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**Is It Time to Let go of the Past? Effect of Clean Slate
Regulation on Employment and Earnings**

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Is It Time to Let go of the Past? Effect of Clean Slate Regulation on Employment and Earnings

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Abstract

We focus on New Zealand's clean slate legislation to analyze whether automatic concealment of criminal records improves ex-offenders' labor market outcomes. Based on the legislation's eligibility requirements, we utilize detailed court charges information to identify comparable groups of ex-convicts who are subsequently linked to a population-wide tax register that documents monthly employment information. We use a difference-in-differences framework to compare clean slate-eligible individuals to former convicts who are approaching eligibility. Our analysis reveals that the clean slate scheme has no statistically relevant impact on employment propensity. However, we find a significant 2-2.5% increase in monthly wages during the post-implementation period.

JEL Classification: C21; J08; K14.

Keywords: Clean Slate; Court Charges data; Monthly Tax Records; Conviction; Employment; Earnings; Difference-in-Differences; Triple-difference

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1 Introduction

We utilize uniquely detailed administrative information to investigate whether removal of past criminal records improves formerly convicted individuals' labor market prospects. Our analysis focuses on New Zealand's 'clean slate' initiative that was formally enacted as the Criminal Records Act in November 2004. The clean slate regulation allows automatic concealment of ex-convicts' criminal records if they did not have any further convictions within seven consecutive years following the date when they were last sentenced.¹ In general, our analysis evaluates the efficacy of rehabilitative reforms enacted for individuals with a prior criminal background - a group that often experiences labor market discrimination, even after having served their court-ordered sentence obligations.

The existing literature shows that past criminal records (e.g., formal arrests or court-based convictions) have a scarring effect on ex-offenders' future socio-economic well-being. Individuals with a criminal history often encounter high entry barriers in the labor market, which substantially reduce their employment prospects (Grogger, 1992, 1995; Stoll and Bushway, 2008; Agan and Starr, 2018). Consequently, the commonly observed firms' reluctance to hire ex-offenders have prompted policymakers to adopt legislative initiatives that restrict or defer employers' access to their job applicants' past criminal records. A well studied example of such rehabilitative interventions is the United States' (US) state-specific 'Ban-the-Box' policy (BTB). The BTB reform restricts employers from asking about a job applicant's criminal background during the initial stages of a hiring process (see Doleac and Hansen, 2016; Agan and Starr, 2018; Craigie, 2020; Doleac and Hansen, 2020; Rose, 2020).²

Relative to the US's BTB intervention, several European countries along with Australia, Canada, New Zealand, and South Africa have enacted legislation that

¹There are certain additional eligibility requirements, which are accounted for in our analysis and discussed later in a greater detail.

²The restriction on employers' access to applicants' criminal history is usually imposed by removing the criminal history questions from job application forms.

allows expungement of past criminal convictions (see Loucks et al., 1998; Naylor, 2005; Mujuzi, 2014; McAleese and Latimer, 2017; Gollogly et al., 2019). These regulations may vary by country-specific eligibility requirements. In the US too, a few states (such as Pennsylvania, Utah, and Michigan) have recently enacted clean slate initiatives that automatically remove outdated criminal records from the respective states' existing crime registry.³

We contribute to the relevant international literature by providing empirical evidence using comprehensive national-level administrative data on criminal convictions and labor market characteristics. Furthermore, while numerous studies have analyzed the effectiveness of the BTB policy, New Zealand's (NZ) alternative legislative approach constitutes a novel case study.

We begin by documenting employment and earnings trends for convicted individuals before and after their first conviction. The adverse labor market implications of having a prior criminal record have been unequivocally confirmed in existing international literature (Borland and Hunter, 2000; Agan and Starr, 2018; Rose, 2020). We draw NZ-specific evidence by making use of variation in the timing of the first criminal conviction. Not surprisingly, we observe that on average, any first-time conviction leads to a statistically significant decline in the likelihood of employment (by 2.2 percentage points) and a drop of monthly earnings from wages & salaries (by approximately NZ\$ 125). When excluding traffic-related first-time convictions from our sample, the adverse labor market effects are further amplified.

The implementation of NZ's clean slate regulation provides a quasi-experimental setting to estimate the impact of employers' access to past criminal records on labor market outcomes. The detailed national register of all court

³Pennsylvania was the first state to pass a bill on clean slate initiative in 2018. Utah implemented clean slate legislation in 2019. In October 2020, Michigan became the latest of the three states to have enacted the clean slate legislation. The state respectively allows 7- and 10- rehabilitative periods for misdemeanors and for felonies. The information has been retrieved (on March 20, 2021) from the Crime and Justice Institute's website. For further details, see <https://www.cjinstitute.org/news-article/michigan-governor-signs-historic-clean-slate-legislation/>

charges allows us to precisely identify a sample of clean slate-eligible individuals and a reference group of ex-convicts who are yet to be eligible for having their criminal records automatically concealed. We link both the groups to their monthly administrative tax records to document individuals' employment and earnings trajectories. To estimate the causal relationship of our interest, we apply a difference-in-differences (DID) framework.

Focusing on prime-aged males (aged 25-64)⁴, key regression estimates indicate that while the clean slate scheme had no relevant impact on ex-offenders' employment propensity, the post-implementation period saw a significant rise in their monthly wage earnings- an increase of approximately 2-2.5%. As we illustrate in forthcoming sections, our key empirical findings are robust to several sensitivity tests and alternative empirical specifications. Furthermore, the key findings are supported by a difference-in-difference-in-differences (triple difference) strategy wherein the third comparable group is comprised of a randomly selected sample of non-convicts. This sample was identified from a population-based pool of all non-deceased prime-aged males who were never criminally charged (and therefore convicted) of any offense.

Although our administrative data sources do not allow us to objectively identify the possible mechanisms for the observed effects, we do find indirect empirical evidence in support of the conjecture that the intervention may have increased employed ex-offenders' bargaining power in wage negotiation with their employers. One straightforward policy implication of our finding is that while wiping off records does have benefits in terms of increased wages, a wait of seven years is likely too long a period for it to have any impact on employability.

Finally, in drawing an analogy with the existing BTB-based literature (e.g., Agan and Starr, 2018; Doleac and Hansen, 2020), we test whether NZ's clean slate regulation is likely to trigger (racial or) ethnic disparities in labor market outcomes. Upon separately comparing each of the three most relevant ethnic mi-

⁴We restrict the upper age limit to 64 as Kiwi residents are eligible for a publicly funded pension scheme from the age of 65, which may additionally impact workers' labor market activities.

minority groups in NZ (i.e. Māori, Pacific Peoples, and Asians)⁵ to the largest ethnic group (NZ Europeans), we do not find any statistically relevant differences in overall employment rates and earnings. These results provide suggestive evidence that clean slate initiatives in jurisdictions may not trigger the risk of statistical discrimination from employers. As such, our analysis highlights a key contrast in terms of the potential inadvertent effects across different rehabilitative policies studied in the standard literature.

2 Related Literature

At the foundation of our analysis, and probably the main motivating factor for regulations that support expungement of past criminal records, are several previous studies that investigate the impact of having a criminal history on ex-offenders' labor market outcomes. Generally, these studies find a significant negative impact of a criminal past. In one of the seminal papers, Grogger (1992) analyzes arrest and employment information of young men (aged 17-26) in California. The study finds that each arrest decreases the probability of employment in the subsequent year by 2 percentage points and that, such disemployment effect likely persists much longer than a year.

Borland and Hunter (2000) study the effect of arrest on employment status of indigenous Australians using 1994 National Aboriginal and Torres Strait Islander Survey. The authors find that having been arrested lowers the employment probability by 10% to 20% for males, and 7% to 17% for females. The authors conclude that differences in arrest rates explain about 15% of the difference in employment rates between indigenous and non-indigenous Australians. Dobbie et al. (2018) use administrative data from court and tax records for Philadelphia

⁵Unlike the US, NZ's Census survey does not include information on race as a demographic information. In NZ, ethnicity is one of the most important attributes of cultural identity. The major ethnic groups in NZ are the Europeans (or NZ Europeans), Māori, Pacific Peoples, Asians, Middle Eastern/Latin American/African (or MELAA), and other ethnicity. See <http://archive.stats.govt.nz/Census/2013-census/profile-and-summary-reports/ethnic-profiles.aspx#gsc.tab=0>.

and Miami-Dade in the US to estimate the effect of pre-trial detention on various subsequent outcomes, including labor market performance. The authors find that pre-trial detention decreases employment prospects in the formal sector and leads to a reduction in the receipt of employment and tax-related public benefits.

The negative effects of criminal records (documented in the form of arrest, conviction, or detention) are most likely due to a combination of continued unemployment spells resulting from offenders' contact with the criminal justice system and employers' access to individuals' criminal records. In recent years, researchers have used experimental designs to gauge the effect of having access to criminal records on employers' hiring decision. Among those studies, one of the most comprehensive analyses comes from Agan and Starr (2018), who employ a field experiment by sending out around 15,000 fictitious job applications to employers in New York and New Jersey before and after the adoption of the BTB policy. The authors observe that for young men, having a criminal record is a major barrier to employment. Specifically, employers asking about applicants' criminal background information were 63% more likely to call applicants with no records.

Existing crime literature also shows that after a period with clean records, the chances of an ex-felon committing crime decline substantially and eventually converge toward that of general population (Kurlychek et al., 2006; Blumstein and Nakamura, 2009; Kurlychek et al., 2012). Focusing on youth aged 16 to 20, Blumstein and Nakamura (2009) find that the estimated time to converge varies from 3.2 years to 8.5 years, depending on age at the time of first crime. Work opportunity itself, along with a number of other life events such as marriage, child birth, ageing etc., is found to be a key driver of redemption (Uggen, 2000; Laub and Sampson, 2001).

A likely motivation for employers to not hire individuals with a criminal past is to avoid economic liabilities potentially associated with future crimes. In this context, a natural research question is whether restricting employers' access to use criminal records as a screening device would help improve ex-offenders' la-

bor market wellbeing.⁶ Some of the most relevant evidence for the effect of restricting records on labor market outcomes comes from researchers analyzing the effects of BTB policies in the US. BTB policies prevent employers from asking conviction-related questions at the beginning of hiring process and these policies vary greatly across jurisdictions within the US. Recently, Craigie (2020) analyzes conviction and employment records of individuals surveyed in the National Longitudinal Survey of Youth 1997 Cohort in a DID framework to study the effect of BTB policies in the US. The author finds that such policies raise the probability of public employment for those with convictions by about 30% on average. However, not all studies paint a rosy picture for BTB policies. For example, Agan and Starr (2018) find that BTB policies may backfire by encouraging racial discrimination as employers resort to statistical discrimination in lack of explicit individual-level information. In line with this finding, Doleac and Hansen (2020) conclude that BTB policies decrease the employment probability of young, low-skilled black men by 5.1%.

The recent study by Rose (2020) is the most germane to our analysis. The author uses administrative records of employment and conviction records to evaluate the labor market implications of BTB policy. Specifically, Rose (2020) focuses on a 2013 Seattle law barring records until after an initial screening and finds that the law had negligible impacts on ex-offenders employment and earnings. The author conjectures that employers respond to the law by deferring background checks to a later stage in the hiring process. However, NZ's clean slate regulation, described in detail in the following section, is genuinely different from the widely studied

⁶We want to note that there are arguments for allowing employers access to criminal records. For example, Bushway (2004) discusses a model of statistical discrimination to argue that allowing access to criminal records actually increases market wages for individuals without record, including the average wage for groups with a large number of convicted individuals. Additionally, there are also ethical and moral issues related to the use of such records. Lam and Harcourt (2003) discuss in detail the arguments for and against legal protection of ex-offenders by limiting employers' access to use of criminal background. The authors highlight issues ranging from employers' rights, employers' obligations to their employees and customers, ex-offenders rights, and unfair discrimination against ex-offenders and its social costs. Petersen (2016) discusses ethical issues related to the use of criminal records for statistical and structural discrimination by employers.

BTB program, as it completely wipes away the data instead of simply creating a hurdle at the beginning of the interview process. This allows us to explore the previously unanswered policy-relevant question: what would be the labor market effects of expanding BTB-like policies to completely seal criminal records from employers at all stages of the hiring process?

3 Institutional Background

The clean slate scheme of NZ was established by the enactment of the Criminal Records Act, which came into effect in November 29, 2004. The main underlying rationale for the enactment of the legislation is to mitigate the social barriers (such as labor market discrimination) commonly experienced by individuals with a criminal past.⁷ As already mentioned, the clean slate initiative allowed automatic concealment (i.e. without the need to apply) of formerly convicted individuals' past criminal records, provided they did not have any further convictions in at least seven years since the date when they were last sentenced (defined as the 'rehabilitation period').⁸ Furthermore, as listed below, the eligibility for the clean slate scheme is conditional on certain additional criteria. Based on the Criminal Records Act's provisions, an individual must -

- not have any convictions within the previous seven years since the last sentencing.
- not have received any custodial sentences (prison, corrective training, preventive detention, borstal training).
- not have convictions for sexual offence (defined as 'specified offence').

⁷Criminal background checks are quite common in NZ. The Ministry of Justice in NZ processes over 500,000 requests annually for criminal conviction history checks, a substantial portion of which likely comes from potential employers. The Ministry of Justice's annual reports provide annual estimates of criminal conviction check requests processed from 2012 until 2019; See <https://www.justice.govt.nz/about/about-us/corporate-publications/>.

⁸See details in <http://www.legislation.govt.nz/act/public/2004/0036/latest/DLM280840.html>; Retrieved on May 5, 2020.

- have paid in full all financial penalties and criminal offence obligations (e.g., compensation, reparation costs) as ordered by the court.
- not have received any indefinite disqualification from driving vehicles.
- not have been ordered by the court to be admitted to hospital for mental health treatment instead of being sentenced.⁹

In the next section, we demonstrate how we utilize the Ministry of Justice’s detailed court charges information to identify the sample of clean slate-eligible and a comparable control group for estimating the causal impact of the clean slate act on labor market outcomes. Even if the individual meets the above criteria, there are some instances where otherwise eligible individuals may still have to provide their full criminal history. These exceptions include traveling outside the country (e.g., for processing visa applications) and applying for jobs in certain public administration services related to national security, law enforcement, corrections, and justice.¹⁰

Furthermore, for certain offenses, individuals may also apply for convictions to be disregarded (e.g., sexual offences that received non-custodial sentences and decriminalized offences such as homosexual offences). As will be highlighted in the next section, our administrative court charges data provide sufficient information to rule out criminal offenses that could render a former offender ineligible to have their criminal records concealed under the clean slate act.

⁹The list of eligibility criteria is also provided in <https://www.justice.govt.nz/criminal-records/clean-slate/>; Retrieved on May 10, 2020.

¹⁰Table A1 provides industry-wise distribution of employed convicted and non-convicted individuals present in our regression samples. We observe the the proportion of non-convicted individuals employed in public administration and safety services (is almost double the percentage of convicted individuals employed in that sector, regardless of their clean-slate eligibility.

4 Data - The Integrated Data Infrastructure

We utilize data from a large-scale database known as the Integrated Data Infrastructure (IDI). Administered by Statistics NZ, the IDI houses a wide range of linked administrative and survey-based microdata about individuals and households in NZ. These data are collected from various government agencies and non-government organizations, and each individual has a unique confidentialized identifier that can be used to link them across the different datasets.

To identify our primary sample, we begin with the Ministry of Justice's court charges data, which records all charges that were processed in NZ criminal courts since 1992. The court charges data provide individual-level information on offense date, offense type, outcome type (e.g., convicted or acquitted) and additional court proceeding details including court identifiers, plea type, hearing and outcome dates, sentence type, and so on. The selection of relevant sample of eligible ex-offenders and a comparable control group is performed by conforming to the list of criteria stated in the preceding section.

In Table 1, we show the chronological steps of selecting the initial sample of male convicts from the court charges register.¹¹ The monthly period considered in our analysis spans from January 2000 through December 2009. The time selection ensures that we have equivalent and sufficient data points for the pre- and post-clean slate implementation period. We begin by selecting all individuals who had their last recorded court hearing date (for any offense) between 1992 and 2003. The last court hearing date usually refers to the date when individuals, if convicted, receive their sentence, if there is any. The eligibility under the clean slate regulation is conditional on the elapsed time since the last sentence. As such, the selected range of last court hearing dates ensures that we have sufficiently large number of observations below and above the seven-year threshold, which

¹¹Although we initially select a broader male sample of ex-convicts from the court charges data, in our empirical analysis, we focus on prime aged males (25-64) as that group tends to have stable labor market conditions. See Van Ours (2007); Greenstone and Looney (2011); Moffitt and Gottschalk (2012).

eventually determines the treatment status in the post-implementation period.

We link the selected individuals to the Department of Internal Affairs' death registers to remove all deceased individuals. From the resultant non-deceased sample, we select only those individuals whose last observed court charge led to a conviction. This can be identified using the "outcome type" information of the court charges data, which indicates whether an individual has been convicted or acquitted.

Next, using "sentence type" information from the court charges data, we remove all individuals whose last conviction resulted in a custodial sentence (e.g., imprisonment or home detention), driving disqualification, or a court order for mental health treatment in rehabilitative facilities. As the last step, we remove any individual who was convicted due to any sexual or violence-related offenses. The identification of such cases was facilitated by the "offence code" information, which is provided by a highly detailed crime classification system developed by the Ministry of Justice as a part of the ministry's New Zealand Crime and Safety Survey.¹² The final court charges sample is comprised of 57,915 males who had their last recorded conviction between 1992 and 2003.

It is worth noting that convicted individuals with financial penalties are also required to fulfill their monetary obligations in full to be clean slate-eligible (see preceding section). However, there are no administrative information on whether convicted individuals who were subjected to monetary penalties such as reparation costs or fines successfully executed their court-ordered obligations. As a further verification, we retrieve relevant information from the Ministry of Justice (MoJ) upon submitting a request under the Official Information Act 1992. Table A3 indicates that convicted offenders in NZ have a high compliance level with respect to settling their court-imposed financial penalties. For instance, as per the MoJ records dated November 11, 2020, the proportion of offenders who successfully indemnified all of their financial obligations imposed by an NZ court between

¹²The alternative and a broader classification system is called the Australian and New Zealand Society of Criminology (ANZSOC) classification system. We re-confirm our exclusion of sexual and violence-related offence by referring to the ANZSOC classification as well.

2000 and 2014 varied between 96 and 100 percent. For financial penalties imposed in more recent years (i.e. post-2015), the percentage appears to decline marginally. One explanation is that some of the payment periods assigned to the convicted individuals were still in progress.

The selected MoJ sample of 57,915 former male convicts is then linked to Stats NZ's Personal Details files for individual-level demographic information including birth dates (for age) and ethnicity. We also link the identified sample of convicts with the national census held in March 2013, to incorporate additional individual characteristics like educational attainment. However, the link rate between the selected MoJ sample and census 2013 data is $\sim 50\%$. This is likely due to non-response or physical absence during the time of survey. As such, controlling for census-specific individual characteristics may limit our analysis to a potentially selective sample. Nonetheless, as will be shown later, our regression estimates are consistent across various specifications that incorporate different combinations of individual-level covariates and samples. However, our preferred specification incorporates the broader MoJ sample of male convicts before they were matched with the census data.

The longitudinal panel of the MoJ sample is then created such that the individuals could be linked to the Inland Revenue's (IR) monthly tax records from January 2000 through December 2009. The IR data allows us to create a dichotomous employment indicator and inflation-adjusted estimates of monthly earnings from wages and salaries. Finally, to ensure that our analysis is based on individuals who are physically present inside NZ, we link the longitudinal version of the MoJ sample with the NZ's border movements data administered by the Ministry of Business, Innovation and Employment (MBIE). The border movements data allows us to create monthly indicators of whether an individual was travelling outside NZ within the study period. The primary empirical analysis is performed using a final sample of non-deceased formerly convicted males aged 25-64 who were physically present in the country during the evaluation period.

4.1 Descriptive statistics

As shown in Table 1, the 57,915 convicted individuals in our sample have 85,359 convictions in total, indicating that there are some repeat offenders. Based on the list of ANZSOC’s broad offence classification, most convictions are in the ‘dangerous acts’ category (27.58%).¹³ Criminal offenses ‘against justice’ make up approximately one-fourth of all convictions, followed by traffic-related convictions (11.25%) and ‘fraud & deception’ (9.91%). Table 2 shows the complete list of all (last convicted) offenses for which the individuals in our main sample were convicted.

Our baseline regression analysis sample has 1,264,860 person-month observations. Summary statistics for this sample in the pre-clean slate months are presented in Table 3. We separate the descriptive statistics for individuals that are eligible for clean slate (treated), those that are not yet eligible for having a clean slate (untreated), and an additional sample of randomly selected non-convicted individuals (used in a triple difference model). We see that while monthly earnings for treated individuals are higher than those for untreated, there is no difference in terms of employment rate. Moreover, there is no real difference between the two groups in terms of ethnic composition, age, or education.

5 Identification Strategy

To estimate the labor market implications of the clean slate legislation, we incorporate a standard DID approach. The eligibility for treatment is determined by time since a convicted individual received their last sentence. Utilizing the

¹³The detailed definitions and examples of each broad category of ANZSOC offense type can be found in the following link: <http://datainfolplus.stats.govt.nz/Item/nz.govt.stats/a10413bf-f78a-4f17-a9c1-55e7717ab91d>; Retrieved on August 28, 2020

selected MoJ sample of former male convicts, we estimate:

$$Y_{it} = \alpha_0 + \alpha_1.(Post_t * Eligible_{it}) + \alpha_2.Post_t + \alpha_3.Eligible_{it} + \mathbf{X}_i'.\alpha_4 + \lambda_t + A_{it} + \Omega_i * t + \varepsilon_{it} \quad (1)$$

such that

$$Post_t = \begin{cases} 1 & \text{if time} \geq \text{December 2004} \\ 0 & \text{otherwise} \end{cases}$$

and

$$Eligible_{it} = \begin{cases} 1 & \text{if time elapsed since} \\ & \text{last sentence} \geq 7 \text{ years} \\ 0 & \text{otherwise} \end{cases}$$

In Equation (1), Y_{it} represents the dependent variable, which is a binary indicator when we analyze the impact of the clean slate scheme on the likelihood of being employed. For analyzing the wage effects, Y_{it} is a continuous measure represented by logarithm of total monthly earnings from wages and salaries (log earnings). The continuous measures of earnings are adjusted for inflation by using 2017 estimates of the consumer price index. The parameter α_1 represents the estimated relationship between clean slate regulation and relevant labor market outcomes. The vector \mathbf{X}_i' incorporates time-invariant individual-level characteristics such as ethnicity and educational attainment. Finally, in all our models, we control for time- and age-specific fixed effects (represented by λ_t ¹⁴ and A_{it} respectively) and age-specific linear time trends ($\Omega_i * t$). We test the consistency of the estimate of parameter α_1 across multiple empirical specifications ranging from a parsimonious baseline specification to a more saturated model that controls for individuals' educational attainment (when linked to Census 2013 data). Finally, we account for individual-specific unobserved characteristics using individual fixed

¹⁴We also estimate separate specifications where time fixed effects are replaced by time since last sentence fixed effects and age-specific linear time trends are replaced by age-specific linear trends of time since last sentence. Our results do not vary.

effects regressions.

Unfortunately, the IR data does not provide information on hours worked. Therefore, it is difficult to identify full-time (or part-time) employment. As such, we focus on a relatively homogeneous sample of prime-aged males who are most likely to be in full-time employment.¹⁵ To ensure comparability between eligible and ineligible groups, we allow the elapsed time since the last sentence to vary between five to nine years (to maintain a bandwidth of 2 years below and above the seven-year threshold). As shown later, our results do not qualitatively vary when estimating DID regressions using narrower bandwidths of time since the last sentence. As a final restriction, we limit our regression models to months where individuals were not observed to travel outside NZ. This condition allows us to account for the possibility that people might be employed outside the country for certain monthly spells, which is unobserved. As such, the inclusion of months where individuals are observed to have travelled outside NZ might lead to underestimating the true impact of the clean slate initiative. Importantly, our results do not vary if we instead control for an individual-specific time-variant dichotomous indicator which equals 1 if individual i was observed to be traveling outside in month t .

To empirically examine the validity of the causal interpretation of the DID estimate of α_1 in Equation 1, we test for parallel trends assumption using an event study design (Angrist and Pischke, 2009). To be specific, using separate time dummies representing periods prior to and post-clean slate legislation, we check whether there's any meaningful variation in labor market outcomes of eligible individuals during the pre-implementation periods leading up to the passage of the clean slate scheme. As such, statistically significant coefficients for pre-intervention time dummies for the clean slate-eligible group can be suggestive of

¹⁵Organisation for Economic Co-operation and Development (OECD) data on NZ suggests that over 95% of working age men (25 and above) work full-time in NZ. The information was retrieved from <https://stats.oecd.org/> on June 25, 2020.

policy endogeneity. In particular, we estimate:

$$Y_{it} = \beta_0 + \sum_{s=-23}^{24+} \gamma_s \cdot (D_{it}^s * Eligible_{it}) + \sum_{s=-23}^{24+} \Gamma_s \cdot D_{it}^s + \beta_1 \cdot Eligible_{it} + \mathbf{X}_i' \cdot \beta_2 + \lambda_t + A_{it} + \Omega_i * t + e_{it} \quad (2)$$

, where D_{it}^s is a time dummy which equals 1 when individual i at time t is s months from implementation of the clean slate act. Estimate of parameter γ_s represents the difference in the labor market outcomes between eligible and ineligible groups at the s^{th} month relative to clean slate intervention. The binned pre-implementation period representing 24 or more months prior to enacting the clean slate act is considered the omitted category in our parameterized event analysis.

The DID approach compares the labor market outcomes of eligible and convicted individuals who are approaching eligibility. Because the treatment status is conditional on having a criminal conviction, the intervention allows us to test the robustness of our key findings by comparing our treated group with an additional group of prime-aged males who never received a criminal conviction. The inclusion of a third difference allows us to eliminate possible confounding influences from unaccounted heterogeneities that may affect labor market outcomes in the DID specifications (see Hamermesh and Trejo (2000)).

For the third control group, we randomly select a comparable sample size of 30,000 non-deceased males from the population-based pool in the IDI. For the purpose of our analysis, we make sure that the individuals were born between 1936 and 1984 and were never criminally charged (or convicted). The selection of birth dates ensures that most of the non-convicted individuals, like the DID sample, are aged between 25 and 64 in our analysis. Similar to our DID specification, we also make sure all non-convicted individuals in our regression sample

were present in NZ. The triple difference specification is:

$$Y_{it} = \rho_0 + \rho_1.(Convict_i * Post_t * Eligible_{it}) + \rho_2.(Convict_i * Post_t) + \rho_3.(Post_t * Eligible_{it}) + \rho_4.(Convict_i * Eligible_{it}) + \rho_5.Convict_i + \rho_6.Post_t + \rho_7.Eligible_{it} + \mathbf{X}'_i.\rho_8 + \lambda_t + Age_i + \Omega_i * t + v_{it} \quad (3)$$

where

$$Convict_i = \begin{cases} 1 & \text{if ever convicted} \\ 0 & \text{otherwise} \end{cases}$$

In Equation 3, the triple difference estimate ρ_1 represents the difference in labor market outcomes estimated by comparing eligible former convicts to ineligible former convicts and non-convicts before and after the clean slate enactment. It is worth noting that clean slate-eligibility only applies to a person having a criminal conviction in the past. Therefore, the variable $Eligible_{it}$'s value is zero for all non-convicted individuals. As such, the variable results in being omitted from our regression when interacted with $Convict_i$. Nonetheless, the estimate ρ_1 appears to remain consistent with our key DID-based findings. However, to circumvent the collinearity issue, we randomly assign 'fake' dates of last sentence to the group of non-convicted individuals to superficially determine their eligibility status. This is conditional on the reasonable assumption that the clean slate legislation should not affect non-convicted individuals' labor market engagements. The triple difference strategy provides an additional check to ensure that the DID-based estimates of the legislation's impact on labor market outcomes are genuine.

6 Results

6.1 Effect of conviction on labor market outcomes

To understand the relevance of legislative measures such as the clean slate regulation, it is first important to evaluate the potential impact of criminal convictions on

labor market outcomes. To this end, we begin by visualizing raw trends in monthly earnings and the employment rate of ex-convicts before and after their first conviction in Figure 1. We perform separate analysis by excluding traffic-related offenses from our sample to focus on relatively more serious convictions.¹⁶ In Figure 1, distinct trends are presented for all convictions and all non-traffic convictions. The plots show that the wage earnings and the employment prospects for ex-offenders were steadily declining until reaching the first conviction. The respective trends experience a sharp drop right after the first conviction before gradually recovering over time. In Figure 2, we plot the differences between outcomes of convicts and non-convicts to envisage how convicts' labor market outcomes evolve relative to that of non-convicts. The plots of the relative measures indicate a significant negative impact of a conviction on wages and employment prospects around the first conviction.

To estimate the causal impact of convictions on labor market outcomes, we adopt an empirical strategy similar to Rose (2020). Utilizing variation in the dates of first conviction across offenders, we look at the effect of such criminal conviction using four measures of labor market outcomes. These variables include two binary indicators of whether an individual is employed and whether an individual's monthly earnings exceeded the monthly full-time inflation-adjusted minimum wage rate. The other two variables include continuous measures of actual monthly wages and salaries. In the first measure, non-employed individuals' earnings are treated as zero, while second variable represents monthly earnings of employed individuals only (such that the non-employed individuals' earnings are treated as missing).¹⁷

Using the court charges data, we look at a sample of male convicts who received their first criminal conviction at any month during the five-year window

¹⁶Traditionally, based on seriousness scores assigned to each ANZSOC classification by the MoJ, traffic-related offenses have the lowest average score.

¹⁷See discussions by Mocetti (2007) and Jenkins (2011). Furthermore, the latter measure of is more in line with the standard labor literature (e.g., see Jensen and Shore, 2015), which utilizes logarithmic values of wage earnings, intuitively treating non-employed individuals' earnings as missing.

between 2010 January and 2014 December.¹⁸ We estimate:

$$Y_{it} = a + \mu_i + \delta \cdot FirstConv_{it} + \mathbf{Z}'_{it} \cdot b + u_{it} \quad (4)$$

, where μ_i represents individual fixed effects that account for time-invariant individual-specific unobserved heterogeneities. The binary indicator $FirstConv_{it}$ equals 1 for an individual i if time t indicates a period after receiving his first conviction. We prepare our sample in a way such that we can track a person's labor market outcomes for 72 months (i.e. 6 years) before and 72 months after their first conviction. Similar to Rose (2020)'s analysis, we exclude periods between the date of the offense and the date of conviction. The vector \mathbf{Z}'_{it} incorporates time-varying individual characteristics such as indicators of age and of future convictions.

Consistent with our main specification, estimation of Equation 4 is restricted to individuals aged 25-64. Furthermore, we make sure that all individuals are non-deceased and physically present in NZ during the months in our regression analysis.¹⁹

We substantiate the above empirical evidence by comparing the first-time convicts to a randomly selected group of non-convicted individuals²⁰ in a more tra-

¹⁸The period was selected to ensure IR data availability (which starts from April 2000) and also allows us to identify periods between the date an individual committed the offense and the date they received court-ordered conviction. Offense dates can be additionally obtained from the NZ Police register which starts from July 2009.

¹⁹Similar to Rose (2020), we also estimate a dynamic model :

$$Y_{it} = a + \mu_i + \sum_{s=-72}^{72} \delta_s \cdot T_{it}^s + \mathbf{Z}'_{it} \cdot b + u_{it}$$

, where estimates of δ_s represent the dynamic effects of first conviction on labor market outcome. We prepare our sample in a way such that we can track a person's labor market outcomes for a maximum of 72 months (i.e. 6 years) before and 72 months after their first conviction. The data is binned at the two terminal time points. The month of conviction is the omitted category, such that the regression estimates of δ_s can be interpreted as effects of conviction relative to the month of first conviction. Although the results are not provided here, the dynamic effects for each labor outcome closely resemble the descriptive trends in Figure 1.

²⁰In the IDI server, we randomly selected 30,000 non-deceased males from the Census 2013

ditional DID-type setting (similar to equation 1). The key explanatory variable is given by the interaction between two dichotomous indicators of whether an individual i is a convict and whether month t represents the time after the first conviction. To construct the second indicator that equals 1 for all post-conviction months, we randomly assign artificial first conviction dates to the sample of non-convicts. Like the sample of convicts, these superficial dates are within the time range of January 2010 to December 2014. Also, we restrict our analysis to individuals aged 25-64 who were present in NZ during the months under evaluation.

Table 4 presents our regression results for the effect of first conviction on employment prospects and wages for our ‘all convictions’ and ‘non-traffic convictions’ samples. The results across all samples show that first conviction has a statistically significant negative impact on employment and wage earnings. As expected, the coefficient estimates are bigger for ‘non-traffic convictions’ sample than ‘all convictions’ sample. For instance, any first-time conviction is associated with a 2.2 percentage point decline in the probability of being in an employed job. In comparison, the magnitude of the decrease in the likelihood rises up to 3.4 percentage point for people with non-traffic convictions. Furthermore, when non-employed individuals’ are considered to have zero wage earnings rather than missing, the average decline in monthly wages and salaries for individuals with any conviction is approximately NZ\$ 125. But after excluding traffic-related convictions, the loss in monthly earnings mounts up to around NZ\$ 171. These findings are strongly supported in the traditional DID specifications, where we include the randomly selected non-convicts as the control group (see Panel B).

6.2 Clean slate regulation and labor market outcomes

After confirming the negative effects of conviction on labor market outcomes in the context of New Zealand, we turn to our main objective of assessing the impact of the clean slate regulation. We present results from our baseline DID regressions

who were never observed to be charged or convicted of any offense in the court charges data. Further, the selected birth dates ensured that the majority of this sample are aged 25-64.

in Table 5 (employment propensity) and Table 6 (log earnings). We estimate five distinct model specifications for each outcome and show results in columns (1) through (5). Model I represents the least saturated specification where we control for demographic characteristics (ethnicity) and fixed effects of age and time. In model II, we add age-specific linear time trends to control for unobserved age-specific heterogeneities evolving linearly over time. Model III is based on census-linked individuals of our relevant MoJ sample of formerly convicted males such that we could additionally control for their educational attainment. Finally, in models IV and V, we estimate DID specifications by additionally accounting for time-invariant individual fixed effects. The only difference in the two individual fixed effects regression analyses is that we additionally control for one-period lag of our dependent variable in model V. Furthermore, in the individual fixed effects regression, we include time-variant macro indicators of unemployment rate and overall conviction rate in NZ.

Regarding the binary indicator of employment, our linear probability regression estimates in Table 5 show that the clean slate scheme does not have a statistically significant effect on ex-offenders' likelihood of being employed. These results hold in non-linear (Probit) specifications as well. Although the signs on the coefficient estimates vary across specifications, the effect sizes are small enough to rule out the possibility that automatic concealment of criminal records after seven years since the last conviction has any meaningful impact on the overall employability of ex-offenders.

On the other hand, when we look at log earnings from wages and salaries, regression estimates in Table 6 indicate an approximate 2%-increase in earnings of ex-offenders following the implementation of the clean slate scheme. Our preferred specification (model II) indicates a 2.2%-increase in monthly earnings of ex-offenders. The effect is statistically significant at the 1% level. We find qualitatively similar results across all other specifications. Moreover, in the individual fixed effects specifications estimated in models 6 and 7, the effect sizes increase to 2.4 and 2.8% of monthly earnings, respectively.

The monthly earnings measure used in our analysis is the aggregate of wages and salaries earned from all jobs. As such, if a person holds multiple jobs, we would not be able to identify from Table 6 whether the increase in the earnings is driven by variation in wages and salaries from the highest-paid employment (or the ‘main employment’). Therefore, in Table 7, we repeat our Table 6 analysis by estimating the impact of the legislation on the maximum of the wages and salaries received from all jobs an individual held in a month. Naturally, the maximum monthly earnings measure equals the total monthly earnings (used as the dependent variable in Table 6) when a person had only one job. Reassuringly, the results provided in Table 7 are very similar to our findings in Table 6, indicating that the observed wage effects of the clean slate scheme are due to the increase in earnings from a person’s highest paid job.

The observed empirical findings in Table 6 and Table 7 do not considerably vary when we additionally control for employers’ industry classifications and people’s geographic locations (represented by NZ’s territorial authorities). These additional results are available upon request as we do not report the supplemental analysis for the sake of brevity.

6.2.1 Testing parallel trends

As a necessary precondition, we examine whether our DID estimates in Tables 5 and 6 are likely affected by anticipatory changes in labor market activities during pre-implementation periods leading up to the passage of the clean slate legislation. This is tested by estimating Equation 2. As indicated in Equation 2, our regressions include leads and lags between -23 through +24 with -24 (or prior months) as the omitted category. In Figure 3, we present visual plots of the regression estimates (along with confidence intervals) obtained from our event analysis for employment and log earnings. The unreported numerical estimates of the regression coefficients of the event analysis are available upon request.

We further test the overall significance of our leads (pre-implementation time dummies) and lags (post-implementation time dummies) separately and provide

the relevant statistical tests at the bottom of each graph in Figure 3. Focusing on the sum of leads, the lack of statistical relevance of the F-values signals a likely absence of significant anticipatory effects, suggesting that our analysis meets the standard parallel trends assumption. Furthermore, consistent with our baseline DID findings in Table 6, and those in Table 7, several post-implementation coefficients in regressions for log earnings are positive and statistically significant. While the joint F-value for all the post-implementation time dummies in the log earnings regression is statistically significant at the 10% level, the joint F-statistic for the binary time indicators representing a post-policy period one year after the implementation of the clean slate legislation (i.e. 12-24+ months) is statistically significant at the 5% level. This indicates that the incremental effect of the clean slate regulation on monthly earnings is likely to be realized over a longer time horizon rather than being a short-term occurrence.

6.2.2 Robustness checks using alternative specifications

We perform several tests to verify the consistency of our DID-based findings. First, we re-estimate the DID models by applying alternative criteria of selecting our comparable groups and present our results in Table 8.

To provide details on the various empirical specifications estimated in the additional analysis, it is important to remember that the control group in our baseline DID models was selected so that the elapsed time since the last sentence is bounded from below at 60 months (or 5 years). We first begin with two specifications where we adjust the lower bound of the elapsed time since the last conviction to 66 months (5.5 years) and to 72 months (6 years). Increasing the lower bound to higher time since last conviction thresholds may enhance the comparability of our control group as the individuals in that group are closer to clean-slate eligibility. We perform two additional specifications by considering treated and control groups based on offense types. Referring to the major crime classifications under which individuals in our MoJ sample were convicted, we run separate DID regressions using homogeneously chosen sample of individuals who were convicted

only under dangerous acts and those who were convicted only under traffic-related offense.²¹ Finally, as our main study period overlaps with the global economic recession, the economic downturn may have a differential impact on our treated and control groups and thereby influence the identification of the true impact of the clean slate scheme on labor market outcomes. To test this hypothesis, in our final DID specification, we restrict the end date of our study period to November 2007 (the last month before the onset of the great recession of 2008).

Overall, our results in Table 8 provide consistent findings. To be specific, while we do not find any statistically significant impact on the likelihood of employment, we observe a significant (approximate) 2%-increase in monthly earnings across most specifications, except for the sample that includes traffic offenders only. While the marginal effect on monthly earnings for traffic offenders do not vary much in magnitude, the coefficient is not statistically significant. This likely indicates that traffic offenses may not be considered as a serious crime and as such, unlike other offenses, does not possibly alter individuals' labor market prospects. Additionally, the results in Table 8 hold in the census-linked sample of formerly convicted individuals, allowing us to incorporate educational achievement as a covariate.

Furthermore, in Table 9, we present estimates from our triple difference regressions (as represented by Equation 3) for the three labor market outcomes considered in Tables 5-7 (i.e. employment, earnings, and earnings from main employer). As already mentioned, the third control group comes from a randomly chosen sample of 30,000 non-convicts. The results of these regressions are presented in Table 9 and markedly substantiate our findings from the baseline DID regressions. In other words, while we continue to find no statistically meaningful impact on the likelihood of being employed, we do find around 2%-increase in total monthly wages and salaries in the broad MoJ sample. In the census-linked sample where we control for individuals' educational attainment, the estimated

²¹In the sample of individuals who were convicted only under one offense type, the two most prevalent offences are classified under dangerous acts and traffic-related cases.

wage effect marginally increases to 2.5% of monthly earnings. Our analysis with respect to maximum monthly earnings yields qualitatively similar results.

It is worth noting that in our primary analysis, we assume non-employed individuals' monthly earnings as missing (see discussions by (Mocetti, 2007, page 7) and Jenkins (2011)). As such, it can be argued that the earnings regressions are restricted to a non-random sample of employed individuals only. However, as long as the implementation of the clean slate act is independent of unobserved influences that are correlated with individual's labor market characteristics, the identification of the true relationship of our interest would not be affected. Our event analysis partly supports this exogeneity assumption. This is because we do not find any significant differences in employment between eligible and control groups in periods prior to the enactment of the clean slate act. Nonetheless, we test the robustness of our key findings by estimating additional specifications that arguably relax selectivity of the earnings regression sample used in the baseline analysis.

First, instead of using log values (as in Table 6), we regress actual measures of total monthly earnings on the clean slate policy variable. To check if the nature of the clean slate regulation's impact on earnings varies depending on the sample specification, we first restrict our analysis to employed individuals by treating earnings of non-employed individuals as missing. Estimates from this regression can be compared to the Table 6 findings. Not surprisingly, the regression estimates in columns (1) and (2) of Table 12 are consistent with our key findings presented in Table 6. In the second sample, we allow non-employed individuals to be included in our regressions by equating their earnings to zero. The regression estimates for unconditional measures of monthly earnings in columns (3) and (4) continue to be positive and statistically significant at the conventional levels, thereby adding support to the empirical validity of our main analysis. As the mean monthly wages in the unconditional sample is relatively lower than the employed sample, we observe a smaller incremental effect on monthly earnings in the second specification.

As an additional robustness exercise, we modify our analyzed data to resemble an annual labor force survey by keeping information pertaining to the month of October only. This allows the individuals to have variation in their employment status during the other months in our study period. We provide our findings in Appendix Table A2. Once again, our results do not appreciably deviate from the key findings obtained in the baseline DID analysis.

Finally, we conduct a falsification test to see if randomly assigning a placebo treatment to convicted individuals gives us the same result as the effect of the clean slate scheme. The results of this falsification exercise are presented in Figure 4. The graphical plots of the regression coefficients show that the placebo treatment effect estimates are concentrated around zero (except for a couple of significant ones that would be accepted to occur simply by chance) in the case of both employment and earnings regressions.

6.2.3 Possible mechanisms

Following our main findings, it is important to test some of the underlying mechanisms that could explain the observed increase in earnings. For instance, upon having their criminal records concealed, individuals might look to explore better labor market prospects by switching to higher paid employment opportunities. Alternatively, as individuals with no conviction are likely to have better labor market outcomes than individuals with observable criminal records, the change in the criminal record history induced by the clean slate legislation might increase the wage bargaining power of eligible ex-offenders. Consequently, if an individual choose to remain with their current employers even after having their criminal records concealed, the firms might ‘reward’ that person by increasing their pay in return.

While our administrative data source does not allow us to look into specific details on the possible variation in employment characteristics that one might experience once their criminal records are expunged (such as job promotions or interactions with employers), we use the employer identifiers and industry classi-

fication of the IR data to investigate some of the aforementioned mechanisms. By utilizing monthly information on the highest paid employment of an individual, we estimate Equation 1 with two outcome variables. These variables represent monthly indicators for whether an employed individual changed their main employer and whether they changed their main industry with respect to their last observed employment.²² We analyze two samples for each of these outcomes: one where all individuals in our sample are included and another where only employed individuals are included. The results from this test are presented in Table 10. Our estimates are statistically indistinguishable from zero for both samples. Overall, our results support the conjecture that the observed wage hike during the post-clean slate implementation period is likely due to ex-offenders' increased bargaining capacity rather than due to a change in their employment.

6.3 Ethnic differences in employment and earnings

As several US-based studies point to potential discrimination due to BTB policies, we investigate the same in our sample. To study if clean slate act likely triggers ethnic disparity in labor market outcomes, we separately compare three prominent ethnic minority groups (Māori, Pacific Peoples, and Asians) with NZ Europeans. Basically, we separately focus on subsamples that incorporate an ethnic minority of interest and the reference group of NZ Europeans. We estimate a specification similar to the triple difference model provided by Equation (3), where the indicator $Convict_i$ is replaced by an indicator of whether an individual belongs to the NZ European ethnicity. We repeat this analysis for each ethnic minority group.

The results from our analysis are presented in Table 11. Overall, we do not find any statistically significant evidence of differences in employment propensity as well as in monthly earnings between NZ Europeans and any of the three ethnic minority groups. These findings corroborate our hypothesis that in a jurisdiction where employers can easily access people's criminal record information, reforms that permit concealment of one's criminal records are less likely to trigger statisti-

²²See Table A1 for industry distribution of individuals in our sample.

cal discrimination based on demographic attributes. However, it is also important to note that the clean slate legislation applies to a specific group of ex-offenders who, in addition to serving their court-ordered sentences, have not been involved in criminal activities for a considerable period. On the other hand, by restricting employers' right to inquire into applicants' past criminal background, public programs like the BTB policy apply to a much broader population of criminal offenders, thereby leaving more room for statistical discrimination.

7 Conclusion

Internationally, there has been a rise in rehabilitative interventions that allow automatic expungement of ex-offenders' criminal records. Our study presents policy-relevant insights into the efficacy of such regulations. To the best of our knowledge, this is the first analysis to utilize national-level administrative data to investigate labor market implications of a country's clean slate initiative. Our analysis provides compelling evidence in support of the positive impact that clean slate regulation may have on monthly earnings of employed ex-offenders, despite having no relevant impact on employment propensity.

The empirical findings provide adequate support to two specific hypotheses. First, the mandated seven-year threshold might induce too long a wait for ex-offenders to experience positive employment prospects in their desired jobs. Given that criminal past reduces labor market prospects, ex-offenders might be economically better off taking up less desirable job opportunities (e.g. in low paid sectors) rather than waiting for seven or more years to have their criminal records concealed. From the demand side, firms might also be reluctant to offer someone their preferred job if the person lacks prior experience required for the relevant profile. Secondly, despite the null effect on employment propensity, having a 'clean slate' can enhance employed ex-convicts' bargaining capacity in negotiating wages with their employers. As the administrative data used in our study do not allow a more intuitive assessment of the possible mechanisms underlying our

key findings, this study opens up an important scope for future research to further explore the plausibility of our hypotheses.

Our analysis on NZ's clean slate scheme also complements the burgeoning literature on U.S.'s BTB policies. The two alternative legislative approaches allow us to highlight some of the key differences in the intended rehabilitative outcomes as well as in the inadvertent social consequences of the existing regulations. As such, future studies could focus on exploring further conclusive evidence on the differences in the current public policies adopted to reduce socio-economic barriers that ex-criminals often face.

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Table 1: Selection of the initial MoJ sample

Selection criteria	Unique individuals
-Individuals with last recorded court charges between 1992 and 2003	296 085
-Individuals who were not deceased during the study period	275 154
-Last court charge received conviction	146 658
-Individuals with no custodial sentence, driving disqualification, or court orders for mental health treatment	61 839
-Individuals with no sexual or violence-related offence	57 915

Table 2: Crime classification of sample of interest

ANZSOC broad classification	Percent	Number of Convictions
Dangerous acts	27.58	23,547
Against justice	24.88	21,237
Traffic	11.25	9,603
Fraud, deception	9.91	8,457
Miscellaneous	7.87	6,720
Drugs	6.69	5,715
Public order	5.10	4,353
Theft	3.79	3,234
Property damage	1.64	1,401
Burglary, unlawful entry	0.68	582
Weapons	0.58	498
Robbery, extortion	0.02	12
Total convictions of 57915 individuals		85359

Notes: ANZSOC is abbreviation for Australian and New Zealand Standard Offence Classification. The individuals in our sample of interest were convicted at least once under the above-mentioned offence categories. Since an individual may have multiple convictions, the number convictions exceed the number of individuals in the sample of interest. Offences classified under ‘miscellaneous’ are either not well-defined or unknown.

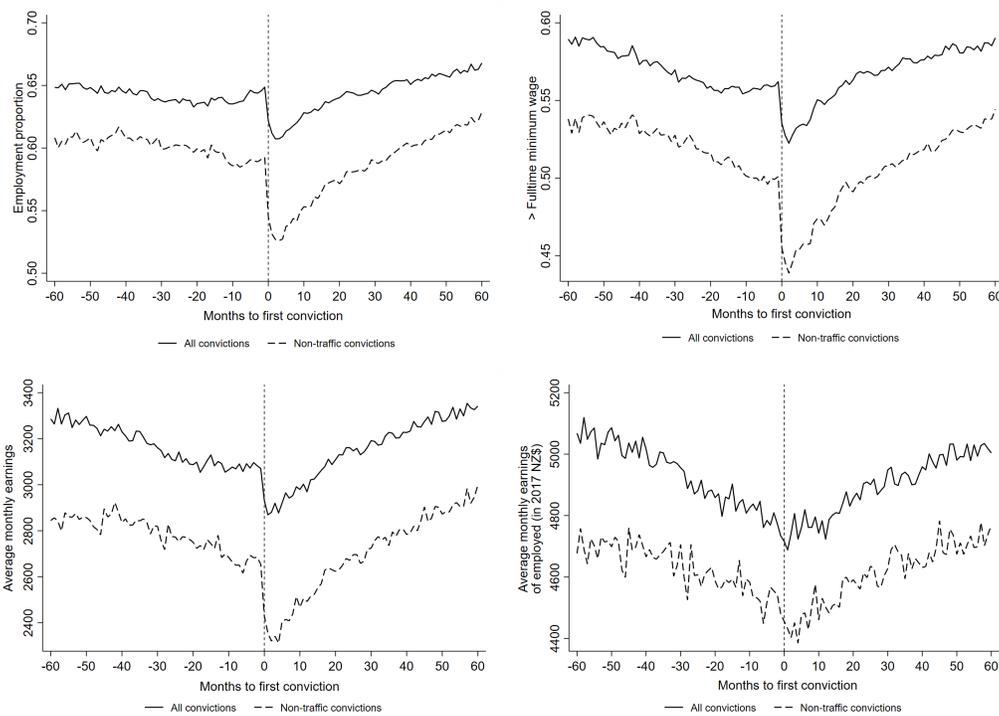
Table 3: Summary statistics of regression samples

Variable	Untreated Pre-CS		Treated Pre-CS		Non-convicts Pre-CS	
	Mean/Prop	SD	Mean/Prop	SD	Mean/Prop	SD
Aggregate of monthly earnings	4790.400	3788.210	4880.703	3851.890	5577.498	4971.492
Maximum of monthly earnings	4648.930	3734.969	4740.625	3803.108	5510.411	4896.896
Employed	0.565	0.496	0.565	0.496	0.572	0.495
European	0.629	0.483	0.636	0.481	0.757	0.429
Māori	0.078	0.268	0.077	0.267	0.060	0.237
Pacific	0.047	0.211	0.044	0.205	0.007	0.086
Asian	0.051	0.220	0.043	0.202	0.006	0.077
MELAA	0.004	0.064	0.003	0.056	0.000	0.015
Age	39.315	10.478	39.365	10.349	43.962	10.665
Certificate 1-4	0.503	0.500	0.500	0.500	0.371	0.483
Diploma	0.074	0.262	0.078	0.267	0.008	0.092
Bachelor	0.079	0.270	0.081	0.273	0.012	0.108
Post-graduate	0.034	0.181	0.035	0.183	0.282	0.450
Observations	1264860				1114521	
Unique individual	Convicts: 37731				Non-convicts: 25044	

Notes: CS: Clean slate regulation; SD: Standard Deviation; and MELAA: Middle Eastern/ Latin American/ African.

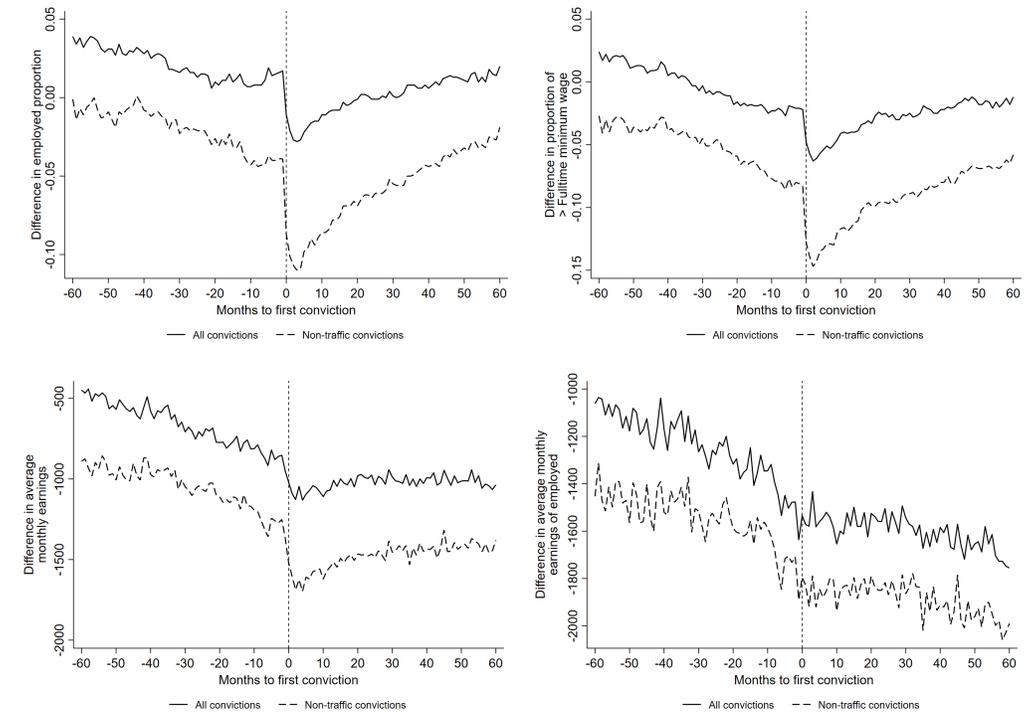
The above table presents descriptive information based on the largest regression samples used in our analysis. The estimates of average monthly earnings are based on employed individuals only as for non-employed workers, we treat the earnings information as missing in our main analysis. Since an individual may hold multiple jobs each month, “aggregate of monthly earnings” sums up monthly income from wages and salaries across all jobs, while “maximum of monthly earnings” considers the highest paid employment only. For individuals with only one job, the two measures are the same.

Figure 1: Trends in employment and earnings of convicts before and after first conviction



Notes: The top-left figure looks at employment proportion trends before and after first conviction. The graph in the top-right corner shows trends in the proportion of individuals earning more than full-time inflation-adjusted minimum wage. The bottom graphs present trends in inflation-adjusted monthly earnings. In the bottom-left figure, earnings for non-employed individuals are considered to be zero and in the right-hand side figure, the same are considered to be missing. The graphs are based on data from 60 months before and 60 months after individuals' first conviction. The data at the 60th month before and 60th after are based on 12-month average preceding and succeeding the 5-year terminal periods, respectively.

Figure 2: Trends in employment and earnings of convicts before and after first conviction relative to non-convicts



Notes: The above figure incorporates graphs that present trends in the differences in labor market outcomes between first-time convicts and non-convicts. The artificially assigned first-time conviction dates for non-convicts are randomly generated for each individual who was never observed to be convicted of a crime in the court charges data. The top-left figure looks at the relevant trends in employment proportion. The graph in the top-right corner shows trends in the proportion of individuals earning more than full-time inflation-adjusted minimum wage. The bottom graphs present trends in inflation-adjusted monthly earnings. In the bottom-left figure, earnings of non-employed individuals are considered to be zero and in the right-hand side figure, the same are considered to be missing. The graphs are based on data from 60 months before and 60 months after individuals' first conviction. The data at the 60th month before and 60th after are based on 12-month average preceding and succeeding the 5-year terminal periods, respectively.

Table 4: Effect of first ever criminal conviction on labor market outcomes

<i>Panel A - Sample of convicts</i>								
Dependent variable:	Employment		Exceed min. wage		Monthly earnings		Monthly earnings of employed	
	All	Non-traffic	All	Non-traffic	All	Non-traffic	All	Non-traffic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-conviction SM	0.642	0.600	0.570	0.521	3170.872	2776.945	4935.732	4628.022
First conviction	-0.0223*** (0.0037)	-0.0342*** (0.0061)	-0.0202*** (0.0037)	-0.0280*** (0.0060)	-125.0990*** (22.4269)	-170.9755*** (33.4802)	-59.2511*** (22.8665)	-139.0814*** (34.5167)
Observations	1716306	743370	1716306	743370	1716306	743370	1109499	443607
No. of individuals	22986	9954	22986	9954	22986	9954	22020	8439
<i>Panel B - Sample of convicts & non-convicts</i>								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-conviction SM	0.627	0.616	0.572	0.563	3606.276	3640.177	5748.238	5906.510
First conviction	-0.0208*** (0.0021)	-0.0302*** (0.0050)	-0.0200*** (0.0035)	-0.0284*** (0.0050)	-305.7476*** (26.6396)	-403.3352*** (33.6605)	-358.5043*** (25.0373)	-492.9671*** (32.6937)
Observations	4373427	3400491	4373427	3400491	4373427	3400491	2786529	2120634
No. of individuals	50805	37776	50805	37776	50805	37776	41532	29952

Notes: SM = Sample mean; FE = Fixed effect. For each labor market indicator, we estimate two specifications- one which looks at any type of convictions (defined as “All”) and the other considers only non-traffic-related convictions (defined as “Non-traffic”).

All regressions in the above table control for individual fixed effect, age fixed effect, time (in month) fixed effects and binary indicator of future convictions. Regression models in columns (5) and (6) treat earnings of non-employed individuals as zero. Regression models in columns (7) and (8) treat earnings of non-employed individuals as missing. Panel A analysis has been performed using a sample of non-deceased males aged 25-64 who had their first formal conviction sometime within the five-year period from January 2010 to December 2014. The empirical specification tracks individuals’ labor market characteristics 6 years (or 72 months) before and after first conviction. Panel B analysis compares convicts to a randomly selected sample of non-convicts. For non-convicts, the first conviction dates are randomly assigned between January 2010 and December 2014. In both the panels, the robust standard errors (reported in parentheses) are clustered on the individual-level. All the regressions control for individual, age, and time fixed effects along with time-varying indicator of future convictions following individuals’ first conviction. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effect of clean slate regulation on employment

Dependent variable: Binary employment indicator					
Model specification	I	II	III	IV	V
Pre-act untreated proportion:	0.565	0.565	0.636	0.565	0.566
Clean slate act (Post*Treat)	0.0025 (0.0053)	0.0025 (0.0053)	-0.0033 (0.0062)	-0.0026 (0.0027)	-0.0000 (0.0010)
Treat	-0.0027 (0.0031)	-0.0026 (0.0031)	0.0076** (0.0037)	0.0013 (0.0018)	0.0004 (0.0007)
Post	0.0635*** (0.0086)	0.1729*** (0.0498)	0.1740*** (0.0574)	0.0012 (0.0030)	0.0003 (0.0011)
Observations_{it}	1,264,860	1,264,860	883,101	1,264,860	1,233,879
Unique individual_i				37,731	37,653
Demographic information	Yes	Yes	Yes	-	-
Time fixed effect	Yes	Yes	Yes	Yes	Yes
Age fixed effect	Yes	Yes	Yes	Yes	Yes
Age-specific linear trends	-	Yes	Yes	Yes	Yes
Individual fixed effect	-	-	-	Yes	Yes
Macro indicator	-	-	-	Yes	Yes
Education characteristics	-	-	Yes	-	-
Lagged employment	-	-	-	-	Yes

Notes: The unit of analysis in the above linear probability regression models is at the individual level (i) observed for each month (t) in the period between January 2000 and December 2009. Robust standard errors are clustered on individuals and are presented in parentheses. Demographic indicator includes ethnicity. Owing to data availability issues, the regression model (Model III) that additionally controls for an individual's education is restricted to the relevant sample of ex-convicts who were observed in the Census 2013 data. In the individual fixed effect regressions (Models IV and V), we additionally control for time-variant economy-wide indicators including unemployment rate and overall conviction rate. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Effect of clean slate regulation on monthly earnings

Dependent variable: Log of total monthly earnings					
Model specification	I	II	III	IV	V
Pre-act untreated earnings:	4791.40	4791.40	4850.96	4791.40	4870.54
Clean slate act (Post*Treat)	0.0211*** (0.0081)	0.0218*** (0.0081)	0.0207** (0.0090)	0.0241*** (0.0044)	0.0276*** (0.0031)
Treat	0.0097* (0.0051)	0.0093* (0.0051)	0.0045 (0.0056)	-0.0110*** (0.0032)	-0.0106*** (0.0022)
Post	0.3189*** (0.0160)	0.4658*** (0.1556)	0.4204*** (0.1611)	-0.0111** (0.0048)	-0.0115** (0.0034)
Observations_{it}	727,827	727,827	577,311	727,827	691,497
Unique individual_i				26,460	25,746
Demographic information	Yes	Yes	Yes	-	-
Time fixed effect	Yes	Yes	Yes	Yes	Yes
Age fixed effect	Yes	Yes	Yes	Yes	Yes
Age-specific linear trends	-	Yes	Yes	Yes	Yes
Individual fixed effect	-	-	-	Yes	Yes
Macro indicator	-	-	-	Yes	Yes
Education characteristics	-	-	Yes	-	-
Lagged log earnings	-	-	-	-	Yes

Notes: The unit of analysis in the above linear regression models is at the individual level (i) observed for each month (t) in the period between January 2000 and December 2009. Earnings of non-employed individuals are treated as missing. Robust standard errors are clustered on individuals and are presented in parentheses. Demographic indicator includes ethnicity. Owing to data availability issues, the regression model (Model III) that additionally controls for an individual's education is restricted to the relevant sample of ex-convicts who were observed in the Census 2013 data. In the individual fixed effect regressions (Models IV and V), we additionally control for time-variant economy-wide indicators including unemployment rate and overall conviction rate. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Effect of clean slate regulation on maximum of monthly earnings

Dependent variable: Log of maximum of monthly earnings					
Model specification	I	II	III	IV	V
Pre-act untreated earnings:	4648.93	4648.93	4714.29	4648.93	4726.19
Clean slate act (Post*Treat)	0.0214** (0.0088)	0.0222** (0.0088)	0.0208** (0.0098)	0.0271*** (0.0052)	0.0299*** (0.0039)
Treat	0.0103* (0.0055)	0.0098* (0.0056)	0.0050 (0.0061)	-0.0136*** (0.0038)	-0.0121*** (0.0029)
Post	0.3209*** (0.0175)	0.3656** (0.1617)	0.3311** (0.1662)	-0.0193*** (0.0056)	-0.0174*** (0.0043)
Observations_{it}	727,827	727,827	577,311	727,827	691,497
Unique individual_i				26,460	25,746
Demographic information	Yes	Yes	Yes	-	-
Time fixed effect	Yes	Yes	Yes	Yes	Yes
Age fixed effect	Yes	Yes	Yes	Yes	Yes
Age-specific linear trends	-	Yes	Yes	Yes	Yes
Individual fixed effect	-	-	-	Yes	Yes
Macro indicator	-	-	-	Yes	Yes
Education characteristics	-	-	Yes	-	-
Lagged log max. earnings	-	-	-	-	Yes

Notes: The unit of analysis in the above linear regression models is at the individual level (i) observed for each month (t) in the period between January 2000 and December 2009. Earnings of non-employed individuals are treated as missing. Robust standard errors are clustered on individuals and are presented in parentheses. Demographic indicator includes ethnicity. Owing to data availability issues, the regression model (Model III) that additionally controls for an individual's education is restricted to the relevant sample of ex-convicts who were observed in the Census 2013 data. In the individual fixed effect regressions (Models IV and V), we additionally control for time-variant economy-wide indicators including unemployment rate and overall conviction rate. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 3: Event studies for the effect of Clean Slate on labor market outcomes

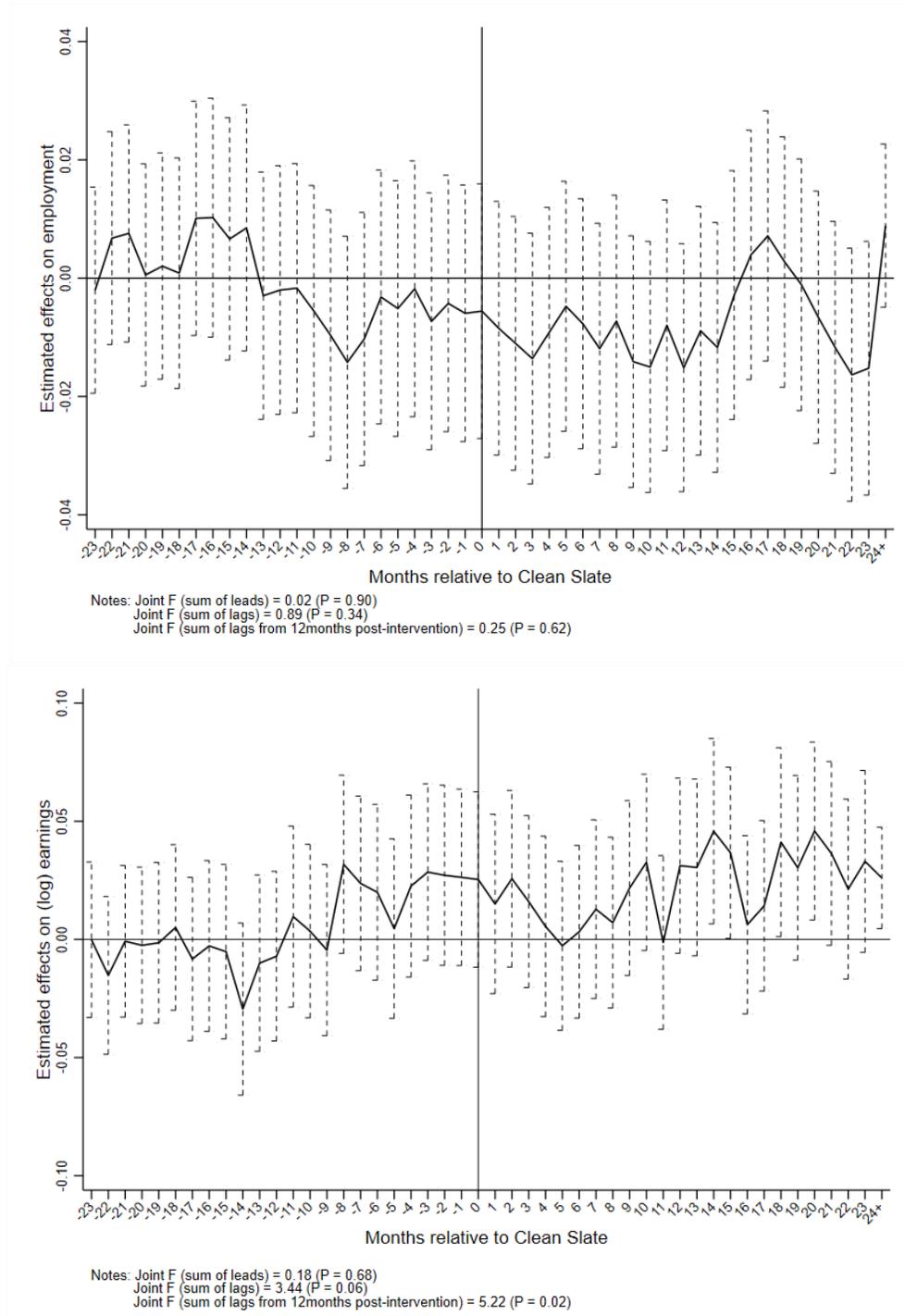


Table 8: Sensitivity analyses using alternative samples

Specification:	Effect on employment				
	Time to LC ≥ 66 months (1)	Time to LC ≥ 72 months (2)	Traffic offence only (3)	Dangerous acts only (4)	Removed GFC years (5)
Clean slate act	0.0008 (0.0051)	-0.0010 (0.0048)	0.0166 (0.0139)	-0.0062 (0.0081)	-0.0059 (0.0065)
Treat	-0.0005 (0.0030)	0.0014 (0.0029)	0.0032 (0.0080)	-0.0023 (0.0048)	-0.0026 (0.0031)
Post	0.1750*** (0.0520)	0.1660*** (0.0545)	0.0285 (0.1300)	0.1850*** (0.0698)	0.1780*** (0.0541)
Observations	1,140,060	1,002,027	184,119	532,977	1,072,971
	Effect on log earnings				
	Clean slate act	0.0211*** (0.0078)	0.0184** (0.0076)	0.0298 (0.0216)	0.0248** (0.0119)
Treat	0.0069 (0.0050)	0.0061 (0.0048)	-0.0081 (0.0132)	0.0098 (0.0072)	0.0095* (0.0051)
Post	0.4560*** (0.1690)	0.4150** (0.1850)	0.3080 (0.4300)	0.4250** (0.2080)	0.1690 (0.1890)
Observations	655,875	576,657	100,662	336,660	616,113

Notes: Time to LC - Months since last conviction; GFC - Global Financial Crisis (2007 December-2009 December).

All regression models control for ethnicity, time and age fixed effects, age-specific linear time trends, and ethnicity. Our findings do not qualitatively vary when we apply the same specifications on census-linked sample that additionally allows us to include educational characteristics as covariates. The census-linked sample results are available upon request. Robust standard errors are clustered on the individual level. In the model (column 5) where we try to remove the potential effects of the GFC, the analysis is restricted to the study period January 2000-November 2007. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9: Triple difference estimates using randomly selected non-convicted individuals

Sample specification:	Employed		Log monthly earnings		Log max. earnings	
	Broad sample (1)	Census-linked (2)	Broad sample (3)	Census-linked (4)	Broad sample (5)	Census-linked (6)
DDD (Post*Treat*Convicted)	-0.0031 (0.0066)	-0.0071 (0.0077)	0.0214** (0.0108)	0.0251** (0.0117)	0.0220* (0.0114)	0.0257** (0.0123)
Treat*Post	0.0051 (0.0041)	0.0038 (0.0047)	-0.0013 (0.0073)	-0.0064 (0.0077)	-0.0012 (0.0074)	-0.0064 (0.0078)
Convicted*Treat	0.0121* (0.0066)	0.0247*** (0.0075)	0.0120 (0.0105)	0.0077 (0.0113)	0.0127 (0.0109)	0.0080 (0.0117)
Convicted*Post	0.0145** (0.0060)	0.0089 (0.0069)	-0.0393*** (0.0094)	-0.0383*** (0.0103)	-0.0347*** (0.0100)	-0.0340*** (0.0110)
Treat	-0.0143** (0.0059)	-0.0169** (0.0066)	-0.0027 (0.0092)	-0.0017 (0.0099)	-0.0027 (0.0094)	-0.0016 (0.0101)
Post	0.1666*** (0.0226)	0.1775*** (0.0272)	0.5578*** (0.0758)	0.5623*** (0.0797)	0.5355*** (0.0773)	0.5410*** (0.0811)
Convicted	-0.0440*** (0.0054)	-0.0406*** (0.0065)	-0.0963*** (0.0084)	-0.0943*** (0.0097)	-0.1413*** (0.0088)	-0.1360*** (0.0102)
Observations	3,511,458	2,638,035	2,037,258	1,713,165	2,037,258	1,713,168
Demographic	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Age-specific linear trends	Yes	Yes	Yes	Yes	Yes	Yes
Education characteristics	-	Yes	-	Yes	-	Yes

Notes: Robust standard errors are clustered on the individual-level. In regression models based on 'Broad sample', we control for age and time fixed effect, ethnicity, and age-specific linear time trends. In specifications based on 'Census-linked' sample only, we additionally control for education characteristics. *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Effect of clean slate act on the likelihood of changing main employer or industry

Labor market indicator: Sample specification	Changed employer		Changed industry	
	Overall (1)	Employed (2)	Broad (3)	Employed (4)
Clean slate act	0.0002 (0.0011)	0.0003 (0.0019)	0.0015 (0.0015)	0.0021 (0.0024)
Treat	-0.0011 (0.0007)	-0.0014 (0.0012)	-0.0016* (0.0009)	-0.0022 (0.0016)
Post	0.0523*** (0.0079)	0.0998*** (0.0225)	0.0753*** (0.0121)	0.1154*** (0.0339)
Observations	1,264,860	727,827	1,264,860	727,827

Notes: Robust standard errors are clustered on the individual level. In regression models based on ‘Broad sample’, we control for age and time fixed effect, ethnicity, and age-specific linear time trends. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Ethnic difference in employment and earnings

Ethnic combination	Employment			Log of monthly earnings		
	Māori-Erpn	Pacific-Erpn	Asian-Erpn	Māori-Erpn	Pacific-Erpn	Asian-Erpn
Pre-policy untreated S.M.	0.628 (1)	0.628 (2)	0.611 (3)	4859.023 (4)	4897.426 (5)	4942.559 (6)
Clean Slate*European	-0.0044 (0.0122)	0.0163 (0.0147)	0.0098 (0.0137)	-0.0041 (0.0167)	0.0267 (0.0176)	-0.0281 (0.0246)
Observations _{it}	875,514	835,758	846,303	560,193	534,828	527,415
Individuals _i	26223	25107	25686	20256	19308	19326

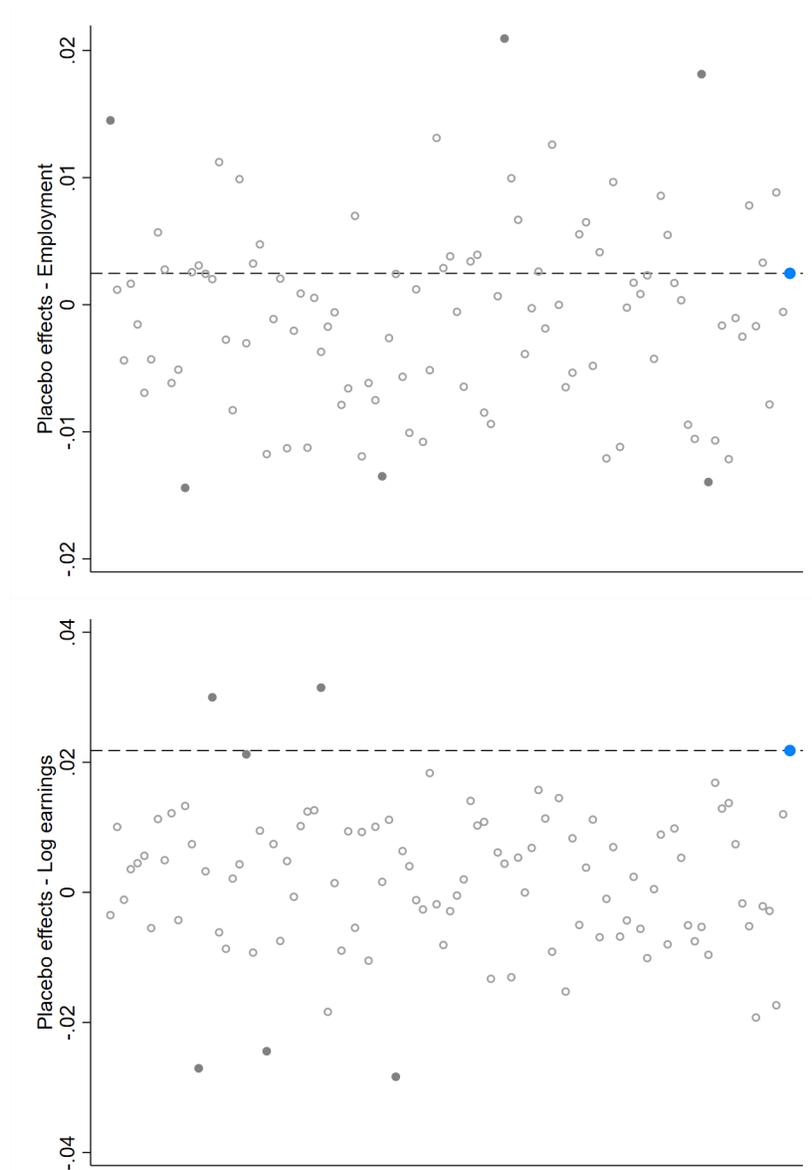
Notes: Erpn: NZ European. All regression models incorporate age and time fixed effects and age-specific linear trends. Robust standard errors are clustered on the individual level. *** p<0.01, ** p<0.05, * p<0.1.

Table 12: Difference-in-differences estimation of the impact of clean slate regulation on actual measures of monthly earnings

	Conditional earnings		Unconditional earnings	
Pre-policy SM	4840.041	4840.041	2734.367	2734.367
Pre-policy SM for untreated	4791.395	4791.395	2708.485	2708.485
	(1)	(2)	(3)	(4)
Clean slate act	87.837**	90.841**	69.056*	70.324**
	(42.062)	(42.369)	(36.220)	(36.421)
Treat	50.939**	49.305**	17.876	17.336
	(23.954)	(24.050)	(20.485)	(20.518)
Post	1366.522***	1769.171***	1071.022***	1510.279***
	(87.931)	(718.3625)	(66.814)	(322.421)
Observations	727,827	727,827	1,264,860	1264860
Demographic characteristics	Yes	Yes	Yes	Yes
Age fixed effect	Yes	Yes	Yes	Yes
Time fixed effect	Yes	Yes	Yes	Yes
Age-specific linear trends	-	Yes	-	Yes

Notes: In models (see columns 1 and 2) labelled ‘Conditional earnings’, the earnings from wages and salaries for non-employed individuals are treated as missing. Therefore, the model only considers earnings of employed individuals. In contrast, the specifications (see columns 3 and 4) labelled ‘Unconditional earnings’, monthly earnings from wages and salaries for non-employed individuals are treated as zero. Robust standard errors are clustered on the individual-level. *** p<0.01, ** p<0.05, * p<0.1.

Figure 4: Falsification test for the effect of clean slate act on labor market outcomes of convicts



Notes: The above figure is generated based on 100 simulations. Results are robust to 200 and 500 simulations as well. The blue dot represents true estimate (see Model II of Table 5 and Table 6, respectively). The filled gray circles represent statistically significant coefficient estimates (at least at the 10 percent level). The hollow circles represent statistically insignificant estimates.

Disclaimer

The results in this paper are not official statistics, they have been created for research purposes from the Integrated Data Infrastructure (IDI), managed by Statistics New Zealand. The opinions, findings, recommendations, and conclusions expressed in this paper are those of the authors, not Statistics NZ.

The results are based in part on tax data supplied by Inland Revenue to Statistics NZ under the Tax Administration Act 1994. This tax data must be used only for statistical purposes, and no individual information may be published or disclosed in any other form, or provided to Inland Revenue for administrative or regulatory purposes. Any person who has had access to the unit record data has certified that they have been shown, have read, and have understood section 81 of the Tax Administration Act 1994, which relates to secrecy. Any discussion of data limitations or weaknesses is in the context of using the IDI for statistical purposes, and is not related to the data's ability to support Inland Revenue's core operational requirements.

Access to the anonymised data used in this study was provided by Statistics NZ in accordance with security and confidentiality provisions of the Statistics Act 1975. Only people authorised by the Statistics Act 1975 are allowed to see data about a particular person, household, business, or organisation, and the results in this paper have been confidentialised to protect these groups from identification. Careful consideration has been given to the privacy, security, and confidentiality issues associated with using administrative and survey data in the IDI.

Further detail can be found in the Privacy impact assessment for the Integrated Data Infrastructure available from www.stats.govt.nz.

Data availability statement

The empirical analysis utilizes data from a large research database known as Integrated Data Infrastructure (IDI) that holds micro-data about individuals, households, and business enterprises. The data disclaimer is provided in the previous page. Access to the IDI data can be obtained through applications to Statistics New Zealand but is strictly restricted to users who are physically based in New Zealand and can only access the information at approved facilities (the Data Lab) located in New Zealand. Given these access restrictions, while it is not in our control to make the data available for public use and for replication purposes, we would be able to provide all our codes and programs used for empirical analysis in the study, conditional on the acceptance of the study for publication. - For detailed procedure on accessing the IDI, see <https://www.stats.govt.nz/integrated-data/apply-to-use-microdata-for-research/>.

Table A1: Industry classification of samples of employed individuals

Industry	Ineligible convicts	Eligible convicts	Never convicted
Agriculture	6.62	6.42	5.63
Mining	0.44	0.44	0.55
Manufacturing	22.84	23.32	20.69
Electricity, Gas, Water & Waste services	1.07	0.99	0.90
Construction	11.86	11.71	9.35
Wholesale trade	7.91	8.12	7.77
Retail trade	7.81	7.58	7.31
Accommodation & Food services	3.21	2.99	2.01
Transport, Postal & Warehousing	10.31	9.19	6.73
Information Media & Telecommunication	1.78	1.98	2.19
Financial & Insurance Services	1.94	2.09	3.00
Rental Hiring & Real Estate Services	1.36	1.32	1.38
Professional, Scientific & Technical	4.67	4.99	6.72
Administrative and Support services	3.55	3.18	2.48
Public administration & Safety	4.36	4.78	8.60
Education & Training	2.98	3.17	6.20
Health Care and social assistance	2.41	2.56	3.44
Arts & Recreation	1.08	1.08	1.46
Other services	3.60	3.91	3.33
Unknown	0.22	0.19	0.24
Observation	326226	401343	1309428
Unique individuals	20910	22956	18558

Table A2: Effect of clean slate regulation on labor market outcomes observed in October of 2000-2009

	Employed		Monthly earnings	
	(1)	(2)	(3)	(4)
Pre-clean slate sample mean of untreated	0.570	0.640	4007.81	4064.31
Clean Slate	0.0071 (0.0062)	-0.0020 (0.0073)	0.0244** (0.0118)	0.0301** (0.0129)
Treat	-0.0059* (0.0036)	0.0066 (0.0042)	0.0035 (0.0073)	-0.0028 (0.0080)
Post	0.1079** (0.0522)	0.1005* (0.0595)	0.3079 (0.2288)	0.2633 (0.2415)
Observations	102,801	71,616	59,082	46,812
Demographic Indicator	Yes	Yes	Yes	Yes
Time fixed effect	Yes	Yes	Yes	Yes
Age fixed effect	Yes	Yes	Yes	Yes
Age-specific linear time trends	Yes	Yes	Yes	Yes
Education indicator	-	Yes	-	Yes

Notes: Robust standard errors are clustered on the individuals and are presented in parentheses. The above analysis is based on IR data as observed in the month of October in the years 2000-2009. This additional analysis is performed to resemble commonly used large-scale annual labor force surveys that incorporate data for a specific month(s) of the surveyed years (like the National Longitudinal Survey of Youth or the British Household Panel Survey). The individuals in the regression sample do not vary from the main analysis. We select the month of October since it is less likely to be affected by seasonal variations or macroeconomic NZ labor policies such as changes in minimum wages. The survey-based design of the data relaxes the requirement of individuals to be continuously employed in our analysis of monthly earnings unlike our main regressions. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A3: Descriptive information on court-imposed fines and reparations in NZ

Year Imposed	Profiles with court-imposed fines or reparations	Profiles that satisfied court-imposed fines or reparations	Percentage
	<i>a</i>	<i>b</i>	$(b/a) * 100$
2000	60011	59338	99%
2001	50846	50203	99%
2002	64878	64576	100%
2003	67156	66760	99%
2004	69708	69049	99%
2005	65984	65403	99%
2006	66747	66057	99%
2007	71961	71054	99%
2008	73265	72224	99%
2009	74835	73396	98%
2010	87682	86161	98%
2011	93240	91700	98%
2012	89086	87012	98%
2013	82955	80506	97%
2014	76129	73053	96%
2015	69760	65927	95%
2016	70196	65108	93%
2017	70573	63646	90%

Notes: The above information was released by the Ministry of Justice pursuant to our request submitted under the Official Information Act of 1992. A profile is classed as ‘satisfied’ if as of November 11, 2020, there is no balance outstanding on any court-imposed fine(s) and/or reparation that was imposed in that given year, regardless of how or when they were satisfied (i.e. paid or remitted). It is important to note, that information on the timing of the payment along with payment method as required by the court (e.g., whether it is a one-time lumpsum fee or a recurring payment over a certain period) are missing. A fine or a reparation could be satisfied either by means of payments (including voluntary or enforcement arrangements such as automatic payment system or deductions from earnings, respectively), or remission or a combination of both.