, employed non-offenders, on average, experienced faster growth in earnings than employed ex-offenders. This result casts doubt on the validity of using the difference-in-differences estimator in this case.

We run the three regression specifications using this conditional earnings measure as the dependent variable. The estimated coefficient on *Post* × *Treat* is negative, but very close to zero, and highly insignificant. Given the measurement issue that affects this dependent variable, it is hard to draw a definitive conclusion from this analysis about the pure earnings effect that is attributable to the ban the box policy.



Source: Authors' calculations.
Note: Earnings are in 2015 dollars. We use the inverse-probability weights.
Average quarterly earnings are calculated conditional on individuals who are employed in each cell in each year-quarter.