The Impact of Criminal Records on Employment, Earnings, and Tax Filing *

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Abstract

This paper combines IRS tax return data with administrative court records from several jurisdictions to measure the impact of criminal records on taxpayer earnings and filing behavior. Our data construction allows us to examine a wide range of interactions with the criminal justice system including charges that led to convictions and those that did not. We document sharp and persistent declines in the propensities to have W-2 reported earnings and to file a 1040 return around an initial criminal charge, even in the case of charges that do not lead to convictions. We also find that self-employment earnings of Schedule SE and 1099-reported nonemployee compensation both fall in close proportion to W-2 employment in most cases. There is evidence that criminal record remediation for individuals who have had records for multiple years leads to increases in platform-mediated gig work reported on 1099 returns, although in most cases those gig earnings are not reported on Schedule SE. Aside from gig work, we do not find evidence that remediation is associated with increased filing rates or earnings.

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1 Introduction

Millions of people across the United States are charged with crimes each year. The prevalence of criminal records could have important implications for tax administration. The consequences of these records for income reporting and tax filing may be substantial. Employment rates are lower for individuals with a criminal record than the general population (Mueller-Smith 2015; Looney and Turner 2018) and recent studies suggest that records may be preventing about one third of working-age males from contributing to the formal economy, leading to substantial lost income and tax revenue (Looney and Turner, 2018). Without access to standard jobs, individuals with a criminal history may rely more heavily on alternative work like independent contracting and self-employment, which could increase workers' tax filing burden and reduce their compliance with tax filing requirements. Recent work by Finlay et al. (2022) finds that earners with criminal histories are disproportionately likely to report Schedule C income. Further, interactions with the criminal justice system might directly impact filing behavior, credit take-up, and compliance with the tax code.

This paper combines IRS tax return data with publicly-available administrative court records from several jurisdictions to measure the impact of criminal records on taxpayer earnings and filing behavior. Our data construction allows us to examine a wide range of interactions with the criminal justice system beyond imprisonment previously studied by Looney and Turner (2018)—including charges that led to convictions and those that did not. By observing charge-level data rather than imprisonment data, we are able to assess the impact of a criminal record *per se*, apart from sentencing decisions—which might have distinct effects on subsequent earnings and filing behavior. A complementary companion paper by Garin et al. (2022) studies the direct impacts of incarceration (for a given charge) on taxpayer behavior.

As a first look at the impacts of criminal history events on tax filing behavior, we conduct event-study analyses that examine how taxpayer reporting and third-party-reported earnings change around charges and convictions. Importantly, we document that individuals who have interactions with the criminal justice system have low filing rates and earnings levels even prior to their first criminal charge, particularly for those charged with felonies. Nonetheless, we find that the propensity to file a tax return falls sharply around an initial criminal charge. These declines in 1040 filing are associated with similar drops in the probability of having W-2 reported earnings. Strikingly, we observe drops even in the case of charges that do not lead to convictions, suggesting that the mere presence of a criminal record might impact earnings prospects. In all cases, these declines persist through several years after the initial charge.

However, we do not find that individuals with criminal records are more likely to be self-employed.[1] Conditional on participating in the workforce—i.e. earning labor income reported on a W-2 or Schedule SE—we never observe self-employment rates above ten percent; this is below the self-employment rate in the broader workforce (Collins et al., 2018). With the exception of misdemeanor convictions, the probability of reporting self-employment earnings on Schedule SE declines in close proportion to W-2 reported wages around initial criminal history events, such that the relative prevalence remains mostly constant.[2] An important caveat is that Schedule SE earnings are self-reported and therefore changes may reflect reporting behavior rather than in underlying earnings. However, we see nearly identical changes in the probability of having *third-party-reported* payments for contract work on 1099-MISC returns, suggesting the observed evolution in Schedule SE earnings reflect changes in actual work and not changes in reporting or compliance.

These findings raise an important question: does the ongoing presence of a criminal record itself drive the persistent declines in W-2 receipt and tax filing that we observe? To isolate the impacts of an observable criminal record itself versus alternative mechanisms, we make use of institutional features that alter the information visible to employers on criminal background checks. In particular, we investigate the impact of retroactive changes in the severity of a charge for eligible felonies in California under Proposition 47, the removal of charges that did not lead to convictions from background checks conducted by consumer reporting agencies (CRAs) under the Federal Credit Reporting Act (FCRA), and removal of charges that did not lead to convictions following a Clean Slate law in Pennsylvania.

Our findings are consistent across these different analyses. Rather than finding that individuals shift out of alternative work arrangements, like gig work, and into traditional jobs when records are remediated, we find consistent evidence that remediation increases the rate of electronically-mediated gig platform work, albeit from a very low base. In practice, however, only a minority of the individuals entering platform work report their earnings on a 1040 Schedule SE.

However, with the exception of very recent convictions, we find no evidence that a change in how or whether a record is reported impacts the filing behavior or reported earnings of a typical person with a record. We find little evidence that a reduction from a felony to a misdemeanor changes non-gig outcomes, on average. We also find no evidence that removal of records from criminal background checks, even for individuals with no other convictions that would still be reported, increases the likelihood of having W-2 reported earnings. This

[1] This finding contrasts with Finlay et al. (2022), in part because we do not condition on filing a tax return when measuring the prevalence of self-employment.

[2] We find no change in the propensity to report Schedule E earnings around an initial misdemeanor conviction.

finding holds across almost every outcome, for both non-convictions and for convictions, and for felonies and misdemeanors. These results are further supported by the analysis of the Pennsylvania Clean Slate law where we also find no effect of having non-conviction records sealed on average filing behaviors or earnings outcomes. The evidence presented strongly points towards the conclusion that in the years after a criminal justice event, lower W-2 reporting is not due to the reporting of criminal records, but to other factors.

The paper is organized as follows. Section 2 describes the IRS data and the publicly-accessible court records we collect from several jurisdictions. Section 3 examines the evolution of taxpayer behavior around different types of initial criminal history events. Section 4 describes several interventions that remediated criminal records and presents our evaluation of the impact of these interventions on taxpayer behavior. Section 5 offers some concluding remarks.

2 Data

In this section, we describe the restricted-access tax data and publicly-available administrative court records used in our analysis.

2.1 IRS Tax Return Data

We study de-identified federal income tax records from the years 2000 to 2019. The tax records include all individuals with a Social Security Number or Individual Taxpayer Identification Number. We draw both on Form 1040 individual income tax filings and third-party reported information returns. Our primary wage and salary earnings and employment outcomes are drawn 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. 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. Beginning in 2012, we break out payments from online platform companies following the method in Collins et al. (2018).[3]

[3] 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. (Forthcoming) 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.

We also examine outcomes directly reported by individual taxpayers themselves to the IRS 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. 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.

2.2 Court Data for FCRA, Maryland, and Clean Slate Analysis

We collect publicly-available administrative court records from Maryland, New Jersey, Pennsylvania, and Bexar County, Texas. These data include information that make it possible to link defendants across cases over time. They include all charges filed with the court within the time period we have data for (both misdemeanor and felony, with the exception of New Jersey, which just contains felonies), the date of the charges, the disposition date and ultimate outcome (dismissed, convicted, etc.), as well as other defendant and case characteristics. We also have publicly-available data on Proposition 47 petitions in San Joaquin County that were filed by the Public Defender's Office in conjunction with the Office of the District Attorney to reduce eligible felonies to misdemeanors.

2.2.1 Bexar County, Texas

The data from Bexar County include all charges filed between 1975 and 2018. 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).

2.2.2 Maryland

The Maryland criminal records were drawn from several publicly-accessible tables hosted on the Maryland Volunteer Lawyers Service (MVLS) database.[4] The Maryland data cover all cases filed between 1990 and 2018.

[4] Access to the database was provided by Matthew Stubenberg, the Associate Director of Legal Technology at the Access to Justice Lab (A2J Lab) at Harvard Law School. Stubenberg created the database for the MVLS using publicly available public court records. Mr. Stubenberg may be contacted at mstubenberg@law.harvard.edu. See https://a2jlab.org/ for more information on the A2J Lab. See https://mvlslaw.org/ for more information on the MVLS.

2.2.3 New Jersey

We obtained New Jersey court data from the Superior Court of New Jersey via a Public Access Information Request.[5] 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.[6] These files contain records from January 1, 1980, to May 30, 2018, which was the last day of the month of the request for the records.

2.2.4 Pennsylvania

The Pennsylvania court data was obtained 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 Common Pleas Court system (which handles felonies) filed between May 2008 and April 2018. We also obtained case numbers that can be accessed through Public Access requests; the case numbers in the AOPC data were current as of when we obtained them in December 2020.[7] Merging this with our original AOPC request, we can see all cases that no longer exist in the data and our conversations with the DA’s office imply that these are all records that were sealed by Clean Slate between June 2019 and June 2020. Our matching indicates that 57% of the nearly 7.5 million non-convictions charges in our AOPC data were sealed, with the ones that were not sealed presumably due to individuals still owing fines and fees at the time of the Clean Slate implementation.[8] This group will serve as our comparison group.

2.2.5 San Joaquin County, California

The data on Proposition 47 petitions filed in San Joaquin County, CA are available on the county website. The data were made available to the public by the Office of the Public Defender of San Joaquin (OPDSJ), CA. The dataset reports all Proposition 47 petitions

[5]According to New Jersey Court Rule 1:38, names, date of births, and locations are not considered confidential personal identifiers. These reports are available for sale to the general public.

[6]To get data on “disorderly person”/misdemeanor offenses would require obtaining data from each of the hundreds of municipal courts in NJ.

[7]The Philadelphia DAO provided these data to us directly to facilitate a quicker turnaround, though all records (in their most current version) are available through standard Public Access requests.

[8]Our research team looked up a small, random subset of case information online for non-convictions that were not sealed and were able to confirm that for that subset, each one owed fines and fees.

that were filed between December 2014 to December 2018 and were successfully reduced by September 2019. We exclude all individuals who were currently serving sentences or under supervision (parole/probation) at the time of the filing as these were generally prioritized and done quickly soon after the law went into effect. With this restriction, we have data on 8,155 successful petitions in San Joaquin.

Most of these petitions were filed proactively on behalf of eligible defendants without the need for the defendants to request the petitions, through a joint effort between the OPDSJ and the San Joaquin County District Attorney's Office. The OPDSJ reported that a small fraction of these petitions were filed on behalf of people who contacted the office to request a reduction, but the data do not directly identify self-petitioners with the exception of 96 individuals who were referred to the OPDSJ for reduction through a local "Justice Fair." OPDSJ reports that they started reductions for those who had eligible drug crimes ("Health and Safety"or HS) as this list was the largest; they received the lists of eligible offenders in alphabetical order by last name and worked through the lists in an alphabetical fashion. Thus, we take a data-driven approach to identifying likely self-petitioners from the proactive petitions by using this alphabetical nature of the order in which petitions were filed. Petitions filed before the "surge" corresponding to the first letter of their last name are identified as likely self-petitions. Appendix B describes how we identified those beginning dates for each surge for each letter on the HS crime list. Throughout the paper, we refer to individuals who had their reductions filed during or after a surge as those whose records were reclassified by "proactive reductions." We refer to individuals whose petitions were filed *before* these clear surge periods as "possible self-petitioners." While there are other idiosyncratic reasons that individuals' petitions would be filed before the office got to their letter of last name, this group is likely to have the bulk of any potential self-petitioners in it. We confirm that our approach is sensible using information we have on Justice Fair participants, who we know were self-petitions and prioritized by the OPD. Among Justice Fair participants in our analysis sample, 70% are accurately flagged as likely self-petitioners using our data-driven methodology.

OPDSJ reported that the office started with the lists of eligible drug crime offenders. Because alphabetical ordering was not preserved for other lists our main analysis sample focuses on individuals with an drug crime: this represents 6,626 of the successful petitions (81.3%). Among this analysis sample, we identify 4,978 as proactive petitions (89%) and 644 as likely self-petitions (11%).

2.3 Data Linkage

The IRS data are linked to the criminal records data using individual name, date of birth, and geographic location and are subsequently de-identified. In the case of San Joaquin County, we were able to match 84% of our main sample to the IRS data. We were able to match 86%, 73%, 81%, and 91% of the data in Bexar County, Maryland, New Jersey, and Pennsylvania, respectively. Further details on our matching algorithm to IRS records can be found in Appendix C. For each jurisdiction, our match rate is comparable or higher than past work matching to IRS or Unemployment Insurance data (Dobbie et al. 2018; Travis et al. 2014). Appendix C also compares characteristics of matched and non-matched individuals in San Joaquin County, CA and the distribution of last criminal history events in the full versus matched sample for our FCRA analysis.

3 Tax Reporting and Earnings Around Initial Criminal History Events

We begin by documenting reporting behaviors and earnings before and after individuals' initial criminal history events. 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. Table 1 Panels (a)-(d) examines mean outcomes around four distinct types of events, pooling data across the jurisdictions with data on each event: (1) misdemeanor charges that do not lead to a conviction, (2) felony charges that do not lead to a conviction, (3) misdemeanor charges that do lead to a conviction, and (4) felony charges that do lead to a conviction; each type of event is presented in a separate panel.

The first column in each panel presents means two years *prior* to the initial event. Prior to their first event, those eventually charged with a misdemeanor start out more likely to file tax returns and have earnings reported on information returns than those eventually charged with a felony, and within each type of charge those who are ultimately not convicted are more likely to file tax returns and have income reported than those who are eventually convicted. Notably, only 55 percent of felony convicts are 1040 filers prior to their initial charge, and nearly 30 percent have no earnings reported on either a W-2 return or on Schedule SE.

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

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

Event time is measured as time since charge for non-convictions, and time since disposition for convictions. α_i is an individual fixed-effect, $\alpha_{a(i,t)}$ are age fixed effects, and α_t is a year fixed effect. Coefficients are relative to two years prior to the event. We plot the β_k coefficients for key outcomes in Figure 1. In the second and third columns of Table 1, we add these estimates to the mean outcome levels two years prior to the event to estimate the outcome levels one year and five years after the events, adjusted for life-cycle changes and macroeconomic conditions.

Figure 1 shows that even non-convictions are associated with substantial and persistent reductions in 1040 filing rates and W-2 earnings. For misdemeanor non-convictions, the probabilities of filing a 1040 or receiving a W-2 decline are five percentage points lower five years after the charge. While the changes in filing and W-2 earnings around misdemeanor convictions are larger in the short run, the long run changes are nearly identical to what we see for misdemeanor non-convictions—with the exception of 1099-reported contract work and Schedule SE self-employment, which, interestingly, decline for those with misdemeanor non-convictions but *not* those with misdemeanor convictions. We find that the impacts of felony non-convictions are more severe than misdemeanor non-convictions in the short run, but the effects converge in the long term.

The observed changes are more pronounced for individuals who are convicted of felonies, and may face incarceration. The probability of filing a 1040 return falls by over 10 percentage points after an initial conviction, amounting to more than a 20 percent decline relative to the already low baseline rate. The decline in 1040 roughly mimics the decline in employer-reported W-2 payments. Strikingly, in the short run, average W-2 reported earnings (including zeros) fall by nearly half. While 1040 filing rates rebound significantly in the long run, W-2 wage earnings only recover slightly by seven years out.

We do not find that individuals with criminal records are more likely to be self-employed. We see in Table 1 that both before and after criminal history events, no more than seven percent of individuals have self-employment income reported on 1040 Schedule SE. Conditional on participating in the workforce—i.e. earning labor income reported on a W-2 or Schedule SE—we never observe self-employment rates above ten percent; this is below the self-employment rate in the broader workforce (Collins et al., 2018). With the exception of misdemeanor convictions (where we observe no change in Schedule SE filing around an initial event), the probability of reporting self-employment earnings on Schedule SE declines in close proportion to W-2 reported wages, such that the relative prevalence remains mostly constant. An important caveat is that Schedule SE earnings are self-reported and therefore changes may reflect reporting behavior rather than changes in underlying earnings. However, in this case, we see nearly identical changes in the probability of having *third-party-reported* payments for contract work on 1099 returns; this suggests the observed changes in Schedule SE earnings reflect changes in actual work and not changes in compliance.

The results in Figure 1 and Table 1 pool individuals across jurisdictions. However, we see highly similar patterns across each jurisdiction, as shown in Appendix Figure A.1. We examine sensitivity to studying changes around individuals' *most recent* events in the data instead of their first events in Appendix Table A.2 and Appendix Figure A.2—the primary difference being that one's most recent event may follow prior events.[9] We find similar effects when looking at latest events, though we see less long-term scarring in the case of felony convictions; we interpret this result as suggesting that initial felony convictions lead to larger long-run declines in earnings than subsequent convictions.[10]

We caution that these patterns in employment around the time of either the first or last criminal history event do not necessarily reflect the causal impact of having a record. These persistent trends could be the direct result of the criminal charge or conviction, or they could also be the result of other unobservable events in an individual's life that caused both the criminal charge and a decline in employment, such as recent drug addiction or job loss. The reduction in any wage employment for convictions could also be the result of incapacitation effects stemming from incarceration, though this is not the case for non-convictions which would not result in incarceration sentences.

In summary, these patterns show that formal labor sector engagement falls sharply and persistently after a criminal charge or conviction (even when there may be other criminal charges in the person's past). A key finding is that changes in taxpayer filing behavior closely follows changes in *firm-reported* earnings. In particular, the changes in 1040 filing generally follow the evolution of W-2 earnings, and changes in Schedule SE filings generally follow the evolution of 1099-MISC non-employee compensation. This finding suggests that changes in taxpayer behavior around criminal history events largely reflect the persistent decline in employment and earnings following those events, which we examine in more depth in the next section.[11]

[9] When examining non-convictions, we omit cases where individuals have prior *convictions* but not cases where they have prior non-convictions, since these are the cases directly impacted by the FCRA law studied below.

[10] Using information on defendant race from the case records, we plot effects by race in Figures A.3 and A.4 and find larger drops in employment for Black individuals around initial events. We also run separate event studies by industry of the payer of the largest W-2 for non-convictions and convictions and report the event study coefficient for the year of the conviction in Appendix Figures A.11 and A.12. We find that the employment impact for non-convictions varies across industries. For convictions, a similar decline is seen across nearly all industries.

[11] The patterns also establish that the matching procedure between criminal records and the IRS data are accurate in that they yield the expected patterns in the data post-event.

4 Tax Reporting Implications of Criminal Record Remediation

We find that taxpayer earnings and filing behavior rates fall persistently after criminal history events—even charges that do not lead to convictions. There are several reasons employment rates may be lower for individuals with criminal records. A criminal record itself can be a direct barrier to employment. Some industries, such as healthcare or education, legally prohibit the hiring of individuals with certain criminal records. Across several surveys, over 90 percent of employers state that they perform background checks for all or some of their positions (Society for Human Resource Management 2012; HireRight 2015).

In this section, we use institutional features of the criminal justice system to examine the implications of remediating criminal records on tax filing and compliance. We begin by presenting results for the federal Fair Credit Reporting Act (FCRA), estimating the impact of removing non-convictions from criminal background checks at seven years. We then present estimates for California's Proposition 47 in San Joaquin County where criminal justice agencies proactively petitioned to have eligible felonies reclassified as misdemeanors in cases covered by the new law. Finally, we present our findings for Pennsylvania Clean Slate, which initially automated the sealing of all non-convictions for individuals that did not owe fines and fees.

4.1 Evidence from the Federal Consumer Reporting Act

4.1.1 Research Design

FCRA prohibits reporting of criminal charges that did not lead to a conviction after seven years for jobs that pay less than $75,000 a year. We study this policy using publicly-available administrative criminal records data from Maryland, New Jersey, Pennsylvania, and Bexar County, Texas. We provide additional detail on FCRA and related state laws in Appendix B.

We use the seven-year rule under FCRA to estimate the effect of having a non-conviction record cleared from a CRA-run employment background check. Under this rule, for an individual who has no convictions on their record, their criminal record should be completely cleared seven years after the last criminal charge. 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. Accordingly, we examine the evolution of outcomes following a criminal charge that is eventually dismissed with a focus on the sharp seven-year change in reporting, comparing outcomes for individuals before and after this non-conviction charge is removed.

In the baseline analysis, our focus is on charges that did not lead to a conviction, when that non-conviction charge is no longer reportable by a CRA. We therefore restrict our analysis of non-convictions to individuals with no other conviction in that jurisdiction because nothing should be reported on these individuals' CRA background checks at the seven year mark. We define event-time in relation to the year in which the record is cleared (the FCRA event). We run a fully saturated event-study specification, and balance the sample three years prior to the FCRA event and one year post the event, so that the estimated coefficients around the event are not driven by changes in sample composition. Our main analysis separately examines last events that are felony non-convictions, misdemeanor non-convictions, and convictions. Our main specification is given by:

$$y_{it} = \sum_k \beta_k \mathbb{I}\{E_i = t + k\} + \alpha_i + \alpha_{a(i,t)} + \alpha_t + \epsilon_{it} \tag{2}$$

Event time is measured as time since last criminal history event. $\alpha_{a(i,t)}$ are age fixed effects, and α_t is a year fixed effect.

For the FCRA analysis, we use separate analysis samples to analyze the clearance of a non-conviction and conviction. The FCRA rule allows us to estimate the effects of having a non-conviction record cleared seven years after the last criminal history event. 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. To reduce measurement error associated with measuring no 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. We focus on the last event and limit to individuals with no other conviction in that jurisdiction 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 study convictions, we restrict our sample to individuals whose last event was either a felony or misdemeanor conviction. Reporting of convictions does not change after seven years in Texas, New Jersey and Pennsylvania. In Maryland, convictions are removed from employer criminal record searches for low-income workers after 7 years.

Appendix Table A.1 Panel (a) presents summary statistics for the FCRA analysis samples. 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 (or alternatively, two years before the potential FCRA removal event). 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.

4.1.2 Results

In Figures 2 and 3 Panels (a)–(b), we plot event-study coefficients for the removal of a felony and misdemeanor non-conviction, respectively, which occurs seven years after the original event. Figure 2 reports share with any 1040 filing and Figure 3 reports share with any W-2 wages. Even though non-conviction events are associated with significant drops in 1040 filing and W-2 reported employment, 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 or tax filing.

Figures 2 and 3 Panels (c)–(d) show the same event-study plots for last felony and misdemeanor convictions on record. Recall that in Maryland these convictions are no longer reportable seven years after their disposition, but in the other jurisdictions they are reportable indefinitely. And yet across jurisdictions we see similar patterns of no divergence in the likelihood of having either a 1040 return or W-2 reported earnings around the seven year mark.

Tables 2 and 3 report formal tests of whether the event study coefficients reported in Figures 2 and 3 seven and eight years after the last event are different from a linear trend implied by our event-study coefficients, for both 1040 reporting, any W-2 wages, and other employment outcomes.[12] Event-time coefficients will identify any trends around the event. Because a criminal history event mechanically occurred seven years earlier than our FCRA event, individuals could still be recovering from the initial event. For exposition purposes, we pool the jurisdictions. Table 2 Panel (a) reports results for misdemeanor non-convictions

[12] 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, β_{+4}. If β_{+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 be suggestive that FCRA is having a positive impact.

and Panel (b) reports results for felony non-convictions. Table 3 Panel (a) reports analogous tests of deviations from trend in year 7 and 8 for convictions in Bexar County, New Jersey, Pennsylvania and Panel (b) reports results separately for Maryland, which has a state FCRA law limiting the reporting of convictions after seven years.

Consistent with Figures 2 and 3 Panels (a) and (b), Table 2 shows that for non-convictions (both felony and misdemeanor), we find no detectable deviation from the pre-trend trend around the timing of the record removal. There is similarly no significant discontinuity or deviation from trend for any type of earnings or reporting behavior. With respect to convictions, Table 3 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 1). Table 3 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 3 and Appendix Figure A). 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. We see similar trends for tax filing, and little response on our measures of self-employment or independent contracting.

These results are highly robust. Our finding of no discrete change in trend in W-2 reported earnings seven years after a criminal charge (or conviction) is removed holds across subsamples by industry (see Appendix Figure A.13) and by crime type of the last event (see Appendix Figure A.14). Both of these Appendix figures report whether the event study coefficients seven years after last charge are different from the linear pre-trend. We look separately at effects for defendants identified as Black versus all others (see Appendix Figure A.7).[13] We also look seperately at effects by gender and age in Appendix Figure A.8 and in Appendix Figure A.9. In Appendix Figure A.10 we also consider the impact of these criminal history removals on employment in large firms (firms with 10,000 or more workers) and small firms (firms with less than 100 workers). Finally, we find null effects at year seven using an alternative differences-in-differences estimator following Sun and Abraham (2021) (see Appendix Figure A.16). Overall, these results imply that removing criminal history from CRA background checks after seven years does not result in positive impacts on employment and tax-filing outcomes for affected individuals.[14]

[13] Race data is available in public court records in Bexar County, Texas, Maryland, and Pennsylvania.

[14] One explanation for the null result might be that employers are asking applicants about records, and any information obtained in a background check 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.6 shows that even in post-BTB years, there remains a null effect after seven years of record sealing.

An important exception to the pattern of null results is non employee compensation. For non-convictions, we see increases in 1099 work that are above trend. For convictions, we see an increase in online-platform mediated gig work. While this type of employment is a small share of overall employment, gig jobs are interesting because, for the most part, gig platform work does not involve an interviewing process or an evaluation of one's work history. If an applicant can pass the initial requirements necessary to be on the platform, they are allowed to begin to earn money on the platform. Gig platform work, such as ride-sharing and app-based delivery services, has increased dramatically in recent years, and may provide opportunities that were not previously available. Because this type of gig work has only been prevalent since circa 2012, for this part of our analysis, we restrict our sample to years since 2012 and FCRA events beginning in 2015.

Event-study results pooling all criminal history events and jurisdictions over the period since 2012 are plotted in Figure 4. The left-hand 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 patterns around the initial criminal history event for wage employment, and is suggestive that the criminal record limits employment opportunities in gig work. The right hand panel of Figure 4 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).

While gig work is a new form of work activity, we find evidence that removal of a criminal record via FCRA has a large (in percent terms) impact on gig work for this particularly disadvantaged group, many of whom are likely entering self-employment for the first time. The increase in Schedule SE filing is only about one-quarter the size, suggesting low earnings after expenses or compliance issues[15].

4.2 Evidence from Proactive Felony Reductions After California Proposition 47

4.2.1 Research Design

We next study the labor market impacts of criminal record remediation efforts under California's 2014 Proposition 47, which reclassified certain theft and drug possession felonies to

[15] Collins et al. (2018) document that only a small share of platform-based gig workers in the broader population file a Schedule SE and discuss several potential explanations.

misdemeanors. While largely prospective in nature, Proposition 47 also allowed individuals with eligible offenses to petition to have their previous felonies reclassified as misdemeanors, with an estimated one million Californians eligible for a record reduction under the law.

We focus our analysis in San Joaquin County, CA, where criminal justice agencies worked to *proactively* reduce tens of thousands of eligible felonies to misdemeanors without involvement or notification to eligible individuals. Starting in December 2014, the Office of the Public Defender of San Joaquin (OPD) and the San Joaquin County District Attorney's Office (DAO) coordinated to file petitions on behalf of all eligible defendants without requiring effort, intervention, or even knowledge from the defendant. As of September 2019, this effort resulted in the reduction of approximately 10,000 felony convictions under Proposition 47. They have posted information about these reductions on their website publicly. For a large subset of defendants, the order in which their proactive reductions were filed was unsystematic, based on the first initial of their surname, giving rise to plausibly exogenous variation in the timing of record reductions among a sample that did not self-select into treatment. We use this quasi-experimental variation to estimate the causal effect of these reductions on labor market and tax outcomes. We describe Proposition 47 and the reductions in San Joaquin County in detail in Appendix B. As described in Section 2.2.5, we also take a data-driven approach to identify individuals who were more likely to have self-requested a petition rather than have their petitions filed proactively, leveraging this alphabetical ordering, a group we call likely self-petitioners.

For this analysis, we estimate conventional event-study models for our labor market and tax outcomes. We define the event as the year a person in the estimation sample obtains a Proposition 47 reduction of their felony to a misdemeanor. Our focus will be on "proactive" reductions, that is, those reductions that were initiated by the Public Defender and District Attorney without knowledge or involvement by the affected individuals, but we also make comparisons to the sample of possible self-petitioners as well as the pooled sample that combines the two groups. In the main Proposition 47 analysis, we estimate the following event-study specification:

$$y_{it} = \sum_k \beta^k 1\{E_i = t + k\} + X_{it}^{\prime}\gamma + \alpha_i + \alpha_t + \varepsilon_{it} \tag{3}$$

where y_{it} is the outcome of interest (e.g. any wages, any self-employment, etc.) for individual i in year t. $1\{E_i = t+k\}$ is an indicator for the Proposition 47 reduction occurring k periods from t, with negative k indicating a future event date, and positive k indicating the event occurred k years in the past. α_i are individual fixed effects, α_t are year fixed effects, and

X_{it} includes a quintic in age. Standard errors are clustered at the individual level. The coefficients of interest are β^k, which trace out the labor market impact of a Proposition 47 reduction. For consistency across policies, we omit $k = -2$ so that the estimated β^k coefficients are relative to two years before the reduction.

Summary statistics on individuals matched to the IRS data are presented in Appendix Table A.1. Table A.1 Panel (b) presents summary statistics for our estimation sample of individuals who received reductions under Proposition 47 in San Joaquin County, CA, pooled (columns 1-2) and separated by individuals who we identify as likely self-petitioners using the procedure described above (column 3) and those who received proactive reductions without their knowledge or involvement (column 4). Compared to those who received proactive reductions, likely self-petitioners are much more likely to receive reductions within seven years of the original conviction (18.0 percent versus 6.1 percent).[16] In terms of baseline outcomes measured at two years prior to the Proposition 47 reduction, likely self-petitioners are slightly younger and are negatively selected in terms of wages, with annual baseline earnings of \$6,003 compared to \$7,920 for those who received proactive reductions. These differences indicate that likely self-petitioners are not a random subset of eligible individuals.

4.2.2 Results

Figure 5 Panel (a) plots event-study coefficients around the Proposition 47 reduction for all proactive drug offense petitions (N = 4,978), along with 90 and 95 percent confidence intervals. We do not find any statistically significant increase in W-2 wage earnings in the years after the reduction occurs. This null effect is precisely estimated and a 90 percent confidence interval rules out effect sizes larger than a 3.6 percentage point increase in the year of reduction. Consistent with the unsystematic ordering of these proactive reductions, there are parallel trends between the treated and comparison group in the pre-reduction periods. Figure 5 Panel (b) reports analogous event-study coefficients for the subsample we identified as likely self-petitioners (N = 644). In contrast to Panel (a), we find that in the year of the reduction, any wage employment is 3.7 percentage points higher than in the year prior to the reduction (a nearly 10% increase, $p < .10$). This effect can be ruled out by the 90 percent confidence interval of the estimates from Panel (a) for proactive reductions. By two years after the reduction, this increase relative to the year prior to the reduction drops to 1.7 percentage points. However, consistent with selection into treatment, Panel (b) documents increases in any wage employment that begin in the several years *before* the reduction.

To formally compare our findings for those who received proactive reductions to likely

[16] Panel(c) provides comparisons of the standard deviations of baseline characteristics in both of these two groups.

self-petitioners, Figure 5 Panel (c) plots event-study estimates where we interact time since event with an indicator for being a likely self-petitioner. The reported coefficients estimate the differential effect of the reduction for self-petitioners versus proactive reductions by year since event. The findings document notable and statistically significant differences in both pre-trends and post-treatment effects among these two groups.

Figure 5 Panel (d) presents results combining the proactive reductions and likely self-petitioners. As can be seen, there is an uptick in any wage employment in the one to two years following the Proposition 47 reduction. This increase is significant at the 10% level. Notably, even a small number of observations that were self-selected into treatment can be influential for the conclusions, highlighting the importance of accounting for selection. Moreover, when we focus further on the subset of likely self-petitioners whose reductions were obtained more than seven years after the original conviction—at which point their convictions can no longer be reported by CRAs under California's ICRAA law—we continue to observe an increase in employment despite the fact that their charges would have largely been hidden for most purposes regardless of their Proposition 47 reduction (panel e). There also remain large pre-trends, with significant rises in employment several years preceding the reductions. While the post-treatment employment increase could be due to these self-petitioners specifically working in jobs that require occupational licenses, these results are also consistent with selection driving the results for likely self-petitioners, rather than real treatment effects of the reduction.

To examine the broader set of tax reporting behavior, Table 4 collapses the event-studies estimates for proactive reductions to a single "treated" coefficient. This specification is given as follows:

$$y_{it} = \beta \text{Treated}_{it} + \sum_{k \in K \leq -2} \delta^k 1\{E_i = t + k\} + X_{it}^{'}\gamma + \alpha_i + \alpha_t + \varepsilon_{it} \tag{4}$$

Under this specification, the coefficient of interest is β, which estimates the average impact of a Proposition 47 reduction in all observed post-treatment years.[17] We report impacts of the proactive reductions for a range of employment outcomes, including any wage employment and wage employment across certain income thresholds, as well as measures of self-employment.

Consistent with Figure 5, we cannot rule out a null effect of PD-initiated reductions on any wage employment in Panel (a). There are also no statistically significant effects for other employment outcomes, including earning wages above $7,500 or $15,000 or any other 1099 work. The results in Column (3) show this finding holds even among individuals with only

[17] Tabulate the full δ^k coefficients in Appendix Table **??**.

one felony.[18]

We further explore whether tax reporting effects are different depending on the time since original conviction. In California, a criminal conviction can only be reported on an employment background check for seven years after the latter of disposition date, release date, or parole violation, unless another law requires employers to look more deeply into the employee's background. To capture potential dynamics in treatment effects, in Table 4 Panel (a) Column 4 we interact our Post indicator with the number of years since conviction at the time of proactive reduction.[19] For the main employment outcomes, we find evidence consistent with the hypothesized relationship. The estimated interaction term is indeed negative, and strongly significant implying that the benefits of a felony reduction are diminishing with time since initial disposition.

As in the FCRA analysis, one exception to the pattern of null average effects is gig platform taxable income. In Table 4 column (5) of Panel (a), we see that a proactive Proposition 47 reduction is associated with a 0.4 percentage point increase in the rate of gig work that is statistically significant at the 5% level. This increase doubles the rate of gig work prior to reduction.

Panels (b) and (c) report show complementary results for likely self-initiated petitioners and for the pooled sample. The tabulated results consistent with our findings in Figure 5. We present effects on additional outcomes in Appendix Figure .

To understand whether part of the reason we see no impact of felony reductions is due to lack of knowledge on behalf of impacted individuals, we analyze the impacts of an effort by the Public Defender's office (along with resources from the District Attorney's office) in San Joaquin county to notify individuals about these reductions. The notifications took place in randomized waves, with 4086 individuals with reductions being chosen to be notified at the time of the analysis (notifications are ongoing). The notifications we analyze took place between June 2019 and March 2020. Of the 4610 individuals randomly chosen to be contacted in this first-wave, contact information could be located for 3982 (86.3%). Between June 2019 and March 2020, SJOPD personnel with carefully written scripts attempted to call these 3982 individuals; in January 2020 letter were mailed to individual homes (with self-addressed postcards included to return upon receipt); and in January 2020 e-mails were

[18] In Appendix Figure A.15, we examine whether our event-study estimates are biased due to treatment effect heterogeneity by constructing alternative estimators following Sun and Abraham 2021. In Appendix Table **??** we display results from specifications with alternate age controls. In both cases continue to find null effects of the reductions on any wage employment for those that received proactive reductions. In Appendix Table , we explore an alternative approach where the year of reduction is instrumented with the year of the Public Defenders surge for last names beginning with the same letter in the pooled sample, but we obtain imprecise estimates.

[19] We do not have data on release dates.

sent as well.[20]

In Appendix Figure A.17, we present raw trends in outcomes for the group that was notified as well as the group that was not.[21] We also present intention-to-treat (ITT) estimates of employment outcomes in 2019 and 2020 between the notified and not notified groups.[22] We present results for any wage employment and wages > \$15,000. This figure reveals remarkably similar raw trends in any wage employment and wages exceeding \$15,000 in the years before and after notification. The COVID-19 pandemic began in the second year of the post-treatment period, but only small dips in employment rates are observed in that year and the notification and comparison groups respond similarly. Our intent-to-treat (ITT) estimates confirm that individuals chosen for notification did not experience detectable improvements in labor market outcomes compared to those not chosen for notification. Appendix Table A.8 presents the full set of employment outcomes associated with notification. We find null effects across all outcomes. In Appendix Table A.9, we present estimates of the average treatment effect of *successful* notification on the treated (TOT) using notifications assignment as an instrument for successful contact, but continue to find no evidence of an effect. In sum, these results imply that lack of knowledge about a Proposition 47 reduction is unlikely to be the main driver of our null result among individuals who received proactive reductions.

4.3 PA Clean Slate Law

4.3.1 Research Design

The last institutional feature we study is Pennsylvania's Clean Slate Law of 2018, which legislated automated sealing of *all* non-convictions and some older low-level convictions. PA Clean Slate automated the sealing of non-conviction records with no waiting period for individuals who did not owe fines and fees to the court at the time of the initial set of sealings. We provide additional details about the Clean Slate Law in Appendix B. We use individuals that owed fines and fees as a comparison group for individuals who received automatic

[20] In Appendix figure A.7 we show covariate balance across people who were notified in the first wave and those who were not.

[21] We downloaded the list of contacted defendants from the public link at the website of the San Joaquin County public defender's office (Last accessed 6/20/22). See: https://www.sjgov.org/department/pubdef/programs- services/proposition-47.

[22] We estimate: $y_{it} = \beta$ Notified$_{it}$ + $X_{it}^{'}\gamma +$ OneFelony$_i$ + ε_{it} where y_{it} is the outcome of interest for individual i in year t, where we run separate regressions for each year of interest. $Notified_i$ is an indicator treatment, i.e. being notified in the first wave. X_{it} includes a quintic in age, and $OneFelony_i$ is an indicator for being on the one felony list. We control for an indicator for one felony because the randomization was stratified in this dimension. Standard errors are clustered at the individual level. β captures the causal effect of notification of Proposition 47 reduction on labor market outcomes.

sealings to difference out any trends in employment for people with criminal system contact during this time period. Summary statistics on individuals matched to the IRS data in both groups are presented in Panel (d) of Appendix Table A.1.

Our analysis focuses on the subset of individuals who only have non-convictions on their records, as these individuals' entire criminal histories are cleared 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 cleared between June 2019 and June 2020 due to the fines and fees. Our main specification is given as follows:

$$\begin{aligned} y_{it} = {} & \beta \text{Cleared}_i \times \mathbf{1}\{Year_t \in \text{2019-2020}\} + \sum_{k \in 2016,2017} \delta_k \text{Cleared}_i \times \mathbf{1}\{Year_t = k\} \\ & + X_{it}^{\theta} \gamma + \alpha_i + \alpha_t + \varepsilon_{it} \end{aligned} \tag{5}$$

where Cleared_i is an indicator for an individual having their record cleared (i.e. an ever-treated indicator), and $\mathbf{1}\{Year_t \in \text{2019-2020}\}$ is an indicator for being in the period after records were cleared. The interpretation of β is the difference-in-difference estimator, comparing the change in the outcome in the post period between those who had all their misdemeanor charges cleared with those who did not because they owed fines and fees. The interactions between Cleared_i and earlier years provide a test for pretrends.[23]

To examine if the effect varies with the time elapsed since the last charge, we also estimate a triple difference specification including additional interations with months since the latest charge (calculated as months since June 2019):

$$\begin{aligned} y_{it} = {} & \beta_1 \text{Cleared}_i \times \mathbf{1}\{Year_t \in \text{2019-2020}\} + \beta_2 \mathbf{1}\{Year_t \in \text{2019-2020}\} \times \text{Months since charge}_i \\ & + \beta_3 \text{Cleared}_i \times \mathbf{1}\{Year_t \in \text{2019-2020}\} \times \text{Months since charge}_i \\ & + \sum_{k \in 2016,2017} \delta_k \text{Cleared}_i \times \mathbf{1}\{Year_t = k\} + X_{it}^{\theta} \gamma + \alpha_i + \alpha_t + \varepsilon_{it} \end{aligned} \tag{6}$$

The interpretation of the coefficient on $\text{Cleared}_i \times \mathbf{1}\{Year_t \in \text{2019-2020}\}$ is now the out of sample predicted effect at 0 months since the initial charge. The earliest charge reductions 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.[24]

[23] 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 our charge data, which begins in 2008.

[24] We do not know the exact date the sealing took place within this date interval.

4.3.2 Results

Table 5 Panel (a) presents these differences-in-differences results, where we interact a Post (2019-2020) indicator with a treatment indicator for individuals whose records were cleared. Panel (b) presents estimates including a triple interaction with months since original charge to test whether more recently cleared charges had a differential effect on employment. We present graphical event-study estimates in Appendix Figure A.18. While these non-conviction records were generally cleared less than seven years before the original event (on average cleared 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.[25]

5 Discussion and Conclusion

Our paper documents that initial criminal history events are associated with sharp and persistent declines in the propensities to have W-2 reported earnings and to file a 1040 return, even in cases where charges did not lead to convictions. We find no evidence that remediation for individuals who have had criminal records for multiple years improves filing rates or earnings. Our findings point to the conclusion that older records do not directly suppress individuals' earnings and filing rates, which could be because the initial short-term impacts of a record—for example, resume gaps and loss of experience—lead to longer term labor-market scarring that can be difficult to undo.

An initial motivation for this work was our hypothesis that individuals with criminal histories facing barriers to traditional employment might shift towards alternative work arrangements like gig work and other self-employment where they bear the full burden of tax compliance. In practice, we do not find this to be true. Rather, in most cases self-employment earnings of Schedule SE and 1099-reported nonemployee compensation both decline in close proportion to W-2 employment after most types of criminal history events. Contrary to our expectations, we find that clearing records leads to *increases* in platform-mediate gig work reported on 1099 returns, though in most cases those gig earnings are not reported on Schedule SE. These findings suggest that non-traditional work arrangements, which have typically been unstudied, may be an increasingly important avenue of work for

[25] 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.10 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. We also estimate effects separately by race in Appendix Tables A.11 and A.12.

those who have previously had a criminal record–but an avenue that places the burden of tax compliance on the individual, potentially leading to compliance challenges.

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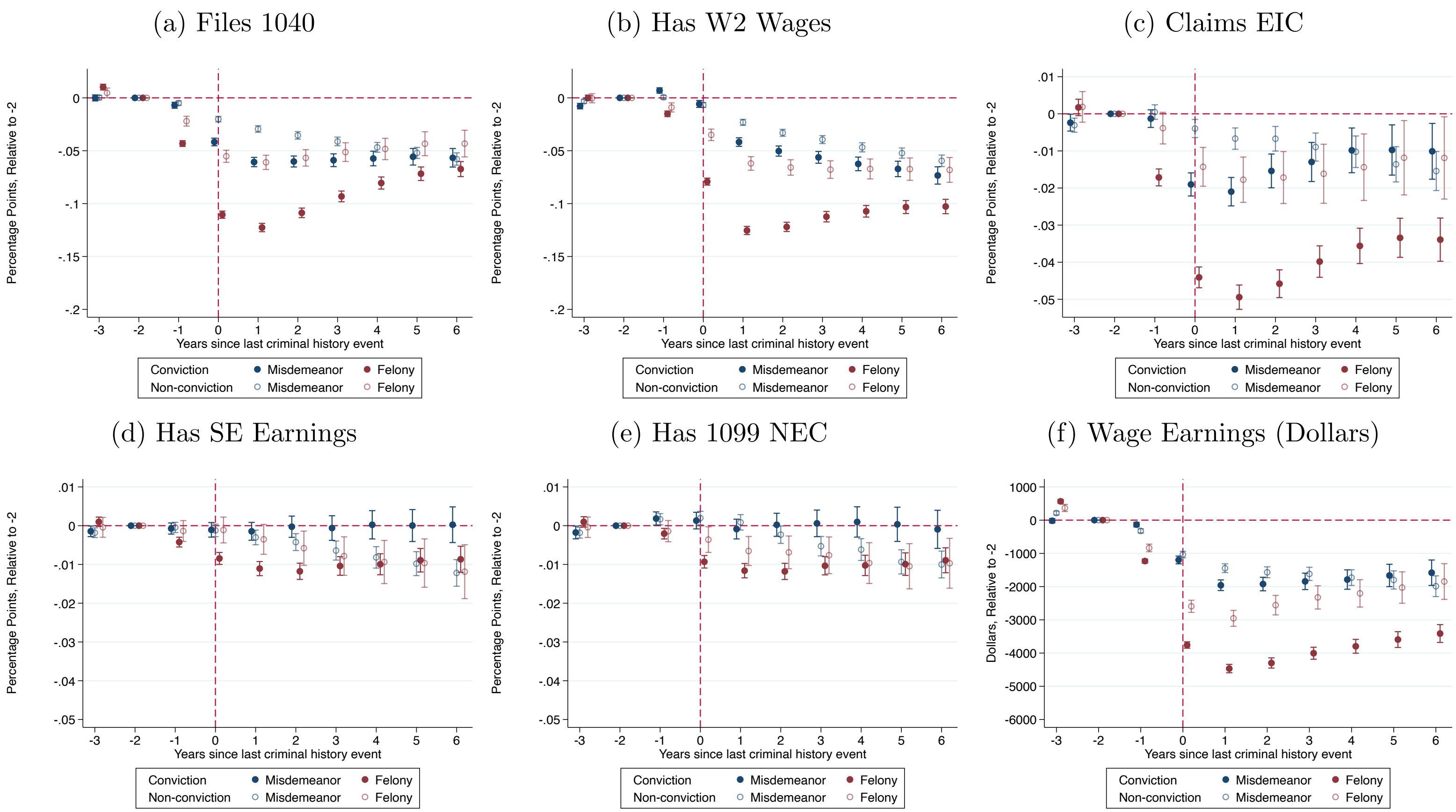
Figures and Tables

Figures

Figure 1: Reductions in Tax Filing After First Event

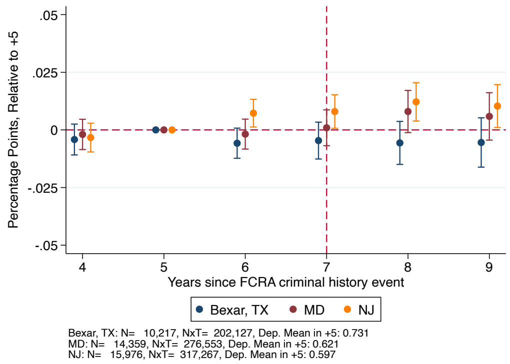


Notes: Each panel plots selected event study coefficients for the specified outcome after an initial criminal history event following specification 1 in the text, where the type of event is as specified in the legend. For this analysis, we restrict the full sample to have been 18 by the time the first 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.

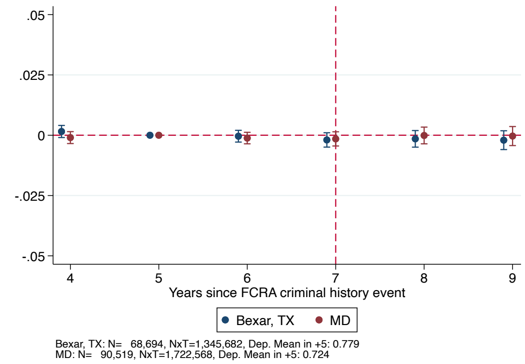
Figure 2: FCRA Event Study of Any 1040 Around Removal (Year 7)

Note: MD has State FCRA for Convictions

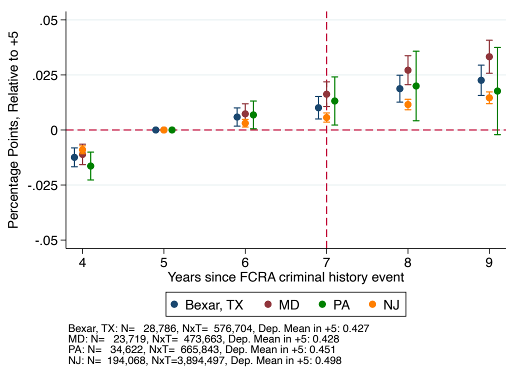
(a) Felony Non-Convictions, no other convictions



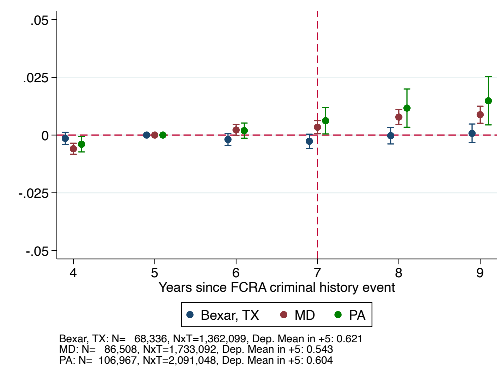
(b) Mis. Non-Convictions, no other convictions



(c) Felony Convictions



(d) Mis. Convictions

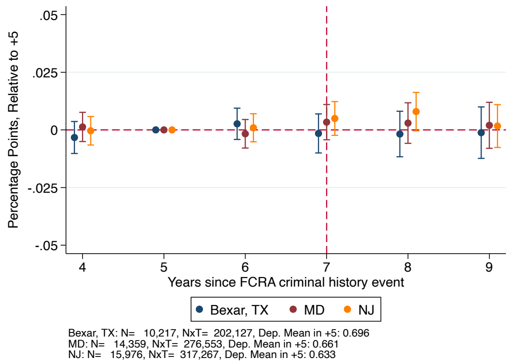


Notes: Each panel plots selected event study coefficients for the share with any 1040 filing around 7 years after the event, following specification 2 in the text. Timing from the event is based on the charge date for non-convictions and disposition date for convictions. Coefficients are relative to +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.

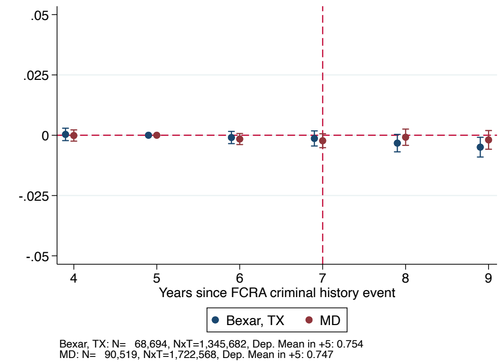
Figure 3: FCRA Event Study of Any Wages Around Removal (Year 7)

Note: MD has State FCRA for Convictions

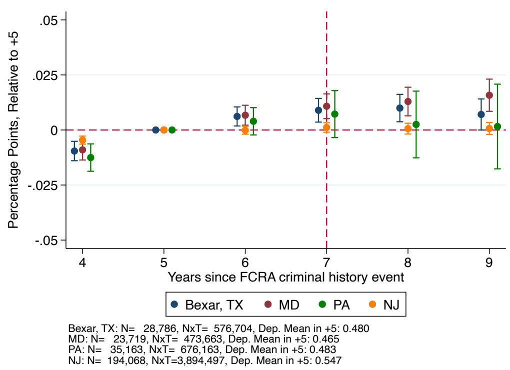
(a) Felony Non-Convictions, no other convictions



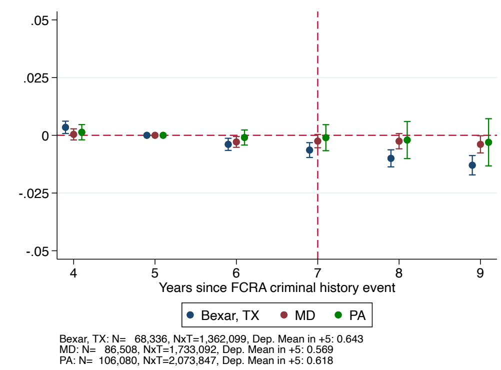
(b) Mis. Non-Convictions, no other convictions



(c) Felony Convictions

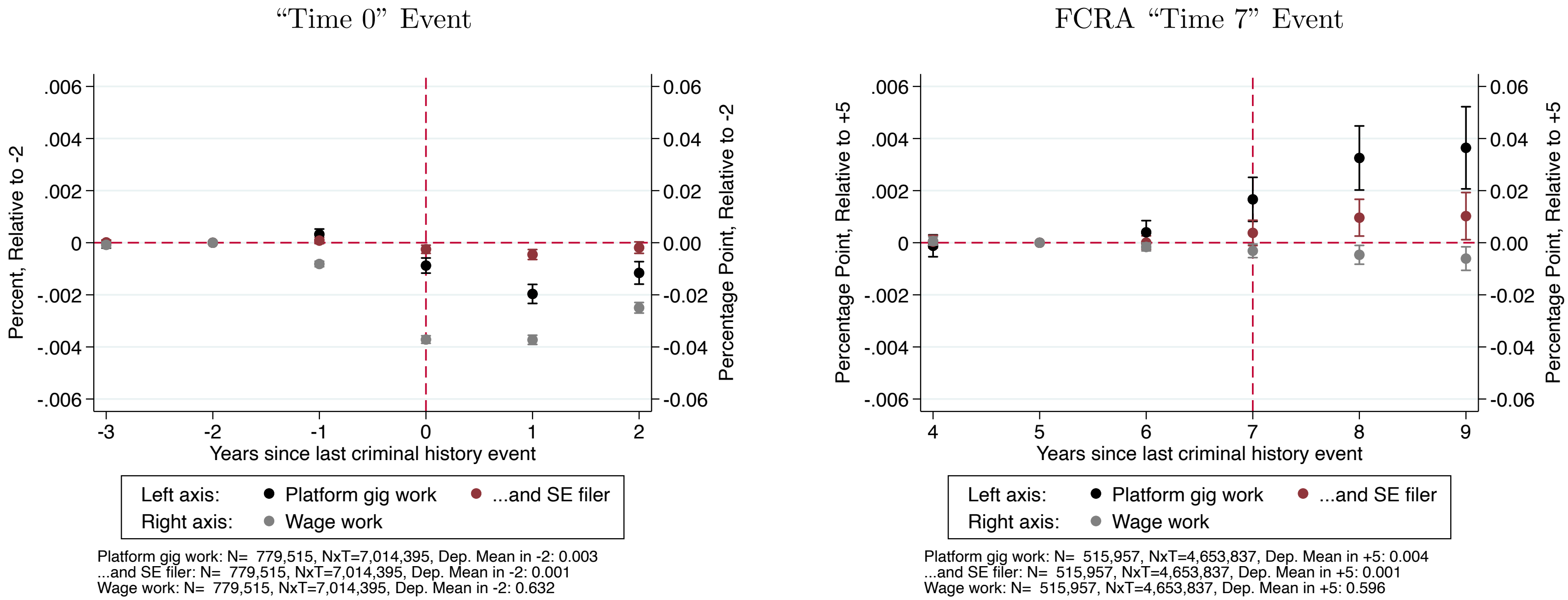


(d) Mis. Convictions



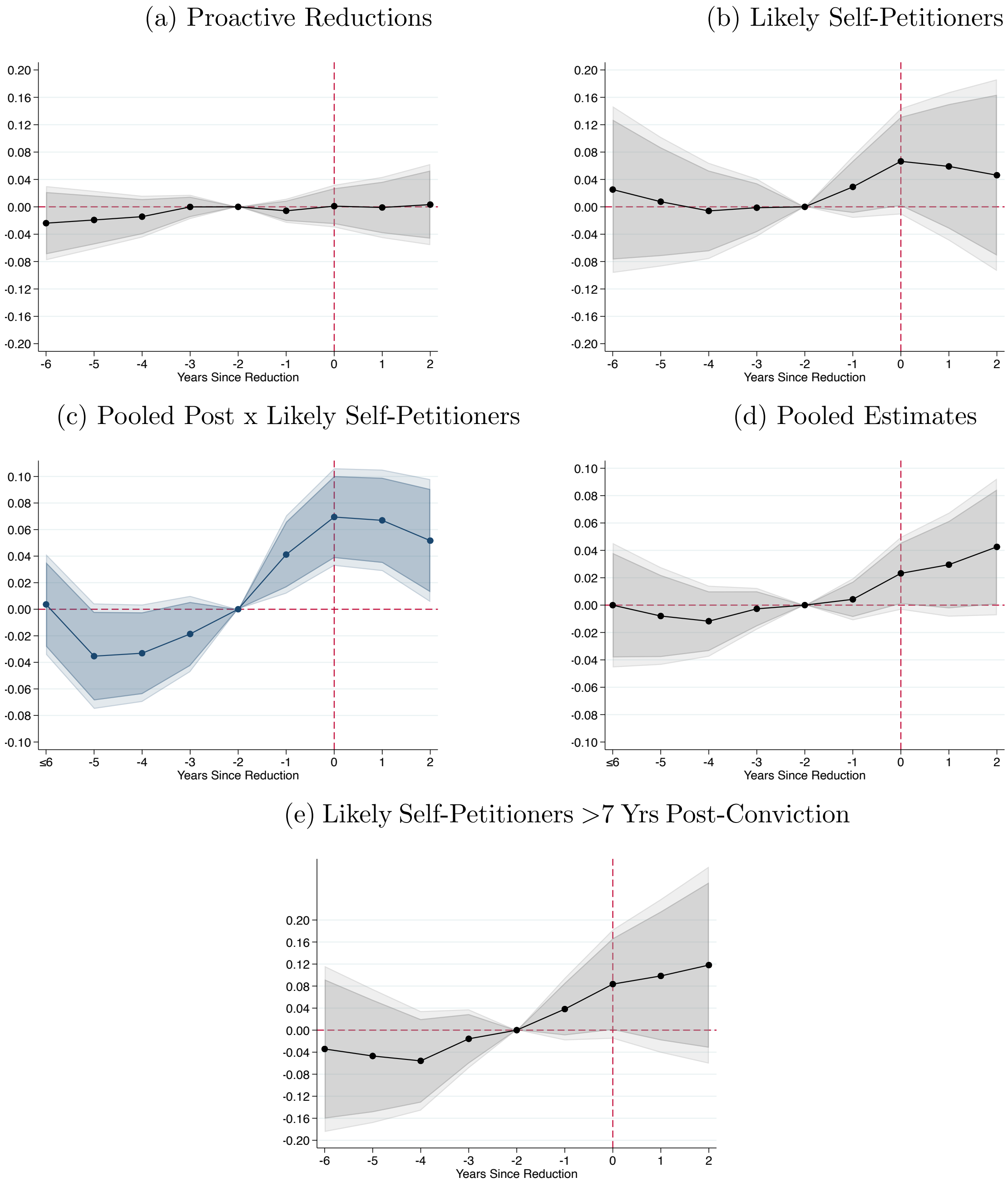
Notes: Each panel plots selected event study coefficients for the share with any wages > \$0 around 7 years after the event, following specification 2 in the text. Timing from the event is based on the charge date for non-convictions and disposition date for convictions. Coefficients are relative to +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 4: FCRA Event-Study Estimates: Any Gig Platform Work



Notes: Each panel plots selected event study coefficients for the share with any gig 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 5: Any Wage Employment Around CA Prop 47 Reductions in SJ County



Notes: Figure shows event-study coefficients for having any wage employment around Proposition 47 felony reductions in San Joaquin County, CA. Panels A and B report event-study coefficients from separate regressions following equation 3 in the text for proactive reductions and likely self-petitioners, respectively. Panel C plots event-study coefficients from an interaction of post-reduction and likely self-petitioner, representing the differential labor market impacts for likely self-petitioners compared with individuals who received proactive reductions. Panel D pools all individuals (both likely self-petitioners and proactive reductions) in our main analysis sample of individuals with HS charges. Panel (E) plots event-study coefficients for likely self-petitioners who received reductions more than seven years post-conviction. Darker shading shows 90 percent confidence intervals, and lighter shading extends out to 95 percent confidence intervals.

Tables

Table 1: Tax Reporting Behavior Before and After First Criminal History Events

(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. Table also presents mean outcomes one and five years after the specified event implied by our event study estimates of 1, which estimate the change relative to period -2 controlling for aging and macroeconomic conditions. Specifically, we add our estimates of β^1 and β^5 from 1 for each event type (displayed in Figure 1) 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 2: 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 3: 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 4: Impact of Proposition 47 Reductions on Employment Outcomes

(a) Public Defender Initiated Petitioners

	(1) Any Wages>$0	(2) ...>$15,000	(3) ...>$0	(4) ...>$0	(5) Any Gig	(6) Files SE
Prop 47 Reduction	0.003	0.002	0.003	0.054*	0.004*	0.003
	(0.014)	(0.012)	(0.015)	(0.026)	(0.002)	(0.006)
Prop 47 Reduction × 1 Felony			-0.006			
			(0.026)			
Prop 47 Reduction × Years Since Crime				-0.004**		
				(0.001)		
Dep. Mean (-1)	0.338	0.195	0.338	0.335	0.002	0.029
N	4,967	4,967	4,967	4,336	4,967	4,967
NxT	94,373	94,373	94,373	82,384	94,373	94,373
Age Controls	X	X	X	X	X	X
Indiv. FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X

Standard errors clustered on individual in parentheses
[a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

(b) Likely Self-Initiated Petitioner

	(1) Any Wages>$0	(2) ...>$15,000	(3) ...>$0	(4) ...>$0	(5) Any Gig	(6) Files SE
Prop 47 Reduction	0.034	0.019	0.036	0.024	-0.004	0.019
	(0.036)	(0.029)	(0.036)	(0.051)	(0.006)	(0.013)
Prop 47 Reduction × 1 Felony			-0.018			
			(0.054)			
Prop 47 Reduction × Years Since Crime				-0.000		
				(0.003)		
Dep. Mean (-1)	0.334	0.160	0.334	0.330	0.005	0.031
N	655	655	655	615	655	655
NxT	12,445	12,445	12,445	11,685	12,445	12,445
Age Controls	X	X	X	X	X	X
Indiv. FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X

Standard errors clustered on individual in parentheses
[a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

(c) Pooled

	(1) Any Wages>$0	(2) ...>$15,000	(3) ...>$0	(4) ...>$0	(5) Any Gig	(6) Files SE
Prop 47 Reduction	0.024[a]	0.006	0.024[a]	0.078***	0.004*	0.006
	(0.013)	(0.010)	(0.013)	(0.022)	(0.002)	(0.005)
Prop 47 Reduction × 1 Felony			-0.002			
			(0.024)			
Prop 47 Reduction × Years Since Crime				-0.004***		
				(0.001)		
Dep. Mean (-1)	0.338	0.191	0.338	0.334	0.002	0.029
N	5,622	5,622	5,622	4,951	5,622	5,622
NxT	106,818	106,818	106,818	94,069	106,818	106,818
Age Controls	X	X	X	X	X	X
Indiv. FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X

Standard errors clustered on individual in parentheses
[a] $p<0.1$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$

Notes: This table reports coefficients on receiving a Prop 47 reduction indicator following Equation 4. Panel (a) presents results for Public Defender Initiated Petitioners, Panel (b) presents results for likely self-initiated petitioners, and Panel (c) presents results for the pooled sample.

Table 5: 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: Summary Statistics

(a) 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

(b) San Joaquin Felony Reductions Estimation Sample

	(1)	(2)	(3)	(4)
	All Crimes	Has HS Crime		
		All	Likely Self-Initiated Petitioner	PD Initiated Petitioner
Male	0.735	0.751	0.766	0.749
Reduction<7 Years from Conviction	0.087	0.075	0.177	0.061
2 Years Prior to Reduction:				
Age	47.63	47.55	45.09	47.88
Any Wages	0.331	0.332	0.296	0.337
Wages>$15k	0.179	0.181	0.139	0.187
Avg. Wages	7,674	7,697	6,003	7,920
Any 1099 NEC	0.033	0.032	0.020	0.034
Filed Taxes	0.303	0.310	0.301	0.311
Any SE Income	0.031	0.030	0.029	0.030
Total Obs	6,729	5,622	655	4,967

(c) Additional Summary Statistics, San Joaquin Felony Reductions Estimation Sample

	(1)	(2)
	Likely Self-Initiated Petitioner	PD Initiated Petitioner
Male	0.766	0.749
	(0.424)	(0.434)
Reduction<7 Years from Conviction	0.177	0.061
	(0.382)	(0.239)
2 Years Prior to Reduction:		
Age	45.09	47.88
	(10.79)	(10.55)
Years Since Conviction	12.176	14.474
	(5.668)	(5.322)
1 Felony	0.095	0.073
	(0.293)	(0.260)
Any Wages	0.296	0.337
	(0.457)	(0.473)
Wages>$15k	0.139	0.187
	(0.346)	(0.390)
Any Platform Gig	0.002	0.001
	(0.039)	(0.032)
Any Filed SE Income	0.029	0.030
	(0.168)	(0.172)
Total Obs	655	4,967

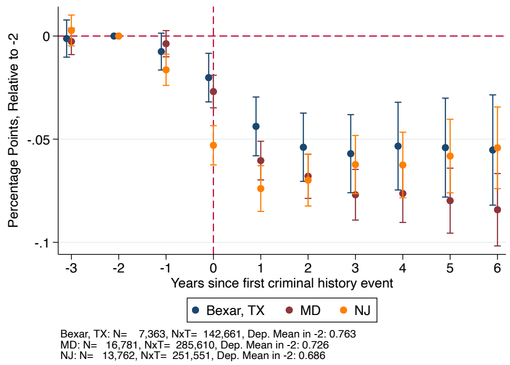
(d) PA Clean Slate Estimation Sample, Summary Statistics in 2018

	Pooled	Treated	Control
Male	0.645	0.631	0.682
	(0.478)	(0.483)	(0.466)
Black	0.303	0.314	0.276
	(0.460)	(0.464)	(0.447)
Age	26.39	26.77	25.45
	(3.677)	(3.674)	(3.512)
Years Since Charge	4.781	5.122	3.941
	(2.958)	(2.976)	(2.738)
Any Wages	0.809	0.798	0.835
	(0.393)	(0.402)	(0.371)
Wages>$15k	0.480	0.478	0.484
	(0.500)	(0.500)	(0.500)
Avg. Wages	19,531	19,566	19,445
	(20,747)	(20,950)	(20,235)
Any 1099 NEC, non platform	0.056	0.056	0.056
	(0.229)	(0.229)	(0.230)
Any platform gig	0.011	0.011	0.010
	(0.104)	(0.106)	(0.100)
Filed Taxes	0.713	0.706	0.731
	(0.452)	(0.456)	(0.443)
Any SE Income	0.051	0.053	0.047
	(0.221)	(0.225)	(0.211)
Total Obs	45,877	32,677	13,221

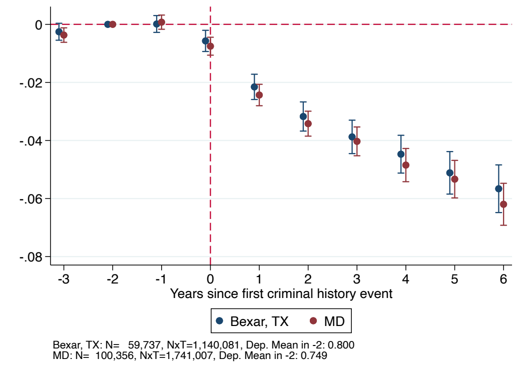
Notes: Panel (a) uses charges in the case of non-convictions, or disposition dates in the case of convictions. Sample is restricted to charges or dispositions from 1996 to 2011. Panel (b) reports summary statistics for our main estimation sample in San Joaquin County, CA. Likely self-initiated petitions are those whose petitions were filed before the "surge" for the first letter of their last name, the rest are classified as PD initiated petitions (see text for more detail). Panel (c) provides additioan information on the focal sub-sample of individuals with HS ("health & safety") crime as those were the petition list the DPD started with and make up a majority of petitions (84%). In panel (c), standard deviations are in parenthesis. Panel (d) presents summary statistics for PA estimation sample. This sample had charges that were dismissed or withdrawn, and were 18-25 at time of the charge.

Figure A.1: Reduction in W2 Employment after *First* Charge, By State

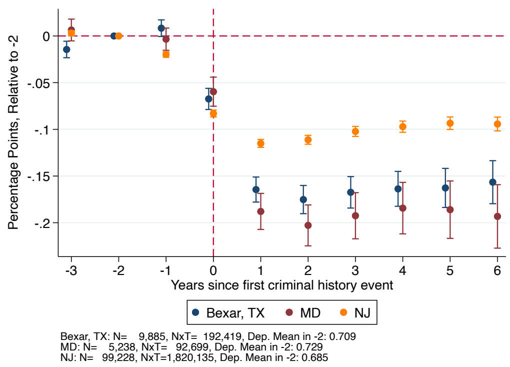
(a) Felony Non-Convictions



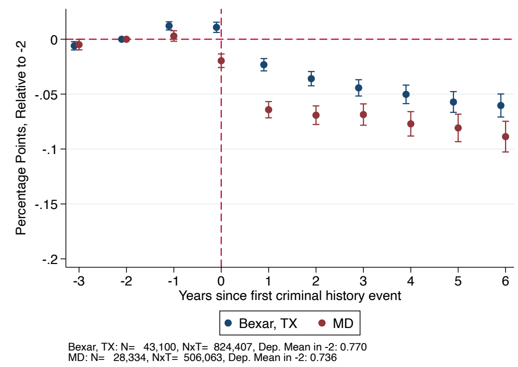
(b) Misdemeanor Non-Convictions



(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.

Table A.2: Summary Statistics: Last-Event Analysis Estimation Sample, Two Years Before Last Charge/Disposition.

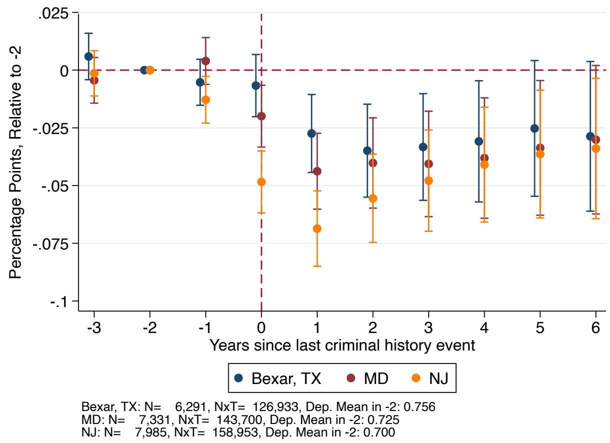
(a) Last-Event Analysis Estimation Sample

	(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
Wages>$15k	0.399	0.323	0.360	0.239
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

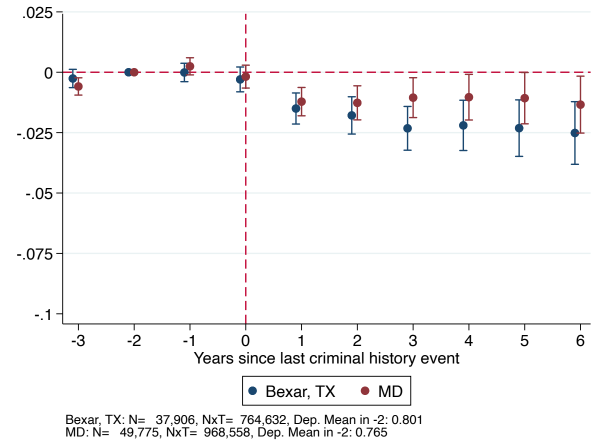
Notes: Table displays summary statics for sample in Figure A.2, pooling individuals across the states presented within each panel of Figure A.2.

Figure A.2: Reductions in W2 Employment After Latest Criminal History Event

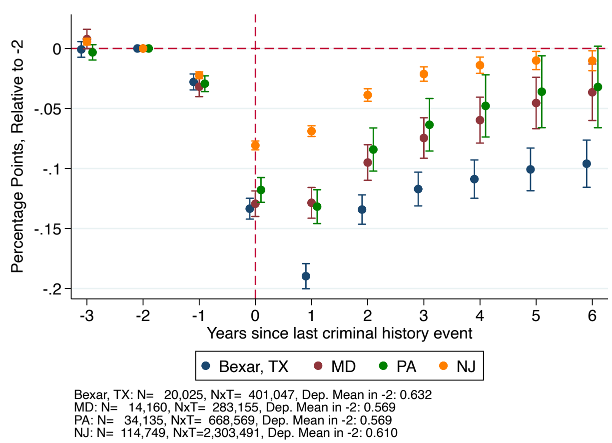
(a) Fel. Non-Convictions, no earlier convictions



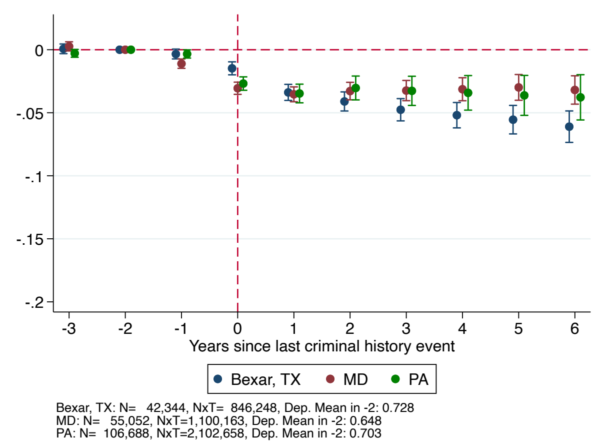
(b) Mis. Non-Convictions, no earlier convictions



(c) Fel. Convictions



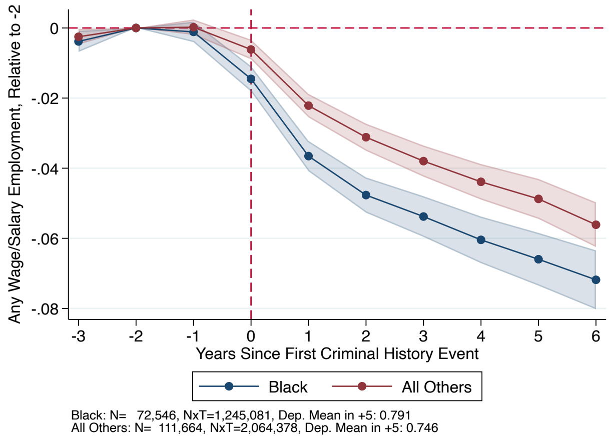
(d) Mis. Convictions



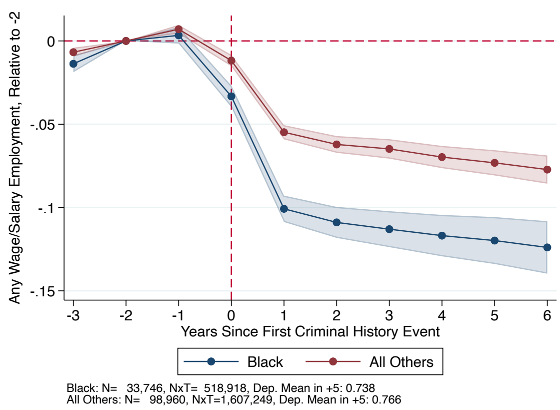
Notes: Each panel plots selected event study coefficients for the share with any wages > \$0 around the latest event, following specification 1 in the text. This event is the charge date for non-convictions and disposition date for convictions. Coefficients are relative to 2 years before the event. We 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 A.3: Event Study of Any Wages Around First Criminal History Event, By Race

(a) Non-Convictions



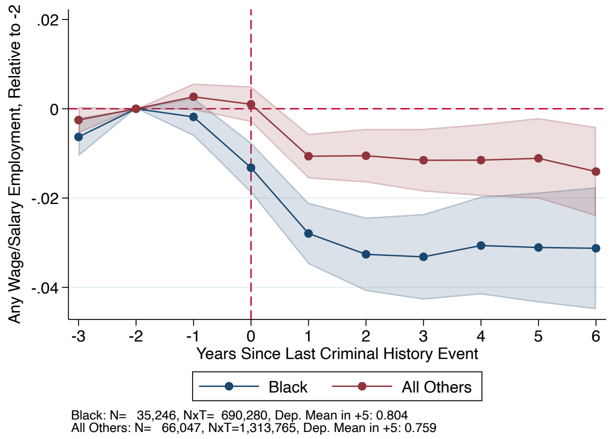
(b) Convictions



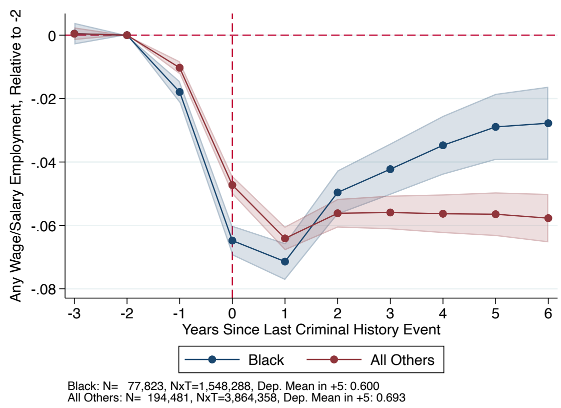
Notes: Each panel plots selected event study coefficients for the share with any wages around the first criminal history event, following specification 2 in the text. Timing from the event is based on the charge date for non-convictions and disposition date for convictions. Coefficients are relative to 2 periods before the event. We run separate event studies for each state in each panel. 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 for Black individuals and all those of all other racial identities.

Figure A.4: Event Study of Any Wages Around Latest Criminal History Event, By Race

(a) Non-Convictions



(b) Convictions



Notes: Each panel plots selected event study coefficients for the share with any wages around the last criminal history event, following specification 2 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 2 periods 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 2003-2018 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.

Table A.3: Impact of Proposition 47 Reductions on Employment Outcomes: Including Pre-Trend Estimates

(a) Main Results-Proactive Reductions

	(1) Any Wages>$0	(2) ...>$7,500	(3) ...>$15,000	(4) Any Gig	(5) Any Other 1099	(6) Files 1040	(7) Files SE
Treated	0.00270	-0.00395	0.00234	0.00380*	0.00117	-0.0133	0.00270
	(0.0145)	(0.0128)	(0.0119)	(0.00167)	(0.00653)	(0.0142)	(0.00586)
-2	0.00471	-0.00164	-0.00624	-0.00129	-0.00647	0.00607	0.00124
	(0.00909)	(0.00794)	(0.00735)	(0.00137)	(0.00415)	(0.00883)	(0.00404)
-3	0.00470	-0.00800	-0.0167	-0.000557	-0.00842	0.00628	0.00546
	(0.0155)	(0.0134)	(0.0124)	(0.00160)	(0.00704)	(0.0151)	(0.00657)
-4	-0.00948	-0.0183	-0.0226	-0.000203	-0.00672	0.00499	0.00971
	(0.0213)	(0.0187)	(0.0169)	(0.00165)	(0.00932)	(0.0212)	(0.00910)
-5	-0.0143	-0.0154	-0.0208	-0.000200	-0.0118	0.0126	0.0122
	(0.0267)	(0.0229)	(0.0209)	(0.00163)	(0.0114)	(0.0267)	(0.0111)
$\leq$ -6	-0.0189	-0.0134	-0.0190	-0.0000890	-0.0160	0.00329	0.0136
	(0.0319)	(0.0273)	(0.0248)	(0.00167)	(0.0133)	(0.0315)	(0.0129)
Dep. Mean (-1)	0.338	0.243	0.195	0.002	0.038	0.304	0.029
N	4,967	4,967	4,967	4,967	4,967	4,967	4,967
NxT	94,373	94,373	94,373	94,373	94,373	94,373	94,373
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

Standard errors clustered on individual in parentheses
[a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

(b) By Years Since Conviction

	(1) Any Wages>$0	(2) ...>$7,500	(3) ...>$15,000	(4) Any Gig	(5) Any Other 1099	(6) Files 1040	(7) Files SE
Treated	0.054*	0.050*	0.052**	-0.000	0.008	0.002	0.012
	(0.026)	(0.022)	(0.020)	(0.003)	(0.012)	(0.025)	(0.010)
Treated × Years Since Crime	-0.004**	-0.005***	-0.005***	0.000	-0.001	-0.002	-0.000
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
-2	0.011	0.005	0.000	-0.002[a]	-0.003	0.016	0.002
	(0.010)	(0.009)	(0.008)	(0.001)	(0.005)	(0.010)	(0.005)
-3	0.016	0.003	-0.004	-0.002	-0.006	0.022	0.003
	(0.018)	(0.016)	(0.014)	(0.001)	(0.008)	(0.018)	(0.008)
-4	0.006	-0.002	-0.004	-0.001	-0.004	0.025	0.008
	(0.025)	(0.022)	(0.020)	(0.002)	(0.011)	(0.025)	(0.011)
-5	-0.001	0.003	-0.004	-0.001	-0.012	0.041	0.009
	(0.031)	(0.027)	(0.024)	(0.002)	(0.013)	(0.032)	(0.013)
≤ -6	-0.006	0.007	0.001	-0.001	-0.014	0.035	0.009
	(0.037)	(0.032)	(0.029)	(0.002)	(0.015)	(0.037)	(0.015)
Dep. Mean (-1)	0.335	0.243	0.196	0.002	0.039	0.303	0.028
N	4,336	4,336	4,336	4,336	4,336	4,336	4,336
NxT	82,384	82,384	82,384	82,384	82,384	82,384	82,384
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

Standard errors clustered on individual in parentheses
[a] $p<0.1$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$

(c) By One Felony

	(1) Any Wages>$0	(2) ...>$7,500	(3) ...>$15,000	(4) Any Gig	(5) Any Other 1099	(6) Files 1040	(7) Files SE
Treated	0.003	-0.004	0.001	0.004*	0.001	-0.014	0.002
	(0.015)	(0.013)	(0.012)	(0.002)	(0.006)	(0.014)	(0.006)
Treated × 1 Felony	-0.006	0.006	0.024	-0.000	0.008	0.012	0.020
	(0.026)	(0.025)	(0.022)	(0.005)	(0.015)	(0.028)	(0.016)
-2	0.005	-0.002	-0.006	-0.001	-0.006	0.006	0.001
	(0.009)	(0.008)	(0.007)	(0.001)	(0.004)	(0.009)	(0.004)
-3	0.005	-0.008	-0.017	-0.001	-0.008	0.006	0.006
	(0.016)	(0.013)	(0.012)	(0.002)	(0.007)	(0.015)	(0.007)
-4	-0.010	-0.018	-0.022	-0.000	-0.007	0.005	0.010
	(0.021)	(0.019)	(0.017)	(0.002)	(0.009)	(0.021)	(0.009)
-5	-0.014	-0.015	-0.021	-0.000	-0.012	0.013	0.012
	(0.027)	(0.023)	(0.021)	(0.002)	(0.011)	(0.027)	(0.011)
≤ -6	-0.019	-0.013	-0.019	-0.000	-0.016	0.003	0.014
	(0.032)	(0.027)	(0.025)	(0.002)	(0.013)	(0.032)	(0.013)
1 Felony							
Dep. Mean (-1)	0.387	0.276	0.240	0.000	0.041	0.362	0.028
N	362	362	362	362	362	362	362
NxT	6,878	6,878	6,878	6,878	6,878	6,878	6,878
>1 Felony							
Dep. Mean (-1)	0.334	0.240	0.192	0.002	0.038	0.299	0.029
N	4,605	4,605	4,605	4,605	4,605	4,605	4,605
NxT	87,495	87,495	87,495	87,495	87,495	87,495	87,495
Age Controls	X	X	X	X	X	X	X
Indiv. FE	X	X	X	X	X	X	X
YearXGroup FE	X	X	X	X	X	X	X

Standard errors clustered on individual in parentheses
[a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

(d) Likely Self-Initiated Petitioners

	(1) Any Wages>$0	(2) ...>$7,500	(3) ...>$15,000	(4) Any Gig	(5) Any Other 1099	(6) Files 1040	(7) Files SE
Treated	0.0344	0.0331	0.0185	-0.00375	0.00792	0.00708	0.0194
	(0.0364)	(0.0320)	(0.0293)	(0.00629)	(0.0106)	(0.0352)	(0.0134)
-2	-0.0342	-0.0181	-0.00876	-0.00101	-0.00671	-0.0169	-0.00728
	(0.0228)	(0.0183)	(0.0150)	(0.00145)	(0.00892)	(0.0216)	(0.00734)
-3	-0.0352	-0.0551[a]	-0.0344	-0.00228	-0.00675	-0.0187	-0.0121
	(0.0365)	(0.0293)	(0.0242)	(0.00203)	(0.0147)	(0.0365)	(0.0111)
-4	-0.0402	-0.0858*	-0.0425	-0.00249	-0.0122	-0.0506	-0.0158
	(0.0485)	(0.0391)	(0.0334)	(0.00248)	(0.0185)	(0.0509)	(0.0164)
-5	-0.0269	-0.0790	-0.0335	-0.00285	-0.0122	-0.0628	-0.00773
	(0.0593)	(0.0486)	(0.0407)	(0.00297)	(0.0238)	(0.0643)	(0.0207)
$\leq$ -6	-0.00944	-0.0747	-0.0385	-0.00332	-0.0216	-0.0960	-0.00509
	(0.0713)	(0.0586)	(0.0486)	(0.00361)	(0.0274)	(0.0772)	(0.0231)
Dep. Mean (-1)	0.334	0.217	0.160	0.005	0.021	0.299	0.031
N	655	655	655	655	655	655	655
NxT	12,445	12,445	12,445	12,445	12,445	12,445	12,445
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

Standard errors clustered on individual in parentheses
[a] $p<0.1$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$

(e) Pooled

	(1) Any Wages>$0	(2) ...>$7,500	(3) ...>$15,000	(4) Any Gig	(5) Any Other 1099	(6) Files 1040	(7) Files SE
Treated	0.0242[a]	0.00892	0.00623	0.00388*	-0.000254	-0.000715	0.00631
	(0.0128)	(0.0113)	(0.0103)	(0.00157)	(0.00526)	(0.0125)	(0.00503)
-2	-0.00526	-0.00550	-0.00438	-0.00110	-0.00454	-0.00154	-0.000568
	(0.00790)	(0.00677)	(0.00614)	(0.00107)	(0.00343)	(0.00751)	(0.00336)
-3	-0.00673	-0.0132	-0.0115	-0.000697	-0.00471	-0.00259	0.00224
	(0.0132)	(0.0112)	(0.0101)	(0.00131)	(0.00561)	(0.0127)	(0.00532)
-4	-0.0147	-0.0225	-0.0141	-0.000470	-0.00306	-0.0104	0.00357
	(0.0180)	(0.0155)	(0.0137)	(0.00135)	(0.00734)	(0.0177)	(0.00729)
-5	-0.00995	-0.0137	-0.00753	-0.000475	-0.00644	-0.0104	0.00350
	(0.0224)	(0.0189)	(0.0168)	(0.00134)	(0.00906)	(0.0224)	(0.00887)
$\leq$ -6	-0.000674	-0.00389	-0.00188	-0.000394	-0.0104	-0.0328	0.00323
	(0.0268)	(0.0225)	(0.0200)	(0.00139)	(0.0106)	(0.0268)	(0.0104)
Dep. Mean (-1)	0.338	0.240	0.191	0.002	0.036	0.303	0.029
N	5,622	5,622	5,622	5,622	5,622	5,622	5,622
NxT	106,818	106,818	106,818	106,818	106,818	106,818	106,818
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

Standard errors clustered on individual in parentheses
[a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

Notes: This table reports results from specifications in Table 4 but fully displays year-specific coefficients for leads of the events.

Table A.4: Impact of Proposition 47 Reductions: Alternative Age Controls

(a) PD Initiated Petitioners-No Age Controls

	(1) Any Wages>$0	(2) ...>$15,000	(3) ...>$0	(4) ...>$0	(5) Any Gig	(6) Files SE
PD Initiated Petitioner	-0.003	-0.002	-0.002	0.112***	0.004*	0.002
	(0.015)	(0.012)	(0.015)	(0.026)	(0.002)	(0.006)
PD Initiated Petitioner × 1 Felony			-0.012			
			(0.027)			
PD Initiated Petitioner × Years Since Crime				-0.009***		
				(0.001)		
Dep. Mean (-1)	0.338	0.195	0.338	0.335	0.002	0.029
N	4,967	4,967	4,967	4,336	4,967	4,967
NxT	94,373	94,373	94,373	82,384	94,373	94,373
Age Controls						
Indiv. FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X

Standard errors clustered on individual in parentheses
[a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

(b) PD Initiated Petitioners-5 Year Age Bins

	(1) Any Wages>$0	(2) ...>$15,000	(3) ...>$0	(4) ...>$0	(5) Any Gig	(6) Files SE
PD Initiated Petitioner	0.001	0.002	0.001	0.063*	0.004*	0.003
	(0.015)	(0.012)	(0.015)	(0.026)	(0.002)	(0.006)
PD Initiated Petitioner × 1 Felony			-0.007			
			(0.026)			
PD Initiated Petitioner × Years Since Crime				-0.005***		
				(0.001)		
Dep. Mean (-1)	0.338	0.195	0.338	0.335	0.002	0.029
N	4,967	4,967	4,967	4,336	4,967	4,967
NxT	94,373	94,373	94,373	82,384	94,373	94,373
Age Controls	X	X	X	X	X	X
Indiv. FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X

Standard errors clustered on individual in parentheses
[a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

Notes: This table replicates specifications in Table 4 with either no age controls or with age controlled using 5-year bins instead of the quintic in age included in the benchmark specification.

Table A.5: IV Estimates Using Letter-Specific Surge

	(1) Any Wages>$0	(2) Post Reduction	(3) Any Wages>$0	(4) Any Wages>$0
Post Reduction	0.025*			0.031
	(0.012)			(0.041)
Post Surge		0.379***	0.012	
		(0.012)	(0.016)	
KP Fstat				1253.993
Age Controls	X	X	X	X
Indiv. FE	X	X	X	X
Year FE	X	X	X	X

Standard errors clustered on individual in parentheses
[a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

Notes: This presents IV estimates for the pooled San Joaquin sample where the Post-Reduction indicator (treatment) is instrumented with an indicator of whether the current year is during or after the year of the initial surge of petitions filed by the Public Defender's office for individuals with the same last name. Column 1 presents OLS estimates as in 4 estimated on the sample with valid surge indicators, Column 2 presetns the first stage estimates, Column 3 presents reduced-form effects of the surge indicator, and Column 4 presents IV estimates.

Table A.6: Impacts of Proposition 47: Additional Outcomes

(a) Likely Self-Initiated Petitioner

	(1) Any Wages>$0	(2) ...>$7,500	(3) ...>$15,000	(4) Wages	(5) Any Gig	(6) Any Other 1099	(7) Files 1040	(8) Files SE
Treated	0.0344	0.0331	0.0185	-295.1	-0.00375	0.00792	0.00708	0.0194
	(0.0364)	(0.0320)	(0.0293)	(1148.5)	(0.00629)	(0.0106)	(0.0352)	(0.0134)
Dep. Mean (-1)	0.334	0.217	0.160	6815.554	0.005	0.021	0.299	0.031
N	655	655	655	655	655	655	655	655
NxT	12,445	12,445	12,445	12,445	12,445	12,445	12,445	12,445
Age Controls	X	X	X	X	X	X	X	X
Indiv. FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X

Standard errors clustered on individual in parentheses
[a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

Notes: This table reports coefficients on a "treated" reduction indicator following Equation 4, for only those identified as likely self-petitioners, for a variety of outcomes.

(b) PD Initiated Petitioner

	(1) Any Wages>$0	(2) ...>$7,500	(3) ...>$15,000	(4) Wages	(5) Any Gig	(6) Any Other 1099	(7) Files 1040	(8) Files SE
Treated	0.00270	-0.00395	0.00234	795.7	0.00380*	0.00117	-0.0133	0.00270
	(0.0145)	(0.0128)	(0.0119)	(535.7)	(0.00167)	(0.00653)	(0.0142)	(0.00586)
Dep. Mean (-1)	0.338	0.243	0.195	8460.821	0.002	0.038	0.304	0.029
N	4,967	4,967	4,967	4,967	4,967	4,967	4,967	4,967
NxT	94,373	94,373	94,373	94,373	94,373	94,373	94,373	94,373
Age Controls	X	X	X	X	X	X	X	X
Indiv. FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X

Standard errors clustered on individual in parentheses
[a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

Notes: This table reports coefficients on a "treated" reduction indicator following Equation 4, for only those that received proactive reductions, for a variety of outcomes.

(c) PD Initiated Petitioner-By Years Since Conviction

	(1) Any Wages>$0	(2) ...>$7,500	(3) ...>$15,000	(4) Wages	(5) Any Gig	(6) Any Other 1099	(7) Files 1040	(8) Files SE
Treated	0.054*	0.050*	0.052**	617.516	-0.000	0.008	0.002	0.012
	(0.026)	(0.022)	(0.020)	(939.489)	(0.003)	(0.012)	(0.025)	(0.010)
Treated × Years Since Crime	-0.004**	-0.005***	-0.005***	-40.145	0.000	-0.001	-0.002	-0.000
	(0.001)	(0.001)	(0.001)	(48.801)	(0.000)	(0.001)	(0.001)	(0.001)
Dep. Mean (-1)	0.335	0.243	0.196	8470.711	0.002	0.039	0.303	0.028
N	4,336	4,336	4,336	4,336	4,336	4,336	4,336	4,336
NxT	82,384	82,384	82,384	82,384	82,384	82,384	82,384	82,384
Age Controls	X	X	X	X	X	X	X	X
Indiv. FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X

Standard errors clustered on individual in parentheses
[a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

Notes: This table presents differential impacts for individuals who received proactive Proposition 47 reductions based on years since original conviction for a variety of outcomes.

(d) PD Initiated Petitioner-< 7 Years Indicator

	(1) Any Wages>$0	(2) ...>$7,500	(3) ...>$15,000	(4) Wages	(5) Any Gig	(6) Any Other 1099	(7) Files 1040	(8) Files SE
Treated	0.002	-0.006	0.002	820.429	0.004*	0.001	-0.012	0.003
	(0.014)	(0.013)	(0.012)	(535.615)	0.002	(0.007)	(0.014)	(0.006)
< 7 Years × Treated	0.014	0.023	0.004	-330.894	-0.003	-0.002	-0.021	0.000
	(0.027)	(0.024)	(0.021)	(972.513)	(0.003)	(0.012)	(0.026)	(0.012)
Dep. Mean (-1)	0.338	0.243	0.195	8460.821	0.002	0.038	0.304	0.029
N	4,967	4,967	4,967	4,967	4,967	4,967	4,967	4,967
NxT	94,373	94,373	94,373	94,373	94,373	94,373	94,373	94,373
Age Controls	X	X	X	X	X	X	X	X
Indiv. FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X

Standard errors clustered on individual in parentheses
[a] p<0.1, * p<0.05, ** p<0.01, *** p<0.001

Notes: This table presents differential impacts for individuals who received proactive Proposition 47 reductions. An indicator is included for being < 7 years from the charge.

Table A.7: Notification Analysis Balance Table

	(1)	(2)	(3)	(4)	(5)	(6)
	Notified		Not Notified		Difference (p-value)	
	1 Felony	All Others	1 Felony	All Others	1 Felony	All Others
Male	0.669	0.742	0.688	0.762	-0.018	-0.019
					(0.665)	(0.066)
Outcomes in 2018:						
Age	49.70	49.07	49.46	49.08	0.233	–0.005
					(0.820)	(0.984)
Any Wages	0.364	0.332	0.382	0.330	-0.018	0.002
					(0.683)	(0.896)
Wages>$15k	0.223	0.197	0.231	0.185	-0.008	0.012
					(0.832)	(0.228)
Any SE Income	0.056	0.021	0.022	0.028	0.034	-0.007
					(0.051)	(0.074)
Wages	10,322.01	8,710.60	9,810.20	8,592.98	511.81	117.62
					(0.769)	(0.798)
Total Obs	269	3,486	225	3,175		

Note: This table reports balance tests for the notified and no notified groups.

Table A.8: Notification Analysis, Additional Employment Outcomes

(a) 2019 Outcomes

	(1) Any Wages>$0	(2) ...>$7,500	(3) ...>$15,000	(4) Any Gig	(5) Any Other 1099	(6) Files 1040	(7) Files SE
Notified	0.0110	0.00248	0.00669	0.00130	0.000384	0.00186	-0.00855*
	(0.0116)	(0.0105)	(0.00966)	(0.00179)	(0.00357)	(0.0123)	(0.00377)
Notified × 1 Felony	-0.0460	0.000128	0.0284	-0.00203	-0.00846	-0.0475	0.0176
	(0.0453)	(0.0428)	(0.0406)	(0.00605)	(0.0140)	(0.0468)	(0.0180)
1 Felony	0.0623^{a}	0.0632*	0.0513^{a}	-0.000280	0.00525	0.0256	0.00721
	(0.0337)	(0.0315)	(0.0293)	(0.00460)	(0.0110)	(0.0344)	(0.0127)
Constant	0.333***	0.239***	0.189***	0.00472***	0.0214***	0.503***	0.0283***
	(0.00837)	(0.00757)	(0.00695)	(0.00122)	(0.00257)	(0.00888)	(0.00295)
N	7155	7155	7155	7155	7155	7155	7155

(b) 2020 Outcomes

	(1) Any Wages>$0	(2) ...>$7,500	(3) ...>$15,000	(4) Any Gig	(5) Any Other 1099	(6) Files 1040	(7) Files SE
Notified	-0.00433	0.00834	0.00884	-0.00144	0.00247	-0.00255	-0.00202
	(0.0114)	(0.0104)	(0.00965)	(0.00233)	(0.00331)	(0.0117)	(0.00382)
Notified × 1 Felony	-0.0121	-0.00137	-0.0132	-0.00300	-0.00311	-0.0176	0.00137
	(0.0450)	(0.0418)	(0.0392)	(0.00501)	(0.0149)	(0.0450)	(0.0150)
1 Felony	0.0514	0.0475	0.0440	-0.00532	0.00934	0.0168	0.000840
	(0.0333)	(0.0307)	(0.0290)	(0.00477)	(0.0110)	(0.0334)	(0.0111)
Constant	0.322***	0.228***	0.187***	0.00976***	0.0173***	0.357***	0.0258***
	(0.00829)	(0.00745)	(0.00692)	(0.00175)	(0.00232)	(0.00850)	(0.00282)
N	7155	7155	7155	7155	7155	7155	7155

Note: This table reports additional employment outcomes for the effect of notification. We separately report outcomes in 2019 and 2020.

Table A.9: Notification Experiment: ITT v IV estimates, 2020

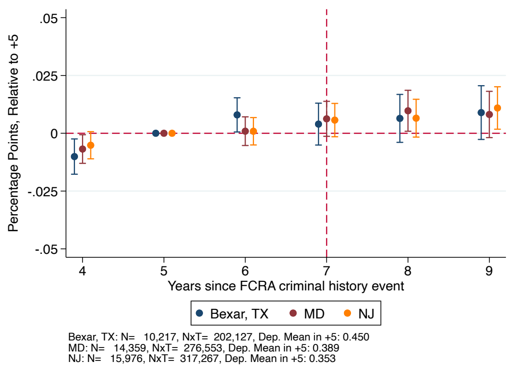
	(1)	(2)	(3)	(4)
	Any Wages>$0	>$0	...>$15,000	...>$15,000
Notified	-0.00433		0.00884	
	(0.0114)		(0.00965)	
Notified × 1 Felony	-0.0121		-0.0132	
	(0.0450)		(0.0392)	
Success		-0.0151		0.0308
		(0.0400)		(0.0336)
Success × 1 Felony		-0.0551		-0.0494
		(0.191)		(0.166)
1 Felony	0.0514	0.0514	0.0440	0.0440
	(0.0333)	(0.0333)	(0.0290)	(0.0289)
Constant	0.322***	0.322***	0.187***	0.187***
	(0.00829)	(0.00829)	(0.00692)	(0.00692)
N	7155	7155	7155	7155
OLS/IV	OLS	IV	OLS	IV
KP F-stat		41.110		41.110

Note: This table reports IV estimates of the effects of successful notifications on 2020 outcomes, using sent notifications (used to estimate ITT effects in Table A.8a) as an instrument for successful contact with individuals.

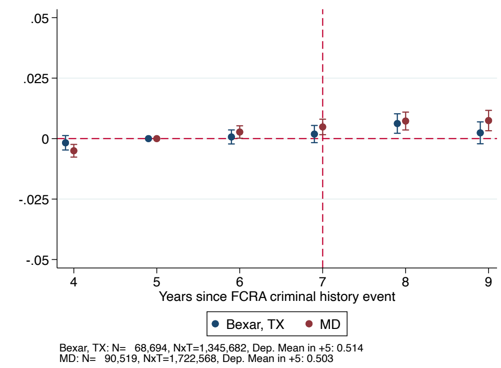
Figure A.5: FCRA Event Study of Any Wages >$15,000 Around Removal (Year 7)

Note: MD has State FCRA for Convictions

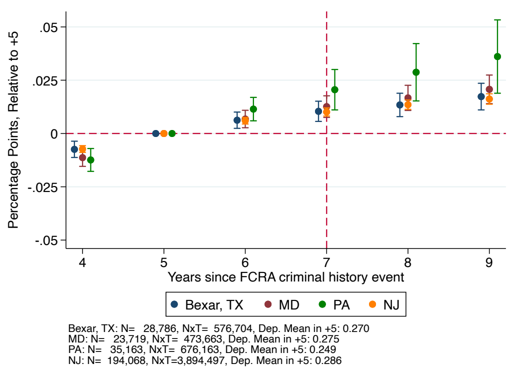
(a) Felony Non-Convictions, no other convictions



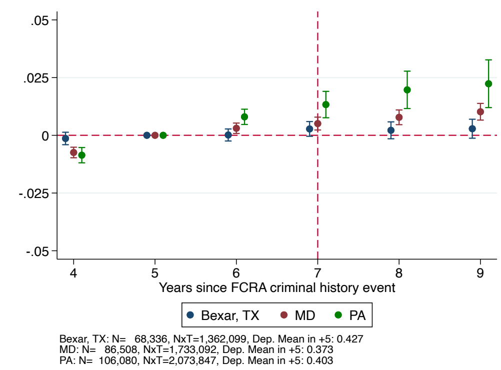
(b) Mis. Non-Convictions, no other convictions



(c) Felony Convictions



(d) Misdemeanor Convictions

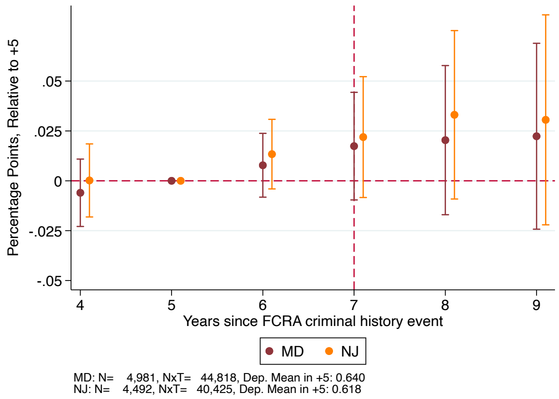


Notes: Each panel plots selected event study coefficients for the share with any wages > $15,000 around 7 years after the event, following specification 2 in the text. Timing from the event is based on the charge date for non-convictions and disposition date for convictions. Coefficients are relative 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.

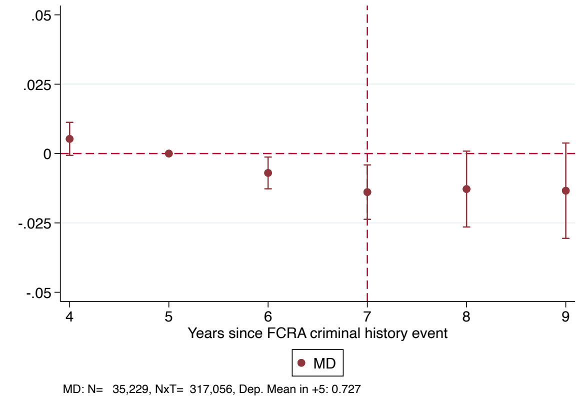
Figure A.6: FCRA Event Study of Any Wages Around Removal (Year 7) occurring between 2015-2018

Note: MD has State FCRA for Convictions

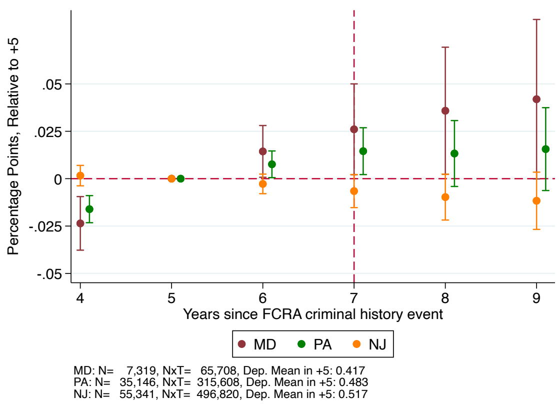
(a) Felony Non-Convictions, no other convictions



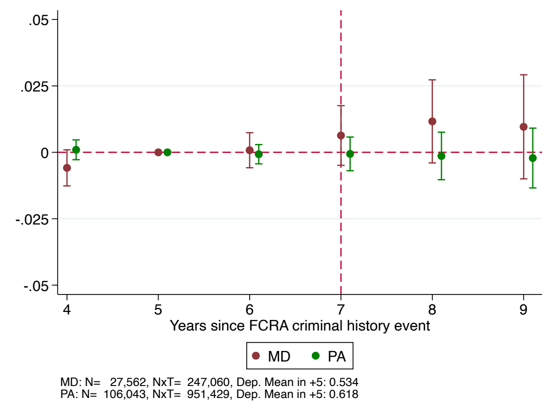
(b) Mis. Non-Convictions, no other convictions



(c) Felony Convictions



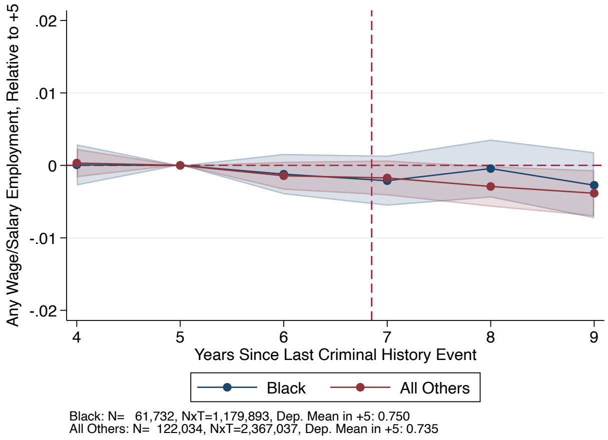
(d) Misdemeanor Convictions



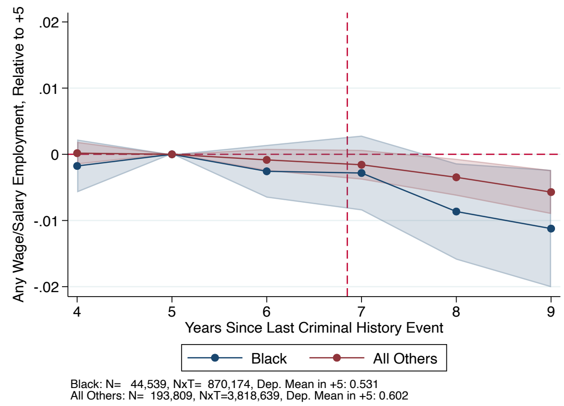
Notes: For this figure, the sample is restricted to events occurring between 2008-2011 (removal (Year 7) occurring between 2015-2018).

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

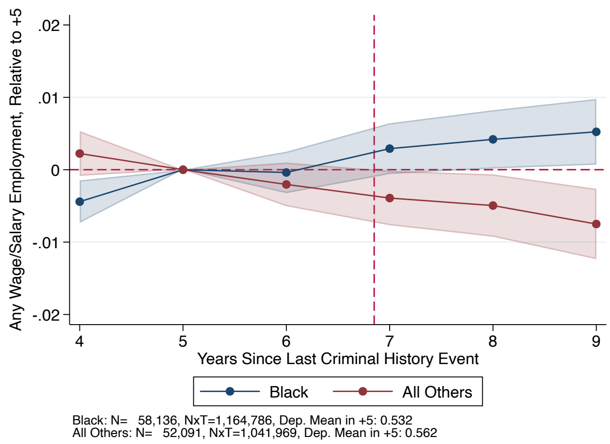
(a) Non-Convictions



(b) Convictions in PA, and TX



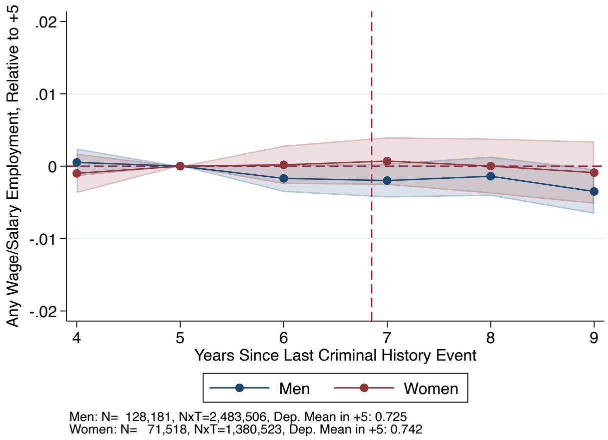
(c) Convictions in MD



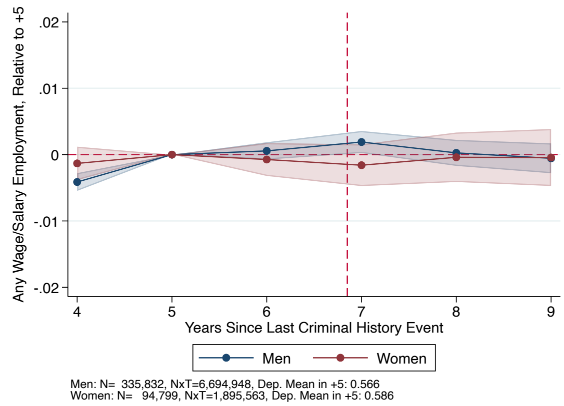
Notes: Race data is available in public court records in Bexar County, Texas, Maryland, and Pennsylvania. Each panel plots selected event study coefficients for the share with any wages around 7 years after the event, following specification 2 in the text. Timing from the event is based on the charge 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 other races.

Figure A.8: FCRA Event Study of Any Wages Around Removal (Year 7), By Gender

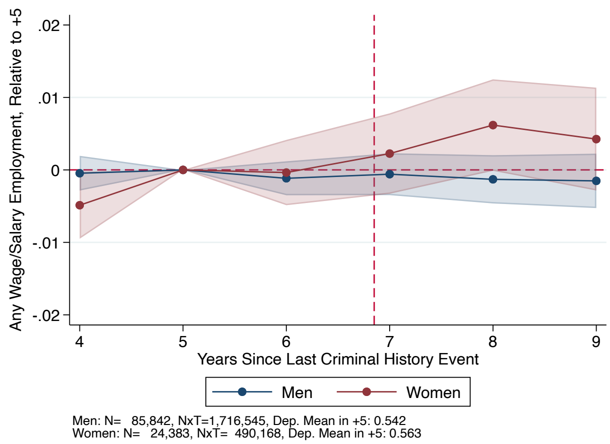
(a) Non-Convictions



(b) Convictions in PA, NJ and TX



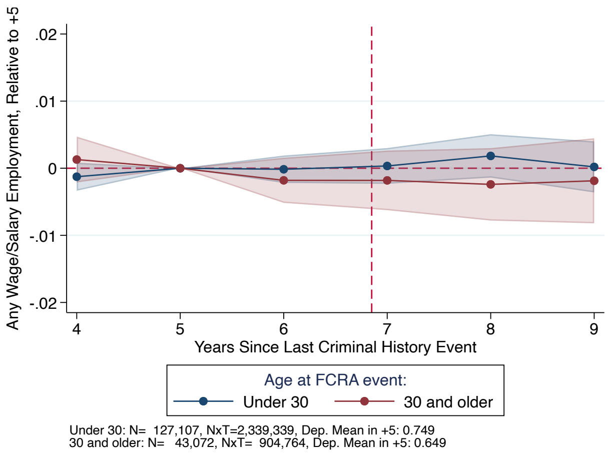
(c) Convictions in MD



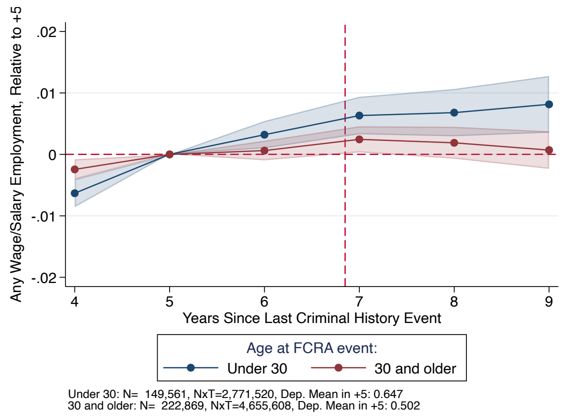
Notes: Each panel plots selected event study coefficients for the share with any wages around 7 years after the event, following specification 2 in the text. Timing from the event is based on the charge date for non-convictions and disposition date for convictions. Coefficients are relative to +5 periods 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 men and women (gender based on SSA records).

Figure A.9: FCRA Event Study of Any Wages Around Removal (Year 7), By Age

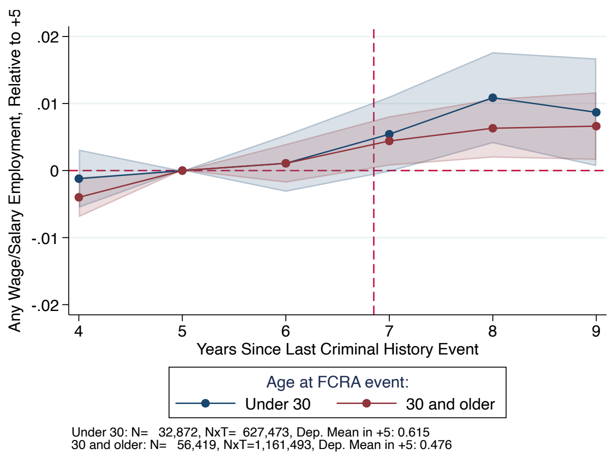
(a) Non-Convictions



(b) Convictions in PA, NJ and TX



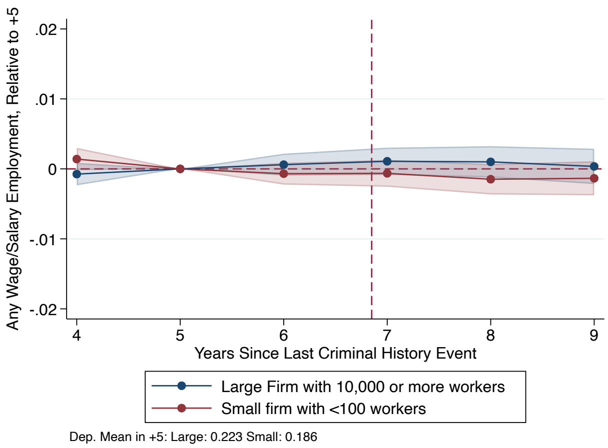
(c) Convictions in MD



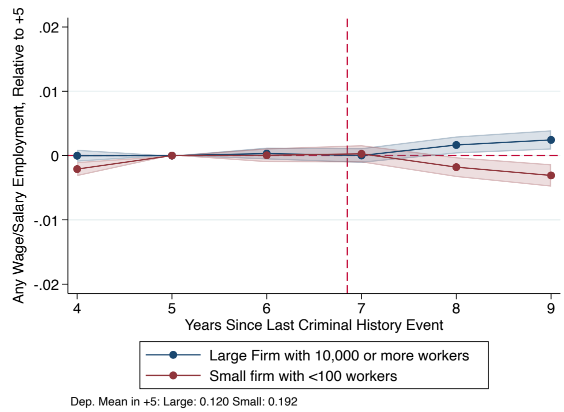
Notes: Each panel plots selected event study coefficients for the share with any wages around 7 years after the event, following specification 2 in the text. Timing from the event is based on the charge date for non-convictions and disposition date for convictions. Coefficients are relative to +5 periods 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 individuals under 30 and for individuals 40 and over.

Figure A.10: FCRA Event Study of Any Wages Around Removal (Year 7), by Firm Size

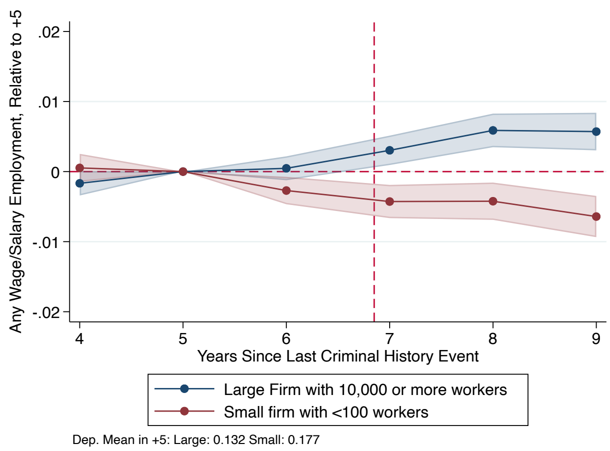
(a) Non-Convictions



(b) Convictions in PA, NJ and TX



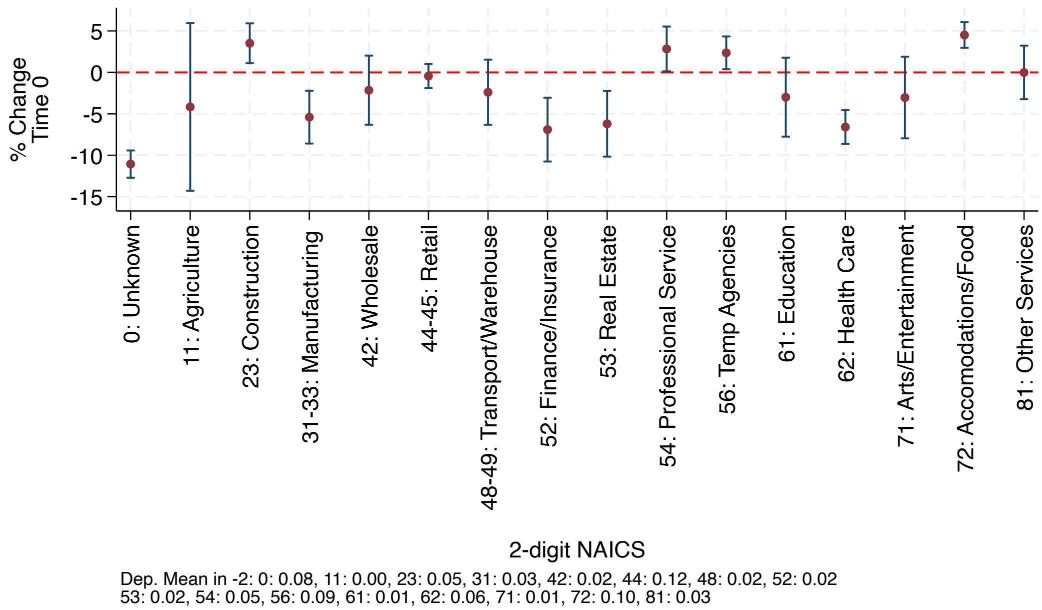
(c) Convictions in MD



Notes: Each panel plots selected event study coefficients for the share with any wages from a large or small firm around 7 years after the event, following specification 2 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.11: Event Study of Any Wages Around First Event, By 2-digit NAICS Industry

(a) Non-Convictions



(b) Convictions

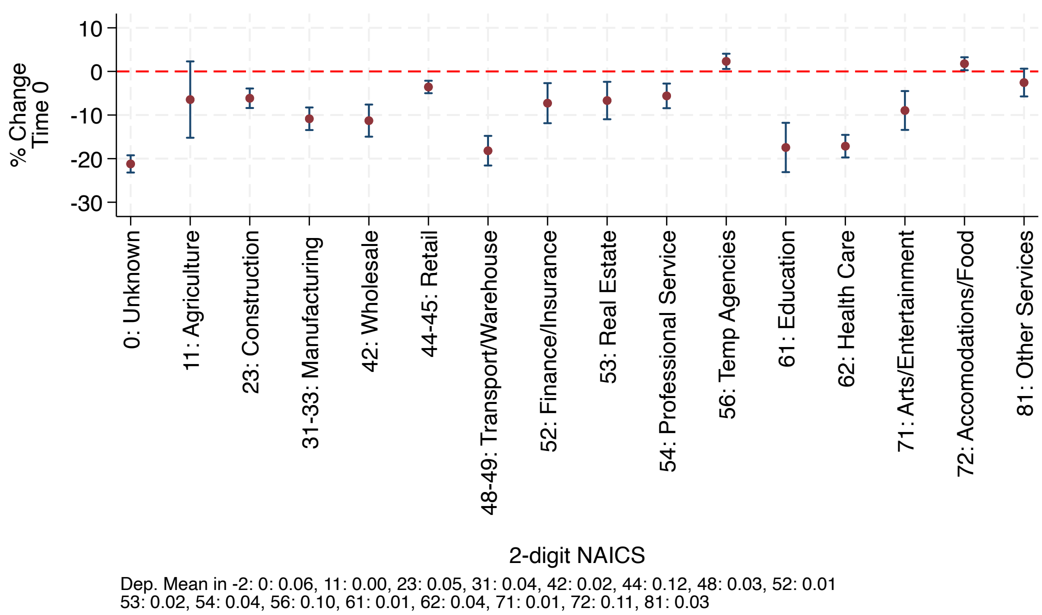
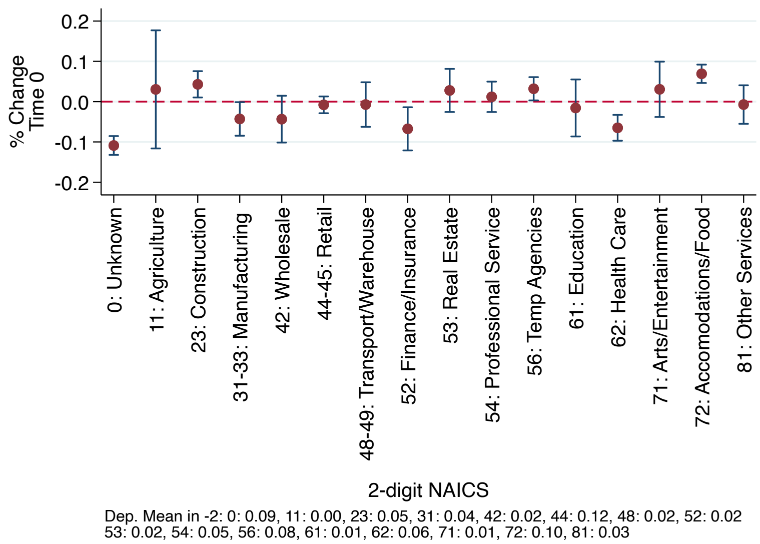


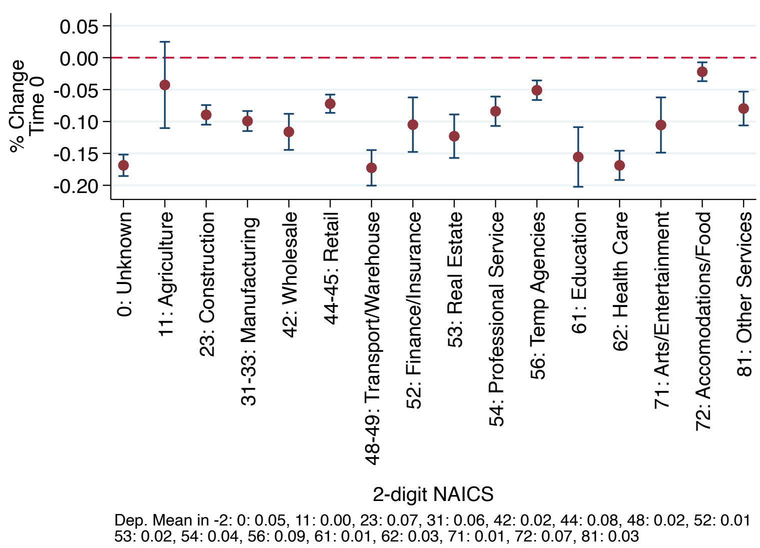
Figure reports event study estimates in the year of someone's first criminal history event of a W-2 issued by a payer firm in the specified 2-digit NAICS code based on the firms' tax return in that year. For this analysis, we restrict the full sample to have been 18 by the time the first charge appears 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 A.12: Event Study of Any Wages Around Last Criminal History Event, By 2-digit NAICS Industry

(a) Non-Convictions



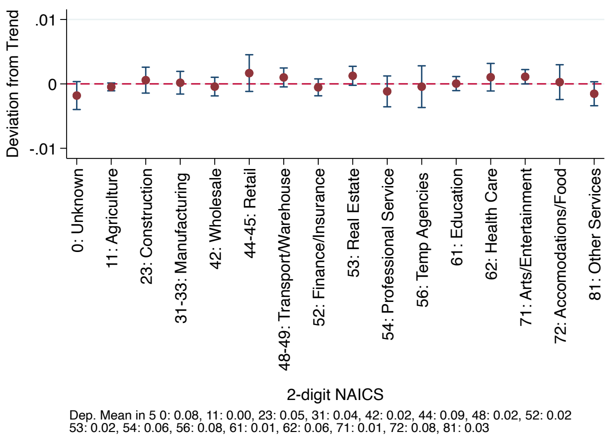
(b) Convictions



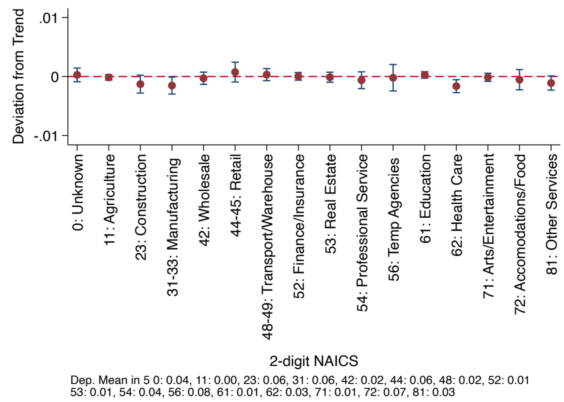
Notes: Figure reports event study estimates at time 0 of W-2 issued by a payer firm in the specified 2-digit NAICS code based on the firms' tax return in that year. We run separate event studies by industry around the criminal history event, and divide by the mean share in the in the industry in -2 to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

Figure A.13: FCRA Event Study of Any Wages Around Removal (Year 7), Deviation from Trend, By 2-digit NAICS Industry

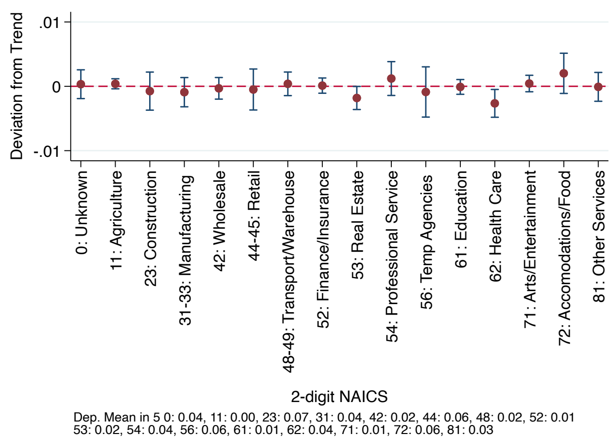
(a) Non-Convictions



(b) Convictions in PA, NJ and TX



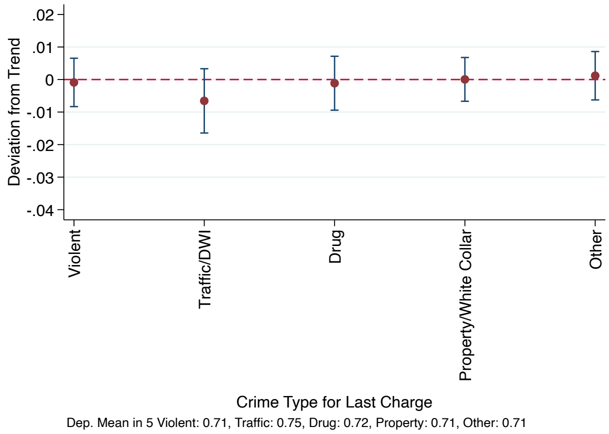
(c) Convictions in MD



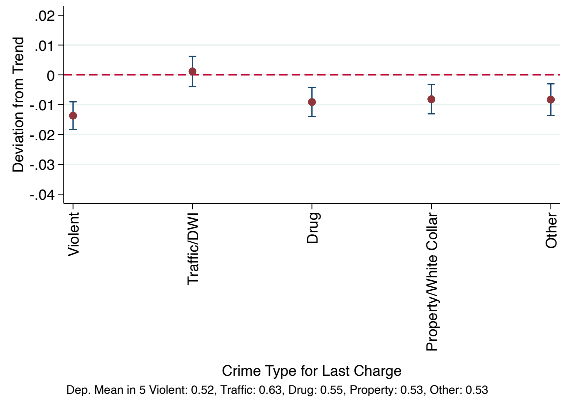
Notes: Figure reports results from a test of whether the event study coefficients 7 years after the last charge are different from a linear trend. Specifically, figure reports $2 \times \beta_{+4} + \beta_{+7}$.

Figure A.14: FCRA Event Study of Any Wages Around Removal (Year 7), Deviation from Trend, By Crime-Type of Last Conviction

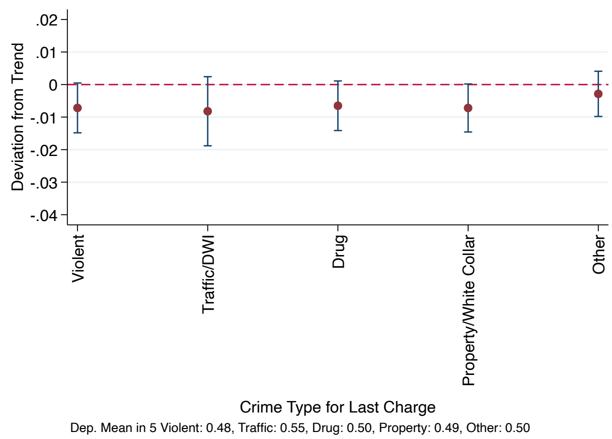
(a) Non-Convictions



(b) Convictions in PA, NJ and TX



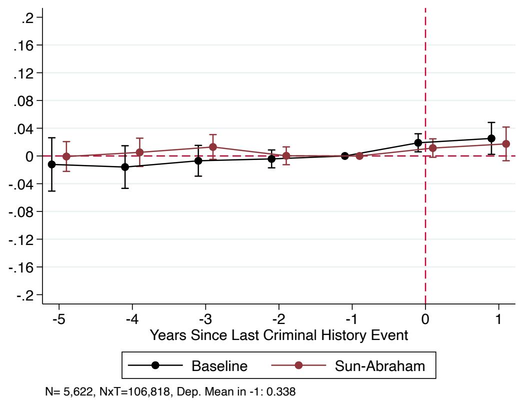
(c) Convictions in MD



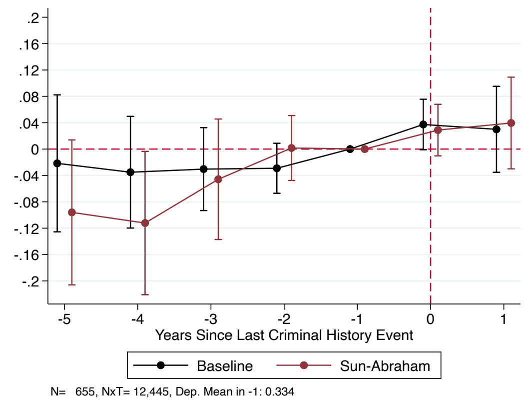
Notes: Figure reports results from a test of whether the event study coefficients 7 years after the last charge are different from a linear trend. Specifically, figure reports $2 \times \beta_{+4} + \beta_{+7}$.

Figure A.15: Robustness to Alternative DD-Estimators: San Joaquin Analysis

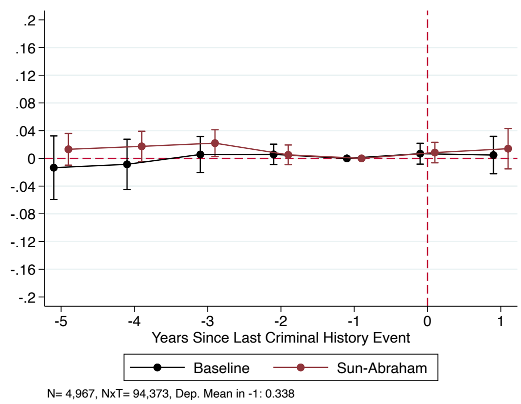
(a) Pooled



(b) Likely Self-Petitioners



(c) Proactive Reductions

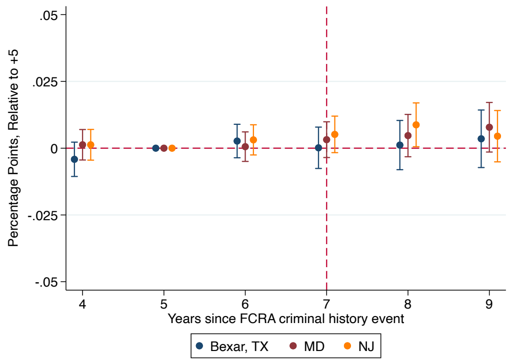


Notes: Figure shows event-study coefficients for having any wage employment around Proposition 47 felony reductions in San Joaquin County, CA. We report our baseline estimates alongside event-study coefficients using the estimator proposed by Sun-Abraham (2020). The error bars report ninety percent confidence intervals.

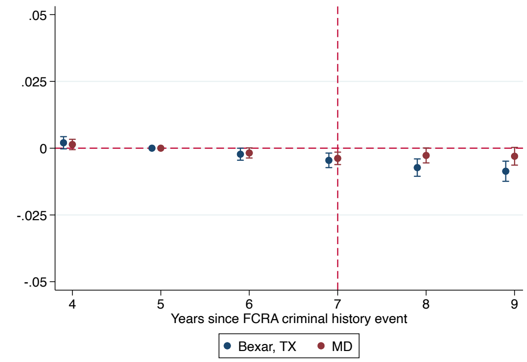
Figure A.16: Robustness to Alternative DD-Estimators: FCRA Event Study of Any Wages Around Removal (Year 7)

Note: MD has State FCRA for Convictions

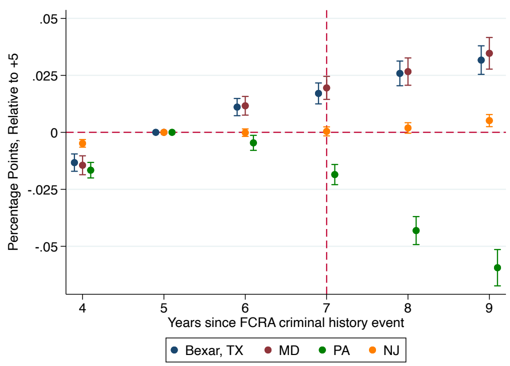
(a) Felony Non-Convictions, no other convictions



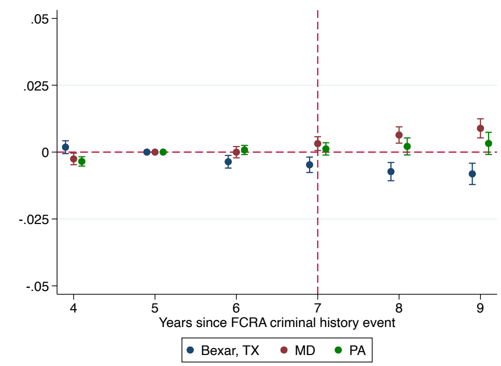
(b) Mis. Non-Convictions, no other convictions



(c) Felony Convictions



(d) Mis. Convictions

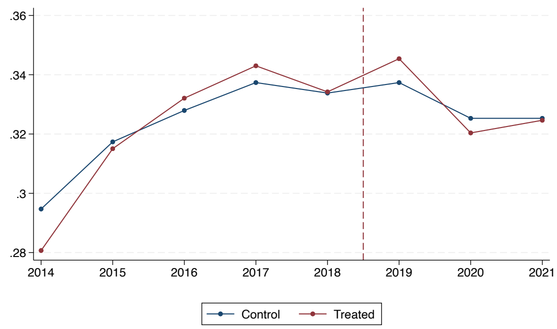


Notes: Each panel plots selected event study coefficients for the share with any wages > \$0 around 7 years after the event, from event-study estimation following Sun and Abraham (2020). Year 7 events occurring in 2021 are used as the last treated group. Timing from the event is based on the charge date 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.

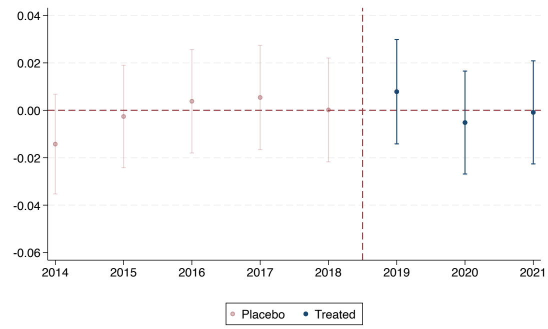
Figure A.17: CA Prop 47 Reductions in SJ County: Effect of Notifications

Any Wages > $0

(a) Levels

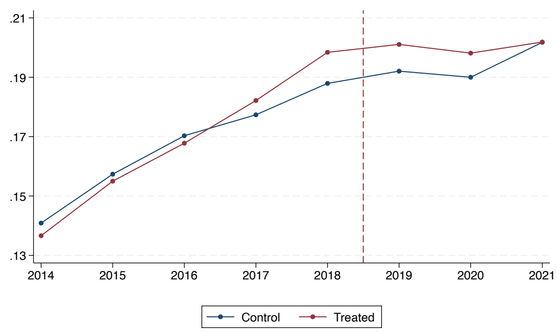


(b) Differences

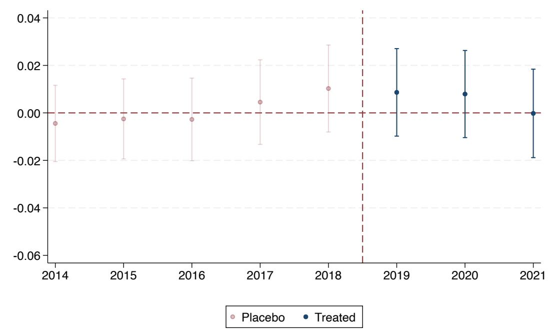


Any Wages > $15,000

(c) Levels



(d) Differences

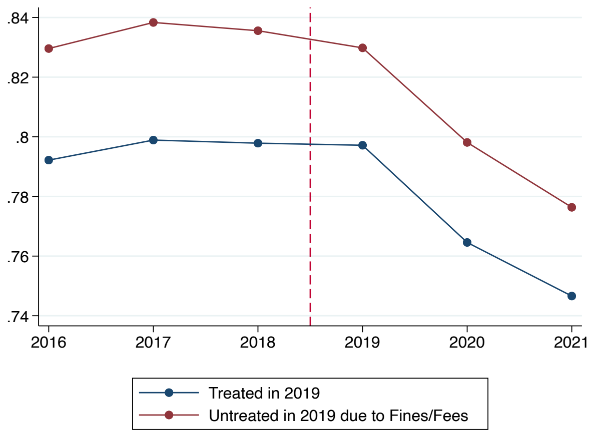


Notes: The treatment group are those who the PD's office attempted to notify about their reduction (N=3,755), control received no attempted notification (N=3,400). Figures (a) and (c) show raw probability of wages $> \$0$ and wages $> \$15,000$ for treatment (attempted notification) and control groups for each year. Figures (b) and (d) show ITT regression coefficients of the effect of notification from Equation **??** in the text run separately for each year. Notifications took place in 2019 and 2020, the dashed red line indicates the end of the pre-period, before any notifications took place.

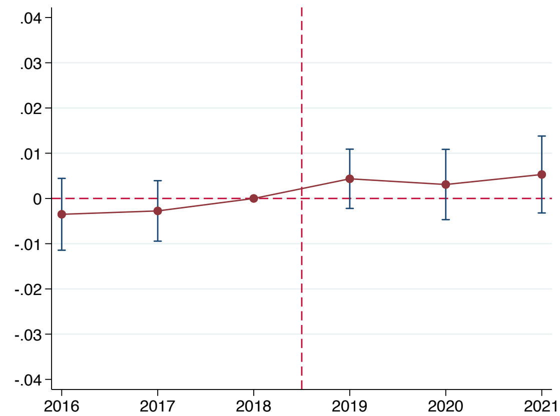
Figure A.18: Impact of PA Clean Slate Reductions on Employment Outcomes

Any Wages > \$0

(a) Raw Data

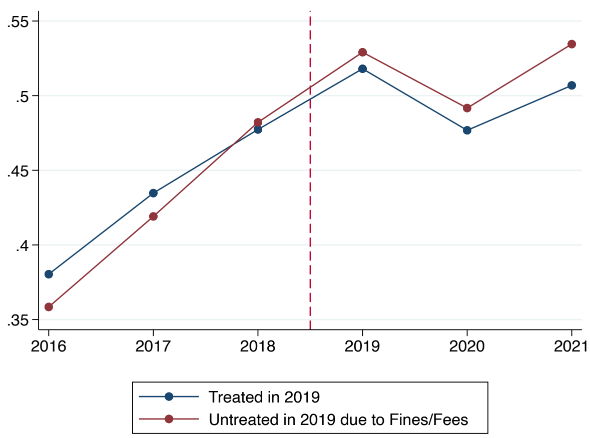


(b) Event-Study Estimates

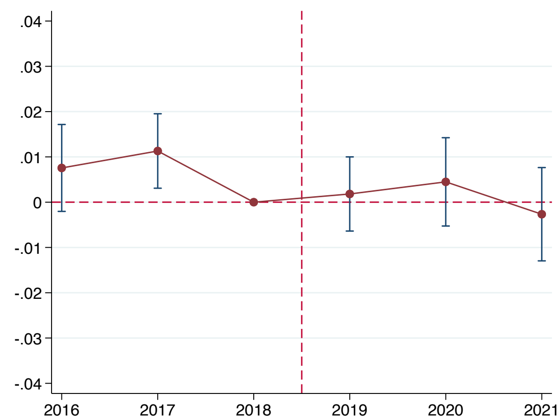


Any Wages > \$15,000

(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.

Table A.10: Impact of PA Clean Slate Reductions on Employment Outcomes - Excluding Philadelphia

(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	0.00448	-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) 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	0.00282	-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.11: Impact of PA Clean Slate Reductions on Employment Outcomes - Black Individuals

(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 2016-2021. 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.12: Impact of PA Clean Slate Reductions on Employment Outcomes - All Others

(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 2016-2021. 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

B Criminal Record Remediation Policies

In this appendix, we describe the policies that we study with a focus on the details of the institutional background and setting that we use to formulate the research designs.

B.1 Fair Credit Report Act

The Fair Credit Report Act (FCRA) is a federal law that governs the type of information reported by consumer reporting agencies (CRAs) to potential employers. CRAs include the major credit bureaus and also employment background check companies including those that provide criminal background checks to employers.[26] FCRA only applies to criminal background checks performed by CRAs—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.[27]

We use the seven-year rule under FCRA to estimate the effect of having a non-conviction record cleared from an employment background check. Under this rule, a record should be cleared 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.

B.2 Maryland Credit Report Law

Nine states—California, Kansas, Maryland, Massachusetts, Montana, New Hampshire, New Mexico, New York, and Washington—have a seven-year limit on reporting convictions, with exceptions.[28] Our study includes Maryland which, in 1976, passed a law which states that employers cannot request 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.[29] While this income threshold is low, it binds for the majority of individuals with criminal records. 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. In our administrative tax data, in Maryland,

[26] There are two types of background checks an employer can choose to run: fingerprint checks and name searches. When an individual is booked by police (e.g. arrested), they are fingerprinted. In a fingerprint-based search, these arrest records will appear regardless of whether the arrest leads to a formal charge. A name-based search queries court records either via an online system or in-person at a court house. This kind of 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.

[27] 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.

[28] For example, California allows full look-backs for convictions for Transportation Network Companies (e.g. Uber, Lyft). In New York, continued reporting of criminal convictions is allowed when the employer is hiring the individual for an annual salary of $25,000 or more.

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

the average W-2 earnings for individuals matched to criminal records data and who are working is $14,000 per year (approximately $7,000 per year median, rounded to the nearest thousand). 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 (Checkr) indicate that agency's default policy is to not provide prior convictions more than seven years old from date of disposition to any employer in Maryland, and the vast majority of employers do not opt out of the default.

B.3 California's Proposition 47

In November 2014, California voters passed a law through referendum known as Proposition 47: The Safe Neighborhoods and Schools Act. Proposition 47 implemented three broad changes to felony sentencing laws within California. First, it prospectively reclassified certain theft and drug possession offenses from felonies to misdemeanors. Broadly speaking, these eligible offenses include theft offenses where the value of property stolen does not exceed $950, such as shoplifting, grand theft, receiving stolen property, forgery, fraud, and drug offenses including the personal use of most illegal drugs.[30] Second, it authorizes defendants currently serving sentences for felony offenses that would have qualified as misdemeanors under the proposition to petition courts for resentencing under the new misdemeanor provisions. Third, it authorizes defendants who have completed their sentences for felony convictions that would have qualified as misdemeanors under the proposition to petition to reclassify those convictions to misdemeanors.[31] For the purposes of our study, we focus on this third change under Proposition 47—the retroactive reclassification/reduction for individuals who have completed their sentences.

Like many criminal record remediation efforts in the United States, retroactive reclassification under Proposition 47 is mainly available by petition to an appropriate court, where the petitioner must establish that he or she committed a crime which, had Proposition 47 been in effect when committed, would be a misdemeanor. If the court grants the request to reclassify the offense as a misdemeanor, the crime will be treated as a misdemeanor for all purposes except for the right to own or possess firearms.[32]

[30] Eligible offenses were communicated to us by a Deputy Public Defender in San Joaquin County, and verified via online sources to the extent possible. For theft offenses, value taken or intended to be taken must be <$950: Commercial burglary during business hours (Penal Code 459); Theft (PC 484(e)(a),484(e)(b), 484(e)(d), 484(g), 484(h), 487(a), 487(b), 487(c), 487(d)(1),487(d)(2), 487(e), 487(g)-487(i)); Receiving stolen property (PC 496(a)); Forgery (PC 470; 471; 472; 475; 476; 484(f); 484(i)(b)); Insufficient Funds [unless previously convicted of 3 or more other crimes] (PC 476a); Petty theft with a prior conviction (PC 666); Grand Theft (PC 489); Auto Theft (Vehicular Code 10851). Eligible drug offenses include possession of methamphetamine (Health & Safety Code Section 11377);possession of controlled substance (H&S 11350); possession of Concentrated Cannabis (H&S 11357(a)).

[31] Some individuals, such as those who had previous convictions for sexually violent offenses, murder, or sex offenses that require registration, were not eligible for the new resentencing, or reclassification provisions of Proposition 47.

[32] While Proposition 47 does not completely clear a person's record, an employer conducting a criminal background check will no longer be able to see the original felony conviction after a reduction. Instead, only a misdemeanor conviction remains. Appendix Figure 1 shows four redacted examples of official court criminal record searches after Proposition 47 reductions. This figure shows that following reduction, Proposition 47 charges are listed as misdemeanors by statute and dispositions denote that the eligible charge was "Reduced

Proposition 47 has been regularly argued to give eligible Californians an opportunity to remove barriers to employment (in addition to housing and other outcomes) through reclassification of a felony to misdemeanor.[33] For example, advocates who helped draft Proposition 47 argue that "[w]e created a system where there's so many collateral impacts to having a felony conviction on your record that you cannot sustain yourself....You cannot find employment. You cannot find housing. You cannot integrate back with your family. These are all things that lead to recidivism."[34]

A Proposition 47 reduction can also allow individuals to obtain certain occupational licenses that previously excluded those with felony convictions. In California, licensing laws can categorically exclude the hiring of individuals with certain criminal records in hundreds of professions, such as healthcare and education, regardless of whether the offense is relevant to the practice of the occupation or poses a substantive risk to public safety, and regardless of the age of the record.[35] Even individuals who receive job-specific training while incarcerated are excluded by licensing restrictions in these occupations. As one example, individuals with a felony conviction are barred from obtaining a Californian alcoholic beverage license.[36]

The interaction of Proposition 47 and another law, the California Investigative Consumer Reporting Agencies Act (ICRAA), implies that the any benefit of retroactive reductions under Proposition 47 should be declining in the time since conviction. This is because under the ICRAA, criminal convictions can be reported for only seven years from the latest of the date of disposition, date of release, or date of violation of parole from the original case (versus indefinitely under federal law), unless another law requires employers to look more deeply into the employee's background.[37]

B.3.1 San Joaquin County, CA and Research Design

Starting in December 2014, the Office of the Public Defender of San Joaquin (OPD) and the San Joaquin County District Attorney's Office (DAO) coordinated to proactively file petitions on behalf of all eligible defendants without requiring effort, intervention, or even knowledge from the defendant. As of September 2019, this effort has resulted in the reduction of approximately 10,000 felony convictions under Proposition 47. As we discuss below, the timing of these reductions was unsystematic and, crucially, most of the reductions were not from individuals who self-selected into treatment, facilitating evaluation.

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to Misd" on a particular date. We have verified that this is the underlying data and process that criminal background check companies use when running a background check.

[33] See, for example, "Finding a Job with a Felony Conviction is Hard. California May Make it Easier," available at https://fivethirtyeight.com/features/finding-a-job-with-a-felony-conviction-is-hard-california-may-make-it-easier/.

[34] See https://www.desertsun.com/story/news/crime_courts/2016/12/14/prop-47-former-felons-new-jobs/94636088/.

[35] Compared to just 5 percent in the 1950s, occupational licensing now covers over one quarter of the U.S. workforce nationally and it is estimated that nearly 30 percent of California jobs require licensure, certification, or clearance by an oversight board or agency for approximately 1,773 different occupations. See https://www.bot.ca.gov/board_activity/meetings/20180524_material_3d_3e.pdf.

[36] See California Business and Professions Code Section 23952.

[37] see https://help.checkr.com/hc/en-us/articles/360000725967-Lookback-periods-How-far-back-are-criminal These exceptions under the ICRAA would apply for certain types of jobs such as in the health industry, or any job requiring an occupational license.

Petition timing: The county's agencies took a multi-step approach to implementing Proposition 47. First, the agencies focused on resentencing for individuals currently serving sentences or under supervision (parole/probation) for eligible felony offenses. Since there is no exogenous variation in when reductions occurred across these individuals, these individuals will not be the focus of our analysis. Second, after reducing records for those currently serving sentences or under supervision, the OPD compiled a comprehensive list of all people in the county with eligible criminal charges who had already completed their sentences from relevant state agencies.[38] There were separate lists for each eligible charge which meant that the same individual could in theory appear on multiple lists. For example, if a person had a petty theft conviction and a drug possession conviction, each of these charges would be listed on the respective "crime lists."

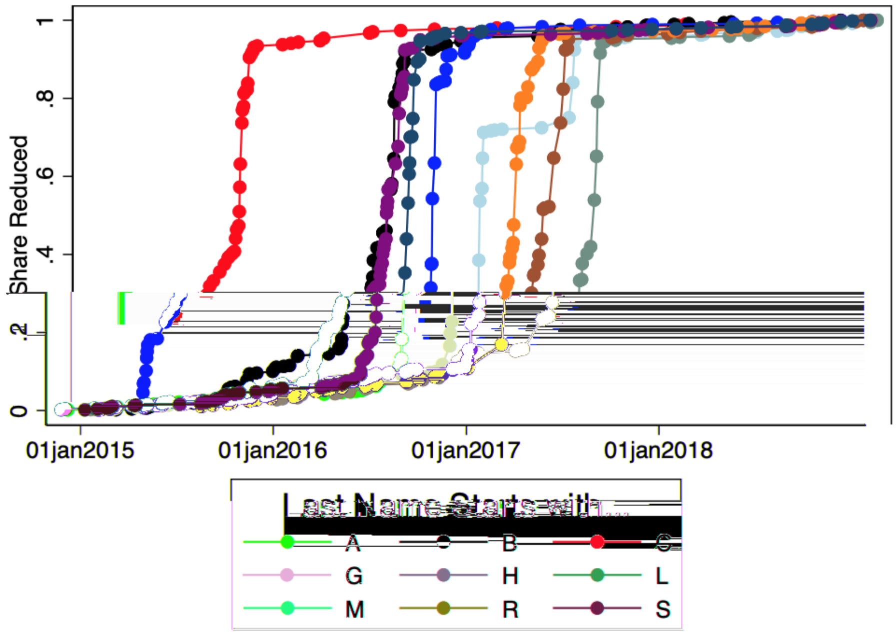
The OPD started with the largest crime list, consisting of individuals with felony drug convictions (designated as "health and safety" or HS crimes). Nearly 85% of individuals for whom petitions were filed had a crime on this HS list. These lists were alphabetical by last name of the eligible individual and OPD personnel worked through these lists in various chunks, initially starting alphabetically with A, although sometimes switching to the other end of the alphabet to reduce workload with filing clerks who split petitions between A-L last names and M-Z last names. This process was effectively quasi-random, with the crime list and the first letter of last name dictating when an individual's proactive petition would be filed.

Figure B.1 depicts the impact of the first letter of last name on the order of petition filing. This figure presents the timing of petitions filed for individuals on the HS crimes list, as described above. The figure presents cumulative density functions (CDFs) for the proportion of petitions filed by date for each first letter of last name. One can see a very clear pattern whereby a vast majority of petitions for those with, say, "A" last names, were filed within a few months of each other during a "surge" period.[39]

[38] The list was obtained from a court record system known as CJIS, which only digitized records going back to 1990. As a result, any eligible charge from before 1990 was not on the list.

[39] Appendix **??** presents these CDFs for each crime list, where analogous "surges" can be seen for each letter of last name. Occasionally, there were deviations from the alphabetical ordering for idiosyncratic reasons, which include the someone mistyping the court case number or name and the mistyped case was also eligible; codefendants on an eligible case are also eligible and would be filed together; referrals of eligible clients to the public defender's office from attorneys; referrals of eligible individuals from local organization "Justice Fairs." Deviations from alphabetical ordering within a crime list also occurred if an individual appeared on more than one crime list, as the OPD filed a petition for all of each individual's eligible charges at the same time.

Figure B.1: Alphabetical Ordering for HS (Drug) Crime Petitions



Notes: The timing of proactive felony reductions in San Joaquin County was determined in an alphabetical manner. Figure shows the CDF of felony reductions for HS crimes, by the indicated first letter of last name. Appendix **??** contains the CDFs for other letters.

After preparing a petition for each eligible individual, the OPD sent the petition to the DAO for review. Throughout this process, there was a general understanding between the two agencies that the vast majority of petitions would be approved. The approved petition would then be sent to a judge to officially secure the Proposition 47 reduction. This was a time- and labor-intensive process.

Not all petitions filed by the SJOPD were done through proactive reductions. Some individuals directly called the OPD to inquire about their eligibility to receive a reduction under Proposition 47 and to ask for a petition to be filed on their behalf.[40] Collectively, these individuals were prioritized by the SJOPD and had their petitions filed soon thereafter. We leverage the alphabetical nature of the proactive petition filing to identify likely self-petitioners, as we describe in Section 2.2.5. In Section 4, we will turn to the sample of self-petitioners to assess the importance of selection bias.

The Public Defender's office made an effort (with the aid resources from the District Attorney's office) to notify at least a subset of individuals about these reductions. The notifications took place in randomized waves, with 4086 individuals with reductions being chosen to be notified in the first wave. These notifications took place between June 2019 and March 2020. Contact information was collected by the Public Defender's office from Transunion's

[40] Another small group of individuals were referred to the SJOPD based on their participation in local "record change and justice fairs," during which community members receive free legal consultations to see if they are eligible for reduction under Proposition 47 (N=96).

TLO product which provides most recent addresses, e-mails, and phone numbers. Of the 4610 individuals randomly chosen to be contacted in this first-wave, contact information could be located for 3982 (86.3%). Between June 2019 and March 2020, SJOPD personnel with carefully written scripts attempted to call these 3982 individuals in a random order; in January 2020 letter were mailed to individual homes (with self-addressed postcards included to return upon receipt); and in January 2020 e-mails were sent as well. Text messages were sent between December 17, 2019 and May 15, 2020.[41] Text messages were staggered randomly so as to not overwhelm the SJOPD call center.[42]

Through this effort, SJOPD was able to confirm successful contact, either by phone, return of postcard, or email, of 1,175 individuals (29.5% of those with contact information, and 25.5% of the full first-wave notification group). The true contact rate is likely higher since not everyone who received a letter called the SJOPD or mailed back the included pre-addressed postcard. SJOPD reported that 411 individuals were surveyed by phone who had received proactive reductions and asked them if they were previously aware of having received a reduction. Only 6.1 percent of the group responded that they were aware.

B.4 Pennsylvania Clean Slate Law

In 2018, Pennsylvania enacted the Clean Slate Law (Act 56 of 2018), which implemented automated sealing of all non-conviction records with no waiting period, as well as certain low-level conviction records after ten years. Those who still owed court fines and fees were not (initially) eligible.[43] Eligible records were sealed between June 2019 and June 2020 and the law has 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.

C 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.

C.1 Step 1

We first search for 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

[41] The text messages said: "This is the San Joaquin County Public Defender's Office. We have good news to share with you. Please call DPD [name redacted] at [number redacted]".

[42] We downloaded the list of randomly contacted defendants from the website of the San Joaquin County Public Defender's Office last accessed 6/20/22). See https://www.sjgov.org/department/pubdef/programs-services/proposition-47.

[43] In 2020, PA eliminated court fees and fines as a barrier to sealing so long as restitution is paid.

Taxpayer Identification Number. The database includes a history of up to nine Name Controls ever-associated with an individual (for example, women 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.

C.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.

C.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 analysis of notifications. 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		

C.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.

C.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%

C.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%.

C.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%.

C.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 C.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.