NBER WORKING PAPER SERIES

# CAN YOU ERASE THE MARK OF A CRIMINAL RECORD? LABOR MARKET IMPACTS OF CRIMINAL RECORD REMEDIATION

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Working Paper 32394
http://www.nber.org/papers/w32394

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2024

All results in this paper using IRS data are drawn from the publicly available working paper "The Impact of Criminal Records on Employment, Earnings, and Tax Filing," which is available on the IRS Statistics of Income Tax Stats website. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors and do not necessarily reflect the views or the official positions of the U.S. Department of the Treasury or the Internal Revenue Service. All results have been reviewed to ensure that no confidential information is disclosed. Researchers Agan, Garin, and Koustas received funding for this research through the Clean Slate Initiative, a project of the New Venture Fund. We thank staff at the Philadelphia DA's office, especially Michael Hollander and Betsey Carroll for help with the Clean Slate data and analysis. We also thank the numerous interns who worked on this project: Camilla Adams, Kaan Cankat, Sarah Frick, Jared Grogan, Bailey Palmer, Kalie Pierce provided instrumental research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 32394
May 2024
JEL No. J0,K0

**ABSTRACT**

We investigate whether removing a previously-obtained criminal record improves employment outcomes. We estimate the causal impact of criminal record remediation laws that have been widely enacted with the goal of improving employment opportunities for millions of individuals with records. We find consistent evidence that removing an existing record does not improve labor market outcomes, on average. A notable exception is participation in gig work through online platforms, which often screen workers based on their records but not their employment histories. The evidence is consistent with records initially scarring labor market trajectories in a way that is difficult to undo later.

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# 1 Introduction

Millions of people across the United States are charged with crimes each year, with each charge adding additional "marks" to an individual's criminal record.[1] The employment consequences of these records could be substantial. In several surveys, 70-95% of employers state that they perform background checks for most positions (Society for Human Resource Management 2012, HireRight 2015, hr.com 2017, Agan et al. Forthcoming) and many job applications ask about criminal history when permitted (Denver et al. 2018; Agan and Starr 2018). Consistent with these practices, past research has shown that employment rates are lower for individuals with a felony conviction than the general population (Garin et al. 2022b; Mueller-Smith 2015; Looney and Turner 2018) and field experiments have found lower callback rates for applicants with criminal records (Pager 2003; Pager et al. 2009; Uggen et al. 2014; Agan and Starr 2018). Our own survey also finds that employers state they are less likely to hire individuals with records (Table 1). The magnitudes of the number of people with records and potential impacts on employment rates are so large that they could have potential aggregate implications for the employment rate in the US, particularly for men and communities of color (Looney and Turner 2018; Brame et al. 2014; Shannon et al. 2017).[2]

To increase opportunities for individuals with criminal records, jurisdictions across the country have expanded initiatives to expunge or seal criminal records. We refer to these efforts collectively as "record remediation policies." These policies offer a great promise: if records alone are preventing people from accessing employment opportunities, clearing a record can be done with relatively little cost, and in some jurisdictions can be done wholesale.

In this article, we study the labor market impacts of three major criminal record remediation policies in the United States that exogenously change or limit the availability of criminal history information reported in employment background checks. Two of these policies focus on remediating non-conviction records, and one remediates conviction records. All three remediate both felony and misdemeanor level charges. While much of the past research focuses on felony convictions, most charges filed are misdemeanors (Mayson and Stevenson, 2020) and conditional on charges being filed by the prosecutor, many cases will end up in a non-conviction, usually via dismissal.[3] Even these lower-level charges and non-conviction

[1] Nearly 97 million individuals have a criminal history record stored in the Federal Bureau of Investigation's (FBI) Interstate Identification Index (III) based on records maintained by the FBI and state criminal record repositories, covering over one-third of the adult working-age population (Goggins and DeBacco 2020).

[2] Having a criminal record can also affect access to housing and public benefits (Evans and Porter 2015; Leasure and Martin 2017; Yang 2017; Tuttle 2019), which can further affect employment prospects.

[3] According to the Court Statistics Project, 77% of cases in state courts in 2022 were misdemeanors (`https://www.courtstatistics.org/court-statistics/interactive-caseload-data-displays/csp-stat`). In 2009 in a random sample of cases from the 75% largest counties in the United States, 25% of felony charges were dismissed, another 10% were acquitted or given diversion (Reaves 2013). Amongst

outcomes can show up on criminal background checks, potentially reducing employment opportunities for people with records.

To examine employment trajectories before and after criminal legal contact and estimate the labor market impacts of the criminal record remediation policies we study, we link publicly-available individual-level criminal records data from four jurisdictions—Maryland, New Jersey, Pennsylvania, and Bexar County, Texas—to administrative tax records at the Internal Revenue Service (IRS).

We begin by providing evidence of large and persistent drops in formal employment at the time of both misdemeanor and felony charges. These patterns are observed for convictions and also, perhaps more surprisingly, for non-convictions. Though these drops may coincide with other shocks happening at the time of the criminal events, these findings are consistent with previous data from audit studies that has shown that employers react negatively to felony convictions and also to misdemeanor arrests (Pager 2003; Uggen et al. 2014; Agan and Starr 2018). Further, a survey of hiring decision-makers at firms that we conducted supports the interpretation that these patterns at least partially reflect the negative impact of having a record. Hiring professionals report markedly reduced likelihood of hiring someone with a drug or theft charge, even if it resulted in a non-conviction.

We then study whether criminal record remediation can increase employment. The first policy we study is the federal Fair Credit Reporting Act (FCRA) which prohibits reporting of criminal charges that did not lead to a conviction (mainly dismissals) after seven years for jobs that pay less than $75,000 a year. In addition, several states have a stronger version of the law that applies to convictions, in particular Maryland, which is one jurisdiction in our study.[4] These laws apply to all consumer reporting agencies (CRAs), which include the major credit bureaus and employment background check companies. We examine the evolution of labor market outcomes after the last criminal charge or conviction with a focus on the sharp seven-year change in reporting, comparing the outcomes for individuals before and after the record is removed. In our first analysis, we focus on individuals whose last criminal charge is a non-conviction and who also have no other convictions in that jurisdiction, such that FCRA gives these individuals a clear criminal record on CRA-run background checks seven years after the last event. Using the same design, we examine reporting prohibitions for convictions, contrasting the experience of Maryland—a state that prohibits the reporting of convictions after seven years for lower earning workers— with other states with no such restrictions.

misdemeanor charges in a sample of 8 jurisdictions that spanned from rural to urban, between 28 - 73% of charges ended in a dismissal or diversion depending on the jurisdiction (Mayson and Stevenson 2020). Amongst misdemeanor charges in Suffolk County, MA, 74% ended in a non-conviction (Agan et al. 2023).

[4] For a full list of different FCRA rules by state, see Appendix D.

We also study Pennsylvania's Clean Slate Law of 2018, which legislated automated sealing of *all* non-convictions. Our data allow us to analyze the impact of these sealings on charges that were at least 18 months old using a difference-in-differences research design, comparing outcomes of individuals who only had non-convictions on their record and had their non-conviction sealed to a comparison group of individuals with non-convictions who did not have their record sealed because they had unpaid fines and fees and thus were not eligible for the first round of automated sealing.

Our findings are consistent across the different criminal record remediation policies we study and across jurisdictions. In all but a few notable cases, we find little evidence that clearing a record improves labor market outcomes for the average affected individual. We find no evidence that removal of non-conviction records from criminal background checks at seven years under FCRA increases the likelihood of having any formal employment income for those whose records are completely clear seven years after their last non-conviction event. This finding holds across almost every employment-related outcome. Similarly, using variation in Maryland, we fail to find a detectable improvement in average employment rates seven years after an individual's last conviction event, when their record is completely in that state. These results are further supported by the analysis of the Pennsylvania Clean Slate Law, where we find no effect of having non-conviction records 18 months or older automatically sealed on employment outcomes amongst those with only non-convictions on their record.

Ultimately, our reading of the totality of the evidence is that existing record remediation policies have limited effects on employment, at least on average. Why, then, are there such persistent impacts of having a criminal record, even for a misdemeanor or non-conviction? Our hypothesis is a scarring mechanism: records initially create gaps in employment or limit access to opportunities with job ladders or long-run employment potential which, in turn, lead to subsequent adverse outcomes separate of the record itself.[5] A notable exception to our general null finding is evidence that record remediation policies increase the rate of electronically-mediated gig platform work, albeit from a very low base.[6] In the case of platform-based gig work, such as ride-sharing, all applicants go through a background check but face few other barriers to participating as a worker. While jobless spells and intermittent employment caused by having a record may impact consideration for many jobs, this type of labor market scarring has little scope to impact an applicant's ability to find work on a gig platform, so long as they pass the background check. And thus the finding of a positive effect there is perhaps indicative that labor market scarring is partially driving our null results.

[5]The labor literature shows that displaced workers tend to experience persistent earnings losses (Jacobson et al. 1993; Lachowska et al. 2020). Kroft et al. (2013) shows that employers are less likely to callback applicants with longer unemployment spells.

[6]We do not find evidence of effects on other types of self-employment work.

Our findings indicate that scarring from records can occur quickly, consistent with recent work on the downstream effects of initial misdemeanor charges (Agan et al., 2023). For example, we find that even record remediation occurring as early as 18 months following a non-conviction event, as in our PA Clean Slate analysis, appears to be too late to undo scarring.

These results mirror results from work studying the impact of sealing bankruptcy flags and clearing medical debt. Using a similar methodological approach, Goldsmith-Pinkham et al. (2017) examine the effects of removing bankruptcy flags on individual outcomes and find limited effects on earnings and employment. Other work using RCTs, such as Kluender et al. (2024), find that medical debt relief, when relieved more than a year after the initial debt was acquired, has no impact on credit outcomes, financial distress, or mental health. In a different experiment when medical debt was relieved immediately before patients left the hospital, there were increases in healthcare use after relief including increases in detection of diabetes and depression (Adams et al. 2022).

Of course, even existing criminal record remediation policies may have beneficial impacts on individuals that are not reflected in the formal labor market earnings we observe in IRS tax records. Remediation of records could directly impact access to housing, civic engagement, quality of life, and other policy-relevant outcomes. At the same time, our analysis suggests that if reintegration and labor market participation are primary objectives, existing policies are not achieving these goals. Our findings may help explain why recent evaluations of Ban-the-Box policies failed to find improvements in labor market outcomes for individuals with records (Rose 2021).

Our work is closely related to several literatures. One literature studies the impact of criminal record remediation laws by studying cases where individuals file their own petitions for record remediation. This work documents a substantial "uptake gap": a very small proportion of individuals that are eligible for this type of remeidation take it up (Chien 2020). Previous work that has studied the labor market impacts of expungement amongst those who choose to petition has found a positive trend in employment after the petition-based expungement (Selbin et al. 2018; Prescott and Starr 2019). Our study builds on this prior work by studying settings where individuals do not select into treatment.

Our study is also closely related to a literature studying the interplay between crime, criminal records, and employment more generally. For example, previous work using tax data has found that formal employment prospects for individuals who have been released from prison are poor both before and after the period of incarceration (Looney and Turner 2018) and conviction (Rose 2021). Quasi-experimental evidence on the employment impacts of incarceration yields mixed results (Kling 2006; Mueller-Smith 2015; Bhuller et al. 2020;

Garin et al. 2022b). There has been more limited work on labor market impacts of criminal justice events before the conviction stage, with Freeman (1991), Grogger (1995), Dobbie et al. (2018), and Mueller-Smith and Schnepel (2021) being notable examples.[7] Finally, our work relates to an experimental literature that has consistently found lower callback rates for those with records, including misdemeanor arrests (Pager 2003; Uggen et al. 2014; Agan and Starr 2018).

Most closely related to our research are Jackson et al. (2017), Dasgupta et al. (2021), and Agan et al. (Forthcoming). Dasgupta et al. (2021) study the enactment of New Zealand's clean slate regulation, which allowed automatic sealing of criminal records for individuals with no further convictions within seven years. Consistent with our findings in the U.S. context, Dasgupta et al. (2021) find null effects on employment outcomes at the seven-year mark. Jackson et al. (2017) study the introduction of a new background check database in Massachusetts that provided an alternative to CRA background checks and also limited access to older records, and similarly find no evidence of employment benefits. Using experimental and quasi-experimental variation, Agan et al. (Forthcoming) find null average effects on employment outcomes for cases retroactively reduced with exogenous timing from a felony to a misdemeanor due to California Proposition 47.

We build on these literatures by studying the impact of record remediation policies that can potentially clear entire records (rather than reduce their severity) on labor market outcomes in the United States. Beyond standard employment outcomes, we also build on earlier work of Collins et al. (2018) to examine whether criminal records may push individuals out of the traditional workforce and into alternative work arrangements such as platform-based gig work and other independent contracting or self-employment.

The article is organized as follows. Section 2 discusses the policies we study, Section 3 discusses the data, and Section 4 describes survey and observational results that document the penalty associated with criminal records. Sections 5 and 6 describe our empirical specifications and results. Section 7 offers some concluding remarks.

# 2 Criminal Record Disclosure Under Reporting Laws

In this section, we discuss the three policies that are central to our analysis on the impacts of the "extensive margin" of record reporting: the federal Fair Credit Reporting Act that sets a seven-year time limit for reporting of non-conviction criminal records by CRAs, the

[7] In particular, Grogger (1995) finds that the impact of an arrest on employment is small and not persistent in the 1980s in California. We note that not all arrests will lead to charges or convictions, and interestingly, since 1975, California is one of the few states that prohibits the reporting of non-convictions which could dampen labor market impacts of an arrest.

stronger law in Maryland that also applies to convictions at the seven-year mark, and the clean slate law in Pennsylvania.[8]

## 2.1 Background

**FCRA**: The federal FCRA governs the type of information reported by CRAs to potential employers.[9] FCRA only applies to criminal background checks performed by CRAs (which includes major credit bureaus and also employment background check companies). A firm that performs an in-house criminal background check is not subject to FCRA requirements. Relevant to our context, under FCRA, non-convictions are only reportable for seven years for jobs with annual expected salary <\$75,000; in contrast, convictions are always reportable regardless of the age of conviction. The seven-year clock for non-convictions starts at the date the charge is filed.[10] We use the seven-year rule under FCRA to estimate how employment changes after non-convictions are cleared from individuals' records. In particular, under this rule, an individual's criminal history should be completely clear seven years after the last criminal history event/charge among individuals who have no convictions on record. This feature of FCRA allows for an event-study design where individuals do not select into the event in the relevant time horizon for estimation. Our approach is in the spirit of Goldsmith-Pinkham et al. (2017) who examine the effects of removing bankruptcy flags on individual outcomes.

Our survey of hiring professionals further discussed in Section 4—in particular the responses in Appendix Table A.1—confirms that this is a relevant policy for understanding the role of records on employment outcomes. There is considerable evidence that background checks are overwhelmingly done through CRAs. Amongst the respondents whose most recent firm did criminal background checks for entry-level positions (70% of the sample), 85% of criminal background checks for entry-level positions were performed by a CRA that would be obligated to follow the look-back provisions of FCRA for reporting non-convictions.[11] While

[8] Appendix Figure A.1 shows an example actual background check from a CRA which includes a dismissed charge.

[9] There are two types of background checks an employer can choose to run: fingerprint checks and name searches. In a fingerprint-based search, arrest records will appear regardless of whether the arrest leads to a formal charge. A name-based search search will turn up court charges even if they did not lead to conviction, although is unlikely to uncover arrests that did not lead to a court charge. Name-based searches are more common for most mainstream types of employment.

[10] In the context of recent litigation, courts have upheld the Federal Trade Commission and Consumer Financial Protection Bureau's interpretation that the FCRA look-back window for non-convictions starts at the date of filing and NOT the date of dismissal.

[11] 78% said this check was performed by an external CRA and they could name the CRA (and we could verify that it was, in fact, a CRA). Only 9% of respondents said they performed the background check "'in-house" by searching court records themselves. The remaining 6% reporting using fingerprint checks or

it is plausible that employers may be doing other types of informal searching on top of CRA checks (Lageson 2020), given the myriad laws surrounding criminal background checks and the large proportion of employers using CRAs, even for entry-level jobs, it seems likely that FCRA's reporting requirements directly affect the information visible to employers.

To more directly verify CRA compliance with FCRA, we obtained data on all background checks performed by a large CRA for 288 firms covering more than 11 million charges. In Figure 1, we plot the distribution of time since criminal event for conviction charges and non-convictions that show up on criminal background checks from this CRA. As seen in the figure, no non-conviction charge is reported after seven years, consistent with the reporting requirements under FCRA. Since FCRA does not regulate the reporting of convictions, most convictions that are older than seven years are reported. However, there is a marked discontinuity in the frequency of convictions reported at seven years because of analogous laws in nine states that further restrict the reportability of criminal convictions at seven years.[12]

**Maryland Credit Reporting Law**: We also study a law in Maryland, passed in 1976, which prohibits employers from requesting arrest or conviction records that are more than seven years old (starting from the date of disposition) for any job that pays less than $20,000 per annum.[13] While this income threshold is low, it binds for the majority of individuals with criminal records. In our administrative tax data, in Maryland, the average earnings for individuals matched to criminal records data and who are working is $14,000 per year ($7,000 per year median).[14] These numbers suggest that part-time jobs and likely even full-time jobs will be largely under this threshold in our population of individuals with records. In addition, our conversations with a major CRA indicate that the agency's default policy is to not provide prior convictions more than seven years old from date of disposition to any employer in Maryland, regardless of the position's expected annual salary, and the vast majority of employers do not opt out of this default.

**Pennsylvania Clean Slate Law**: The last policy we study is Pennsylvania's Clean Slate Law of 2018 (Act 56 of 2018), which legislated automated sealing of *all* non-convictions and some older low-level convictions. The Clean Slate law automated the sealing of non-conviction records with no waiting period, as well as certain low-level conviction records after ten years. Eligible records were sealed between June 2019 and June 2020 and the law has

other official state record searching programs such as CORI in Massachusetts.

[12] See https://help.checkr.com/hc/en-us/articles/360001100688-What-are-state-salary-exceptions-.

[13] MD. CODE. ANN., COM. LAW §14-1203(b)(3) (2010).

[14] Over our sample period of 1999 to 2018, $20,000 is approximately the median annual labor income for employed men without college education in Maryland according to the March Current Population Survey.

since resulted in nearly 40 million criminal records sealed for over 1.2 million individuals. Under this law, these records are automatically shielded from the vast majority of employers, landlords, schools, and the general public, but are still accessible to law enforcement and judicial officers. To estimate the effects of this policy we employ a differences-in-differences design. When the law was initially passed, individuals who owed fines and fees to the court at the time of the initial set of sealings were not (initially) eligible.[15] We therefore compare individuals with eligible records with and without fines and fees, from before the change to the year after the policy in 2021.

# 3 Data

To analyze the evolution of employment around criminal charges and the impact of criminal record remediation on labor market outcomes, we merge publicly-available criminal records data from several jurisdictions to administrative data from the IRS. In this section, we describe the administrative criminal records and tax data we use.

Our administrative court records come from Maryland, New Jersey, Pennsylvania, Maryland, and Bexar County, Texas. They include all misdemeanor and felony charges filed with the court within the relevant time period, with the exception of our New Jersey data, which just contain information on Superior Court cases (equivalent to felonies in other jurisdictions). In addition, the data include the date of each charge, the disposition date, and ultimate outcome (dismissed, convicted, etc.), as well as other defendant and case characteristics. Further details on these data, including how we match defendants across cases and how we categorize charges are available in Appendix C.

**Bexar County, Texas** The data from Bexar County include all charges filed between 1975 and 2018.[16]

**Maryland** The Maryland criminal records were drawn from several tables hosted on the Maryland Volunteer Lawyers Service (MVLS) database. The Maryland data cover all charges filed between 1990 and 2018.

**New Jersey** We obtained New Jersey court data from the Superior Court of New Jersey via a Public Access Information Request. We only have data for "indictable offenses" which

[15] PA subsequently eliminated court fees and fines as a barrier to sealing so long as restitution is paid.

[16] The data are publicly available for download via the court website, which began releasing them as part of an effort to make court records more accessible. Full detailing of this data can be found in Agan et al. (2021) and Freedman et al. (2018).

closely align with what would be felonies in other states, and we use the term felonies to describe them. These files contain records from January 1, 1980, to May 30, 2018.

**Pennsylvania** We obtained Pennsylvania court data from the Administrative Office of Pennsylvania Courts (AOPC) via a Public Access request. The data cover all cases in the Magisterial District Court system (which handles misdemeanors) and Courts of Common Pleas system (which handles felonies) filed between May 2008 and April 2018. Using data from the Philadelphia District Attorney's Office, we are able to identify records that were sealed by PA Clean Slate between June 2019 and June 2020. Our estimates indicate that 57% of the nearly 7.5 million non-convictions charges in our AOPC data were sealed, with the ones that were not sealed due to individuals still owing fines and fees at the time of the initial Clean Slate implementation.[17]

## 3.1 IRS Employment Outcomes

We draw our outcomes from the universe of IRS tax filings, which are linked to the individual-level court record data. The IRS data includes anyone who has ever filed an individual tax return or has had income reported to the IRS on an information return.

We construct main employment and tax filing outcomes standard in the literature using administrative tax returns. We draw information on formal sector wage and salary earnings and employment from W-2 returns issued by employers for each employee in each year. Importantly, W-2 returns are sent by employers to the IRS irrespective of whether and how the employee files their own individual tax return. Individuals with no W-2s or self-reported income in any particular year are assumed to have had no earnings in that year. We examine whether individuals have any wage and salary earnings as well as whether earnings exceed specified threshold levels. We supplement these earnings records with gross payments to non-employee independent contractors and online platform "gig" workers reported by firms on 1099-MISC and 1099K forms. We construct these earnings measures following the methodology in Collins et al. (2018).[18] W-2 based outcomes are available from 1999 to 2019

[17] Our research team along with a team the Philadelphia DA's office looked up a small, random subset of case information online for non-convictions that were not sealed between June 2019 and June 2020 and were able to confirm that for that subset, each one owed fines and fees. In October 2020 a new law was passed eliminating court fines and fees as a barrier to sealing remedy under Clean Slate and these additional cases were sealed in late 2021.

[18] Unlike payments to independent contractors on 1099-MISC, which are subject to a $600 threshold, online platform economy earnings reported on 1099-K are subject to a much higher $20,000 threshold. Garin et al. (2022a) reports that while that most major platforms issued 1099-Ks to all platform participants through 2016 regardless of the earnings level, several large platforms announced that they would adhere to the higher thresholds beginning in 2017. Thus, some smaller payments from these platforms after 2016 may not be observed in the data.

and 1099-based outcomes are available from 2000 to 2019, with the exception of platform-mediated gig work which only became widespread enough to measure separately beginning in 2012.

We also examine outcomes directly reported to the IRS by individual taxpayers themselves on Form 1040 tax returns. These include total gross self-employment revenues and net profits after expenses reported on Schedule C, with self-employment net profits further broken out for each individual within the filing unit (with net profits of at least $433) on Schedule SE.[19]

The IRS data are linked to the criminal records data using individual name, date of birth, and geographic location. We were able to match 86%, 73%, 81%, and 91% of the data in Bexar, Maryland, New Jersey, and Pennsylvania, respectively. Further details on our matching algorithm to IRS records can be found in Appendix E. For each jurisdiction, our match rate is comparable or higher than past work matching to IRS or Unemployment Insurance data (e.g. Dobbie et al. 2018; Travis et al. 2014). Appendix E also compares characteristics of matched and non-matched individuals and the distribution of last criminal history events in the full versus matched sample for our FCRA and Maryland analysis.

# 4 Evidence on the Penalty Associated with Criminal Records

Strong quantitative evidence on the informational content of criminal records in employment decisions comes from audit studies (Uggen et al. 2014, Pager 2003, Agan and Starr 2018). These studies have consistently found a causal penalty in callback rates for an applicant reporting a record, both misdemeanor and felony. For example, Uggen et al. (2014) find that employers are four percentage points (10-14% depending on race) less likely to call back individuals reporting a misdemeanor arrest that did not lead to a conviction; this penalty is substantial but smaller than the penalty for reporting felony convictions (Pager 2003; Agan and Starr 2018).[20] Audit studies are limited, however, because they only rely on information entered on applications and the outcome is limited to callbacks.

In this section, we take two additional approaches to documenting whether and how criminal records may harm employment outcomes: (1) a survey of hiring professionals, and (2) documenting changes in employment and earnings around the time of criminal charges.

[19] Unlike information returns reported by third-parties, individual tax reporting on 1040 forms may be impacted by taxpayer non-compliance or strategic reporting. Nonetheless, reporting behaviors such as filing any 1040 return are of interest to understand how criminal record remediation impacts tax compliance.

[20] Employers were 50% less likely to callback an applicant with a felony conviction in Pager (2003) and 37% less likely for the felony conviction in Agan and Starr (2018).

## 4.1 Survey Evidence

To learn more about the role of criminal record background checks in hiring decisions, we conducted two surveys of hiring professionals. The first survey (N=808) took place in May 2021 and asked about practices related to criminal background checks at the individual's most recent firm for entry-level positions. The second survey (N=505) took place between September to October 2021 and presented hiring scenarios to a different group of hiring professionals who reported having recent hiring experience in the United States.[21]

Survey results for the first survey are presented in Appendix Table A.1. 70% of respondents reported that their firm did criminal background checks for entry-level positions (and 7% were unsure), and of these, 59% (N=383) reported being knowledgeable about background check procedures. Importantly for our FCRA analysis, we find that of those who perform background checks, 78-85% report that those checks are performed by CRAs, which are regulated under that law.[22]

In the second survey, we presented a vignette that described a hiring scenario for an entry-level position at the respondent's most recent firm. Specifically, we presented the following scenario:

> *"You intend to hire a candidate for an open entry-level position at the most recent firm at which you had hiring experience. Through the hiring process, you decide that this candidate is well qualified for the position. You are ready to extend an offer to the candidate. However, you learn that the candidate was charged with [felony/misdemeanor] [drug possession/theft] X years ago and was [convicted/not convicted]. How likely are you to recommend that the company hire the candidate?"*

We randomly assigned respondents "felony" or "misdemeanor," "drug possession" or "theft," and "convicted" or "not convicted." The number "X" varies from 1 to 10 for each respondent.

These results from the second survey are reported in Table 1. Applicants with drug possession and theft charges are substantially penalized in hiring, even if they have not been convicted of the charge. 50 percent of respondents state that they would not hire someone with a misdemeanor drug non-conviction in the previous year. This rate increases to 65 percent for a felony. For theft offenses, the corresponding rates are 62 percent and 68 percent. Clearly, even a non-conviction is viewed quite negatively by these respondents. Convictions are penalized even further, and more for felonies than misdemeanors.

[21] The surveys were conducted using Prolific, which has compared favorably to other platforms for soliciting survey respondents (Peer et al., 2017). See Appendix B for details on the survey sample.

[22] 7% are performed by an external agency, but the respondent could not name the agency so we could not verify it was a CRA.

Table 1 also shows that the penalty employers assign to convictions and non-convictions diminishes with the time elapsed since the event. In our survey, depending on the charge, 15 to 25 percent of hiring professionals report that they would pass on qualified applicants with non-convictions for drug and theft charges at year seven, compared to 15 to 35 percent who report they would do so for applications with convictions. This finding, which is relevant for all of the policies we consider, provides an additional reason why the expected effect of having a record sealed or reduced may vary with time since charge.[23]

## 4.2 Employment Around a New Charge

How does employment evolve around the time of criminal legal charge? We examine this in our linked microdata sample focusing first on the (known) first-event for an individual in our sample within each jurisdiction. We then also study how employment evolves around the charge that is the latest observed event for an individual with records to ensure that changes in employment outcomes after the event are not solely due to additional charges or convictions.

We document the change in earnings around these events using an event-study framework.[24] We estimate the model:

$$y_{it} = \sum_k \beta_k \mathbb{1}\{E_i = t + k\} + \alpha_i + \alpha_{a(i,t)} + \alpha_t + \epsilon_{it} \tag{1}$$

We do this separately for convictions and non-convictions and for felonies and misdemeanors. We assign the charge date as the the event date for non-convictions and assign the disposition date of the last criminal history event for that defendant in a particular jurisdiction for convictions. $\alpha_i$ is an individual fixed-effect, $\alpha_{a(i,t)}$ are age fixed effects, and $\alpha_t$ is a year fixed effect. We bin endpoints for the period more than 5 years prior and 10 or more years post event. For the non-conviction sample, we limit the latest non-conviction charge to individuals with no earlier conviction on record in the same jurisdiction within our data. The estimates are shown separately for each jurisdiction for which we have complete criminal records data

[23]These survey findings are largely consistent with the past literature. For example, using employer surveys administered in various large metropolitan areas in the 1990s to early 2000s, Holzer et al. (2003) find that employers are much more averse to hiring ex-offenders than they are towards any other disadvantaged group, and that employer criminal background checks are increasingly used. On an online labor platform that third-party businesses use to connect with workers seeking short-term jobs, Cullen et al. (2023) employ an incentive-compatible survey and similarly find that employers are reluctant to work with people with criminal records (only 39% willing), and this reluctance holds across both misdemeanor and felony convictions, although demand is higher for those with a misdemeanor (e.g. 51% willing to work with a worker with a drug-related misdemeanor, only 27% with a drug-related felony). They did not explicitly ask about non-convictions.

[24]Rose (2021) also uses an event-study approach to estimate the effect of convictions on labor market outcomes.

for a sufficiently long panel: Bexar County, TX, Maryland, Pennsylvania, and New Jersey (for felonies only). We present these estimates using data from 2000 to 2020, restricted to "last events" that occur between 2003 to 2011.

We present summary statistics for this sample in Appendix Tables A.2-A.3, broken out separately by last events which are non-convictions and convictions, and by last events that are felony and misdemeanors. We see that two years before the first criminal history event, the employment rates of those with misdemeanor charges, regardless of conviction status, are quite similar at around 77%. Among those whose first event is a felony, pre-period employment rates of those who are convicted are slightly lower (69% compared with 72%). Looking at the last criminal event, those whose last event is a non-conviction with no other convictions had baseline employment rates of 72 or 78% depending on whether the last charge was a felony or misdemeanor, respectively. These rates are quite similar to those around the first event. For those whose latest event was a conviction, baseline rates of employment are substantially lower at 60 or 69% for felony and misdemeanor, respectively.

Figure 2 plots the $\beta_k$ coefficients for having any wages relative to two years prior to the *first* criminal legal event for an individual within that jurisdiction. For all types of criminal legal contact, employment falls after an initial charge and remains low persistently even 6 years after the event, even when the defendant was not convicted. After an initial criminal legal charge that leads to a felony conviction, employment rates are lower by between 10 and 20 pp depending on the jurisdiction, which represents an 13-26% decline, felony non-convictions are associated with a persistent is 5-8pp decline in employment (8-11%); misdemeanor convictions is 6-9pp (7-11%); by 6 years out after misdemeanor non-conviction is 6pp (a 7-8% decline). These declines in employment are large and importantly remain persistent 6 years after the initial criminal charge. We examine other types of labor earnings reported to the IRS in Appendix Figure A.2 and find no evidence of offsetting increases in independent contract work or other self-employment around an initial charge.

Some of the persistence in the overall decline could come from subsequent criminal legal contact.[25] Figure 3 plots the $\beta_k$ coefficients for having any wages relative to two years prior to the *last* criminal event. We still see that formal labor sector engagement falls sharply and persistently around the time of a criminal charge, even those that did not lead to conviction. For felony convictions there is some evidence that this is a transitory decline that mimics incapacitation in prison—individuals employment probabilities fall 7-20pp in the year after the conviction and by 6 years have returned closer to pre-conviction baseline. For many people with felony convictions, their last event was preceded by other criminal

[25] Agan et al. (2023) shows that initial misdemeanor charges, even when they do not lead to a conviction, cause large increases in recidivism for marginal defendants.

legal charges—so they are returning to an already depressed baseline from their *first* charge. For felony non-convictions, employment is 3.5 to 5.6 percentage points (4.6-7.9%) lower two years from the last charge. These declines are persistent and only exhibit a limited rebound. For misdemeanor non-convictions, the share of the group with any earnings is 1.3-1.8 percentage points (1.7-2.2%) lower two years following the charge, with no detectable reversion to pre-charge levels.

Figure 4 presents changes in the probability of being employed in a specific sector around the time of an initial charge (Appendix Figure A.3 does so for last charges). Both non-convictions and convictions appear associated with reallocation in employment across sectors. Around the first nonconviction we see declines in manufacturing, finance, real-estate and health care employment. These sectors with relative declines, particularly finance, real-estate, health care and education tend also to be sectors with occupational licensing requirements. Conversely, following a non-conviction charge we see rises in construction, professional services, temp agencies, and accommodation/food employment. The relative changes in sectors for first convictions largely mirror non-convictions, but there is mostly an absolute decline across all sectors due to lower overall employment probabilities. Nevertheless, there is even an absolute increase in the rate of employment in temporary help agencies. Temporary help agencies appear to serve as a stopgap for people experiencing these adverse events.

When interpreting the overall patterns it is important to note that factors other than criminal records, such as addiction and mental health issues, plausibly contribute to adverse outcomes after criminal charges. In the case of felony convictions, there are clearly incapacitation effects, though these are unlikely for misdemeanors and non-convictions beyond the first year. There are reasons to think, though, that records play a substantial role in the observed patterns. First, with the exception of felony convictions after their last event, the declines arise immediately in the year of the event without a clear pre-trend. Second, patterns are consistent with the audit literature that finds experimental evidence on reduced call back rates when criminal histories are reported. Third, the effects exhibit similarities to the patterns observed in the survey evidence described above. In particular, the general patterns for first and last events are consistent with our survey evidence that the majority of employers state that they are unwilling to hire someone with even low-level records, that these preferences are stronger for felonies than misdemeanors and for convictions relative to non-convictions, and they are persistent.

These trends suggest that there is potential scope for policies that clear or reduce records to "work" by removing negative information from a person's background check. To the extent the mark itself was a major cause of a sustained decline in employment, removing it could remediate the negative impacts.[26]

# 5 Empirical Specification and Analysis of the Fair Credit Reporting Act

For our analysis of the extensive margin of record reporting using records cleared from CRA reports, we define event-time in relation to the year of the charge that gets cleared seven years after the non-conviction or conviction. Our main specification is the same as in Equation 1 except now event time is measured as time since *last* criminal history event. We balance the sample three years prior to the clearance event and one year post the event, so that the estimated coefficients around the event are not driven by changes in sample composition. We bin endpoints for the period more than 5 years prior and 10 or more years post event. Our main analysis separately examines "last events" that are felony non-convictions, misdemeanor non-convictions, and felony and misdemeanor convictions. When analyzing convictions, we compare results from Maryland, with the other states without laws governing reporting of convictions.

We use separate analysis samples to analyze the clearance of a non-conviction and conviction. To study non-convictions, we restrict our sample to individuals with a felony or misdemeanor non-conviction as their last event, limited to individuals with no other convictions on their record, because nothing should be reported on these individuals' CRA background checks at the seven year mark, giving FCRA the best chance at improving outcomes for these individuals. To reduce measurement error associated with measuring past convictions, we limit the sample to individuals who were 18 or younger as of the earliest year of data available in the respective jurisdiction to ensure that we can accurately measure their adult criminal record. To study convictions in Maryland versus other jurisdictions in our sample, we restrict our sample to individuals whose last event was either a felony or misdemeanor conviction, regardless of other previous convictions on the record (as those will also be cleared prior to seven years after the last event).

Table 2 presents summary statistics for our analysis samples used in the FCRA and Maryland "last-event" analysis. Columns 1 and 2 present summary statistics for individuals with a felony or misdemeanor non-conviction as their last event, restricted to individuals with no other convictions on their record. Columns 3 and 4 present summary statistics for individuals whose last event was either a felony or misdemeanor conviction, respectively. Summary statistics on baseline outcomes are presented at five years after the charge or disposition date

[26] The patterns also establish confidence in the matching procedure between criminal records and the IRS data given they yield the expected patterns in the data post-event.

(or alternatively, two years before the record clearance under FCRA or the Maryland law). Among the sample of individuals whose last event is a non-conviction with no other convictions, the average age is approximately 31 years of age, and baseline measures of extensive and intensive employment are generally higher for those whose latest non-conviction was a misdemeanor versus a felony. For example, among last-event misdemeanor non-convictions, 74 percent of individuals had any wages at five years post-charge, with average wages of $19,647. In contrast, among last-event felony non-convictions, 66 percent of individuals had any wages at five years post-charge, with average wages of $14,564. Individuals are older among the sample of people whose last event was a conviction. This sample also has relatively low baseline rates of employment and average wages, particularly among individuals whose last event was a felony conviction.

## 5.1 Results

In Figure 5 Panels (a)–(b), we plot event-study coefficients for the removal of a felony and misdemeanor non-conviction, respectively, seven years after the original event. Even though non-conviction events are associated with significant drops in employment rates (as seen in Section 4), this figure shows that there is no evidence that removing the last non-conviction from the record of someone with no other convictions increases employment. We find no increases in employment among any group when examining effects separately by race (Appendix Figure A.4).

Figure 5 Panels (c)–(d) shows the same event-study plots for last convictions on record. We see no change in employment rates at the seven year mark in Maryland, where convictions cease to be reported for the majority of individuals in out sample. There is also no divergence in employment rates between Maryland and other states, who do not prohibit the reporting of convictions at a similar event-time.

Tables 3 and 4 report formal tests of whether the event-study coefficients reported in Figure 5 seven and eight years after the last event are different from a linear trend implied by our event-study coefficients, for both any wages and other employment outcomes. We adjust for pre-trends because a criminal history event mechanically occurred seven years before the FCRA event and individuals could still be recovering from the initial event.[27] For exposition purposes, we pool the jurisdictions. Table 3 Panel (a) reports results for misdemeanor non-convictions and Panel (b) reports results for felony non-convictions. Table 4 Panel (a)

[27] Specifically, we calculate: $d_{+7} = 2 \times \beta_{+4} + \beta_{+7}$ and $d_{+8} = 3 \times \beta_{+4} + \beta_{+8}$ and the standard errors on these sums using the delta method. $d_{+7}$ and $d_{+8}$ report the deviation of our period 7 and 8 event-study coefficients, respectively, from the linear trend implied by our period 4 event-study coefficient, $\beta_{+4}$. If $\beta_{+4}$ is negative, this implies a positive pre-trend. In that case, a positive and statistically significant deviation from trend in periods 7 or 8 would suggest that FCRA is having a positive impact.

reports analogous tests of deviations from trend in year 7 and 8 for convictions in the three states that have no state-FCRA law that prohibit the reporting of convictions, and Panel (b) reports results separately for Maryland, which limits the reporting of convictions after 7 years.

Consistent with Panels (a) and (b) of Figure 5, Table 3 shows that for non-convictions (both felony and misdemeanor), we find no detectable deviation from the pre-trend around the timing of the record removal. There is similarly no significant discontinuity or deviation from trend for other wage thresholds or employment outcomes. With respect to convictions, Table 4 Panel (a) shows that individuals appear to be below trend seven to eight years after the initial charge in states (NJ, PA, and TX) that do not prohibit the reporting of convictions, suggesting the positive trend observed closer to the initial event has slowed (see Figure 3 Panels (c) and (d)). Table 4 Panel (b) shows results for convictions in Maryland. We see evidence of positive trends before the event for any wages at various thresholds (see Figure 5 and Appendix Figure A.5). If anything, these trends are *slowing*, not increasing after the conviction is removed at seven years, as indicated by formal tests of deviation from pre-trends.[28] Notably, this pattern in Maryland is similar to the pattern seen for convictions in states that do *not* have a law that removes convictions after seven years. We see similar trends for tax filing, and little action on our measures of self-employment or independent contracting.[29] In addition, we find no differences by crime type of the last event (see Appendix Figure A.7) or by industry of employment (see Appendix Figure A.8). Overall, the results imply that removing criminal records from CRA background checks after seven years (either non-convictions in NJ, PA, or TX or convictions in MD) does not result in positive impacts on employment and tax-filing outcomes for affected individuals.

However, there is a notable exception for gig platform work. While platform-based gig work is a new form of work activity, we find evidence that removal of a criminal record has an economically large impact on such work for this particularly disadvantaged group, many of whom are likely entering self-employment for the first time.[30] Because platform-based work has only been prevalent since circa 2012, for this part of our analysis, we restrict our sample to years since 2012 and FCRA "last events" beginning in 2015. Event-study results pooling all criminal history events and jurisdictions are plotted in Figure 6. The left-hand

[28]Interestingly, defendants identified as Black have positive employment trends while all other races have a downward trend (see Appendix Figure A.4. There is some visual evidence that employment rates rose for Black defendants in Maryland after having their records cleared, but we cannot reject that this increase is simply a continuation of the prior trend.

[29]We find similar null effects at year seven using an alternative differences-in-differences estimator following Sun and Abraham (2021) (see Appendix Figure A.6).

[30]In the period we study, ride-hailing apps account for the overwhelming majority of platform-based gig work observed in the IRS (Garin et al., 2022a).

panel shows that for those with a criminal history event, any gig platform work peaks in the year before the initial criminal history event, and then falls in subsequent years. The time pattern is similar to the time zero patterns for wage employment and is suggestive that the criminal record limits employment opportunities in gig work. The right hand panel of Figure 6 plots the event-study coefficients around the FCRA event pooling all the data. We find gig employment increases discretely at seven years. The increase is small in percentage points (0.4 percentage points one year after the FCRA removal event), but quite large in percentage terms (an over 100% increase relative to the baseline mean of 0.003 two years before the FCRA event). Column (4) of Tables 3-4 report our test for trend breaks by type of criminal history events for the outcome of any gig work. Interestingly, our results appear to be driven by convictions, and we see a similar effect size in MD and in other states, suggesting that gig companies may be using a general seven-year rule to screen at least some convictions.

# 6 Empirical Specification and Analysis of the Pennsylvania Clean Slate Law

For this analysis we focus on the subset of individuals who only have non-convictions on their records in Pennsylvania, as these individuals' entire criminal histories are sealed by PA Clean Slate (if they do not owe fines and fees). We compare the former group to individuals who also only have non-convictions on their records but whose charges were not sealed between June 2019 and June 2020 due to the fines and fees.[31] This comparison allows us to difference out any trends in employment for people with criminal justice system contact during this time period.

Our main specification is given as follows:

$$\begin{aligned} y_{it} = {} & \beta \text{Cleared}_i \times \mathbb{1}\{Year_t \in \text{2019-2021}\} + \sum_{k \in 2016,2017} \delta_k \text{Cleared}_i \times \mathbb{1}\{Year_t = k\} \\ & + X_{it}^{'}\gamma + \alpha_i + \alpha_t + \varepsilon_{it} \end{aligned} \tag{2}$$

where $\text{Cleared}_i$ is an indicator for an individual having their record sealed (i.e. an ever-treated indicator), and $\mathbf{1}\{Year_t \in \text{2019-2021}\}$ is an indicator for being in the period after records were cleared. The interpretation of $\beta$ is the difference-in-differences estimator, comparing the change in the outcome in the post period between those that had all their misdemeanor

[31] We do not know the exact date of sealing for each case, only whether it was sealed between June 2019 and June 2020.

charges sealed with those who did not because they owed fines and fees. The interactions between $\text{Cleared}_i$ and earlier years provide a test for pre-trends. We use data since 2016 for this analysis and restrict our analysis sample to individuals aged 18 to 25 to ensure that they had no other prior convictions by the start of the PA charge data, which begins in 2008.

In contrast to FCRA and the Maryland Credit law, the Pennsylvania law allows us to estimate the effect of sealing non-conviction records that are more recent than seven years from the initial charge. To examine if the effect varies with the time elapsed since the last charge, we also estimate a triple-difference specification including additional interactions with months since the latest charge (calculated as months since June 2019):

$$\begin{aligned} y_{it} &= \beta_1 \text{Cleared}_i \times \mathbb{1}\{Year_t \in \text{2019-2021}\} + \beta_2 \mathbb{1}\{Year_t \in \text{2019-2020}\} \times \text{Months since charge}_i \\ &+ \beta_3 \text{Cleared}_i \times \mathbb{1}\{Year_t \in \text{2019-2021}\} \times \text{Months since charge}_i \\ &+ \sum_{k \in 2016,2017} \delta_k \text{Cleared}_i \times \mathbb{1}\{Year_t = k\} + X_{it}'\gamma + \alpha_i + \alpha_t + \varepsilon_{it} \qquad (3) \end{aligned}$$

The interpretation of the coefficient on $\text{Cleared}_i \times \mathbf{1}\{Year_t \in \text{2019-2021}\}$ is now the out-of-sample predicted effect at zero months since the initial charge. The earliest sealings we are able to see in our data occur approximately 18 months after the charge, as we have data through 2018 and the sealings took place June 2019–June 2020.

Summary statistics for our estimation sample are presented in Table 5. We break these out by those who are cleared in 2019-2020 ("Treated") and those who did not because they owed fines/fees ("Control.") While the two groups are broadly similar, the treated are less likely to be male, more likely to be Black, and had somewhat older charges. They are also slightly less likely to be employed, although the probability of making \$15,000 per year (approximately a full-time/full-year job at the minimum wage) and average wages (including 0s) are similar across the two groups.

## 6.1 Results

Figure 7 panel (a) and panel (c) show raw trends in having any employment, or any employment earning more than \$15,000 per year, over our time period, by treatment and control groups. Panels (b) and (d) report the event-study estimates. While both the treatment and control groups were hit hard during COVID, we do not see any differential trends among those with a cleared record.

Table 6 Panel (a) presents the main pooled differences-in-differences results and Panel (b) presents estimates from our triple-differences specification to test whether more recently-sealed charges have a differential effect on employment. While these non-conviction records were generally sealed less than seven years before the original event (on average sealed 5-6 years after the charge), we find no detectable effect of the automated record clearance for treated versus control individuals for a range of employment outcomes, although we do find a marginally significant effect for gig platform work for those with more recent charges.[32] Of course, these automated sealings took place at the beginning of the COVID-19 pandemic, making it difficult to draw conclusions about the potential impacts of sealings in a non-pandemic period. Appendix Tables A.5-A.6 shows results by race. While the overall effects are similar, there is some suggestive evidence that clearing the records earlier is beneficial for Blacks.

Overall, these findings suggest that full and automated sealing of non-convictions from criminal records does not result in improved employment outcomes, even for those with relatively recent non-convictions that are around 18 months old.

# 7 Discussion and Conclusion

In this article, we study the impact of criminal record remediation policies. With a few exceptions, these policies did not improve the labor market prospects of individuals with records, on average, despite their great promise.

There could be a myriad of factors for why these policies are not producing their intended results. If lower level convictions or non-convictions are not associated with declines in employment when they occur, then there is no reason to believe that remediating them later would help. But we provide evidence of large and persistent declines in employment around the time of both first and last charges, even those that led to non-convictions. We also provide evidence that many employers perform background checks and that most use a CRA, with less than 10% saying they do background checks "in-house". In other work, we also found that of a policy to reduce past felonies to misdemeanors had no statistically significant impact on employment for those whose reductions were done exogenously by public defender without their input (Agan et al. Forthcoming). Like the policies we study here, this could have been because the recipients of the remediation did not know; however an RCT to notify these individuals did not improve outcomes for the notification group over the control, which may imply limited scope for lack of knowledge to be driving our results (Agan et al. Forthcoming). Another explanation for the null result might be that employers are asking applicants about records, such that any information obtained in a background check

[32] To ensure our results are not driven by Philadelphia, which generally does not levy fines and fees for non-convictions and thus had very few defendants in the control group, in Appendix Table A.4 we exclude Philadelphia and find similar results, although the result for gig platform work is attenuated from removing this large urban county where gig work would be concentrated.

is superfluous. To examine this mechanism, we estimate the main FCRA specifications for New Jersey after the state Ban the Box (BTB) law went into effect in March 2015. Appendix Figure A.9 shows that even in post-BTB years, there remains a null effect after seven years of record sealing.[33]

The findings point to the conclusion that older records do not have an independent penalty on employment for most jobs. Our survey results suggest that this could be, in part, because employers largely are willing to overlook older records. However, this fact alone would suggest that the employment impacts of a record should be short-lived, whereas we observe persistent negative impacts following both charges that lead to convictions and non-convictions. A more complete account is that charges can lead to scarring such as resume gaps, loss of experience, discouragement, and reduced search (Smith and Broege 2019) that can be difficult to undo.

The scarring mechanism is interesting in light of the sectoral shifts that we observe post-charge. For both non-convictions and convictions there is a relative shift towards interim type jobs, namely temp agencies, retail and food and accommodations. In an audit study, Farber et al. (2019) find significantly lower call-back rates for workers in interim jobs. The evidence suggests that charges may lead to changes in labor market outcomes from path-dependence due both to loss of employment and a move to sectors that result in permanently reduced job finding rates.

We also find large increases in the probability an individual has earnings from a gig platform after remediation. These results for gig platform work provide insight into the scarring mechanism since a record can be the principal barrier to employment in this line of work. Our findings on gig work suggest that non-traditional work arrangements, which have typically been unstudied, may be an increasingly-important avenue of work for criminal justice-involved individuals.

If well-intended record remediation initiatives do not improve labor market outcomes, what policies might work? Experimental evidence on well-known programs that provide transitional jobs or mentoring and work readiness training for those recently released from prison suggest that these initiatives have also been largely ineffective at improving employment outcomes. For example, the Reintegration of Ex-Offenders (RExO) project, a joint initiative between various federal agencies including the Department of Labor and Department of Justice, provides individuals with records with a range of services: mentoring, work

[33] Of course we cannot rule out other possible mechanisms for our findings. Criminal records leave other types of digital footprints that can be hard to erase (Lageson 2020) and if employers are searching for these records they may, in essence, "undo" the remediation. However, a large majority of employers would need to be undertaking these sorts of costly searches to fully explain our null results. For larger employers this would be highly impractical.

readiness training, and job placement, among others. Yet, a randomized evaluation of RExO found no evidence of positive impacts of the program on either recidivism or labor market outcomes within three years of program enrollment. Qualitative evidence from the study suggested that a comprehensive and intensive approach that helps address the wide array of other issues present in the ex-offender population (housing, substance abuse etc.) may be needed (Wiegand and Sussell 2016). Similarly, a randomized evaluation of the New York City-based Center for Employment Opportunities (CEO) program, which provides transitional employment and other supportive services for recently released individuals, was also largely ineffective at increasing longer-term employment (Valentine and Redcross 2015).

Our findings, coupled with this literature, suggest that policies that clear records earlier, or which offer more intensive wrap-around programs that actively seek to connect those with records to stable long-term employment, may generate larger labor market impacts. The evidence from our research suggests that scarring from records can occur quickly, consistent with Agan et al. (2023) who find that negative impacts of a misdemeanor charge on recidivism occurs within the first 15 months. As a result, automated clean slate policies with shorter waiting periods, such as immediate sealing of non-convictions and convictions, could be more fruitful. We could not study immediate sealings in Pennsylvania due to data limitations.

Of course, any benefits from a policy that shortens the reporting period on records would have to be weighed against potential costs. Records could provide information that may be useful to employers given concerns about risk and productivity (Cullen et al., 2023). For example, using our data from Bexar County we find that having a recent record, even a non-conviction, is highly predictive of having another criminal event occur in the following five years.[34] Interestingly, by six years after their last event, people who had been convicted of a crime or been charged but not convicted have the same likelihood of committing a crime as a random person in the population. Future research is needed to determine whether the benefits of earlier record remediation policies outweigh the potential costs for employers. Given the persistent labor market impacts following criminal events, which are not undone by record sealing several years later, developing and evaluating remediation policies with shorter waiting periods, such as an evaluation of immediate non-conviction sealing under Pennsylvania Clean Slate, is an important area for future work.

[34]See Appendix Figure A.10.

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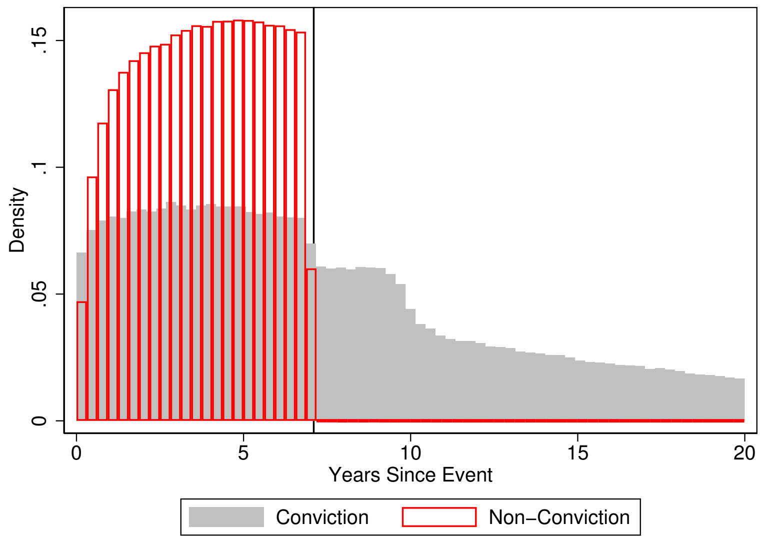
# Figures and Tables

# Figures

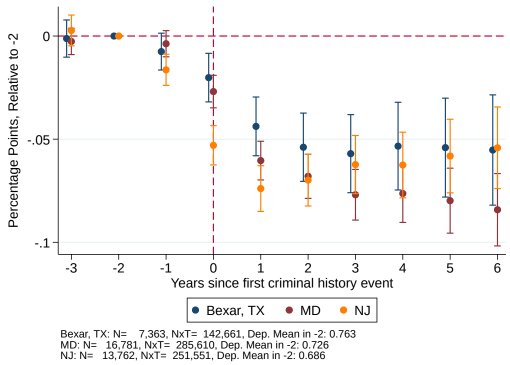
Figure 1: Years Elapsed for Charges Reported on Criminal Background Checks



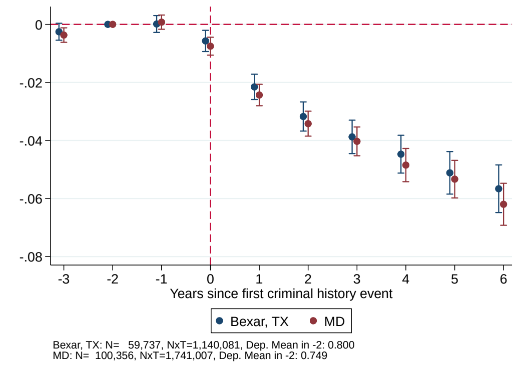

Notes: Data obtained from Agan et al. (2022). Includes information on all criminal background checks performed by a large CRA for 228 firms. These background checks include > 11 million charges reported to employers. 28% of charges reported were dismissed charges, 60% conviction charges, and 12% other. This figure reports the distribution of time since event (charge for non-conviction, conviction date for convictions) for the dismissed and conviction charges.

Figure 2: Persistent reductions in employment after *initial* charge

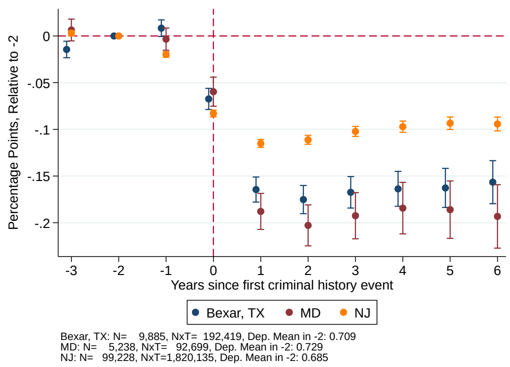
(a) Felony Non-Convictions



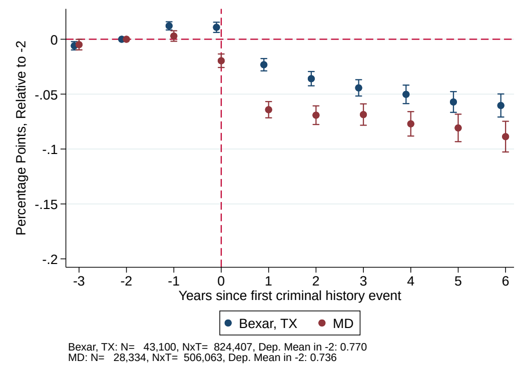

(b) Misdemeanor Non-Convictions




(c) Felony Convictions



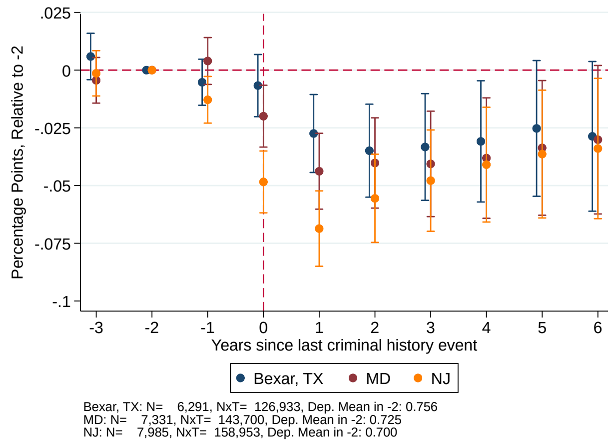

(d) Misdemeanor Convictions



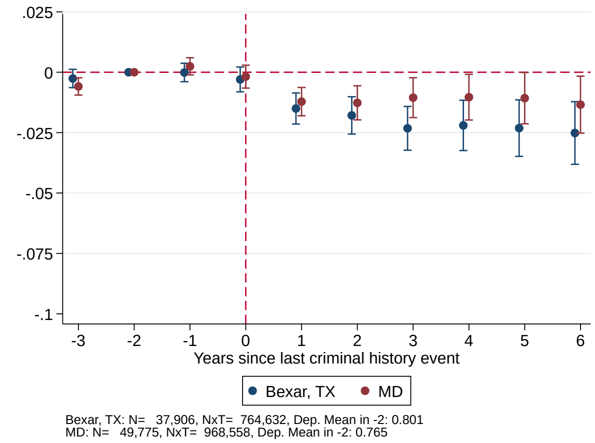

Notes: Each panel plots selected event study coefficients for the share with any wages around an individual's first charge, following specification 1 in the text. For this analysis, we restrict the full sample to have been 18 by the time the first charge appears in the data for both non-convictions and convictions. Coefficients are relative to -2 periods before the first charge. We run separate event studies for each state in each panel. Data from 2000-2020. The sample is restricted to events occurring between 1996-2011 to ensure the regression is balanced in the event window.

Figure 3: Reductions in Employment After Latest Event

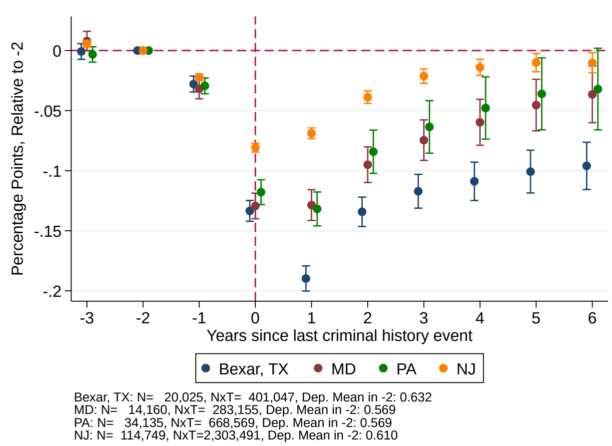
(a) Fel. Non-Convictions, no earlier convictions



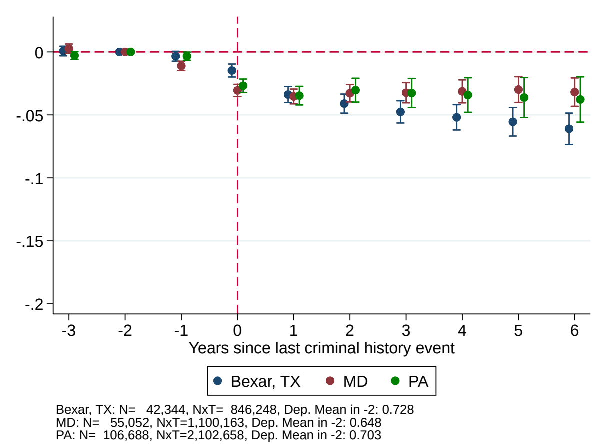

(b) Mis. Non-Convictions, no earlier convictions




(c) Fel. Convictions



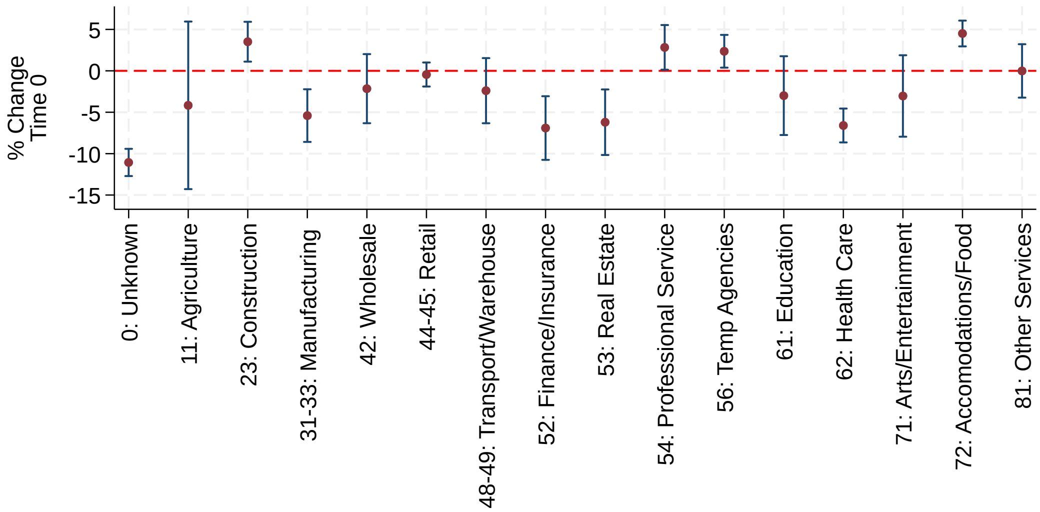

(d) Mis. Convictions




Notes: Each panel plots selected event study coefficients for the share with any wages > $0 around the latest event, following specification 1 in the text. This event is the charge date for non-convictions and disposition date for convictions. Coefficients are relative to -2 years before the event. We run separate event studies for each state in each panel. Data from 2000-2020. The sample is restricted to events occurring between 2003-2011.

Figure 4: Event Study of Any Wages Around First Criminal History Event, By 2-digit NAICS Industry

(a) Non-Convictions




(b) Convictions

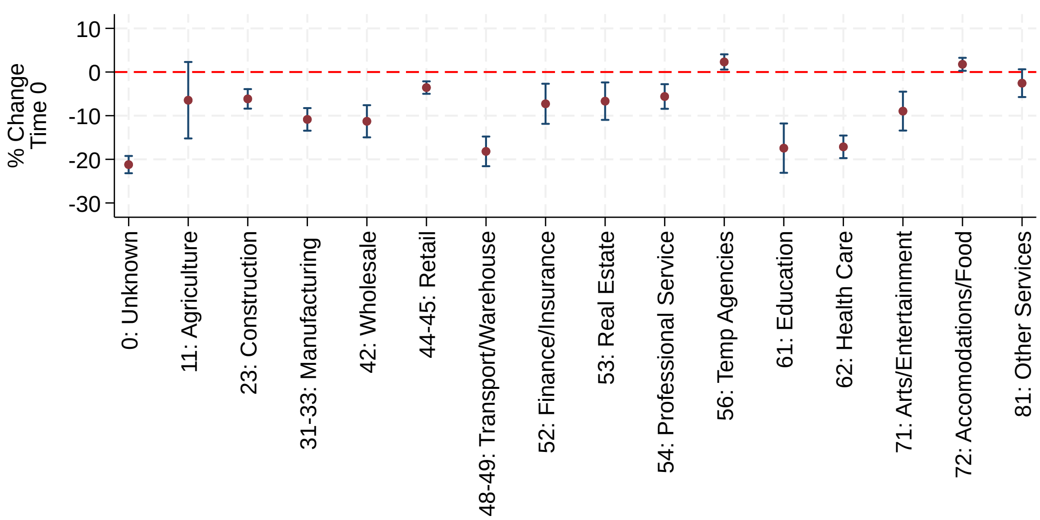
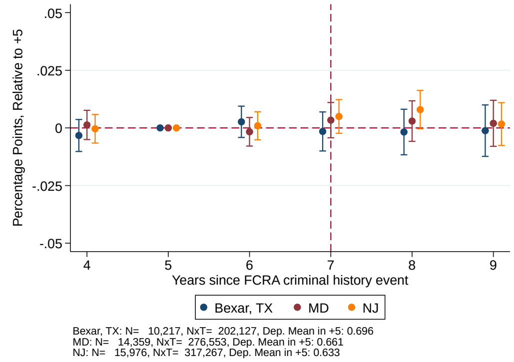



Figure reports event study estimates in the year of someone's first criminal history event of a W-2 issued by a payer firm in the specified 2-digit NAICS code based on the firms' tax return in that year. For this analysis, we restrict the full sample to have been 18 by the time the first charge appears in the data for both non-convictions and conviction. Data from 2000-2020. The sample is restricted to events occurring between 2003-2018. We run separate event studies by industry around the criminal history event, and divide by the mean share in the in the industry in -2 and multiply by 100 to convert to a percent change.

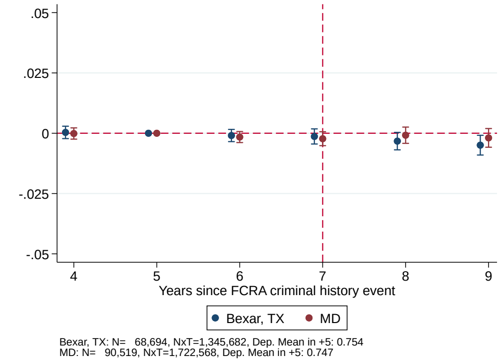
Figure 5: FCRA Event Study of Any Wages Around Removal (Year 7)

Note: MD has State FCRA for Convictions

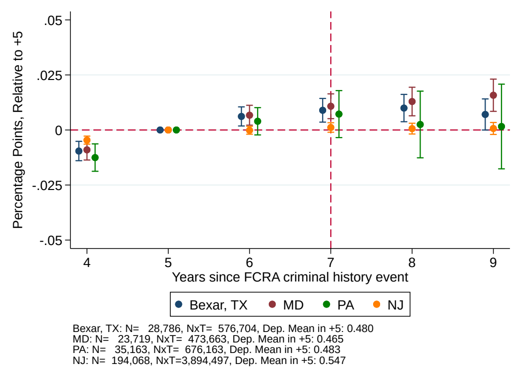
(a) Felony Non-Convictions, no other convictions



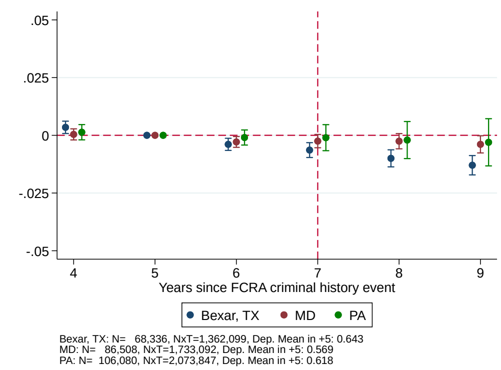

(b) Mis. Non-Convictions, no other convictions




(c) Felony Convictions



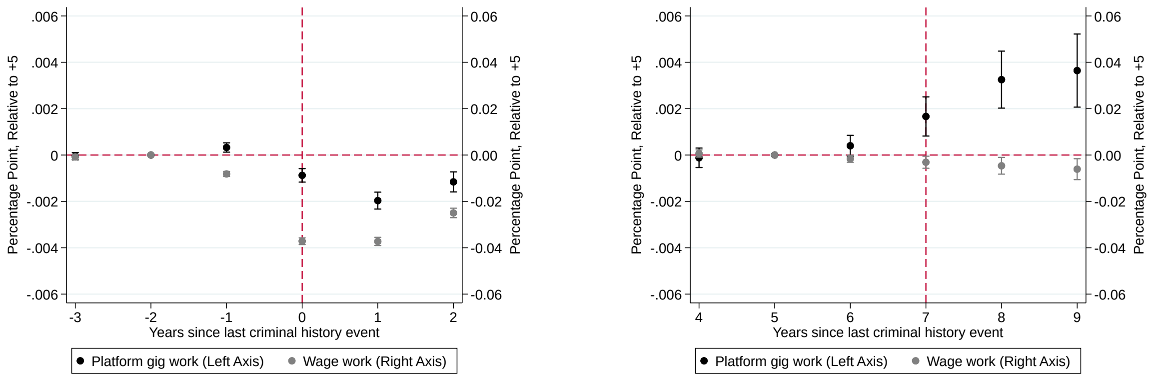

(d) Mis. Convictions




Notes: Each panel plots selected event study coefficients for the share with any wages > \$0 around 7 years after the event, following specification **??** in the text. Timing from the event is based on the charge date for non-convictions and disposition date for convictions. Coefficients are relative to +5 periods after the event (2 years prior to the year 7 FCRA event, if applicable). We run separate event studies for each state in each panel. Data from 2000-2020. The sample is restricted to events occurring between 1996-2011 to ensure the regression is balanced in the event window.

Figure 6: FCRA Event-Study Estimates: Any Gig Platform Work

“Time 0” Event FCRA “Time 7” Event

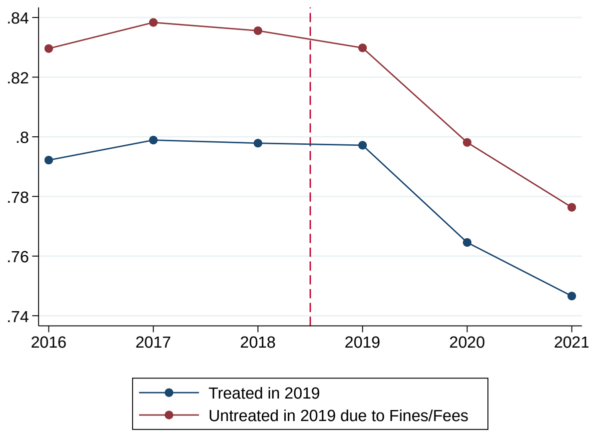



Notes: Each panel plots selected event study coefficients for the share with any gig income > \$0 or wage income > \$0 in different windows around the latest event. Events are defined as charge dates for non-convictions and disposition dates for convictions. The left panel is restricted to latest events from 2015-2018 and focuses on the period around the initial event. The right panel is restricted to latest events from 2008-2011 and focuses on the period 7 years after the latest event. The data are restricted to the period since 2012. We pool all charges for this figure.

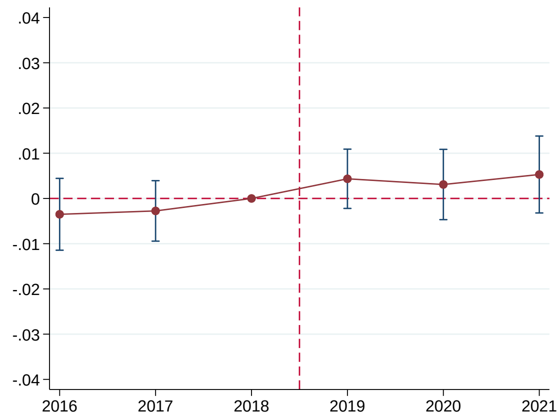
Figure 7: Impact of PA Clean Slate Reductions on Employment Outcomes

Any Wages > \$0

(a) Raw Data

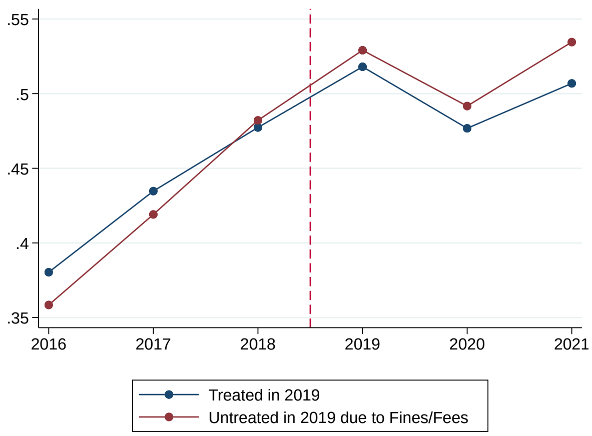



(b) Event-Study Estimates

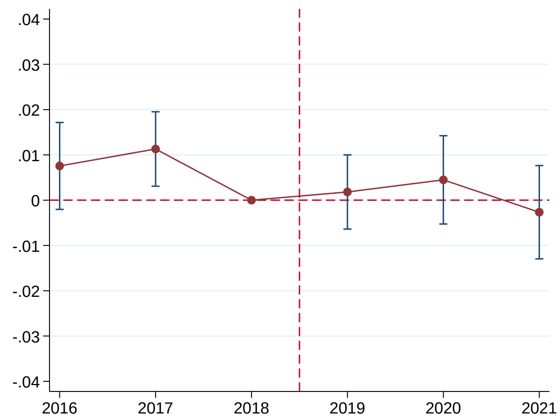



Any Wages > \$15,000

(c) Raw Data




(d) Event-Study Estimates




Notes: Figure reports raw means and event-study estimates for those who had their non-convictions cleared by PA's Clean Slate law by 2020, compared with those who did not. Data from 2016-2021. Sample is restricted to ages 18-25 to ensure they had no other prior convictions by the start of our charge data, which begins in 2008.

# Tables

Table 1: Survey of Hiring Professionals on Willingness to Hire Misd. vs. Felony Convictions

| | Drug Conviction | | | Theft Conviction | | |
|---|---|---|---|---|---|---|
| | Misd | Felony | Misd-Felony | Misd | Felony | Misd-Felony |
| *Prob Would Hire if Crime Was...* | | | | | | |
| 1-3 Years Ago | 0.42 | 0.29 | 0.13** | 0.28 | 0.14 | 0.14*** |
| | | | (0.05) | | | (0.04) |
| 4-7 Years Ago Years Ago | 0.75 | 0.63 | 0.12** | 0.61 | 0.51 | 0.10* |
| | | | (0.05) | | | (0.05) |
| 8-10 Years Ago | 0.90 | 0.82 | 0.08** | 0.82 | 0.76 | 0.06 |
| | | | (0.04) | | | (0.05) |
| N | 119 | 133 | | 125 | 128 | |
| | Drug Non-Conviction | | | Theft Non-Conviction | | |
| | Misd | Felony | Misd-Felony | Misd | Felony | Misd-Felony |
| *Prob Would Hire if Crime Was...* | | | | | | |
| 1-3 Years Ago | 0.60 | 0.49 | 0.11** | 0.45 | 0.42 | 0.03 |
| | | | (0.05) | | | (0.04) |
| 4-7 Years Ago Years Ago | 0.83 | 0.78 | 0.05 | 0.72 | 0.77 | -0.05 |
| | | | (0.05) | | | (0.05) |
| 8-10 Years Ago | 0.89 | 0.86 | 0.03 | 0.86 | 0.90 | -0.04 |
| | | | (0.04) | | | (0.05) |
| N | 127 | 126 | | 124 | 121 | |

Notes: Survey of 1003 hiring professionals with experience in the United States in the past 5 years. Each respondent was randomly assigned to being asked about preferences for hiring someone with one of 8 potential criminal histories: (drug x theft) + (misd x felony) + (conviction x non-conviction). This table focuses on the 505 randomly asked about convictions. The question text was "You intend to hire a candidate for an open entry-level position at the most recent firm at which you had hiring experience. Through the hiring process, you decide that this candidate is well qualified for the position. You are ready to extend an offer to the candidate. However, you learn that the candidate was charged with [**crime type**] [**X years ago**] and was convicted. How likely are you to recommend that the company hire the candidate?" Choices were: Definitely will, probability will, probability will not, definitely will not. The respondent was asked this question for X from 1 through 10 on the same page. This table combines "Definitely will" and "probably will," and shows the average probability the respondent reported would hire in bins of years.

Table 2: Summary Statistics: Last-Event Analysis Estimation Sample

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Last Event is Non-Conv & No Other Conv | | Last Event is Conv. | |
| | Felony | Misdemeanor | Felony | Misdemeanor |
| Male | 0.648 | 0.643 | 0.793 | 0.765 |
| *5 Years After Charge/Disposition:* | | | | |
| Age | 30.95 | 30.61 | 38.18 | 39.69 |
| Any Wages | 0.659 | 0.743 | 0.525 | 0.608 |
| Wages>$15k | 0.390 | 0.499 | 0.279 | 0.399 |
| Avg. Wages | 14,564 | 19,647 | 10,803 | 16,533 |
| Any 1099 NEC | 0.067 | 0.086 | 0.053 | 0.079 |
| Filed Taxes | 0.639 | 0.739 | 0.479 | 0.589 |
| Any SE Income | 0.073 | 0.078 | 0.049 | 0.061 |
| Total Obs | 40,552 | 171,725 | 280,854 | 260,515 |

Notes: Uses charges in the case of non-convictions, or disposition dates in the case of convictions. Sample is restricted to charges or dispositions from 1996 to 2011.

Table 3: Test for Deviations from Trend Around "Year 7" FCRA Event

(a) Mis. non-convictions and no other convictions, MD and Bexar, TX

| | (1) Any Wages>$0 | (2) ...>$7,500 | (3) ...>$15,000 | (4) Any Gig | (5) Any Other 1099 | (6) Files 1040 | (7) Files SE |
|---|---|---|---|---|---|---|---|
| +7 Trend Deviation | -0.002 | -0.004$^{a}$ | -0.004 | -0.001 | 0.004* | -0.001 | 0.001 |
| (S.E.) | (0.002) | (0.003) | (0.003) | (0.001) | (0.002) | (0.002) | (0.002) |
| +8 Trend Deviation | -0.002 | -0.007$^{a}$ | -0.004 | -0.001 | 0.006* | -0.000 | 0.002 |
| (S.E.) | (0.003) | (0.003) | (0.003) | (0.002) | (0.003) | (0.003) | (0.002) |
| N | 159,194 | 159,194 | 159,194 | 57,246 | 159,194 | 159,194 | 159,194 |
| NxT | 3,068,250 | 3,068,250 | 3,068,250 | 515,235 | 3,068,250 | 3,068,250 | 3,068,250 |

(b) Felony non-convictions and no other convictions, MD, NJ, and Bexar, TX

| | (1) Any Wages>$0 | (2) ...>$7,500 | (3) ...>$15,000 | (4) Any Gig | (5) Any Other 1099 | (6) Files 1040 | (7) Files SE |
|---|---|---|---|---|---|---|---|
| +7 Trend Deviation | 0.001 | 0.006 | -0.009$^{a}$ | -0.001 | 0.006$^{a}$ | -0.004 | 0.001 |
| (S.E.) | (0.005) | (0.005) | (0.005) | (0.002) | (0.003) | (0.005) | (0.003) |
| +8 Trend Deviation | 0.002 | 0.008 | -0.013$^{a}$ | 0.000 | 0.006$^{a}$ | -0.003 | -0.000 |
| (S.E.) | (0.007) | (0.007) | (0.007) | (0.003) | (0.005) | (0.007) | (0.004) |
| N | 40,540 | 40,540 | 40,540 | 13,480 | 40,540 | 40,540 | 40,540 |
| NxT | 795,947 | 795,947 | 795,947 | 121,324 | 795,947 | 795,947 | 795,947 |

Notes: Table reports results from a test of whether the event study coefficients 7-8 years after the last charge are different from a linear trend. Specifically, "+7 Trend Deviation" reports $2 \times \beta_{+4} + \beta_{+7}$, and "+8 Trend Deviation" reports $3 \times \beta_{+4} + \beta_{+8}$. With the exception of Column (4), data is from 2000-2020, and the sample is restricted to events occurring between 1996-2011 to ensure the regression is balanced in the window around the event. Column (4) restricts to data from 2012 and events from 2015-2018. Standard errors clustered on individual are reported in parentheses. $^{a}$ p<0.1, * p<0.05, ** p<0.01, *** p<0.001

Table 4: Test for Deviations from Trend Around "Year 7" in MD v All Other States

(a) Convictions in NJ, PA and Bexar, TX [No state FCRA]

| | (1) Any Wages>$0 | (2) ...>$7,500 | (3) ...>$15,000 | (4) Any Gig | (5) Any Other 1099 | (6) Files 1040 | (7) Files SE |
|---|---|---|---|---|---|---|---|
| +7 Trend Deviation | -0.006*** | -0.006*** | -0.003* | 0.002*** | -0.001 | -0.010*** | -0.001 |
| (S.E.) | (0.002) | (0.001) | (0.001) | (0.000) | (0.001) | (0.001) | (0.001) |
| +8 Trend Deviation | -0.011*** | -0.010*** | -0.006* | 0.004*** | -0.002 | -0.012*** | -0.001 |
| (S.E.) | (0.002) | (0.002) | (0.002) | (0.000) | (0.001) | (0.002) | (0.001) |
| N | 430,324 | 430,324 | 430,324 | 227,737 | 430,324 | 430,324 | 430,324 |
| NxT | 8,584,190 | 8,584,190 | 8,584,190 | 2,050,582 | 8,584,190 | 8,584,190 | 8,584,190 |

(b) Convictions in MD [State FCRA]

| | (1) Any Wages>$0 | (2) ...>$7,500 | (3) ...>$15,000 | (4) Any Gig | (5) Any Other 1099 | (6) Files 1040 | (7) Files SE |
|---|---|---|---|---|---|---|---|
| +7 Trend Deviation | -0.003 | -0.008** | -0.010*** | 0.002 | 0.000 | -0.008** | 0.001 |
| (S.E.) | (0.003) | (0.003) | (0.003) | (0.001) | (0.002) | (0.003) | (0.002) |
| +8 Trend Deviation | -0.004 | -0.012** | -0.015*** | 0.004** | 0.002 | -0.009** | 0.000 |
| (S.E.) | (0.004) | (0.004) | (0.004) | (0.001) | (0.003) | (0.004) | (0.002) |
| N | 110,227 | 110,227 | 110,227 | 34,881 | 110,227 | 110,227 | 110,227 |
| NxT | 2,206,755 | 2,206,755 | 2,206,755 | 312,769 | 2,206,755 | 2,206,755 | 2,206,755 |

Notes: Table reports results from a test of whether the event study coefficients 7-8 years after the disposition are different from a linear trend. Specifically, "+7 Trend Deviation" reports $2 \times \beta_{+4} + \beta_{+7}$, and "+8 Trend Deviation" reports $3 \times \beta_{+4} + \beta_{+8}$. With the exception of column (4), data is from 2000-2020, and the sample is restricted to events occurring between 1996-2011 to ensure the regression is balanced in the window around the event. Column (4) restricts to data from 2012 and events from 2015-2018. Standard errors clustered on individual are reported in parentheses. [a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

Table 5: PA Clean Slate Estimation Sample, Summary Statistics

| | Pooled | Treated | Control |
|---|---|---|---|
| Male | 0.645 | 0.631 | 0.682 |
| Black | 0.303 | 0.314 | 0.276 |
| Age | 26.39 | 26.77 | 25.45 |
| Years Since Charge | 4.781 | 5.122 | 3.941 |
| Any Wages | 0.809 | 0.798 | 0.835 |
| Wages>$15k | 0.480 | 0.478 | 0.484 |
| Avg. Wages | 19,531 | 19,566 | 19,445 |
| Any 1099 NEC, non platform | 0.056 | 0.056 | 0.056 |
| Any platform gig | 0.011 | 0.011 | 0.010 |
| Filed Taxes | 0.713 | 0.706 | 0.731 |
| Any SE Income | 0.051 | 0.053 | 0.047 |
| Total Obs | 45,877 | 32,677 | 13,221 |

Summary statistics for PA estimation sample. This sample had charges that were eligible to be dismissed or withdrawn, and were 18-25 at time of the charge.

Table 6: Impact of PA Clean Slate Reductions on Employment Outcomes

(a) DD Estimates

| | (1) Any Wages>$0 | (2) ...>$7,500 | (3) ...>$15,000 | (4) Any Gig | (5) Any Other 1099 | (6) Files 1040 | (7) Files SE |
|---|---|---|---|---|---|---|---|
| Post (2019-2021) × Cleared | 0.00424 | -0.00131 | 0.00123 | 0.000686 | 0.000542 | 0.00767[a] | -0.00415[a] |
| | (0.00326) | (0.00401) | (0.00407) | (0.00150) | (0.00227) | (0.00401) | (0.00214) |
| 2017 × Cleared | -0.00275 | -0.000528 | 0.0113** | -0.000974 | -0.00657** | -0.00575 | -0.00515* |
| | (0.00341) | (0.00426) | (0.00419) | (0.00123) | (0.00249) | (0.00407) | (0.00225) |
| 2016 × Cleared | -0.00350 | -0.000584 | 0.00755 | 0.00155 | -0.00393 | -0.00545 | -0.00234 |
| | (0.00405) | (0.00501) | (0.00490) | (0.00129) | (0.00277) | (0.00468) | (0.00244) |
| Dep. Mean (2018) | 0.809 | 0.618 | 0.480 | 0.011 | 0.056 | 0.713 | 0.051 |
| N | 45,877 | 45,877 | 45,877 | 45,877 | 45,877 | 45,877 | 45,877 |
| NxT | 275,634 | 275,634 | 275,634 | 275,634 | 275,634 | 275,634 | 275,634 |
| Age Controls | X | X | X | X | X | X | X |
| Indiv. FE | X | X | X | X | X | X | X |
| Year FE | X | X | X | X | X | X | X |

(b) By months since charge

| | (1) Any Wages>$0 | (2) ...>$7,500 | (3) ...>$15,000 | (4) Any Gig | (5) Any Other 1099 | (6) Files 1040 | (7) Files SE |
|---|---|---|---|---|---|---|---|
| Post (2019-2021) × Cleared | 0.00249 | -0.00830 | -0.0233* | 0.00724* | -0.00253 | 0.0212* | -0.00202 |
| | (0.00779) | (0.00970) | (0.00953) | (0.00355) | (0.00472) | (0.00924) | (0.00449) |
| Post (2019-2021) × Cleared × Months since charge | -0.00000386 | 0.000126 | 0.000450** | -0.000115* | 0.0000561 | -0.000226 | -0.0000413 |
| | (0.000121) | (0.000147) | (0.000146) | (0.0000548) | (0.0000750) | (0.000141) | (0.0000736) |
| Post (2019-2021) × Months since charge | 0.000340** | -0.000112 | -0.000474*** | 0.0000673 | -0.0000542 | 0.0000323 | 0.0000627 |
| | (0.000109) | (0.000134) | (0.000134) | (0.0000495) | (0.0000694) | (0.000130) | (0.0000675) |
| 2017 × Cleared | -0.00341 | -0.000483 | 0.0116** | -0.000948 | -0.00654** | -0.00550 | -0.00522* |
| | (0.00341) | (0.00426) | (0.00420) | (0.00124) | (0.00249) | (0.00408) | (0.00225) |
| 2016 × Cleared | -0.00482 | -0.000488 | 0.00818[a] | 0.00160 | -0.00387 | -0.00496 | -0.00247 |
| | (0.00406) | (0.00502) | (0.00491) | (0.00130) | (0.00277) | (0.00469) | (0.00244) |

Notes: Table reports difference-in-differences results comparing outcomes for individuals who had all their non-convictions cleared by PA's Clean Slate law by 2020, compared with those who did not. Data from 2016-2021. Sample is restricted to ages 18-25 to ensure they had no other prior convictions by the start of our charge data, which begins in 2008. Standard errors clustered on individual are reported in parentheses. [a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

# Appendix

## A Additional Figures and Tables

Table A.1: Survey 1 of Hiring Professionals on Criminal Background Check Procedures

| | Yes | No | Unsure |
|---|---|---|---|
| *Full Sample, N=808* | | | |
| Perform criminal background check? | 0.70 | 0.23 | 0.07 |
| Knowledgeable about background check procedure? | 0.59 | 0.39 | 0.02 |
| *Background Check & Knowledgeable Sample, N=383* | | | |
| **How Background Check Performed?** | | | |
| Known CRA (able to name) | 0.78 | | |
| Unknown external agency | 0.07 | | |
| Fingerprint/state official court | 0.06 | | |
| Perform "in-house" | 0.09 | | |
| **Distinguish between felony and misdemeanor?** | | | |
| More likely to hire if misdemeanor than felony | 0.73 | | |
| Did not distinguish | 0.25 | | |
| Other/No Response | 0.03 | | |

**Notes:** Survey of 808 individuals with hiring experience in the United States in the past 5 years asked about firms' criminal background check practices for **entry-level** positions.

Table A.2: Summary Statistics: First-Event Analysis Estimation Sample, Before and After First Charge/Disposition

(a) Misdemeanor Non-convictions , MD and Bexar, TX

| | Two Years Before | Year After | Five Years After |
|---|---|---|---|
| Files 1040 | 0.711 | 0.682 | 0.659 |
| | | (0.002) | (0.003) |
| Has Labor Earnings | 0.804 | 0.780 | 0.746 |
| | | (0.001) | (0.002) |
| Has W2 Earnings | 0.768 | 0.745 | 0.716 |
| | | (0.001) | (0.002) |
| W2 Earnings (1000 $) | 14.594 | 13.147 | 12.794 |
| | | (0.067) | (0.139) |
| Has SE Earnings | 0.068 | 0.065 | 0.058 |
| | | (0.001) | (0.002) |
| SE if Has Earnings | 0.084 | 0.084 | 0.082 |
| | | (0.001) | (0.002) |
| Has 1099 NEC | 0.076 | 0.077 | 0.067 |
| | | (0.001) | (0.002) |
| EITC Claimant | 0.247 | 0.240 | 0.233 |
| | | (0.001) | (0.002) |
| N | 160072 | | |

(b) Felony Non-convictions, MD, NJ, and Bexar, TX

| | Two Years Before | Year After | Five Years After |
|---|---|---|---|
| Files 1040 | 0.635 | 0.574 | 0.592 |
| | | (0.003) | (0.006) |
| Has Labor Earnings | 0.754 | 0.691 | 0.682 |
| | | (0.003) | (0.005) |
| Has W2 Earnings | 0.719 | 0.657 | 0.652 |
| | | (0.003) | (0.005) |
| W2 Earnings (1000 $) | 11.593 | 8.639 | 9.564 |
| | | (0.122) | (0.241) |
| Has SE Earnings | 0.067 | 0.063 | 0.057 |
| | | (0.002) | (0.003) |
| SE if Has Earnings | 0.089 | 0.095 | 0.089 |
| | | (0.003) | (0.004) |
| Has 1099 NEC | 0.061 | 0.054 | 0.051 |
| | | (0.002) | (0.003) |
| EITC Claimant | 0.266 | 0.248 | 0.254 |
| | | (0.003) | (0.005) |
| N | 37891 | | |

(c) Misdemeanor Convictions, MD, NJ, PA, and Bexar, TX

| | Two Years Before | Year After | Five Years After |
|---|---|---|---|
| Files 1040 | 0.654 | 0.593 | 0.598 |
| | | (0.002) | (0.004) |
| Has Labor Earnings | 0.792 | 0.749 | 0.725 |
| | | (0.002) | (0.004) |
| Has W2 Earnings | 0.770 | 0.728 | 0.703 |
| | | (0.002) | (0.004) |
| W2 Earnings (1000 $) | 11.084 | 9.125 | 9.419 |
| | | (0.082) | (0.172) |
| Has SE Earnings | 0.046 | 0.045 | 0.046 |
| | | (0.001) | (0.002) |
| SE if Has Earnings | 0.058 | 0.062 | 0.068 |
| | | (0.001) | (0.003) |
| Has 1099 NEC | 0.062 | 0.061 | 0.062 |
| | | (0.001) | (0.002) |
| EITC Claimant | 0.187 | 0.166 | 0.177 |
| | | (0.002) | (0.003) |
| N | 108397 | | |

(d) Felony Convictions, MD, PA, and Bexar, TX

| | Two Years Before | Year After | Five Years After |
|---|---|---|---|
| Files 1040 | 0.550 | 0.427 | 0.478 |
| | | (0.002) | (0.003) |
| Has Labor Earnings | 0.714 | 0.584 | 0.607 |
| | | (0.002) | (0.003) |
| Has W2 Earnings | 0.690 | 0.565 | 0.587 |
| | | (0.002) | (0.003) |
| W2 Earnings (1000 $) | 9.358 | 4.891 | 5.765 |
| | | (0.064) | (0.121) |
| Has SE Earnings | 0.045 | 0.034 | 0.036 |
| | | (0.001) | (0.002) |
| SE if Has Earnings | 0.063 | 0.062 | 0.067 |
| | | (0.001) | (0.002) |
| Has 1099 NEC | 0.046 | 0.034 | 0.036 |
| | | (0.001) | (0.001) |
| EITC Claimant | 0.194 | 0.145 | 0.161 |
| | | (0.002) | (0.003) |
| N | 123429 | | |

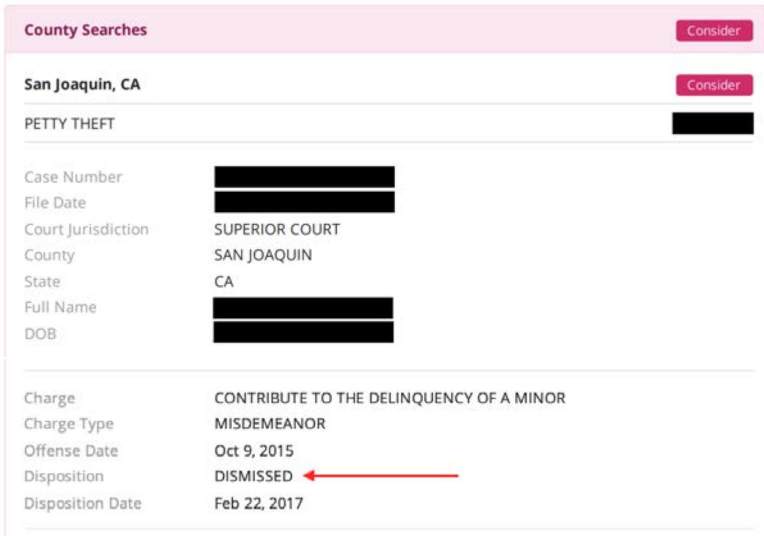
Notes: Table displays mean outcome levels two years prior to an initial criminal history event, where the type of event differs across panels. The sample is the same as in Figure 2, pooling across jurisdictions as specified in table headers. Table also presents mean outcomes one and five years after the specified event implied by our event study estimates of Equation 1, which estimate the change relative to period -2 controlling for aging and macroeconomic conditions. Specifically, we add our estimates of $\beta^1$ and $\beta^5$ from Equation 1 for each event type (displayed in Figure A.2) to the means two years prior to the event. Standard errors reflect estimation of event-study coefficients but not estimation of sample means in period -2. For this analysis, we restrict the full sample to have been 18 by the time the first charge appears in the data for both non-convictions and conviction. Data from 2000-2020. The sample is restricted to events occurring between 1996-2011 to correspond to the event-study sample used to estimate Equation 1. W2 earnings are winsorized at the 99th percentile.

Table A.3: Summary Statistics: Last-Event Analysis Estimation Sample, Two Years Before Last Charge/Disposition.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Last Event is Non-Conv & No Other Conv | | Last Event is Conv. | |
| | Misdemeanor | Felony | Misdemeanor | Felony |
| *2 Years Before Charge/Disposition:* | | | | |
| Any Wages | 0.781 | 0.725 | 0.693 | 0.602 |
| Any 1099 NEC | 0.079 | 0.065 | 0.074 | 0.051 |
| Filed Taxes | 0.716 | 0.639 | 0.627 | 0.475 |
| Any SE Income | 0.059 | 0.061 | 0.050 | 0.040 |
| Total Obs | 87,681 | 21,607 | 204,084 | 183,069 |

Notes: Table displays summary statics for sample in Figure 3, pooling individuals across the states presented within each panel of Figure 3.

Figure A.1: Example Background Check from a Major Provider



**County Searches** Consider

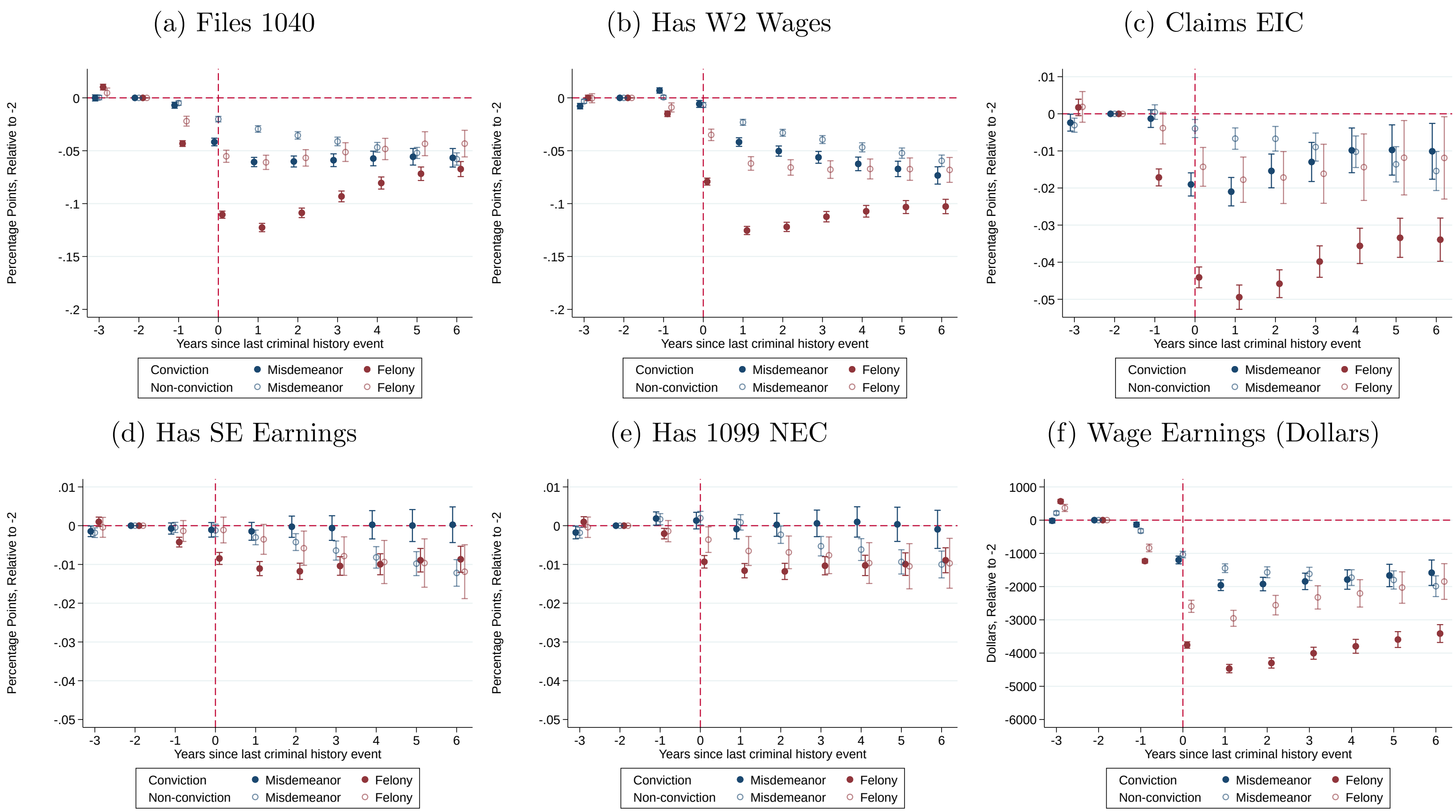
**San Joaquin, CA** Consider

PETTY THEFT [redacted]

| | |
|---|---|
| Case Number | [redacted] |
| File Date | [redacted] |
| Court Jurisdiction | SUPERIOR COURT |
| County | SAN JOAQUIN |
| State | CA |
| Full Name | [redacted] |
| DOB | [redacted] |

| | |
|---|---|
| Charge | CONTRIBUTE TO THE DELINQUENCY OF A MINOR |
| Charge Type | MISDEMEANOR |
| Offense Date | Oct 9, 2015 |
| Disposition | DISMISSED |
| Disposition Date | Feb 22, 2017 |

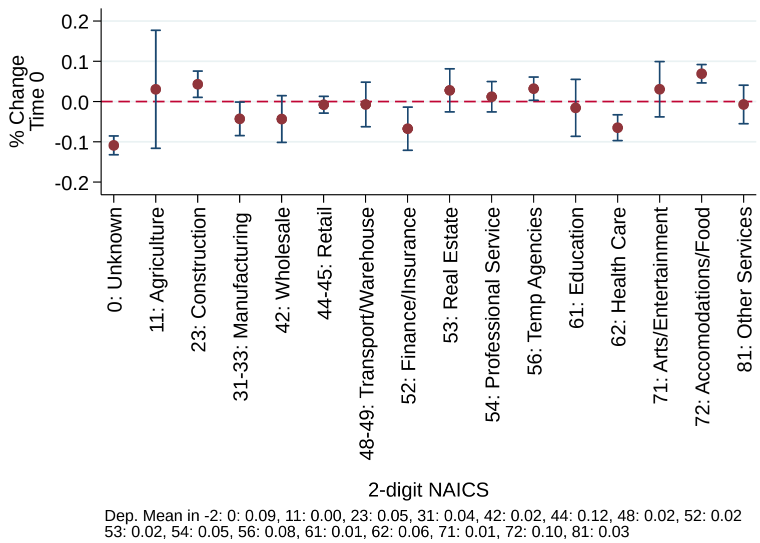
Figure A.2: Reductions in Tax Filing After First Event, Additional Outcomes



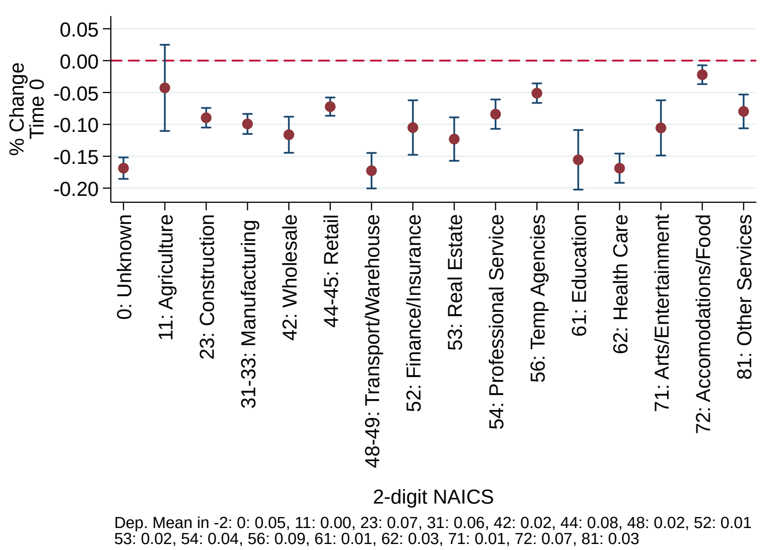

Notes: Each panel plots selected event study coefficients for the specified outcome after an initial criminal history event following Equation 1 in the text, where the type of event is as specified in the legend. Analysis pool data across with data on specified events as described in table headings in Appendix Table A.2. For this analysis, we restrict the full sample to have been 18 by the time the first charge appears in the data for both non-convictions and conviction. This event is the charge date for non-convictions and disposition date for convictions. Coefficients are relative to 2 years before the event. We run separate event studies for each event type and outcome. Data from 2000-2020. The sample is restricted to events occurring between 1996-2011 to ensure the regression is balanced in the event window.

Figure A.3: Event Study of Any Wages Around Last Criminal History Event, By 2-digit NAICS Industry

(a) Non-Convictions



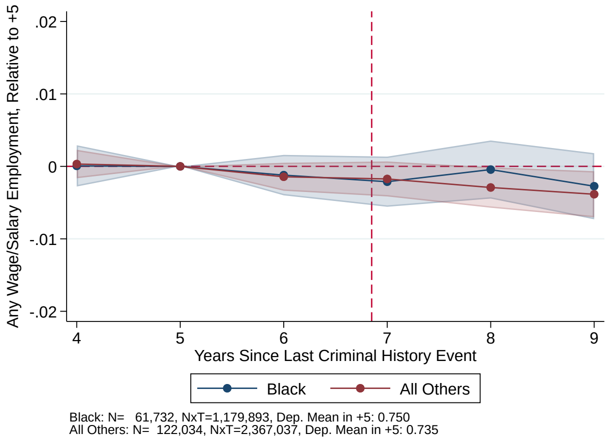

(b) Convictions



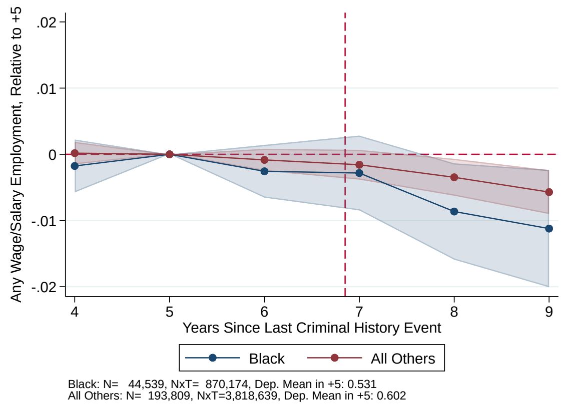

Notes: Figure reports event study estimates at time 0 of W-2 issued by a payer firm in the specified 2-digit NAICS code based on the firms' tax return in that year. We run separate event studies by industry around the criminal history event, and divide by the mean share in the in the industry in -2 to convert to percent.

Figure A.4: FCRA Event Study of Any Wages Around Removal (Year 7), By Race

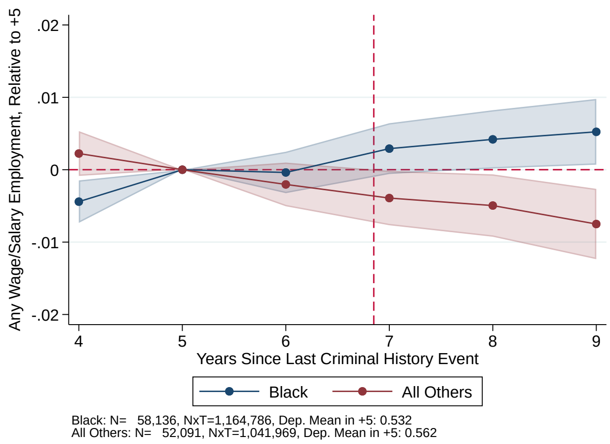
(a) Non-Convictions




(b) Convictions in PA, NJ and TX




(c) Convictions in MD

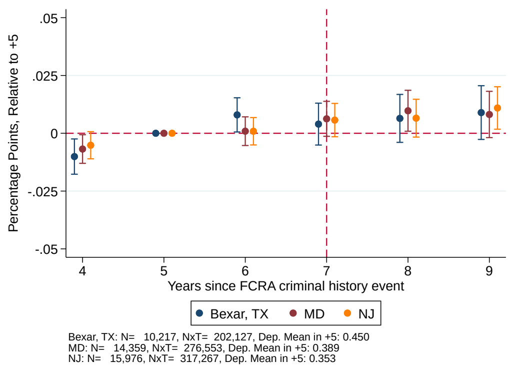



Notes: Each panel plots selected event study coefficients for the share with any wages around 7 years after the event, following specification **??** in the text. Timing from the event is based on the charge date for non-convictions and disposition date for convictions. Coefficients are relative to +5 periods after the event (2 years prior to the year 7 FCRA event, if applicable). We run separate event studies for each state in each panel. Data from 2000-2020. The sample is restricted to events occurring between 1996-2011 to ensure the regression is balanced in the event window. We run separate event studies for Black individuals and all those of all other racial identities.

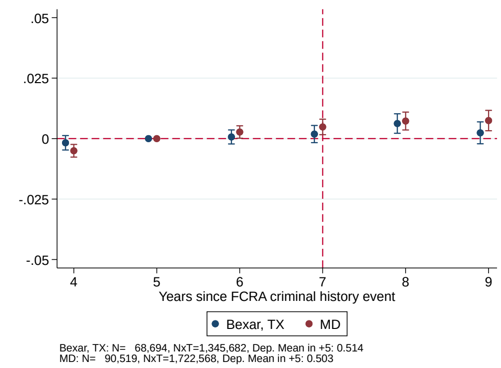
Figure A.5: FCRA Event Study of Any Wages >$15,000 Around Removal (Year 7)

Note: MD has State FCRA for Convictions

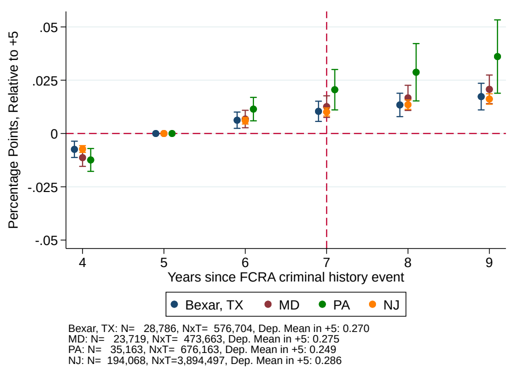
(a) Felony Non-Convictions, no other convictions



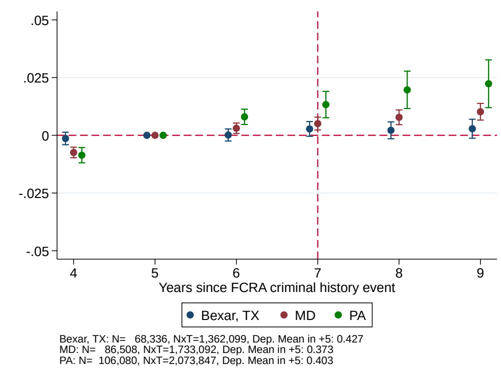

(b) Mis. Non-Convictions, no other convictions




(c) Felony Convictions




(d) Misdemeanor Convictions

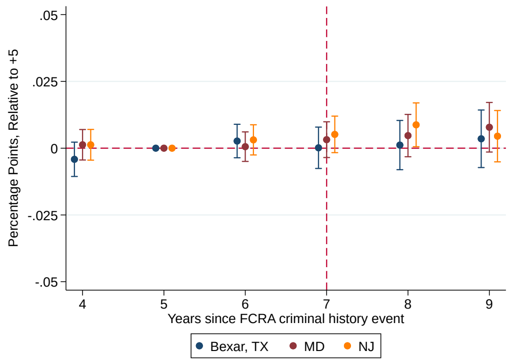



Notes: Each panel plots selected event study coefficients for the share with any wages > $15,000 around 7 years after the event, following specification **??** in the text. Timing from the event is based on the charge date for non-convictions and disposition date for convictions. Coefficients are relative to +5 periods after the event (2 years prior to the year 7 FCRA event, if applicable). We run separate event studies for each state in each panel. Data from 2000-2020. The sample is restricted to events occurring between 1996-2011 to ensure the regression is balanced in the event window.

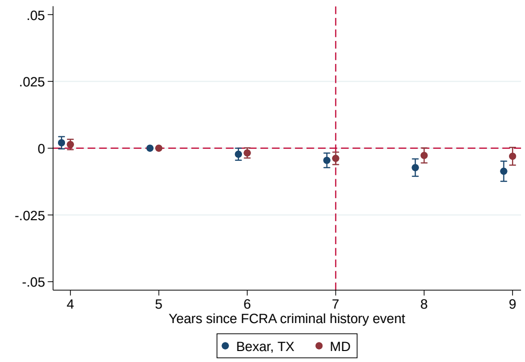
Figure A.6: Robustness to Alternative DD-Estimators: FCRA Event Study of Any Wages Around Removal (Year 7)

Note: MD has State FCRA for Convictions

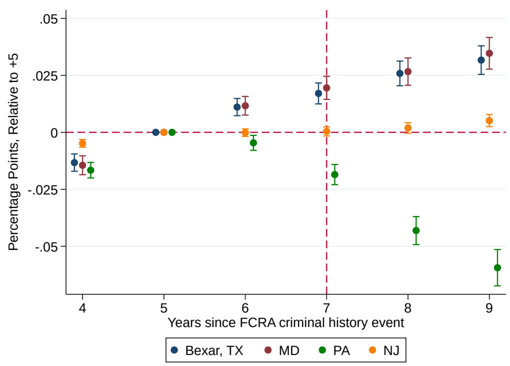
(a) Felony Non-Convictions, no other convictions



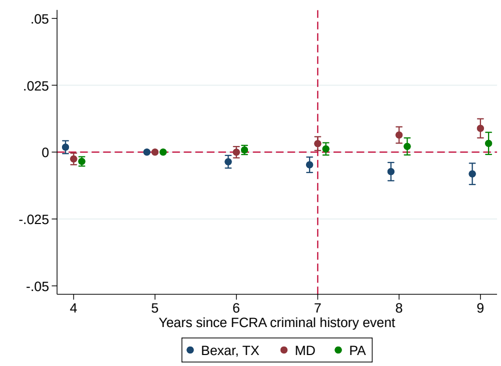

(b) Mis. Non-Convictions, no other convictions




(c) Felony Convictions



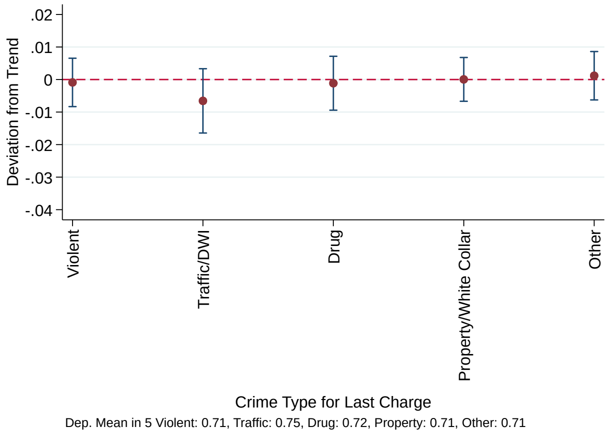

(d) Mis. Convictions



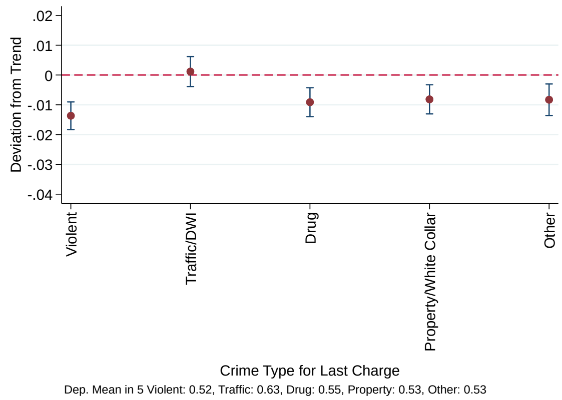

Notes: Each panel plots selected event study coefficients for the share with any wages > \$0 around 7 years after the event, from event-study estimation following Sun and Abraham (2020). Year 7 events occurring in 2021 are used as the last treated group. Timing from the event is based on the charge date for non-convictions and disposition date for convictions. Coefficients are relative to +5 periods after the event (2 years prior to the year 7 FCRA event, if applicable). We run separate event studies for each state in each panel. Data from 2000-2020. The sample is restricted to events occurring between 1996-2014.

Figure A.7: FCRA Event Study of Any Wages Around Removal (Year 7), Deviation from Trend, By Crime-Type of Last Conviction

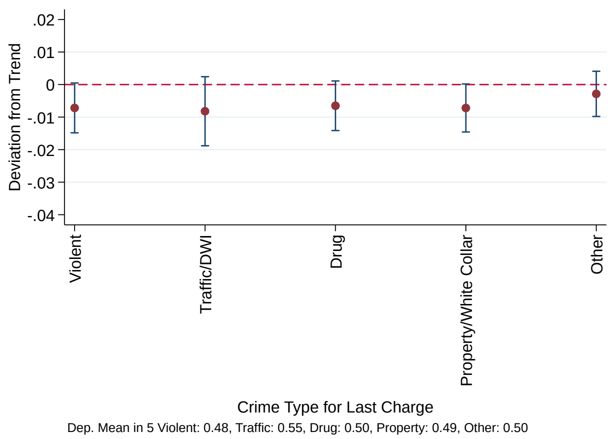
(a) Non-Convictions




(b) Convictions in PA, NJ and TX



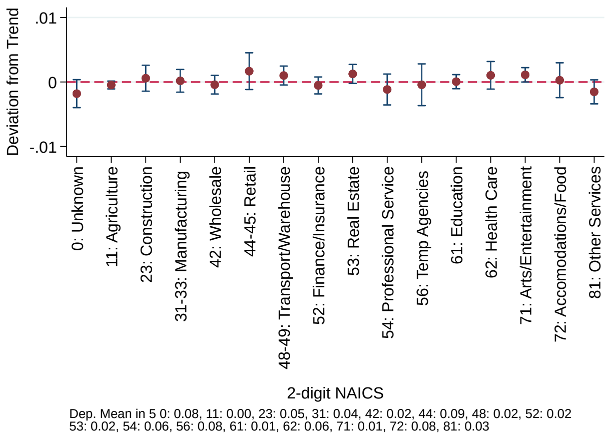

(c) Convictions in MD



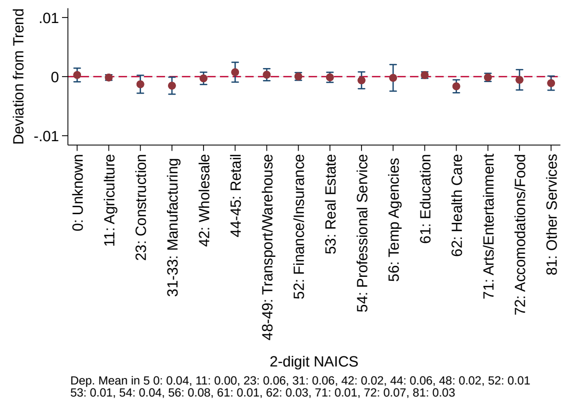

Notes: Figure reports results from a test of whether the event study coefficients 7 years after the last charge are different from a linear trend. Specifically, figure reports $2 \times \beta_{+4} + \beta_{+7}$.

Figure A.8: FCRA Event Study of Any Wages Around Removal (Year 7), Deviation from Trend, By 2-digit NAICS Industry

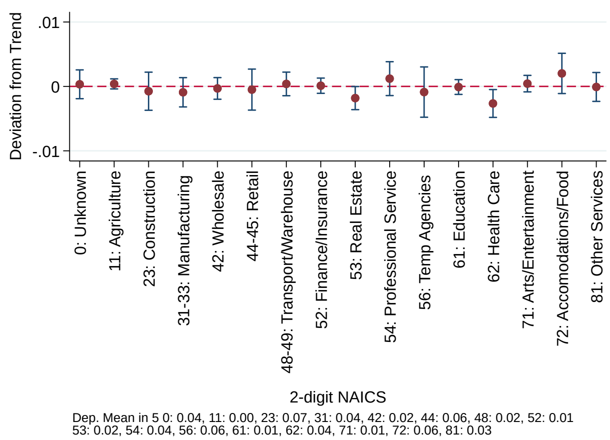
(a) Non-Convictions




(b) Convictions in PA, NJ and TX




(c) Convictions in MD

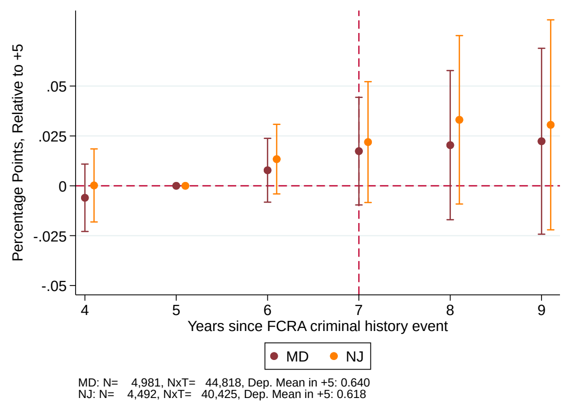



Notes: Figure reports results from a test of whether the event study coefficients 7 years after the last charge are different from a linear trend. Specifically, figure reports $2 \times \beta_{+4} + \beta_{+7}$.

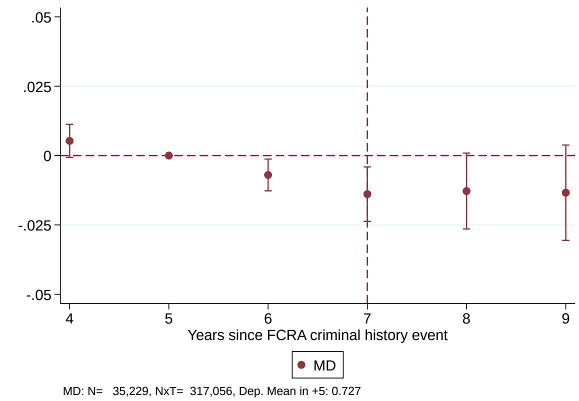
Figure A.9: FCRA Event Study of Any Wages Around Removal (Year 7) occurring between 2015-2018

Note: MD has State FCRA for Convictions

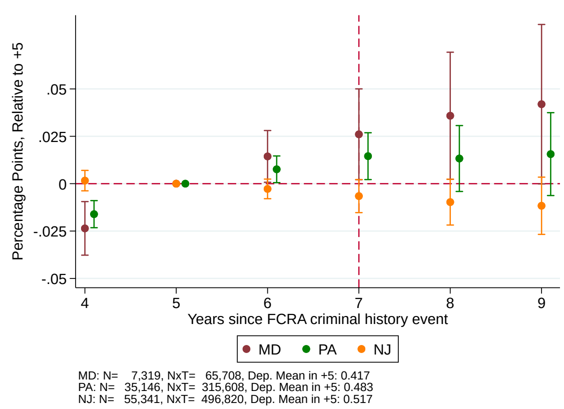
(a) Felony Non-Convictions, no other convictions



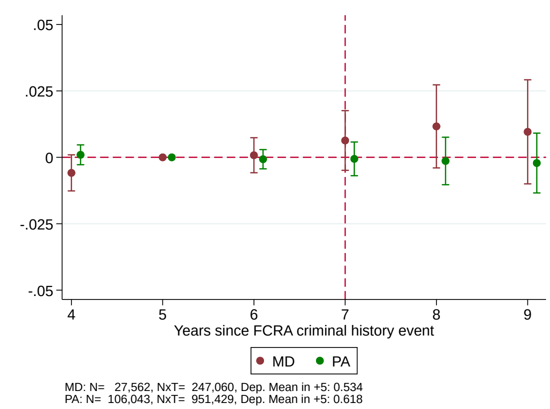

(b) Mis. Non-Convictions, no other convictions




(c) Felony Convictions




(d) Misdemeanor Convictions




Notes: For this figure, the sample is restricted to events occurring between 2008-2011 (removal (Year 7) occurring between 2015-2018).

Table A.4: Impact of PA Clean Slate Reductions on Employment Outcomes - Excluding Philadelphia

(a) DD Estimates

| | (1)<br>Any Wages>$0 | (2)<br>...>$7,500 | (3)<br>...>$15,000 | (4)<br>Any Gig | (5)<br>Any Other 1099 | (6)<br>Files 1040 | (7)<br>Files SE |
|---|---|---|---|---|---|---|---|
| Post (2019-2021) × Cleared | 0.00448 | -0.00285 | 0.00244 | -0.000251 | 0.000949 | -0.00115 | -0.00264 |
| | (0.00338) | (0.00419) | (0.00427) | (0.00151) | (0.00236) | (0.00415) | (0.00224) |
| 2017 × Cleared | -0.00251 | -0.00210 | 0.0101* | -0.00179 | -0.00551* | -0.00593 | -0.00589* |
| | (0.00353) | (0.00445) | (0.00441) | (0.00121) | (0.00259) | (0.00422) | (0.00233) |
| 2016 × Cleared | 0.000720 | -0.00159 | 0.00477 | 0.00146 | -0.00402 | -0.00361 | -0.00253 |
| | (0.00418) | (0.00524) | (0.00514) | (0.00127) | (0.00288) | (0.00484) | (0.00254) |
| Dep. Mean (2018) | 0.824 | 0.638 | 0.498 | 0.009 | 0.055 | 0.739 | 0.050 |
| N | 38,268 | 38,268 | 38,268 | 38,268 | 38,268 | 38,268 | 38,268 |
| NxT | 229,872 | 229,872 | 229,872 | 229,872 | 229,872 | 229,872 | 229,872 |
| Age Controls | X | X | X | X | X | X | X |
| Indiv. FE | X | X | X | X | X | X | X |
| Year FE | X | X | X | X | X | X | X |

(b) By months since charge

| | (1)<br>Any Wages>$0 | (2)<br>...>$7,500 | (3)<br>...>$15,000 | (4)<br>Any Gig | (5)<br>Any Other 1099 | (6)<br>Files 1040 | (7)<br>Files SE |
|---|---|---|---|---|---|---|---|
| Post (2019-2021) × Cleared | 0.00282 | -0.00349 | -0.00873 | 0.00397 | -0.000909 | 0.00232 | -0.000211 |
| | (0.00804) | (0.0102) | (0.0101) | (0.00362) | (0.00489) | (0.00957) | (0.00471) |
| Post (2019-2021) × Cleared × Months since charge | -0.00000208 | 0.0000133 | 0.000218 | -0.0000752 | 0.0000358 | -0.0000575 | -0.0000465 |
| | (0.000125) | (0.000154) | (0.000154) | (0.0000560) | (0.0000778) | (0.000146) | (0.0000771) |
| Post (2019-2021) × Months since charge | 0.000334** | -0.0000298 | -0.000357** | 0.0000541 | -0.0000538 | -0.00000542 | 0.0000666 |
| | (0.000111) | (0.000137) | (0.000137) | (0.0000500) | (0.0000708) | (0.000132) | (0.0000691) |
| 2017 × Cleared | -0.00305 | -0.00206 | 0.0105* | -0.00179 | -0.00546* | -0.00586 | -0.00595* |
| | (0.00353) | (0.00445) | (0.00442) | (0.00122) | (0.00259) | (0.00422) | (0.00234) |
| 2016 × Cleared | -0.000344 | -0.00152 | 0.00546 | 0.00144 | -0.00392 | -0.00347 | -0.00265 |
| | (0.00418) | (0.00524) | (0.00515) | (0.00128) | (0.00288) | (0.00485) | (0.00254) |

Notes: Table reports difference-in-differences results comparing outcomes for individuals who had all their non-convictions cleared by PA's Clean Slate law by 2020, compared with those who did not. Data from 2016-2021. Sample is restricted to ages 18-25 to ensure they had no other prior convictions by the start of our charge data, which begins in 2008. Standard errors clustered on individual are reported in parentheses. [a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

## Table A.5: Impact of PA Clean Slate Reductions on Employment Outcomes - Black Defendants

### (a) DD Estimates

| | (1) Any Wages>$0 | (2) ...>$7,500 | (3) ...>$15,000 | (4) Any Gig | (5) Any Other 1099 | (6) Files 1040 | (7) Files SE |
|---|---|---|---|---|---|---|---|
| Post (2019-2021) × Cleared | 0.00464 | 0.00672 | 0.00656 | 0.00147 | -0.000635 | 0.0137[a] | -0.00539 |
| | (0.00615) | (0.00803) | (0.00782) | (0.00357) | (0.00414) | (0.00832) | (0.00411) |
| 2017 × Cleared | -0.00914 | 0.00222 | 0.0181* | -0.000928 | -0.00922* | -0.0182* | -0.00520 |
| | (0.00621) | (0.00848) | (0.00796) | (0.00306) | (0.00454) | (0.00849) | (0.00422) |
| 2016 × Cleared | 0.00269 | 0.00317 | 0.00925 | 0.00493 | -0.00429 | -0.0110 | -0.00241 |
| | (0.00757) | (0.00957) | (0.00910) | (0.00319) | (0.00499) | (0.00945) | (0.00456) |
| Dep. Mean (2018) | 0.813 | 0.569 | 0.409 | 0.019 | 0.049 | 0.645 | 0.053 |
| N | 13,889 | 13,889 | 13,889 | 13,889 | 13,889 | 13,889 | 13,889 |
| NxT | 83,538 | 83,538 | 83,538 | 83,538 | 83,538 | 83,538 | 83,538 |
| Age Controls | X | X | X | X | X | X | X |
| Indiv. FE | X | X | X | X | X | X | X |
| Year FE | X | X | X | X | X | X | X |

### (b) By months since charge

| | (1) Any Wages>$0 | (2) ...>$7,500 | (3) ...>$15,000 | (4) Any Gig | (5) Any Other 1099 | (6) Files 1040 | (7) Files SE |
|---|---|---|---|---|---|---|---|
| Post (2019-2021) × Cleared | 0.0270[a] | 0.0217 | 0.00419 | 0.00762 | -0.00349 | 0.0317[a] | -0.0156[a] |
| | (0.0151) | (0.0194) | (0.0187) | (0.00806) | (0.00938) | (0.0192) | (0.00898) |
| Post (2019-2021) × Cleared × Months since charge | -0.000520* | -0.000400 | 0.000117 | -0.000130 | 0.0000570 | -0.000595[a] | 0.000179 |
| | (0.000244) | (0.000311) | (0.000305) | (0.000137) | (0.000152) | (0.000305) | (0.000152) |
| Post (2019-2021) × Months since charge | 0.000626** | 0.000243 | -0.000474[a] | 0.000260* | -0.0000506 | 0.000398 | -0.000165 |
| | (0.000212) | (0.000275) | (0.000273) | (0.000123) | (0.000139) | (0.000273) | (0.000135) |
| 2017 × Cleared | -0.0128[a] | 0.00197 | 0.0177* | -0.00280 | -0.00956[a] | -0.0223* | -0.00952* |
| | (0.00665) | (0.00918) | (0.00874) | (0.00324) | (0.00495) | (0.00913) | (0.00461) |
| 2016 × Cleared | 0.00937 | 0.00159 | 0.00406 | 0.00386 | -0.00656 | -0.0111 | -0.00480 |
| | (0.00798) | (0.0104) | (0.00997) | (0.00336) | (0.00540) | (0.0101) | (0.00500) |

Notes: Table reports difference-in-differences results comparing outcomes for individuals who had all their non-convictions cleared by PA's Clean Slate law by 2020, compared with those who did not. Data from 2016-2020. Sample is restricted to ages 18-25 to ensure they had no other prior convictions by the start of our charge data, which begins in 2008. Standard errors clustered on individual are reported in parentheses. [a] $p<0.1$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table A.6: Impact of PA Clean Slate Reductions on Employment Outcomes - Defendants Other Races

(a) DD Estimates

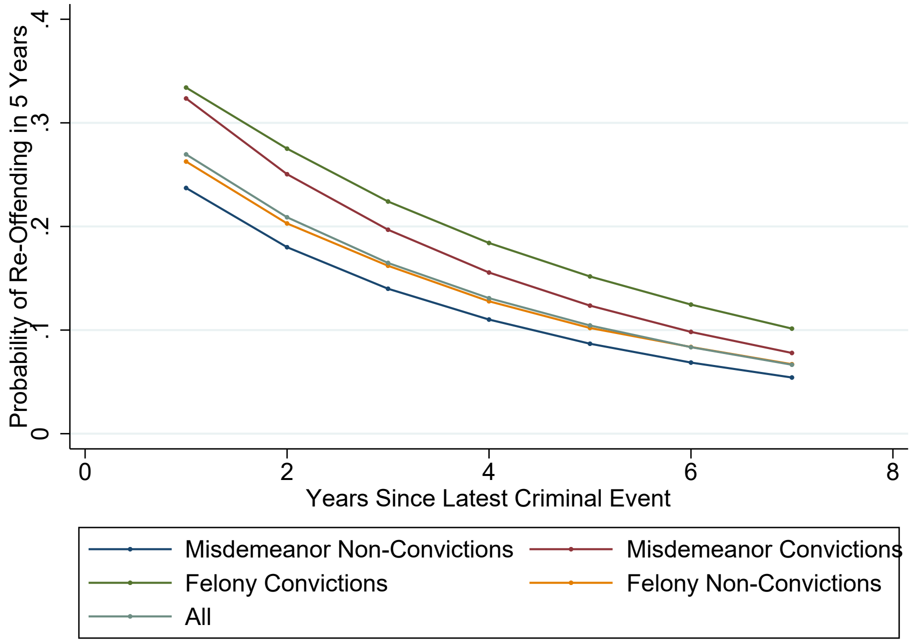
| | (1) Any Wages>$0 | (2) ...>$7,500 | (3) ...>$15,000 | (4) Any Gig | (5) Any Other 1099 | (6) Files 1040 | (7) Files SE |
|---|---|---|---|---|---|---|---|
| Post (2019-2021) × Cleared | 0.00464 | -0.00349 | 0.0000691 | -0.000599 | 0.000459 | 0.00361 | -0.00337 |
| | (0.00385) | (0.00462) | (0.00476) | (0.00153) | (0.00272) | (0.00453) | (0.00251) |
| 2017 × Cleared | 0.000241 | -0.00146 | 0.00878[a] | -0.000956 | -0.00550[a] | -0.000759 | -0.00514[a] |
| | (0.00407) | (0.00491) | (0.00494) | (0.00121) | (0.00297) | (0.00459) | (0.00266) |
| 2016 × Cleared | -0.00515 | -0.00138 | 0.00698 | 0.000429 | -0.00370 | -0.00240 | -0.00232 |
| | (0.00480) | (0.00589) | (0.00581) | (0.00128) | (0.00333) | (0.00536) | (0.00289) |
| Dep. Mean (2018) | 0.807 | 0.639 | 0.511 | 0.008 | 0.059 | 0.742 | 0.051 |
| N | 31,996 | 31,996 | 31,996 | 31,996 | 31,996 | 31,996 | 31,996 |
| NxT | 192,096 | 192,096 | 192,096 | 192,096 | 192,096 | 192,096 | 192,096 |
| Age Controls | X | X | X | X | X | X | X |
| Indiv. FE | X | X | X | X | X | X | X |
| Year FE | X | X | X | X | X | X | X |

(b) By months since charge

| | (1) Any Wages>$0 | (2) ...>$7,500 | (3) ...>$15,000 | (4) Any Gig | (5) Any Other 1099 | (6) Files 1040 | (7) Files SE |
|---|---|---|---|---|---|---|---|
| Post (2019-2021) × Cleared | -0.00672 | -0.0148 | -0.0157 | 0.00318 | 0.000498 | -0.00881 | 0.00577 |
| | (0.00953) | (0.0119) | (0.0120) | (0.00392) | (0.00573) | (0.0110) | (0.00554) |
| Post (2019-2021) × Cleared × Months since charge | 0.000183 | 0.000185 | 0.000286 | -0.0000660 | 0.0000214 | 0.000134 | -0.000129 |
| | (0.000146) | (0.000178) | (0.000180) | (0.0000590) | (0.0000905) | (0.000166) | (0.0000897) |
| Post (2019-2021) × Months since charge | 0.000222[a] | -0.000171 | -0.000351* | 0.00000519 | -0.0000344 | -0.000124 | 0.000150[a] |
| | (0.000131) | (0.000158) | (0.000160) | (0.0000521) | (0.0000822) | (0.000151) | (0.0000804) |
| 2017 × Cleared | 0.000312 | -0.00352 | 0.00781 | -0.00142 | -0.00400 | 0.000107 | -0.00472[a] |
| | (0.00417) | (0.00507) | (0.00512) | (0.00117) | (0.00304) | (0.00470) | (0.00271) |
| 2016 × Cleared | -0.00399 | -0.00285 | 0.00596 | 0.000525 | -0.00304 | -0.000838 | -0.00194 |
| | (0.00491) | (0.00607) | (0.00601) | (0.00125) | (0.00341) | (0.00549) | (0.00295) |

Notes: Table reports difference-in-differences results comparing outcomes for individuals who had all their non-convictions cleared by PA's Clean Slate law by 2020, compared with those who did not. Data from 2016-2020. Sample is restricted to ages 18-25 to ensure they had no other prior convictions by the start of our charge data, which begins in 2008. Standard errors clustered on individual are reported in parentheses. [a] $p<0.1$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$

Figure A.10: Probability of Re-Offending in 5 Years, By Initial Event




Notes: Data are from Bexar County, TX.

# B Survey Details

## B.1 Survey 1

The first survey was conducted in May 2021 through Prolific and designed using Qualtrics. Prolific was selected because it allows for the pre-screening of respondents based off of their responses to prepared questions. We selected people who responded "Yes" to the question: "Do you have any experience in making hiring decisions (i.e. have you been responsible for hiring job candidates)?" And later also added the criteria that the respondents should be located in the USA, after a pilot of the study accidentally included respondents in the UK. We additionally asked (though could not screen participants out based on responses, per Prolific guidelines): "In the past 5 years, have you had experience working in the United States in human resources and/or dealing with hiring processes for a firm with more than 1 employee?"

The survey starts with questions about recent hiring experience and a series of opening questions about the type of office the respondent worked in: the location, size, and industry of the firm. Then the survey asks the same set of questions twice about the most recent position in which the respondent had experience in making hiring decisions: first, for the position "closest to 'entry-level', meaning a job which required the least amount of experience and/or education in that firm," then, later for the position, "closest to 'mid-level', meaning a job which required several years of experience." The respondent's were compensated 2$ for their responses.

### B.1.1 Sample Size, Compensation, and Technical Details

A small pilot was launched in May 2021 which helped us refine questions (results are not used in analysis). On May 26, 2021 we launched with 500 respondents. And on June 2, 2021 we requested an additional 500 respondents. In the full sample of 1000 responses, there are 806 valid responses (had recent hiring experience in the U.S. with a firm with more than 1 employee, of which 77% say their firm conducted a criminal background check. In total, there are 550 respondents who were aware of the background check process.

## B.2 Survey 2

A second survey was designed and launched in late September to early October of 2021. The scope of this survey was broadly the same as the first, but the second survey was not a direct follow-up. The purpose of the second survey was to once again survey people with hiring experience in the United States and see how they would assess the risk of someone with a criminal record re-offending over time by asking them whether they would hire someone with a criminal record after X number of years.

There are two levels of randomization in the survey. The first is the nature of the crime that is posed in the hypothetical scenario asked to the hiring agent. There are four types of crimes: Felony Theft, Felony Drug Possession, Misdemeanor Theft, and Misdemeanor Drug Possession. There are also two possible dispositions: charged and convicted or charged and not convicted. In total, this creates eight options for the randomization, which are selected randomly by Qualtrics. When a respondent is sorted into one of these eight categories, they will only respond to questions responding to that crime/disposition pair.

There is another level of randomization which is that each respondent receives one year since charge that they are asked about before all of the other years. For example, someone might be first asked if they would hire someone with a felony drug possession conviction 6 years ago. Someone else might be asked about conviction 4 years ago. The year value is randomized between 1 and 10.

The respondents also respond to the same question about whether they would hire someone with a charge from X years ago for all 10 years: only the first question is randomized. We ask the question in this format because we want to avoid biasing responses as respondents go through all the years. It also provides us a means through which we can make an assessment of the quality of the response: if the response in the first question does not match the response for the year in the second question, this might be a sign that the response is a low quality response.

The third question relevant to our main results is a question about whether the firm at which the respondent last had hiring experience has any policies about hiring individuals with given crime type and conviction after one through ten years. Unlike the other question, we only ask for the respondent to answer the question for all ten years, and do not present one randomized year.

Respondents were once again compensated 2$ for their participation. It should be noted that the same survey was retained in Prolific so that the same individual could not be surveyed twice through the various versions of the second survey. The same person could be surveyed between the first and the second survey however.

### B.2.1 Sample Size, Compensation, and Technical Details

In total, 2500 people were surveyed for the second survey.

Initially 20 people were part of the pilot of the survey. That number was then raised to 50 people. The first finalized version of the survey was then launched with 500 respondents without any randomization of the type of crime.

For the final version of the survey that included randomization of the type of crime, 1961 people were surveyed. 440 responses were discarded because two typos were found in the survey. Technically, the responses only applied to one eighth of the results, but for the sake of keeping the sample sizes and timings balanced, we discard all responses before 12:04 PM, which was when the typos were corrected.

### B.2.2 Initial Pilot and First Version

The initial pilot was launched on September 29th, 2021 which helped us adjust questions (these results are not used in the analysis). We then launched with 500 participants on September 30, 2021.

### B.2.3 Second Version

The second version of the survey was launched on October 6th, 2021. Following the first survey, a second version of the survey was designed that randomized the crime type and severity of the charge presented in hypothetical scenario. The following adjustments were also made:

- Size bins were changed in the question about firm size
- The industry options were modified in the question about the industry of the firm
- The wording of responses were changed in question 45.

Roughly 460 respondents were surveyed with a survey instrument that had a typo. Then the full sample of 1521 respondents were surveyed. Of these 1521 respondents, 1003 end up being valid responses that are not excluded as a result of any of our criteria. This resulted in roughly 250 respondents in each randomization bin for crime severity, though there is naturally some variation in the exact number for each category.

# C FCRA Data Details

## C.1 Bexar County, Texas

The Bexar County, Texas data was downloaded from the Bexar County Criminal Courts in May 2017. Similar data is also used in Freedman et al. (2018) and Agan et al. (2021). Before matching we do string cleaning of names, addresses, and flag and drop definite businesses.

### C.1.1 Matching Defendants Across Cases

Unique to Bexar County, there is a pre-existing defendant ID in the dataset called "SID". Our matching process assumes that the SID never inaccurately links defendants, but that it could miss links between people. For instance, there are people in the Bexar county dataset how have the same first name, last name, and DOB, but different SIDs.

Since we know SID is unique to a person, we first fill in missing name and DOB information by SID. There are some SIDs that are missing for people who have non-missing name and DOB information. Thus, our naive ID is based off of SID and the grouping on name and DOB for observations that have identifiable ID information but are missing SID.

While we feel it is too risky to match on name and address only[35], we are comfortable filling in missing DOB information if there is a first name, last name, and address match. We match on these factors and generate an ID ONLY to update DOB if missing according to this ID. We do not match individuals or update ID in this step.

Next we match on first name, last name, and DOB.

Next we match if the address and DOB are the same and the combined first last name is less than or equal to a string distance of 3[36]. In other words, we do a row-wise comparison of name string distance for names that have the same address and DOB. We compare up to 3 rows which we feel is very thorough without compromising the efficiency of the code.

After doing this matching we reshape wide to long to wide on the new ID to update

Next we match people that have the same first name, last name, address and a DOB that is "close". DOB errors often appear as a typo in either the day, month, or year. Thus we consider a DOB "close" if at least the two elements of the day, month, and year are the same. This means that we would match two IDs with the same name and address and DOBs of 4/12/1980 and 9/12/1980. We do a row-wise comparison of DOBs to check to see if the are "close" according to the above definition, and match close DOBs that have the same address and name. We compare up to 3 rows.

Unlike other states we do not need to match on name and DOB again. We confirmed that the above matching did not provide any new demographic information such that we would get any updated IDs on a repeated name and DOB match.

### C.1.2 Charge Categorization

There are 1,551,880 charges which link to defIDs that have identifiable information. We create an indicator variable "isConvicted" which is a 1 if the disposition category is "guilty" or the disposition description is "DEF ADJ TERM UNSAT[37]", 0 if the disposition category is "not guilty at trial", "Dismissed", and missing otherwise. Only 4% of charges have a missing isConvicted status.

[35]This could be risky if an address is for a group home or shelter that our address cleaning did not pick up. It is conceivable that multiple people with the same name live in one of these group locations.

[36]More specifically, we look at the levenshtein string distance between the last, first name of the current row to the last, first name of the next [up to 20] rows. The string distance usually ranges from 1 to 35. A string distance less than or equal to 3 is very close.

[37]Unsatisfactory deferred adjudication turns into a conviction.

## C.2 Maryland

The Maryland criminal records were drawn from several tables hosted on the Maryland Volunteer Lawyers Service (MVLS) database. The Maryland data cover all charges filed between 1990 and 2018. The two main courts are the Circuit Court and the District Court. The District Court is the lower criminal court and handles cases such as traffic violations, tenant disputes, or domestic violence. The Circuit Court is the higher criminal court and handles more serious cases. There are 3 separate data sets of higher criminal court hearings originating from Baltimore City, Prince George County, and Montgomery County[38]. Any upper court cases that originate outside of these places are in the circuit regular dataset. For the most part, similar information is contained in the various datasets with some exceptions such as Prince George missing date of birth information.

We clean and standardize name and address entries and standardize abbreviations (e.g. "Street"→"St") in order to decrease the number of unique address entries in the dataset and make the matching on address more accurate.

We are fairly confident that tracking number is unique to a person and it used to link cases that move between courts (e.g. a district case that is elevated to the circuit court will have different case numbers in the district and circuit court, but would have the same tracking number.) For the most part, names and DOBs are consistent with tracking number, but not always. This could be caused by alternate names and DOBs or because a tracking number was used for a co-defendant. Given this, we think it is too aggressive to match people on tracking number, but we do fill in missing name and DOB by tracking number. Recall, Prince George is missing DOB, but we can get DOB if the tracking number links to the district court where DOB is not missing.

### C.2.1 Matching Defendants Across Cases

Our matching algorithm uses tracking number, district case number, DOB, name, and address to link people. We first create a naive ID which is a grouping of first name, last name, and DOB. If name is missing, naive ID is a grouping of DOB and address, and if DOB is missing, naive ID is a grouping of name and address. If naive ID is still missing for an entry, we drop it, since it only has one piece of identifying information (i.e. only name or only DOB or only address), and we cannot reliably match on this.

Recall that while we filled in missing information by tracking number, we did not match by tracking number. Instead, we link IDs if there is a match on tracking number and last name or a match on tracking number and DOB.

Another variable which can be used for matching is the district case number. This variable is found in upper level court court datasets and links to the case number in the district court dataset. Similar to the above, we link IDs if there is a match on district case number and last name or a match on district case number and DOB.

While we feel it is too risky to match on name and address only[39], we are comfortable

[38] These 3 places use a different court number filing system, a single event could have multiple cases (each with multiple charges). In all other places in MD, a single event corresponds to a single case.

[39] This could be risky if an address is for a group home or shelter that our address cleaning did not pick up. It is conceivable that multiple people with the same name live in one of these group locations.

filling in missing DOB information if there is a first name, last name, and address match. We match on these factors and generate an ID ONLY to update DOB if missing according to this ID. We do not match individuals or update ID in this step. For example, if there is a Maria Evanston born on 8/20/1988 who lives at 100 Cherry Hill and another Maria Evanston with a missing DOB, but the same address, we fill in the missing DOB with 8/20/1988. However, if there are two observations with the same name and address but with different DOBs, nothing is updated.

Next we match on first name, last name, and DOB. Recall that we generated the naive ID when data was unique on casenumber and long on demographics, so the naive ID was just based on firstName1 lastName1 and DOB1. Here we compare every combination of name and DOB within a given ID to all other name and DOB combinations within IDS to link ID.

Next we match if the address and DOB are the same and the combined first last name is less than or equal to a string distance of 3[40]. In other words, we do a row-wise comparison of name string distance for names that have the same address and DOB. We compare up to 20 rows which we feel is very thorough without compromising the efficiency of the code.

Next we match people that have the same first name, last name, address and a DOB that is "close". DOB errors often appear as a typo in either the day, month, or year. Thus we consider a DOB "close" if at least the two elements of the day, month, and year are the same. This means that we would match two IDs with the same name and address and DOBs of 4/12/1980 and 9/12/1980. We do a row-wise comparison of DOBs to check to see if the are "close" according to the above definition, and match close DOBs that have the same address and name. We only compare up to 10 rows, which is more than enough (DOB typos are much rarer than name typos.)

Finally we want to do a name and DOB match again since we have updated both of these pieces of information since we last matched solely on name and DOB.

#### C.2.2 Charge Categorization

In total, there are 10,123,687 charge which link to IDs that have identifiable information. A few observations (0.004%) are duplicates in terms of case number and charge number, but are not full duplicates. In most instances, one of the duplicate charges contains more information (e.g. one is missing disposition date and the other is not) so we keep the version with more information. In the regular circuit court, there is a variable called sentence version. For duplicate charges with different sentence versions, we keep the most recent charge by sentence version.

Of note, 20% of the charges are District court charges that were elevated to an upper court. The elevated charges have a case disposition of "Forwarded to Circuit Court" or "Jury trial prayed" (which also means forwarded to the Circuit court). We do not drop these charges, but instead create indicators for "elevated charge" and "matched elevated charge". Matched elevated charges are a subset of elevated charges: in addition to having the case disposition of "Forwarded to Circuit Court" or "Jury trial prayed", they are also a duplicates charge in terms of defID, tracking number, charge number, and charge description.

[40]More specifically, we look at the levenshtein string distance between the last, first name of the current row to the last, first name of the next [up to 20] rows. The string distance usually ranges from 1 to 35. A string distance less than or equal to 3 is very close.

Of the 2,096,072 elevated charges, 15.8% are a matched elevated charge. This percentage may be low because charge descriptions and/or charge number can change when moving from the district court to the upper court. For example if two charges merge when the case is elevated, then the charge number for all other charges may change.

Another thing to note is that while 96% of elevated charges have a missing CHARGE disposition, 4%, have a non-missing charge disposition such as "Dismissed" or "Nolle Prosequi." Thus, not all elevated charges will have a missing Conviction status.

Next we categorize disposition based off of the charge disposition. These are the following categories:

0 "Not Guilty" 1 "Guilty" 2 "Guilty Plea" 3 "Dismissed" 4 "Withdrawn" 5 "CLOSED - JEOPARDY OR OTHER CONVICTION" 6 "STET[41]" 7 "Probation before judgment" 8 "Sent to Juvenile Court" 9 "Charge merged" 10 "Sent to uppercourt" 11 "Other/Missing"

We create an indicator variable "isConvicted" which is a 1 if the disposition category is "Guilty" or "Guilty Plea", 0 if the disposition category is "Not Guilty", "Withdrawn", or "Dismissed", and missing otherwise.

The court data does not have a variable for offense grade or felony/misdemeanor categorization. We are able to categorize 94% of charges using a "cjis-code"[42] (crime code) or the actual crime description.

In the final charge dataset, we keep the following variables: defID, filingdate, dispositiondate_charge, dispositiondate_case, sentencedate, verdictdate, isConvicted, isFelony, viol_prob, traffic_viol, elevated_charge, matched_elevated_charge.

## C.3 New Jersey

The New Jersey criminal records were obtained by a public access information request application submitted to New Jersey courts. These files contain records from January 1st, 1980 to May 30, 2018 and have no restrictions on their usage, though they did require a fee. There are two main files: CCFOCN25, referred to as "Master Defendant List", and CCC1022, "Pleas Guilty, Not Guilty, Dismissed by Judge".

New Jersey does not use a "felony" versus "misdmeanor" distinction, but rather distinguishes between "indictable offenses" and "disorderly person" offenses. Indictable offenses fairly closely align with what would be felonies in other states, and disorderly persons offenses with misdemeanors. On the criminal side, the Superior Court hears cases for "indictable" offenses, and thus we only have data on these types of offenses for New Jersey and throughout the paper we refer to these offenses as felonies to more closely align with terminology from other states.

CCFOCN25 contains data on 1.5 million defendants, including their names, case numbers, defendant numbers, county and date of birth. The data set also includes an information on indictment date and dispositions.

CCC1022 includes data on 3.1 million dispositions. The dispositions correspond to individual charges brought against a defendant, who can have multiple charges brought against

[41] STET means that case is going to be inactive for a period of time (maybe 6 months or a year), usually in order for the defendant to complete some agreed upon conditions like community service hours, counseling courses, anger management classes, payment of restitution, etc.

[42] Montgomery County is missing the cjis-code variable.

them on a case.

We begin by merging the master defendant list with the disposition list on case, indictment number, defendant number and county code. Of 3.2 million person-case-disposition pairs, 163,352 remain unmatched. We assume that this is because of clerical errors that identify multiple defendant numbers in one case or different case numbers with the same indictment level. To remedy these errors, we sort on county, name, indictment and merge status. Under the assumption that the errors are only in the entry of case numbers, this sorting should identify from the master and using data the observations that were not matched to the corresponding entries. We then fill in the missing information, date of birth and indictment date, into the disposition data. This leaves around 71,362 observations still unmatched: 67,006 unmatched from the master data and 4,356 unmatched in the using data.

We attempt to further remedy the number of unmatched cases by identifying unmatched cases in the master and using data respectively that match on indictment number, county, and defendant number. These are the cases where indictment number, county, and defendant number all match, but the defendant name does not, likely because the name was entered in two different forms. We assume that the name in the defendant data set is the "correct" name, since the disposition data set contains multiple names, while the defendant data set contains only one. We then drop the remaining clerical errors, since we have done all that we can do to fix them. This then leaves 67,243 cases unmatched. In total, we have 3,104,529 matched observations at the person-case-disposition level.

We drop all observations for which the indictment date is before January 1st, 1980, since this must have been erroneously included in the data. We also drop date of births that are equal to indictment dates, since these must be errors. We then fix the presentations of names since they are inconsistent across observations: we attempt to drop all observations in which the defendant is a business and remove irregular dashes, commas, and suffixes. As a result, we have information on the individual dispositions—the charges, sentencing and disposition dates, and dispositions result (guilt, not guilty, dismissed), and counts by individual of the total number of cases corresponding to each disposition result—with additional information on the defendants date of birth and indictment date. There is also a range of other information included in the data: including attorney and judge names and whether or not the defendant posted bail.

We also resolve 780 cases where there seem to be errors in the entry of dates: where the indictment date and the sentencing date are the same, and the month and date are equal to the disposition date but not the year. We resolve similar issues for the sentencing date.

### C.3.1 Matching Defendants Across Cases

Note that in NJ, we do not have address information; the most specific geographic information we have is county code. This means that we cannot fill in missing DOB by address and name like we do in other states

We match if the county code and DOB are the same and the combined first last name is less than or equal to a string distance of 2[43]. In other words, we do a row-wise comparison

[43] In other states where we had address, we were less strict about the string distance requirement than we are here when we only have county code

of name string distance for names that have the same address and DOB. We compare up to 20 rows which we feel is very thorough without compromising the efficiency of the code.

Next we match people that have the same first name, last name, county code and a DOB that is "close". DOB errors often appear as a typo in either the day, month, or year. Thus, we consider a DOB "close" if at least the two elements of the day, month, and year are the same AND the DOBs are within a year of each other. This last restriction is not present in MD or Bexar because in those places we had address, but here we only have county code, so we include the extra year restriction to balance out the less specific demographic information. We compare up to 5 rows.

Finally we want to do a name and DOB match since we have updated both of these pieces of information since we last matched solely on name and DOB (the original naiveID was the first match on just name and DOB.

### C.3.2 Charge Categorization

The NJ charge files already have indicators for isGuilty, isNotGuilty, and isDismissed, so we base the isConvicted on these variables where isConvicted=1 for guilty charges, 0 for dismissed or not guilty charges and missing otherwise. isConvicted is missing for 7.31% of charges.

## C.4 Pennsylvania

We obtained Pennsylvania court data from the Administrative Office of Pennsylvania Courts (AOPC) via a Public Access request. The data cover all cases in the Magisterial District Court system (which handles misdemeanors) and Courts of Common Pleas system (which handles felonies) filed between May 2008 and April 2018.

### C.4.1 Matching Defendants Across Cases

We begin by appending the MDJS cases onto the original PA dataset. We then clean names. Next, we link the upper and lower level court cases. Most upper level court cases (hereafter CP) originate at the lower level (MJ or MC). We don't want to double count the cases that have been elevated so we link lower and upper court docket number using the offense tracking number (OTN). About a quarter of CP court cases do not link to the lower level which could be due to court records at the lower level court be expunged.

We drop lower court cases that link to the upper court and are missing disposition or have a charge disposition such as "elevated to the the upper court". For these cases, we merge the lower level birth date (used later as alternate DOB), earliest name, earliest filing date, and all the reshaped docket numbers onto the upper court case. There are 18K OTNs that have different birth dates at the CP and MJ court levels. We check if people differ within OTN by looking at birth dates and last names. If there is an OTN match but there is no last name OR birth date match, we do not link on tracking number. We drop the CP charges that have a disposition indicating they were handled at the lower level such as "Disposed (Lower Court)" and have a merged MJ docketnumber.

We get alternate names and DOBs if either of those differ within the linked OTN and then match people on shortened first and last name (up to 5 alt names) and DOB or alternate DOB. Note that PA does not include any specific address information so we cannot further match on that. After matching and assigning a unique ID, we reshape long and then wide for all demographic information to get modal name, dob and zip code as well as all alternate names, dobs, and zip codes. There is a succinct demographics dataset that just includes unique ID, modal name, dob, and zip code by ID. The wider demographics contains additional (non-modal) name, dob, and zip code information by unique ID.

### C.4.2 Charge Categorization

We then proceed with the charge analysis. We categorize crimes into felonies, misdemeanors, or summary offenses based on the offense grades present in the data. Offense grades that begin with M are misdemeanors, F are felonies, and H are heinous crimes, which are also categorizes as felonies. Offense grades of "S" or "IC" are statue or summary violations which we categorize separately from felonies and misdemeanors. There are 500K missing charge codes, for which we can fill in about 50% using the data from offense descriptions already categorized codes. If an offense description has at least 99% of observations as either of felony or misdemeanor, we use this to fill in the felony/misdemeanor status for the same offense descriptions with missing categorizations.

Next we drop duplicates in terms of the following variables: docketnumber, originatingoffensesequencenumber, offensedescription, offensedisposition, statutetitle, statutetype, statutesection, statutesubsection.

We then categorize dispositions into the following:"Not Guilty" 1 "Guilty" 2 "Guilty Plea" 3 "Dismissed" 4 "Withdrawn" 5 "Proceed to Court" 6 "AMP/ARD" 7 "No Contest" 8 "Nolle Prossed" 9 "Held for Court" 10 "Charge Changed" 11 "Moved to non-traffic". There is a default 12 "other" category which includes drug/veterans treatment courts, failures to appear, transfers, and other odd disposition descriptions.

The main variable of interest is whether the disposition was a conviction or not. We generate the indicator variable, *isConvicted* based off of the offense disposition [44]. *isConvicted* = 1 if the offense disposition is in categories 1, 2, or 7. *isConvicted* = 0 if the offense disposition is in categories 0, 3, 4, 5, 6, 8, 9, 10, or 11. *isConvicted* = . (missing) if the offense disposition is in the other category. The variable, *UnlcearConviction* is an indicator for if offense disposition is in categories 5, 6, 9, 10, or 11 since we are not confident that these dispositions are actually 0. (They may be missing.)

# D Fair Credit Reporting Act Differences by State

At the federal level, FCRA requires that all arrests, indictments, and other records older than 7 years that were dismissed or acquitted cannot be reported in a background check, but only if the person's expected salary is less than $75,000. Convictions older than 7 years can still be reported. These states passed additional legislation that prohibits the reporting of certain criminal record information:

[44] There is also a case disposition, which we only use when offense disposition is missing

Alaska: prohibits the release of any non-conviction or correctional facility records, regardless of the age of the charge

Arkansas: prohibits reporting of non-felony arrest records, and prohibits the reporting of felony arrest records if the arrest is more than 3 years old

California: 7-year limitation on arrest, indictment, and conviction records, regardless of salary. Any arrest, indictment, or misdemeanor complaint information is prohibited (regardless of time) if the charge is still pending or did not result in a conviction

Colorado: 7-year limit on arrest, indictment, and conviction records for salaries of <$75,000

DC: cannot report convictions for which the sentence was completed more than 10 years ago

Kansas: For salaries <$20,000 arrests, indictments, and convictions are prohibited. For salaries $20,001-74,999, the standard FCRA rules apply.

Kentucky: records may only contain information related to a conviction

Maryland: see Kansas

Massachusetts: 7-year limitation on arrests, indictments, and convictions, regardless of salary

Montana: 7-year limitation on arrests, indictments, and convictions, regardless of salary

Nevada: removes salary requirement from FRCA

New Hampshire: For salaries <$20,000, there is a 7-year limitation on arrests, indictments, and convictions. For salaries $20,001-74,999, the standard FCRA rules apply

New Mexico: 7-year rule for arrests, indictments, and convictions. Arrests, indictments that did not lead to a conviction cannot be reported, regardless of time since occurrence

New York: cannot report any criminal information unless the charges are still pending or the charges led to a conviction. For salaries <$25,000 the 7-year rule for convictions also applies

Texas: 7-year rule for arrests, indictments, and convictions if the expected salary <$25,000

Washington: For salaries <$20,000, prohibits reporting of arrests, indictments, convictions more than 7 years old.

# E Match Algorithm

This appendix outlines our approach to matching the names and birth dates from Proposition 47 reductions in San Joaquin County, CA; Bexar County, TX; Maryland; New Jersey, and Pennsylvania to the IRS database. We also report match performance. We rely on a variety of different sources in an iterative process as follows.

## E.1 Step 1

We first search for a possible match in the Social Security database shared with IRS. This database provides date of birth and the first four letters of the last name (a field known as the "Name Control"), for every individual issued a Social Security Number or Individual Taxpayer Identification Number. The database includes a history of up to nine Name Controls ever-associated with an individual (for example, when a woman changes her last name after marriage, this would generate a new entry). We require an exact match on birthdate and first four letters of the last name in the database. For locations where gender is known (most cases in Bexar County, TX and Pennsylvania), we further restrict to gender matches.

## E.2 Step 2

Our procedure so far often results in multiple "hits." To whittle down possible duplicate matches and assess match quality, we match to the database of individual tax returns and the database of information returns (W2s, 1099s, etc), each of which contain full names and ZIP code each time a form is filed. We track match hits to each data source with indicator variables.

Based on these match indicators, we create a priority ranking of matches. The highest quality matches (rank 1) have an exact match on first and last name, birthdate, gender (when available as a match variable) and address (zipcode or state, when available as a match variable). If there is no address information available, or when the address information does not match, we prioritize matches of individuals residing in a state where the legal proceedings occurred. We consider matches on first, last name, and birthdate, but no geographic match, to be the second highest quality matches. The remaining matches will be lower quality: we may have a Name Control, birthdate and geography match, but not an exact match on first and last name; or an exact name and DOB match, but not a geographic match. If there are duplicates, we prioritize the highest quality match. When duplicates remain, we currently throw out all matches.

## E.3 Match performance: San Joaquin County, CA

Below we document match performance in San Joaquin County for the entire universe of possibly eligible crimes in San Joaquin County based on the criteria described above. Note: For San Joaquin County, CA, we prioritize matches that have ever appeared in Northern California, i.e. San Francisco, Sacramento, Palo Alto, San Mateo, Oakland, Berkeley, Richnmond, San Rafael, San Jose, Stockton, Santa Rose, Eureka, Sacramento, Marysville and Redding (zipcodes beginning with 94, 95, or 960).

Starting N (after dropping 427 with missing DOB)= 25,649

| **Highest Match Rank** | **No. Unique Matches** | **% of Matches** | **Cum.** |
|---|---|---|---|
| 1 - DOB, Full name, Northern CA | 18,612 | 85.95 | 85.95 |
| 2 - DOB, Full name | 1,249 | 5.77 | 91.72 |
| 3 - DOB, Name control, Northern CA | 1,444 | 6.67 | 98.39 |
| 4 - DOB, Name control-only | 349 | 1.61 | 100.00 |
| **Total** | 21,654 | | |

Overall match performance: 21,654/25,649 =84.42%

We next compare characteristics of matched and non-matched for our two main estimation samples.

Our first estimation sample is the group who ever receive reductions. A comparison between matched and unmatched for this estimation sample is provided below.

| | (1) Matched | (2) Unmatched | (3) Difference (p-value) |
|---|---|---|---|
| Age in 2014 | 45.23 | 45.98 | -0.753* |
| | | | (0.012) |
| One Felony | 0.086 | 0.144 | -0.0574*** |
| | | | 0.000 |
| Has HS | 0.819 | 0.830 | -0.0104 |
| | | | 0.309 |
| Has 666 | 0.324 | 0.238 | 0.0861*** |
| | | | 0.000 |
| Year of first petition | 2016.2 | 2016.3 | -0.0537* |
| | | | 0.033 |
| Year of reduction | 2016.8 | 2016.9 | -0.0770* |
| | | | 0.010 |
| Latest conviction year, eligible offenses | 2004.7 | 2001.5 | 3.270*** |
| | | | 0.000 |
| Supervised at time of first petition | 0.219 | 0.193 | 0.0259* |
| | | | 0.016 |
| Incarcerated at time of first petition | 0.023 | 0.015 | 0.00832* |
| | | | 0.015 |
| Obs | 8,738 | 1,622 | |
| Unique matches | 8,702 | | |

There are a small number of individuals (36) who are linked to the same SSN. For analysis, we assign the individual the earliest of their reduction dates and minimum of ONE FELONY status.

We have a slightly different estimation sample for the experiment. Randomization occurred earlier, before all reductions had been completed and before we had completed data collection and cleaning. As a result, we separately match the data using the data vintage as of the time of randomization. This full sample starts with 8969 who had received reductions as of the first vintage of our data. We then drop 527 missing date of birth for a starting sample size of 8,442. 7,155 match to the IRS data. A comparison between matched and unmatched for this estimation sample is provided below.

| | (1) | (2) | (3) |
|---|---|---|---|
| | | | Difference |
| | Matched | Unmatched | (p-value) |
| Randomized Into Treatment | 0.525 | 0.505 | 0.020 |
| | | | 0.192 |
| Age in 2014 | 45.89 | 45.11 | -0.786 |
| | | | 0.015* |
| One Felony | 0.069 | 0.124 | -0.055 |
| | | | 0.000*** |
| Obs | 7,155 | 1,287 | |
| Unique matches | 7,128 | | |

## E.4 Match performance: FCRA sample

In this Section we document match performance by location for each location we use in the FCRA analysis based on the criteria described above.

### E.4.1 Bexar County, TX

Starting N =562,434

| Highest Match Rank | No. Unique Matches | % of Matches | Cum. |
|---|---|---|---|
| 1 - DOB, Full name, address zipcode | 282,834 | 58.28 | 58.28 |
| 2 - DOB, Full name, address state | 127,315 | 26.23 | 84.51 |
| 3 - DOB, Full name, TX | 13,652 | 2.81 | 87.33 |
| 4 - DOB, Full name | 21,106 | 4.97 | 92.29 |
| 5 - DOB, Name control, geography | 27,015 | 5.57 | 97.86 |
| 6 - DOB, Name control-only | 10,384 | 2.14 | 100.00 |
| **Total** | 485,306 | | |

Overall match performance: 485,306/562,434=86.3%

### E.4.2 Maryland

Starting N=1,324,226

| Highest Match Rank | No. Unique Matches | % of Matches | Cum. |
|---|---|---|---|
| 1 - DOB, Full name, address zipcode | 569,822 | 58.62 | 58.62 |
| 2 - DOB, Full name, address state | 130,954 | 13.47 | 72.09 |
| 3 - DOB, Full name, MD | 28,547 | 2.94 | 75.03 |
| 4 - DOB, Full name | 76,041 | 7.82 | 82.85 |
| 5 - DOB, Name control, geography | 114,976 | 11.83 | 94.68 |
| 6 - DOB, Name control-only | 51,701 | 5.32 | 100.00 |
| **Total** | 972,041 | | |

Overall match performance: 972,041/1,324,226=73.4%.

### E.4.3 New Jersey

Starting N=778,582

| Highest Match Rank | No. Unique Matches | % of Matches | Cum. |
|---|---|---|---|
| 1 - DOB, Full name, NJ | 458,481 | 72.89 | 72.89 |
| 2 - DOB, Full name | 100,128 | 15.92 | 88.81 |
| 3 - DOB, Name control, NJ | 55,149 | 8.77 | 97.57 |
| 4 - DOB, Name control-only | 15,260 | 2.43 | 100.00 |
| **Total** | 629,018 | | |

Overall match performance: 629,018/778,582=80.8%.

### E.4.4 Pennsylvania

Starting N =1,187,199

| Highest Match Rank | No. Unique Matches | % of Matches | Cum. |
|---|---|---|---|
| 1 - DOB, Full name, address zipcode | 760,782 | 70.2 | 70.2 |
| 2 - DOB, Full name, PA | 197,409 | 18.22 | 88.42 |
| 3 - DOB, Full name | 65,492 | 6.04 | 94.46 |
| 4 - DOB, Name control, geography | 45,473 | 4.2 | 98.66 |
| 5 - DOB, Name control-only | 14,550 | 1.34 | 100.00 |
| **Total** | 1,083,706 | | |

Overall match performance: 1,083,706/1,187,199=91.3%

The following table shows our overall match performance by conviction status for our main estimation sample.

Table E.1: Summary Match Statistics on Last Charge

| | | Last event is Conviction | | Last Event is Non-Conv & No Other Conv | |
|---|---|---|---|---|---|
| | | Felony | Misdemeanor | Felony | Misdemeanor |
| **Bexar** | All | 78,622 | 186,167 | 25,894 | 204,612 |
| | Match | 67,743 | 157,341 | 22,007 | 177,015 |
| **PA** | All | 165,022 | 492,339 | 79,304 | 349,719 |
| | Match | 148,062 | 451,199 | 67,156 | 301,987 |
| **MD** | All | 59,449 | 232,248 | 90,635 | 530,403 |
| | Match | 49,307 | 167,968 | 62,549 | 349,248 |
| **NJ** | All | 517,983 | 0 | 89,026 | 0 |
| | Match | 404,433 | 0 | 61,073 | 0 |

Notes: Table reports total number of individuals in state court records and those matched to IRS data.