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COSTLY MISDEMEANORS: LABOR MARKET EFFECTS OF LOW-LEVEL DRINK-DRIVING VIOLATIONS

Kabir Dasgupta, Marco Francesconi, Jonathan James, Linda Kirkpatrick, and Alexander Plum

Costly Misdemeanors: Labor Market Effects of Low-Level Drink-Driving Violations*

Kabir Dasgupta — Federal Reserve Board
Marco Francesconi — University of Essex
Jonathan James — University of Bath
Linda Kirkpatrick — University of British Columbia
Alexander Plum — Auckland University of Technology

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Abstract

This paper estimates the causal effect of a low-level drink-driving conviction on labor market outcomes. Using linked administrative data and variation in the timing of first convictions, we implement a staggered difference-in-differences design comparing individuals with those convicted later. Employment falls by 6–7 percentage points following conviction, while monthly earnings decline by around 2 percent. Employment losses are concentrated among those seeking new employment, rather than those in existing jobs. Losses are larger in industries where employers are more likely to screen applicants for criminal records, pointing to employer screening as a key driver of the results.

JEL Classification: K42, J24, J31, J15

Keywords: Drink-driving convictions, Criminal records and employment, Labor market effects of crime, Administrative data and causal inference

*Dasgupta, Federal Reserve Board: kabir.dasgupta@frb.gov; Francesconi, University of Essex: mfranc@essex.ac.uk; James: University of Bath: jj412@bath.ac.uk; Kirkpatrick, University of British Columbia: linda.kirkpatrick@ubc.ca; Plum, Auckland University of Technology: alexander.plum@aut.ac.nz. We thank John Abowd, Joe Altonji, Kitt Carpenter, Kevin Lang, Giovanni Mastrobuoni, Thomas Siedler, and participants in several seminars, workshops, and conferences for insightful comments on earlier versions of the paper. We are grateful to Statistics New Zealand for providing the data. The results and opinions expressed in this study reflect the views of the authors and should not be attributed to Statistics New Zealand, the Federal Reserve Board or the Federal Reserve System. All errors are our own. *Stats NZ IDI Disclaimer:* These results are not official statistics. They have been created for research purposes from the Integrated Data Infrastructure (IDI) which is carefully managed by Stats NZ. For more information about the IDI please visit https://www.stats.govt.nz/integrated-data/.

1 Introduction

Many contacts with the criminal justice system involve relatively low-level offenses, most of which do not lead to incarceration.[1] Yet even a low-level conviction can leave a criminal record with lasting consequences long after the sanction itself has been served, particularly when employers use criminal history information in recruitment. This raises the core question of this paper: can a relatively minor conviction generate persistent labor market penalties even in the absence of imprisonment?

This question belongs to a longstanding literature on the economic consequences of crime and criminal justice involvement (Becker, 1968; Freeman, 1999; Draca and Machin, 2015; Chalfin and McCrary, 2017). More recent evidence shows that criminal records can create substantial barriers to employment (Pager, 2003; Uggen et al., 2014; Agan and Starr, 2017, 2018; Hjalmarsson et al., 2024; Rose and Shem-Tov, 2025; Hjalmarsson et al., 2025), while avoiding prosecution for a misdemeanor can substantially reduce subsequent criminal justice involvement, particularly among individuals without prior records (Agan et al., 2023). What remains less well understood is whether acquiring a relatively minor conviction causally affects employment and earnings, and through which labor market margins any such effects arise.

Drink-driving provides an ideal setting in which to answer these questions. Drink-driving offenses span a wide range of severity, from violations involving relatively low alcohol readings and no accident to cases resulting in serious injury or death. We deliberately focus on the former: first-time offenders with alcohol readings at the lower end of the conviction distribution. Breath and blood alcohol concentrations provide an objective measure of offense severity, allowing us to study a relatively homogeneous group of offenders rather than pool individuals convicted of substantially different conduct (Sherman et al., 2016).[2]

Moreover, low-level drink-driving convictions generally result in fines and temporary license suspension, rather than incarceration. The absence of incarceration substantially limits mechanical incapacitation and the direct loss of labor market experience associated with time spent in custody. License suspension itself, however, provides an important competing mechanism: restricted mobility may increase job-search costs or limit access

[1] Low-level offenses account for a large share of criminal justice contact in common-law jurisdictions. In the United States, misdemeanors constitute the vast majority of criminal filings in state courts, and population-level estimates suggest that roughly one in seven Americans has been convicted of a misdemeanor (e.g., Stevenson and Mayson, 2018; Craigie et al., 2020).

[2] Individuals may nevertheless be uncertain about their precise alcohol concentration at the time of driving (Love and Larue, 2025). Most economic research on drink-driving has focused on deterrence and road-safety outcomes (Dee, 2001; Eisenberg, 2003; Albalate, 2008; Francesconi and James, 2021; Portillo et al., 2024; Ramasco, 2026). A smaller literature examines its economic consequences and those of license suspension (Wells-Parker and Cosby, 1988; Knoebel and Ross, 1997; Oksanen et al., 2015). Recent work also exploits objectively measured alcohol content to study judicial treatment of first-time drink-driving defendants, documenting substantial variation in judicial outcomes even among observably similar cases (Denning et al., 2025).

to jobs requiring driving, potentially generating employment effects that persist beyond the suspension period. This setting therefore allows us to examine the consequences of a low-level conviction while distinguishing, to some extent, between the criminal record itself and other consequences of the sanction.

New Zealand provides a particularly useful empirical environment for examining these questions. We exploit an unusually rich linkage of population-wide administrative records that combines detailed information on criminal convictions and sanctions with monthly employment, earnings, and employer histories. Ministry of Justice records identify offense and conviction dates, measured breath or blood alcohol concentration, and the sanctions imposed, allowing us to construct a precisely defined sample of comparable first-time offenders. Inland Revenue tax records then follow these individuals month by month and identify both their earnings and employers. Detailed industry classifications further allow us to distinguish continuing employment relationships from the formation of new matches within and across industries. The data therefore allow us not only to estimate the labor market consequences of acquiring a low-level conviction, but also to locate *where* within workers' subsequent employment trajectories those consequences emerge. Drink-driving convictions are also relatively common in New Zealand: linking the 2023 New Zealand Census to court records, we find that 15.3 percent of men and 5.8 percent of women aged 30–55 have at least one such conviction.[3]

The central empirical challenge is that criminal justice involvement is non-random. Individuals who receive convictions differ from the general population in observed and unobserved characteristics that may independently affect labor market outcomes. Comparisons between the two groups would therefore capture both the causal impact of a criminal record and pre-existing differences in their characteristics. In addition, adverse shocks to health, family circumstances, or economic conditions may affect both drink-driving and employment.[4]

Our main design does not use individuals with no criminal justice involvement as the primary counterfactual. Instead, we exploit variation in the timing of first conviction within a sample of individuals who are all eventually convicted of a similar low-level drink-driving offense. Using the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator, we compare individuals after conviction with not-yet-convicted individuals who will eventually be convicted themselves. The design differences out time-invariant differences across eventual offenders, and does not require conviction timing to be random. Rather, identification requires that earlier- and later-convicted individuals would have followed

[3]Among individuals with a drink-driving conviction, 25.1 percent of men and 43.9 percent of women have only one recorded conviction, indicating that a substantial share have relatively limited drink-driving histories.

[4]More generally, labor market conditions may themselves affect criminal or risky behavior and alcohol consumption, creating a potential reverse-causality channel (MacDonald and Shields, 2001; Auld, 2005; Carpenter, 2005; Bell et al., 2018; Allouard et al., 2026). A related literature examines the association between alcohol consumption and labor market outcomes (e.g., Berger and Leigh, 1988; Zarkin et al., 1998; MacDonald and Shields, 2004).

similar labor market trajectories absent conviction, and that they do not anticipate and respond to conviction in advance. We find no evidence of differential pre-conviction trends in employment or earnings, or of differential deterioration in health-related outcomes before conviction.

A potential concern with the main design is that the not-yet-convicted comparison group may still differ from earlier-convicted individuals in ways related to the timing of conviction itself. We therefore complement this design with an exact-matching event study that uses a different counterfactual population. Convicted individuals are matched to observationally similar New Zealanders with no court conviction during the sample period, based on sex, ethnicity, birth cohort, prior employment, and prior earnings conditional on employment. The two designs therefore rely on distinct comparison groups: future-convicted individuals in the baseline design and observably similar unconvicted individuals in the matched design. The similarity of the resulting estimates makes it less likely that our findings are driven by the particular counterfactual population used in the baseline design.

We find that a drink-driving conviction has substantial and persistent labor market effects. Over the two years following conviction, employment declines by 6.8 percentage points, or approximately 10 percent relative to its pre-conviction level. The decline is driven primarily by an 8.2 percentage-point reduction in entry into employment; employment exits increase by a considerably smaller 2.2 percentage points. Monthly labor income falls by approximately NZ$90, or 2.3 percent. The employment effects persist well beyond the typical six-month license suspension.

The linked employer–employee data allow us to locate these employment losses within workers' subsequent labor market transitions. We find that the losses are concentrated in entry from non-employment and moves to employers in different industries, with little change in retention by the same employer or mobility within the same narrowly defined industry. Thus, the employment penalty arises primarily when workers must form new employment relationships rather than through the dissolution of incumbent matches.

The concentration of employment losses in new-match formation narrows the set of potential explanations but does not, by itself, identify the underlying mechanism. Our leading interpretation is employer screening. Existing employers have accumulated direct information about worker performance, while prospective employers must evaluate applicants using more limited information and may therefore place greater weight on observable signals such as criminal records. This interpretation is consistent with recent evidence that criminal records affect sorting across employers (Hjalmarsson et al., 2025) and that employer demand for workers with criminal records increases when firms receive additional information about worker performance or protection against perceived hiring risks (Cullen et al., 2023). Other mechanisms may nevertheless contribute. Temporary loss of driving privileges may increase job-search costs or restrict the set of accessible jobs, while other changes in worker search behavior or labor market attachment could also reduce the

formation of new matches.[5]

Several additional findings help distinguish among these explanations. Employment losses persist well beyond the period of license suspension and are *not* concentrated in industries where driving is more important for work. Persistence alone does not rule out a role for license loss, since an initial reduction in job finding could have longer-lasting consequences through non-employment and human-capital depreciation. By contrast, the losses associated with forming new matches are substantially larger in industries where employers are more likely to screen applicants for criminal records. Taken together, these results favor an important role for employer screening, while leaving scope for search, mobility, and other worker-side channels to contribute to the employment response.

We formalize the employer-screening interpretation in a simple model of screening and learning. A conviction acts as a negative signal when an outside applicant is evaluated, reducing expected match value and the probability that a new match is formed. Within an existing match, firms accumulate direct information about worker performance, reducing the informational content of the criminal record. This framework rationalizes the empirical combination of large entry effects, much smaller exit effects, and comparatively modest earnings responses among employed workers. We emphasize that it is intended to discipline the interpretation of the screening channel rather than to rule out other mechanisms that may also contribute to the estimated effects.

Most causal evidence on the labor market consequences of criminal justice contact has focused on incarceration and pre-trial detention, with estimates ranging from limited post-release effects to substantial and persistent employment and earnings losses (Kling, 2006; Mueller-Smith, 2015; Dobbie et al., 2018; Grau et al., 2023; Garin et al., 2025).[6] Our setting examines a different margin. Because the offenses we study do not result in incarceration, the estimated effects cannot mechanically arise from time spent out of the labor market while imprisoned. Instead, we study the labor market consequences of acquiring a comparatively minor conviction carrying a non-custodial sanction.

Our analysis also complements research showing that employers respond to criminal histories and that policies regulating access to those histories can alter employment outcomes (Pager, 2003; Agan and Starr, 2018).[7] When information about criminal records is unavailable, employers may substitute toward other signals, while institutions such as occupational licensing can reduce asymmetric information about applicants' criminal his-

[5]Criminal justice contact may affect employment through workers' labor supply, job search, or labor market attachment (Visher et al., 2011; Sugie, 2018; Smith and Broege, 2020; Ramakers et al., 2025).

[6]An important distinction across this literature concerns the roles of incapacitation and prior labor market attachment. Recent administrative evidence suggests that a substantial share of earnings losses surrounding incarceration reflects time incarcerated and weak pre-existing labor market attachment rather than persistent post-release scarring (Garin et al., 2025). Many quasi-experimental studies in this literature also identify effects for defendants whose treatment status responds to judicial severity (e.g., Bhuller et al., 2025).

[7]Evidence from "Ban-the-Box" reforms is heterogeneous across settings and populations (see, for instance, Craigie, 2020; Doleac and Hansen, 2020; Shoag and Veuger, 2021; Rose, 2021; Burton and Wasser, 2025).

tories (Blair and Chung, 2025). Recent work also examines how criminal records affect workers' sorting across employers and firms' willingness to hire workers with records (Hjalmarsson et al., 2025), and decomposes the associated earnings penalty across workers and firms (Rose and Shem-Tov, 2025). A related literature studies the consequences of avoiding prosecution or conviction and of subsequently concealing, reducing, or removing existing criminal records (Mueller-Smith and Schnepel, 2021; Agan et al., 2023, 2024a,b; Dasgupta et al., 2025). Our paper addresses the complementary question of what happens when a relatively low-level conviction is acquired, exploiting linked employer–employee histories to trace workers' subsequent labor market trajectories.

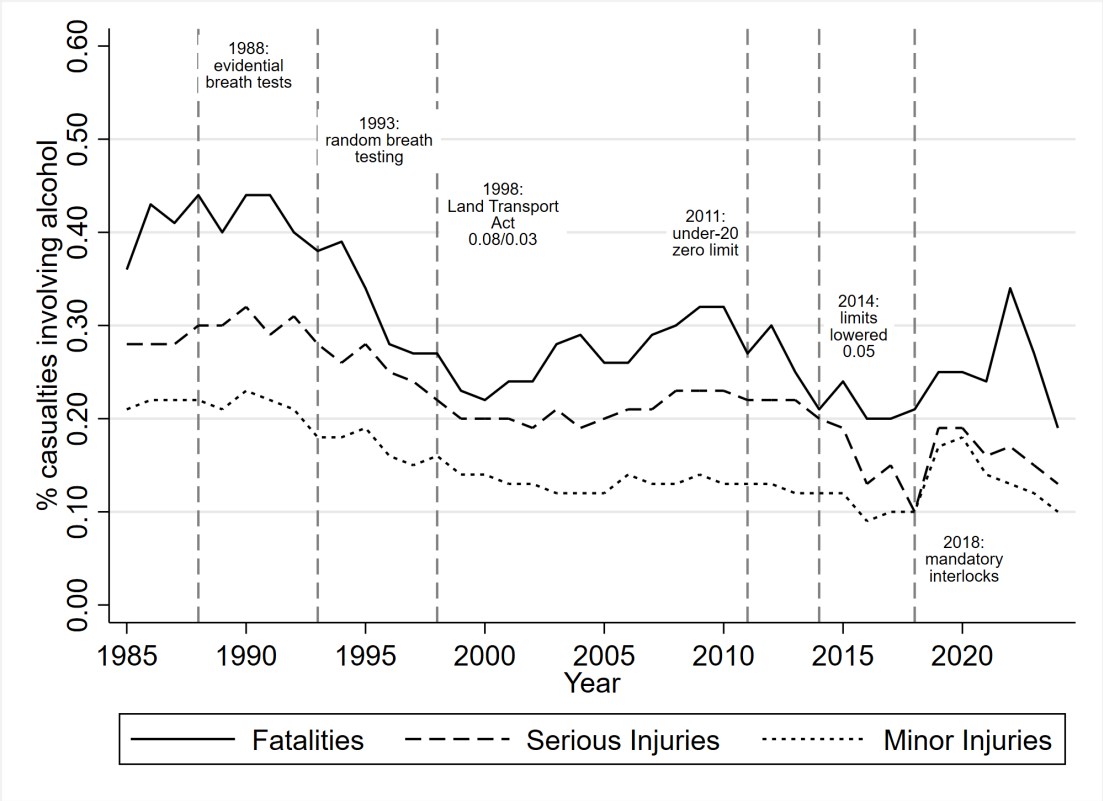
Our study makes three contributions. First, we provide causal evidence on the employment and earnings consequences of acquiring a narrowly defined low-level conviction carrying a non-custodial sanction. Second, the richness of the linked administrative data allows us to go beyond average labor market effects and identify where these consequences emerge: employment losses are concentrated in the formation of new matches rather than the dissolution of existing employment relationships. Third, we provide evidence on potential mechanisms underlying this pattern and a conceptual framework through which to interpret it. New-match losses are substantially larger in industries where employers are more likely to screen applicants for criminal records, while they are not concentrated in industries where driving is more important for work. A simple model of employer screening and learning rationalizes why a criminal record may matter more for prospective than for incumbent employment relationships. The empirical evidence and conceptual framework together support an important role for employer screening, while leaving scope for search, mobility, and other worker-side channels to contribute to the estimated effects.

The rest of the paper is organized as follows. Section 2 describes New Zealand's institutional setting for drink-driving offenses. Section 3 presents the data, sample construction, and empirical strategy. Section 4 presents the main results and robustness checks, and examines heterogeneity. Section 5 explores employer-screening relationship. Section 6 concludes.

2 Institutional Background

Figure 1 summarizes the evolution of New Zealand's drink-driving regime. Over the period shown, enforcement was progressively strengthened through evidential and random breath testing (1988 and 1993), reductions in legal alcohol limits (1998, 2011, and 2014), and mandatory interlocks for repeat offenders (2018). The share of road casualties involving alcohol declined substantially over this period, although drink-driving remains an important road-safety concern.[8]

[8]The figure describes the institutional setting; none of these policy changes provides the identifying variation used in our analysis.



Notes: The figure plots the share of road casualties involving alcohol by year, alongside selected drink-driving policy changes. Vertical dashed lines indicate major changes to alcohol testing, legal thresholds, and sanctions.
Source: Ministry for Cities, Environment, Regions and Transport, https://www.transport.govt.nz/statistics-and-insights/safety-annual-statistics/sheet/alcohol-and-drugs; authors' presentation.

Figure 1: Alcohol-related road casualties and drink-driving policy changes in New Zealand

The Land Transport Act 1998 provides the principal legal framework governing alcohol-impaired driving and associated penalties (New Zealand Government, 2026). Two features of this framework are particularly relevant for our analysis. First, alcohol concentration is measured using standardized breath or blood tests, providing an objective measure of offense severity. Second, legal consequences are threshold-based: lower alcohol readings can result in infringement offenses, whereas readings above specified thresholds lead to court prosecution and can therefore generate a criminal conviction.

Enforcement relies on widespread roadside alcohol testing. Police may stop drivers and administer alcohol tests without reasonable suspicion. Initial roadside screening is followed, where required, by an evidential breath or blood test, which provides the alcohol-concentration measure used for prosecution (Controller and Auditor General, 2013).[9] These

[9]The testing process proceeds through increasingly evidential stages: (i) *passive breath tests* provide an initial roadside indication of alcohol consumption; (ii) *breath screening tests* are administered following failure or refusal of the passive test and determine whether further evidential testing is required; (iii) *evidential breath tests* provide a court-admissible measure of breath alcohol concentration; and (iv) *evidential blood tests* are conducted by a health professional where required. Under the 1990 Bill of Rights Act, drivers who refuse or fail initial screening tests have the right to consult a lawyer before providing further evidence.

procedures provide the measured breath or blood alcohol readings observed in the court records used in our analysis.

The relevant thresholds depend on age and calendar year. Under the original Land Transport Act, drivers under age 20 faced a limit of 30 mg of alcohol per 100 ml of blood, or 150 micrograms (μg) per liter of breath, while the corresponding limits for drivers aged 20 and over were 80 mg and 400 μg, respectively.[10] In 2011, New Zealand introduced a zero-alcohol limit for drivers under 20 (New Zealand Government, 2026). Readings below 30 mg per 100 ml of blood (150 μg per liter of breath) were treated as infringement offenses rather than court offenses, whereas readings at or above that threshold resulted in prosecution and could lead to a criminal conviction.[11]

In 2014, the legal limit for drivers aged 20 and over was reduced from 80 to 50 mg per 100 ml of blood, and from 400 to 250 μg per liter of breath. Readings between the new legal limit and the court-prosecution threshold were treated as infringement offenses, whereas readings above the prosecution threshold resulted in court proceedings. Court sanctions depend on alcohol concentration, prior offenses, and whether injury or death occurred, and can include fines, license disqualification, and, for sufficiently serious offenses, imprisonment.[12]

This distinction between infringement and court offenses is central to our analysis. An infringement can impose a financial penalty or driving restriction without generating the court conviction that constitutes our treatment. We instead study first-time drink-driving offenders whose alcohol readings lead to court prosecution and a formal conviction. We date treatment at the final court date, when conviction status is established, rather than at the earlier offense date. This distinction allows us to separate, conceptually, the criminal-record consequences of conviction from the underlying drink-driving incident and from temporary restrictions on driving.

3 Data and Methods

3.1 Sample Selection and Descriptives

We use administrative data from Statistics New Zealand's (Stats NZ) Integrated Data Infrastructure (IDI), which links individual-level records from multiple government agencies through anonymized identifiers. Our primary sources are Ministry of Justice (MoJ) court records and Inland Revenue employment and earnings records, supplemented with

[10] In 1999, New Zealand reduced the minimum legal alcohol purchasing age from 20 to 18 (Kypri et al., 2006). Subsequent research documents an increase in alcohol consumption among young adults following the reform (Boes and Stillman, 2013; Dasgupta et al., 2022). This change affected access to alcohol but did not alter the drink-driving conviction thresholds that are central to our analysis.

[11] Low-level infringements typically resulted in a fine and demerit points. Drivers accumulating 100 or more demerit points within two years face an automatic three-month license suspension.

[12] See the NZ Transport Agency's "Alcohol and Drugs Limits" and https://www.nzta.govt.nz/driver-licences/driving-offenses-and-penalties/alcohol-and-drug-affected-driving/.

demographic and other administrative data.

The MoJ court charges data contain criminal charges processed in New Zealand courts since 1992, including offense dates, court proceedings, sentencing outcomes, offense types, conviction status, and court location. We link these records to Stats NZ's Personal Details dataset for demographic characteristics, including sex, ethnicity, and date of birth, and to separate MoJ records containing measured alcohol levels for drink-driving offenses.

Because legal alcohol thresholds differ for drivers under age 20, we restrict attention to individuals above this age threshold at the time of the offense. Our analysis sample consists of men and women aged between 20 (excluding the birthday month) and 55 whose drink-driving conviction occurred between January 2007 and December 2019. We impose four additional restrictions to construct a homogeneous sample of individuals receiving comparable convictions and sanctions:[13]

(i) Drink-driving must be the individual's first recorded offense (only one charge), with no prior court appearances, with a measured alcohol level between 400 and 500 μg per liter of breath (or 80–100 mg per 100 ml of blood). This excludes individuals with other recorded criminal justice involvement and restricts the sample to relatively low-level drink-driving offenses.

(ii) Individuals must receive a conviction and the standard sanction of a NZ$400–500 fine and a 180-day driver license suspension. This is the most common sentencing outcome for the offenses in our sample and limits heterogeneity in legal consequences.

(iii) Individuals must not experience an accident in the month of the drink-driving offense, based on Accident Compensation Corporation claims. This restriction reduces the possibility that contemporaneous injury or related health shocks account for subsequent labor market changes.

(iv) Individuals must have been born in New Zealand. Because the MoJ records cover offenses processed in New Zealand courts, this restriction increases confidence that we correctly identify individuals without prior recorded convictions.

We link this sample to Inland Revenue monthly tax records to measure employment and earnings before and after conviction, which we date using the final court date. The resulting estimation sample contains 1,824 individuals.

Table 1 compares the drink-driving sample, measured one month before the offense, with New Zealand-born individuals aged 20–55 in the 2013 Census. Individuals in our sample are younger, more likely to be male, and less likely to be European than the broader population. Although they have higher employment rates, their average earnings are lower and they are disproportionately employed in manufacturing and construction and in trade and retail. These differences underscore that criminal justice involvement

[13] Appendix Table A.1 reports the restrictions used to construct the final sample and the reduction in sample size associated with each step.

	Drink-driving sample[a]	2013 Census[b]
Date	May 2013	March 2013
Age	32.55	38.20
	(10.61)	
Male	0.556	0.474
European only	0.560	0.732
Employed	0.691	0.566
Labor income (in $NZ)	3946.16	4521.95
	(2425.19)	
Labor income relative to industry mean	1.09	1.14
	(0.63)	
Industry		
Manufacturing/Construction	0.292	0.194
Trade & Retail	0.310	0.237
Services & Health	0.284	0.441
Other	0.114	0.128
Breath (vs blood) offense code[c]	0.850	
Alcohol level (relative to legal limit)[d]	1.174	
	(0.044)	
Fine (in $NZ)	428.71	
	(29.22)	
Driver license suspension (in days)	180	
Processing duration[e]	38.05	
	(28.13)	
Unique individuals	1,824	684,927

Notes: Means are reported unless otherwise indicated. Numbers in () are std dev (only for continuous outcomes).

[a] Characteristics of the drink-driving sample are measured one month before the offense date.

[b] The Census comparison sample comprises New Zealand-born individuals aged 20–55 on the 2013 Census date. Labor income is monthly income from wages and salaries among employed individuals; labor income relative to the industry mean is the ratio of individual labor income to the corresponding industry mean. Industry categories follow ANZSIC 2006.

[c] Breath (vs. blood) offense code equals one when the recorded alcohol measure is based on a breath test and zero when it is based on a blood test.

[d] Alcohol concentration is expressed relative to the applicable court-prosecution threshold of 400 μg per liter of breath or 80 mg per 100 ml of blood.

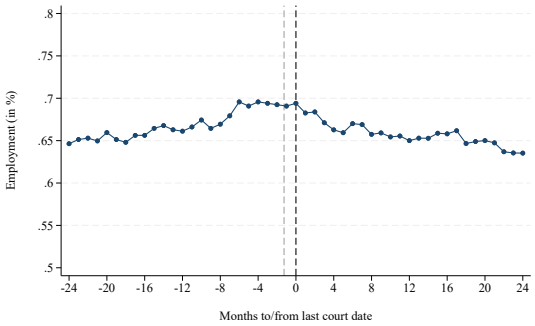
[e] Processing duration is the number of days between the offense date and the final court date.

Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

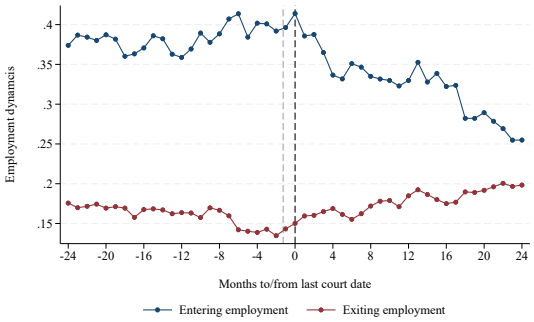
Table 1: Descriptive statistics

is selective: even within the relatively homogeneous group of low-level offenders studied here, convicted individuals are not representative of the general population. Our baseline identification strategy therefore does not use the general population as the counterfactual; instead, it compares individuals convicted at different points in time, as described below.

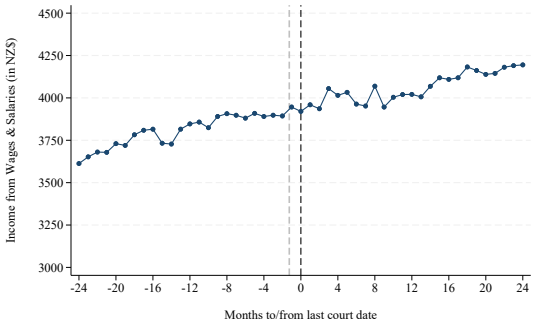
Figure 2 plots raw labor market outcomes over the 24 months before and after conviction. Employment rises modestly before conviction and declines thereafter (Panel (a)). The underlying transitions in Panel (b) show that employment entry exceeds exit before



(a) Employment



(b) Labor Market Transitions



(c) Earnings

Notes: The panels plot raw monthly averages from 24 months before to 24 months after the final court date, with event month $t = 0$ denoting the month of conviction. Entering employment is calculated among individuals who were non-employed at $t = -24$; exiting employment is calculated among individuals who were employed at $t = -24$. labor income is monthly income from wages and salaries among employed individuals. The black dashed line marks the final court date, and the grey dashed line marks the average offense date relative to the final court date.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Figure 2: Economic outcomes before and after the first drink-driving conviction, raw averages

conviction, while entry falls and exit increases modestly afterward. Among those employed, earnings continue to rise over the event window (Panel (c)). These raw trajectories are descriptive and do not identify the effect of conviction. Our empirical design instead identifies effects from differences in outcome dynamics between individuals convicted at different points in time.

3.2 Research Design

Our baseline empirical strategy exploits variation in the timing of individuals' first drink-driving conviction. All individuals in the estimation sample are eventually convicted, but at different dates. We therefore compare changes in labor market outcomes for individuals convicted earlier with contemporaneous changes among individuals who have not yet been convicted. Using not-yet-treated individuals as the comparison group limits concerns that fixed differences between individuals who do and do not experience criminal justice involvement drive the estimates.

Staggered difference-in-differences. Because convictions occur at different dates, treatment is staggered. With heterogeneous treatment effects, conventional two-way fixed effects estimators can incorporate comparisons in which already-treated individuals serve as controls and need not recover an economically transparent average of cohort-time treatment effects (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Roth et al., 2023; Borusyak et al., 2024; Wooldridge, 2025). We therefore use the estimator proposed by Callaway and Sant'Anna (2021), with not-yet-treated individuals serving as controls.

Let $G_i = g$ denote that individual i is first convicted in month g. Let $Y_{it}(g)$ denote the potential outcome at time t if first conviction occurs in month g, and let $Y_{it}(0)$ denote the potential outcome in the absence of conviction. For cohort g at time $t \geq g$, the group-time average treatment effect on the treated is

$$ATT(g,t) = E\left[Y_{it}(g) - Y_{it}(0) \mid G_i = g\right]. \tag{1}$$

Under the identifying assumptions described below, this effect is identified by comparing the change in outcomes for cohort g with the corresponding change among individuals who have not yet been convicted at time t, i.e.:

$$ATT(g,t) = E\left[Y_{it} - Y_{i,g-1} \mid G_i = g\right] - E\left[Y_{it} - Y_{i,g-1} \mid G_i > t\right]. \tag{2}$$

Thus, individuals convicted later provide the counterfactual outcome path for individuals convicted earlier until their own conviction occurs.

For the event-study analysis, we index treatment effects by event time $e = t - g$ and aggregate the group-time effects across cohorts:

$$ATT(e) = \sum_{g:g+e\leq T} \omega_{g,e}\, ATT(g, g+e), \qquad \sum_{g:g+e\leq T} \omega_{g,e} = 1, \tag{3}$$

where $\omega_{g,e}$ weights treatment cohorts according to their relative size among cohorts observed at event time e. In the analysis, we summarize the post-conviction effects over a common 24-month horizon:[14]

$$\overline{ATT}_{0:24} = \frac{1}{25}\sum_{e=0}^{24} ATT(e). \tag{4}$$

This estimand is the average effect of conviction during the first 24 months following the final court date.

Identification. A causal interpretation requires three main assumptions. First, in the absence of conviction, individuals convicted at different dates must have followed parallel

[14]Because event time $e = 0$ denotes the month of conviction, the post-conviction window $e = 0, \ldots, 24$ contains 25 monthly observations.

outcome trends over the periods in which they are used as treated and not-yet-treated comparisons. Importantly, this assumption does not require conviction timing to be random. It allows individuals convicted earlier and later to differ in levels and in fixed characteristics; it requires that shocks determining the timing of conviction do not also generate differential counterfactual changes in labor market outcomes.

This distinction is important in our setting. Adverse life events — such as deteriorating health, family instability, or job loss — could both increase the probability of drink-driving and affect subsequent labor market outcomes. Such shocks threaten identification if they generate differential outcome dynamics around the timing of conviction. We therefore examine event-study coefficients before conviction. The absence of differential pre-conviction labor market trends is consistent with, although it cannot establish, the parallel-trends assumption.

We additionally examine pre-conviction dynamics in health-related outcomes. Online Appendix Figure A.1 reports event studies for prescriptions of nervous-system-related medications and accident-related hospital admissions.[15] If deteriorating health were a common shock driving both conviction timing and labor market outcomes, we might expect these measures to worsen differentially before conviction. We find no such pattern: the estimates are small, imprecise, and display no systematic deterioration before treatment.

Second, we assume no anticipation of the labor market consequences of conviction. The distinction between the offense and conviction dates matters here because the offense generally precedes the final court date. Individuals could respond in the intervening period if they anticipate sanctions or employment consequences. Our treatment of interest, however, is the formal conviction, which establishes criminal-record status and is therefore the event most directly relevant for employer screening. We consequently date treatment at the final court date and use the pre-treatment event-study coefficients to examine whether labor market outcomes begin to diverge before formal conviction.

Third, we assume no interference across individuals: one individual's conviction does not directly affect another individual's potential labor market outcomes. Given the individual nature of treatment and the absence of an obvious mechanism generating economically important spillovers, this assumption appears plausible in our setting.[16]

Alternative matched event-study design. Our baseline design deliberately uses only eventual offenders, thereby avoiding comparisons between offenders and the general population. Its identification, however, requires later-convicted individuals to provide an appropriate counterfactual for those convicted earlier. We therefore implement a complementary design that changes the comparison group. Convicted individuals are matched to observationally

[15]Nervous-system-related medications include antidepressants, antipsychotics, anxiolytics, stimulants and ADHD treatments, analgesics, and treatments for substance dependence.

[16]Spillovers through social or peer networks among drink-driving offenders remain possible but cannot be assessed with the available data. However, using 2013 and 2018 Census data, we verify that no two individuals in our sample share a household identifier, mitigating treatment spillover concerns.

similar New Zealanders who experience no court conviction during the sample period, and we estimate a fixed effects event study within the resulting matched sample.[17]

The control pool consists of New Zealand-born individuals residing in New Zealand between 2007 and 2019 with no recorded court conviction. Each convicted individual is exactly matched to all available controls with the same sex, ethnicity category (European versus non-European), birth cohort (year and quarter), and employment status 24 months before conviction. Conditional on employment, controls must additionally have monthly labor income within approximately NZ$250 of the treated individual's income at that date. Each matched control is assigned the corresponding treated individual's conviction date as a pseudo-event date, ensuring that treated and control outcomes are compared over identical calendar periods.

We obtain matches for 1,818 of the 1,824 convicted individuals. Appendix Table A.2 reports balance before and after matching. The substantial demographic and labor market differences between convicted individuals and the general population are effectively eliminated for the characteristics used in matching. Controls may be matched to multiple treated individuals. Within each matched set, weights are normalized so that the treated individual and the matched controls receive equal aggregate weight. A control individual's effective weight therefore equals the sum of its contributions across all matched sets in which it appears. Using these weights, we then estimate

$$Y_{it} = \xi_i + \sum_{\substack{k=-24 \\ k\neq -1}}^{24} \delta_k \mathbf{1}\{t - t_i^* = k\} + \sum_{\substack{k=-24 \\ k\neq -1}}^{24} \beta_k \mathbf{1}\{t - t_i^* = k\} D_i + \phi_q + \varepsilon_{it}, \tag{5}$$

where ξ_i denotes individual fixed effects, D_i equals one for convicted individuals, t_i^* is the actual conviction date for treated individuals and the assigned pseudo-conviction date for matched controls, and ϕ_q denotes calendar-quarter fixed effects. Event time $k = -1$ is omitted as the reference period. The coefficients β_k therefore measure the differential change in outcomes for convicted individuals relative to their matched controls at each event time.

The matched design does not eliminate the need for a parallel-trends assumption. Rather, it provides a complementary test using a different counterfactual population: instead of comparing earlier- and later-convicted individuals, it compares convicted individuals with observably similar individuals who remain unconvicted. Similar estimates across the two designs would therefore be difficult to attribute solely to the particular comparison group underlying the baseline staggered-adoption design.

[17] This approach follows a growing literature combining matching with event-study or difference-in-differences designs when treatment is non-randomly assigned (e.g., Adams-Prassl et al., 2024; Adams et al., 2024; Jensen and Zhang, 2026).

4 Results

4.1 Baseline Estimates

Table 2 reports the average pre- and post-conviction estimates. The pre-conviction estimates are small and statistically insignificant, with no evidence of systematic differential trends in labor market outcomes before conviction.[18]

Following conviction, employment declines by 6.8 percentage points, or 9.8 percent relative to its pre-conviction level. The decline is associated with a substantially larger response in employment entry than in exit: entry falls by 8.2 percentage points (20.7 percent), while exit increases by 2.2 percentage points (15.4 percent). Average monthly labor income falls by NZ$90, corresponding to a 2.3 percent decline relative to its pre-conviction level.

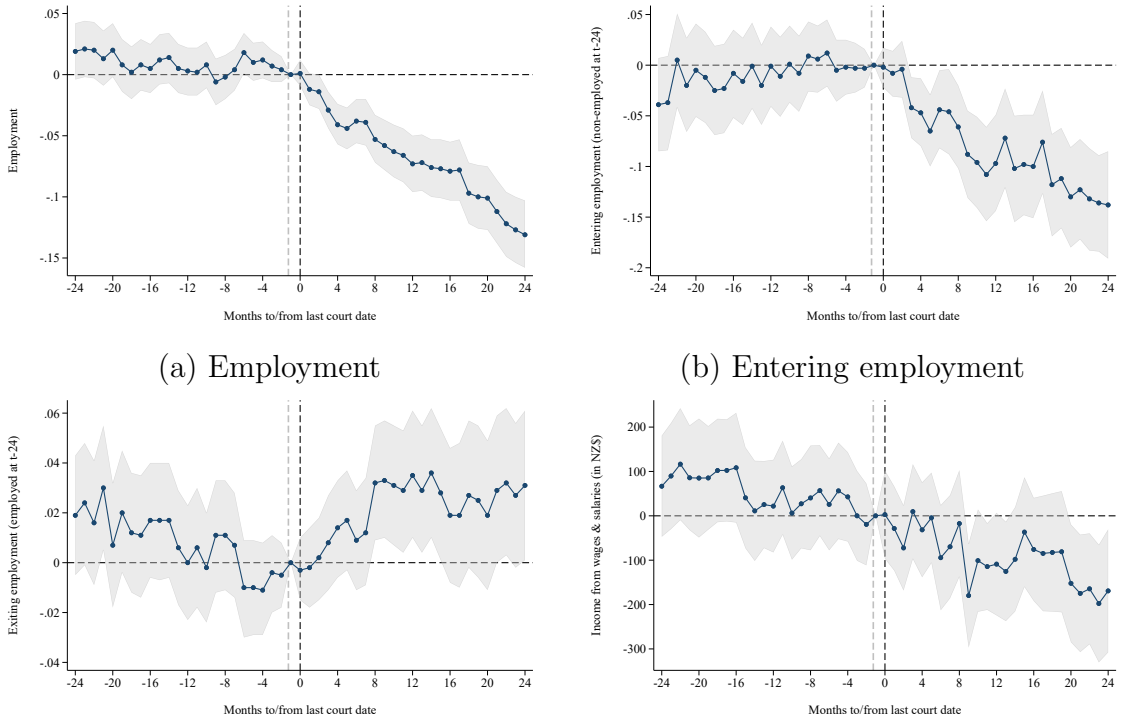
	Months relative to final court date	
	$-24 \leq t \leq -2$	$0 \leq t \leq 24$
Employment	0.009	-0.068***
	(0.008)	(0.009)
Entering employment	-0.009	-0.082***
	(0.015)	(0.015)
Exiting employment	0.008	0.022**
	(0.008)	(0.009)
Labor income	53.933	-90.109**
	(41.093)	(40.853)
Unique individuals	1,824	

Notes: Entries are averages of the event-study coefficients over the pre-conviction period ($-24 \leq t \leq -2$) and the post-conviction period ($0 \leq t \leq 24$). The post-conviction period includes the conviction month and the following 24 months. Estimates use the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator, with not-yet-convicted individuals as the comparison group; the omitted event month is $t = -1$. Entering employment is estimated among individuals who were non-employed at $t = -24$; exiting employment is estimated among individuals who were employed at $t = -24$. labor income is monthly income from wages and salaries among employed individuals. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table 2: Regression results (averages)

Figure 3 shows the corresponding event-study estimates. Employment falls persistently

[18] As an additional falsification exercise, Appendix Table B.1 artificially shifts conviction dates 24 months earlier and re-estimates the model. We find no statistically significant placebo effects on employment, entry into employment, or labor income. The estimate for exit from employment is statistically significant but has the opposite sign from the corresponding post-conviction effect. These results provide additional evidence against pre-existing differential labor market dynamics driving the main estimates.



(a) Employment (b) Entering employment

(c) Exiting employment (d) Labor income

Notes: The panels report event-study estimates from the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator, with not-yet-convicted individuals as the comparison group. The omitted event month is $t = -1$, and shaded areas show 95% confidence intervals. Entering employment is estimated among individuals who were non-employed at $t = -24$; exiting employment is estimated among individuals who were employed at $t = -24$. labor income is monthly income from wages and salaries among employed individuals. The black dashed line marks the final court date ($t = 0$), and the grey dashed line marks the average offense date relative to the final court date.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Figure 3: Economic outcomes before and after the first drink-driving conviction

after conviction, reaching approximately 15 percentage points below the counterfactual two years later (Panel (a)). The dynamics again reveal a pronounced asymmetry between employment entry and exit. Entry falls sharply and persistently (Panel (b)), whereas the increase in exit is considerably smaller (Panel (c)). Labor income also declines after conviction, but by substantially less in proportional terms than employment (Panel (d)).

The timing and margins of adjustment help distinguish the estimated effects from the direct consequences of temporary driver license suspension. If the employment decline arose primarily because convicted workers could not commute or perform driving-related tasks while their licenses were suspended, we would expect a relatively pronounced increase in separations among workers employed at conviction and attenuation as the standard 180-day suspension expired. Instead, the employment effect persists well beyond six months and is driven predominantly by reduced entry rather than increased exit. The evidence therefore points toward impediments to obtaining employment after conviction, rather than primarily the loss of existing jobs during the suspension period.

To examine this distinction more directly, we exploit employer identifiers and five-

digit (ANZSIC) industry codes to decompose post-conviction employment transitions. We distinguish retention with the same employer, moves to another employer within the same five-digit industry, moves across industries, and entry from non-employment. The full decomposition is reported in Online Appendix B.1.

The decomposition reinforces the distinction between continuing and new employment relationships. Appendix Table B.2 shows no detectable effect on retention with the same employer or on moves to another employer within the identical five-digit industry. Instead, the employment losses are concentrated in entry from non-employment, which accounts for approximately 44 percent of the total, and moves across broader industry boundaries, which account for the remainder. Thus, the employment penalty emerges primarily when workers seek new matches outside narrowly defined employment relationships.

This pattern is consistent with employer screening. Existing employers have accumulated direct information about worker performance, while prospective employers hiring workers with closely related industry experience may be better able to infer productivity from relevant skills and employment histories. Employers evaluating less familiar applicants instead face greater informational uncertainty and may therefore place greater weight on observable signals such as criminal records. Importantly, the transition decomposition does not by itself identify employer screening: reduced worker search or other frictions in forming new matches could contribute to the same pattern. The conceptual framework in Section 5 formalizes the screening mechanism and its implications for hiring, incumbent matches, and wages.

The earnings decomposition provides complementary evidence. Appendix Table B.3 shows little evidence of an earnings penalty among workers who remain with the same employer or move within the identical five-digit industry, whereas earnings decline substantially among workers who change industries or enter employment from non-employment. Overall, the employment and earnings decompositions show that the adverse labor market effects are concentrated around the formation of new employment relationships rather than within continuing matches.

4.2 Robustness Checks

Alternative comparison group: exact matching — We first assess whether the results depend on using later-convicted individuals as the comparison group. As described in subsection 3.2, we implement an alternative design that exactly matches convicted individuals to observationally similar individuals who experience no court conviction during the sample period and estimate the fixed-effects event-study specification in Equation (5). Appendix Table C.1 reports the average pre- and post-conviction estimates, while Appendix Figure C.1 compares the dynamic estimates with those from the baseline staggered difference-in-differences design.

The two designs produce similar post-conviction patterns despite relying on different

comparison groups. In both cases, conviction is followed by persistent declines in employment and labor income, driven primarily by lower entry into employment and accompanied by substantially smaller increases in employment exit. The exact-matching estimates for employment and entry are somewhat smaller than the baseline estimates, but their dynamic profiles are similar. One qualification is that the average pre-conviction estimate for employment entry is statistically significant in the matched design. As Appendix Figure C.1 shows, the difference is concentrated earlier in the pre-treatment window and narrows in the months immediately preceding conviction. We therefore view the matched estimates as complementary evidence rather than as eliminating the identifying assumptions of the baseline design. Their similarity is nevertheless reassuring because the two approaches construct the counterfactual from different populations.

Alternative analysis periods — Our baseline sample includes individuals convicted between January 2007 and December 2019, a period spanning changes in New Zealand's drink-driving regime as well as substantial variation in aggregate labor market conditions, including the global financial crisis (Hall and McDermott, 2016; Razzak, 2016). Although the baseline estimator accounts for common calendar-time shocks, comparisons across widely separated conviction cohorts could be problematic if the counterfactual evolution of labor market outcomes differs systematically across institutional or macroeconomic environments.

We therefore re-estimate the Callaway and Sant'Anna (2021) specification over four alternative windows: convictions after December 2014, convictions before December 2014, convictions before 2018, and convictions after 2012. These restrictions alter the cohorts contributing to identification around important changes in the institutional environment, including the 2014 reduction in the adult alcohol limit and the 2018 introduction of mandatory alcohol interlocks. In every specification, we maintain the baseline restrictions to drivers aged 20 and over and alcohol readings above the adult court-prosecution threshold.

Appendix Table C.2 shows that the main results are stable across these alternative windows. The estimated employment decline ranges from 4.9 to 7.9 percentage points and is consistently driven primarily by lower employment entry, for which the estimates range from 8.4 to 10.3 percentage points. Effects on employment exit are positive but smaller, ranging from 2.2 to 3.2 percentage points. Labor-income estimates are negative in all four samples, although statistically significant only in the pre-2018 specification. We also find little evidence of systematic pre-conviction differences. Thus, the central employment result, and especially the asymmetry between entry and exit, does not appear to be driven by a particular conviction cohort or analysis period.

Subsequent convictions — Finally, we examine whether the baseline effects reflect subsequent criminal justice contact rather than the initial drink-driving conviction. We exclude individuals who receive another conviction within 24 months of their first conviction and re-estimate the baseline specification. As shown in Appendix Table C.3, the estimated

effects on employment and employment entry remain very similar. The main results are therefore not explained by subsequent convictions during the post-treatment window.

4.3 Labor-Market Reallocation and Broader Income Effects

Additional labor-market outcomes — We first examine whether conviction affects other margins of labor-market adjustment. Appendix Table C.4 shows no detectable differences in employer switching or wage progression among individuals observed employed both at $t-24$ and at t. Because employment at t may itself respond to conviction, we interpret these conditional estimates descriptively rather than as separate causal effects. We also find no evidence of increased enrollment in college or other higher-education programs. By contrast, conviction increases the probability of employment in a low-paying industry, defined as an industry whose mean earnings lie in the bottom quartile of the industry earnings distribution. This result complements the transition evidence in subsection 4.1: convictions not only impede the formation of new employment matches but also appear to shift employment toward lower-paying parts of the labor market.

Broader income measures and self-employment — Our baseline income measure captures monthly wages and salaries. We therefore examine whether the economic consequences of conviction extend to broader sources of income observed at the annual level. The first two columns of Appendix Table C.5 report estimates for total annual income.[19] Total annual income declines persistently over the four years following conviction, indicating that the economic loss is not confined to monthly wage-and-salary income.

We also examine whether self-employment provides an alternative margin of adjustment. This possibility is particularly relevant given evidence that individuals with criminal records are disproportionately likely to be self-employed (Finlay et al., 2023). We define self-employment as annual self-employment income above NZ$5,000.[20] We find no detectable effect on the probability of self-employment. Self-employment income declines, although the estimate is statistically significant only at the 10 percent level. Thus, there is little evidence that workers offset the loss of wage-and-salary employment by moving into self-employment.

4.4 Heterogeneity

We now examine heterogeneity along three dimensions—ethnicity, gender, and prior university or college enrollment—chosen to assess whether the labor market consequences of

[19] Because Inland Revenue records self-employment and investment income only at the financial-year level, we redefine event time accordingly. Total annual income includes wages and salaries, self-employment and investment income, income-support payments, tax credits, student allowances, paid parental leave, ACC income support, and other taxable income. ACC income support consists of compensation for individuals who cannot work or experience reduced earnings because of a covered injury.

[20] The results are robust to alternative thresholds.

conviction vary with demographic characteristics, labor market attachment and mobility, and observable signals of worker skills.

Ethnicity — We first examine heterogeneity by ethnicity to assess whether the labor-market penalty associated with a similar low-level conviction differs across demographic groups, as might occur if the stigma or signaling value of a criminal record varies across workers.

Conviction is followed by adverse labor market outcomes for both Europeans and Non-Europeans, but the employment decline is substantially larger for the latter. Employment falls by 4.9 percentage points (7.0 percent) among Europeans and by 9.1 percentage points (13.3 percent) among Non-Europeans. The 4.2 percentage-point difference is statistically significant. For Europeans, the employment decline is driven primarily by lower entry, with little detectable change in exit. Among Non-Europeans, both margins contribute: entry falls by 9.2 percentage points and exit rises by 4.1 percentage points. Labor income also falls more among Non-Europeans, although the difference in income effects across the two groups is not statistically significant.

These differences are potentially consistent with heterogeneity in the signaling or stigma associated with a criminal record. Recent evidence shows that even observably comparable drink-driving cases can generate different judicial outcomes across defendants (Denning et al., 2025), illustrating that the consequences attached to similar offenses need not be uniform. Our estimates do not identify why post-conviction effects differ across ethnic groups, however, and differences in employer responses are only one possible explanation. We therefore interpret the larger employment penalty among Non-Europeans as evidence of heterogeneous labor market consequences of conviction, while treating its underlying mechanism more cautiously.

Gender — We next examine heterogeneity by gender, since differences in labor-market attachment and mobility may affect whether a conviction operates primarily through reduced access to new jobs or separation from existing employment.

The overall employment effects are much more similar by gender than by ethnicity. Employment falls by 6.4 percentage points for men and 7.0 percentage points for women, and the difference is not statistically significant. The point estimates nevertheless suggest different margins of adjustment. Among men, the decline is concentrated in employment entry, which falls by 10.7 percentage points. Among women, entry falls by 5.0 percentage points while exit rises by 4.1 percentage points. The differences between men and women in both margins are statistically significant only at the 10 percent level. Labor income falls by NZ$139 among men but is essentially unchanged among women; again, the difference between the two estimates is statistically significant only at the 10 percent level. Figure C.3 presents the corresponding dynamic estimates. Overall, we therefore regard the differences in adjustment margins between men and women as suggestive rather than definitive.

Prior higher-education enrollment. — Finally, we examine whether the effects of con-

viction vary with prior university or college enrollment. This comparison is informative for our screening interpretation (see the next subsection) because education provides prospective employers with an additional observable signal of workers' skills and productivity and may also affect the types of jobs to which workers have access. Prior university or college experience could therefore attenuate the importance of a criminal record in employer screening. At the same time, the consequences of a conviction could be larger in jobs or labor markets in which employers place greater weight on background information, so the direction of the heterogeneity is theoretically ambiguous.

Because enrollment records begin in 1994, this analysis is restricted to individuals younger than 30 at the offense date. Appendix Table C.6 shows somewhat smaller post-conviction employment declines among individuals with prior university or college enrollment. For employment entry, however, the pattern is reversed, with larger reductions among those previously enrolled. The evidence therefore provides no clear indication that prior university or college enrollment insulates workers from the labor market consequences of conviction. Moreover, we find statistically significant pre-treatment estimates in two specifications for individuals without prior enrollment. Given both the restricted sample and these pre-treatment differences, we treat this heterogeneity analysis as exploratory.

5 Employer Screening

The preceding results show that the employment effects of conviction are concentrated in the formation of new employment relationships rather than the destruction of existing matches. We now examine whether this pattern is consistent with employer screening of criminal records. We proceed in two steps. First, we present complementary evidence on the relationship between criminal-record information and hiring. Second, we formalize the underlying mechanism in a simple screening model in which criminal records provide prospective employers with an additional signal about worker productivity. The model deliberately abstracts from incapacitation and human-capital depreciation, mechanisms that are limited in our setting by the non-custodial nature of the convictions we study.

Descriptive evidence — Survey evidence indicates that criminal-record screening is relevant to hiring in New Zealand. In a 2019 survey of 237 firms in the Canterbury region, 51 percent reported conducting criminal background checks (Gilbert et al., 2019).[21] Employer willingness to hire also varies with the nature and severity of the offense, consistent with evidence from other settings (Uggen et al., 2014). This pattern is reflected in screening guidelines, which commonly emphasize the nature and seriousness of the offense, its

[21]The survey focuses on firms in sectors with positions whose skill requirements are likely to overlap with those of justice-involved individuals and is therefore not necessarily representative of all New Zealand employers. In fiscal year 2023–24, the New Zealand Ministry of Justice processed more than 641,000 requests for criminal conviction history checks (Ministry of Justice, 2024), although these requests also include checks initiated by individuals, public agencies, and other organizations.

	European		Non-European			Male		Female		
	Months relative to final court date									
	$-24 \leq t \leq -2$	$0 \leq t \leq 24$	$-24 \leq t \leq -2$	$0 \leq t \leq 24$		$-24 \leq t \leq -2$	$0 \leq t \leq 24$	$-24 \leq t \leq -2$	$0 \leq t \leq 24$	
	(a)	(b)	(c)	(d)	(d)–(b)	(e)	(f)	(g)	(h)	(h)–(f)
Employment	0.011	-0.049***	0.004	-0.091***	-0.042**	0.017	-0.064***	-0.002	-0.070***	-0.006
	(0.010)	(0.011)	(0.012)	(0.014)	(0.018)	(0.010)	(0.012)	(0.012)	(0.014)	(0.018)
Entering employment	0.010	-0.075***	-0.030	-0.092***	-0.017	-0.011	-0.107***	-0.012	-0.050**	0.057*
	(0.020)	(0.019)	(0.022)	(0.024)	(0.030)	(0.020)	(0.023)	(0.021)	(0.020)	(0.030)
Exiting employment	0.003	0.011	0.019	0.041***	0.031	-0.005	0.008	0.025*	0.041***	0.033*
	(0.011)	(0.011)	(0.014)	(0.015)	(0.019)	(0.011)	(0.011)	(0.013)	(0.015)	(0.019)
Labor income	45.239	-72.554	60.005	-120.791**	-48.237	45.862	-138.963**	60.751	-8.990	129.974*
	(57.495)	(57.827)	(57.740)	(54.705)	(79.603)	(58.225)	(58.053)	(53.381)	(52.736)	(78.430)
Unique individuals	1,023		801			1,011		813		

Notes: Entries are averages of the event-study coefficients over the pre-conviction period ($-24 \leq t \leq -2$) and the post-conviction period ($0 \leq t \leq 24$). The post-conviction period includes the conviction month and the following 24 months. Estimates use the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator, with not-yet-convicted individuals as the comparison group; the omitted event month is $t = -1$. Columns (d)−(b) and (h)−(f) report differences in the post-conviction estimates between non-European and European individuals and between women and men, respectively. Entering employment is estimated among individuals who were non-employed at $t = -24$; exiting employment is estimated among individuals who were employed at $t = -24$. labor income is monthly income from wages and salaries among employed individuals. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table 3: Regression results by ethnicity and gender (averages)

relevance to the job, and the time elapsed since conviction.

We complement this survey evidence using linked administrative data to examine whether job seekers with observable criminal records subsequently experience lower employment rates. We link the 2013 New Zealand Census to MoJ court records and Inland Revenue tax records and select individuals who reported being unemployed and actively searching for work in March 2013. To focus on individuals with sustained non-employment rather than very short unemployment spells, we additionally require no wage or salary income between December 2012 and March 2013.

We classify individuals into three groups according to their conviction history and most severe recorded sentence: those with no conviction in the previous seven years; those whose most severe sentence was imprisonment or home detention; and those whose most severe sentence was a non-custodial community-based or monetary sanction. For individuals with a conviction, we restrict attention to convictions occurring within the preceding seven years, corresponding to the relevant horizon under New Zealand's Clean Slate regime (Dasgupta et al., 2025). This restriction focuses the analysis on relatively recent criminal records that are more likely to remain relevant to prospective employers. Within the non-custodial group, we separately examine individuals convicted of traffic offenses, allowing us to assess whether the employment gradient is also present for the category most closely related to the drink-driving convictions in our main analysis.

We measure subsequent employment using indicators for receiving at least NZ$100 in monthly wage and salary income 6, 12, and 24 months after the Census. We estimate linear probability models that control for age, education, ethnicity, nativity, sex, household income and household size, and prior labor market attachment, measured by the number of months employed since March 2012. These regressions are descriptive: conviction histories are not randomly assigned, and the estimates should not be interpreted as causal effects of criminal records. Their purpose is instead to establish whether, among individuals actively seeking work, observable conviction histories predict subsequent employment in the direction implied by an employer-screening mechanism.

Table 4 shows a clear descriptive gradient in subsequent employment by conviction history. Relative to job seekers with no conviction in the preceding seven years, those whose most severe sentence was imprisonment or home detention have employment probabilities that are 2.4–5.4 percentage points lower over the subsequent two years. Job seekers with non-custodial, non-traffic convictions exhibit similarly large differences of 2.2–4.7 percentage points. By contrast, the corresponding differences for non-custodial traffic offenses are smaller, at approximately 1.3–1.7 percentage points, although they remain statistically significant.

These comparisons are not causal, but they provide two pieces of evidence relevant to the mechanism. First, criminal-record information predicts subsequent employment even among individuals who are actively searching for work at a common point in time. Second, the association is present even for non-custodial traffic offenses, the category most closely

	Employed		
	September 2013 (6 months)	March 2014 (12 months)	March 2015 (24 months)
No conviction in previous seven years	*reference*	*reference*	*reference*
Imprisonment/home detention	-0.024***	-0.028***	-0.054***
	(0.008)	(0.009)	(0.010)
Non-custodial sentence	-0.022***	-0.035***	-0.047***
	(0.006)	(0.007)	(0.007)
of which: traffic offense	-0.013**	-0.017***	-0.015**
	(0.005)	(0.006)	(0.007)
Mean employment	0.222	0.296	0.364
Individuals	74,595	74,595	74,595

Notes: The sample comprises respondents to the 2013 New Zealand Census who reported being unemployed and actively seeking work in March 2013 and who received no wage or salary income from December 2012 through March 2013. Conviction history is measured over the seven years preceding the Census. The omitted category is no conviction during this period; traffic offenses are a subset of non-custodial sentences. Employment equals one when monthly wage and salary income is at least NZ$100 and is measured 6, 12, and 24 months after the Census. Estimates are from linear probability models controlling for age, highest educational qualification, ethnicity, New Zealand-born status, sex, total household income, household size, and the number of months employed since March 2012. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table 4: Employment probability of job seekers by conviction history

related to the low-level drink-driving convictions in our main analysis. Together with the survey evidence, these patterns establish that criminal records are both potentially visible to New Zealand employers and predictive of employment outcomes at precisely the hiring margin highlighted by our main results.

Screening intensity and driving requirements — We next return to our main analysis sample and ask whether the formation of new employment matches declines more strongly in industries in which criminal-record screening is plausibly more important. We do not observe firms' screening decisions directly. We therefore classify ANZSIC 2006 industry division-subdivision cells into low, medium, or high screening intensity based on characteristics that plausibly increase the value or likelihood of background checks, including formal regulatory requirements, safety-sensitive activities, responsibility for vulnerable populations, positions of public trust, and unsupervised access to clients.[22] Table 5 reports the classification and the corresponding estimates.

The decline in employment through new matches is substantially larger in medium- and high-screening industries. Following conviction, new-match employment falls by 4.8 percentage points in these industries, compared with 1.9 percentage points in low-screening industries. Because screening behavior is not directly observed and the industry classification is necessarily imperfect, we interpret this heterogeneity cautiously. Nevertheless, its

[22]See Appendix Table A.3 for the list of sectors and their corresponding classifications by screening intensity.

direction is consistent with the screening mechanism: the employment penalty is larger in settings in which criminal-history information is more likely to enter hiring decisions.

A complementary test distinguishes employer screening from a direct consequence of license suspension. If the decline in new employment primarily reflects workers' temporary inability to perform jobs requiring driving, the effect should be concentrated in industries in which driving is an important job requirement. The data show the opposite pattern.[23] Appendix Table B.4 shows that employment through new matches falls by 4.9 percentage points in industries with low driving requirements, compared with 1.9 percentage points in industries with high driving requirements. This pattern is difficult to reconcile with temporary license loss as the primary explanation for the employment decline, although it does not rule out a role for driving restrictions in particular jobs.

Conceptual Framework — To interpret the empirical patterns above, we outline a stylized model of employer screening and learning in which a criminal record affects the information available to prospective employers. The framework is not intended to encompass all channels through which criminal justice involvement may affect labor market outcomes. In other settings, incarceration, human-capital depreciation, changes in labor supply or job search, and restrictions on mobility may also play important roles. Our setting rules out incarceration and substantially limits mechanical incapacitation, while the concentration of employment losses in new-match formation motivates our focus on employer screening and learning. The model is therefore designed to rationalize the central asymmetry in our results: conviction substantially reduces the formation of new employment matches but has much smaller effects on continuing employment relationships. The key distinction is informational. Prospective employers must evaluate applicants using imperfect pre-hire signals, including criminal-record information, whereas incumbent employers can increasingly rely on direct observations of worker performance.[24]

Setup. Consider a labor market with risk-neutral firms and workers. Worker i has productivity

$$y_i = \bar{y} + \varepsilon_i, \qquad \varepsilon_i \sim F(0, \sigma^2),$$

where ε_i captures unobserved worker heterogeneity.[25] Firms cannot directly observe the productivity of job applicants. During recruitment, they observe a signal s_i together with a criminal-record indicator $R_i \in \{0, 1\}$, where $R_i = 1$ denotes a drink-driving conviction. After a match is formed, the employer additionally observes the worker's on-the-job per-

[23] See Appendix Table A.4 for the corresponding classifications of industries by driving-requirement intensity.

[24] The framework combines elements of search and matching (Mortensen and Pissarides, 1999) with statistical discrimination and employer learning (Phelps, 1972; Arrow, 1973; Altonji and Pierret, 2001; Lange, 2007). It is also related to the broader literature on the labor-market consequences of criminal records (Becker, 1968; Pager, 2003; Agan and Starr, 2018; Hjalmarsson et al., 2024).

[25] This term ensures that only a subset of workers satisfy the hiring condition described below. See also a formal discussion in Online Appendix D.1.

Screening intensity	Full sample		European		Non-European		Male		Female	
	months to/from last court date									
	$-24 \leq t \leq -2$	$0 \leq t \leq +24$	$-24 \leq t \leq -2$	$0 \leq t \leq +24$	$-24 \leq t \leq -2$	$0 \leq t \leq +24$	$-24 \leq t \leq -2$	$0 \leq t \leq +24$	$-24 \leq t \leq -2$	$0 \leq t \leq +24$
Low	0.003	-0.019**	0.000	-0.007	0.006	-0.034***	0.008	-0.006	-0.003	-0.035***
	(0.007)	(0.008)	(0.010)	(0.011)	(0.012)	(0.013)	(0.010)	(0.011)	(0.012)	(0.013)
Medium/High	0.012	-0.048***	0.018	-0.044***	0.004	-0.055***	0.016	-0.060***	0.008	-0.031**
	(0.008)	(0.010)	(0.011)	(0.013)	(0.014)	(0.016)	(0.011)	(0.014)	(0.013)	(0.015)
Unique individuals	1,824		1,023		801		1,011		813	

Notes: Each outcome equals one when an individual is employed in month t following entry from non-employment or a move to a different five-digit ANZSIC 2006 industry, and the new industry belongs to the indicated screening-intensity category. Screening intensity is classified at the ANZSIC division-subdivision level using formal regulatory requirements, safety-sensitive activities, responsibility for vulnerable populations, positions of public trust, and unsupervised access to clients; medium- and high-screening industries are combined. Entries are averages of the event-study coefficients over the pre-conviction period ($-24 \leq t \leq -2$) and the post-conviction period ($0 \leq t \leq 24$). Estimates use the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator; the omitted event month is $t = -1$. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table 5: Decomposing employment effects by industry screening intensity (averages)

formance. Hiring decisions therefore depend on imperfect pre-match information, whereas retention and subsequent wage decisions can increasingly incorporate employer-specific information accumulated within the match.

We assume that, conditional on the other information available at hiring, a criminal record lowers the employer's assessment of the expected value of the match. For a prospective worker,

$$\mathbb{E}[y_i \mid s_i, R_i = 1] = \mathbb{E}[y_i \mid s_i, R_i = 0] - \delta, \qquad \delta > 0. \tag{6}$$

The parameter δ should be interpreted as a reduced-form perceived penalty associated with the criminal record. It may reflect employers' beliefs about productivity, reliability, absenteeism, or other job-relevant attributes. Importantly, the model does not require the conviction to reduce the worker's underlying productivity y_i; it requires only that the record affect employers' beliefs when evaluating a prospective match.

Hiring. Firms post vacancies and meet workers through a matching process. Upon meeting worker i, the firm decides whether to form a match. Let the value of a newly formed match be

$$J_i^H = \mathbb{E}[y_i \mid s_i, R_i] - w_i, \tag{7}$$

where wages are determined through Nash bargaining:

$$w_i = (1 - \beta)b + \beta\mathbb{E}[y_i \mid s_i, R_i], \tag{8}$$

with b denoting the worker's outside option and $\beta \in (0, 1)$ the worker's bargaining weight. Substituting (8) into (7) yields

$$J_i^H = (1 - \beta)\left(\mathbb{E}[y_i \mid s_i, R_i] - b\right). \tag{9}$$

Thus, a match forms if J_i^H in Equation (9) is nonnegative, implying the hiring condition:

$$\mathbb{E}[y_i \mid s_i, R_i] \geq b. \tag{10}$$

Using Equation (6), the hiring condition for an applicant with a criminal record becomes

$$\mathbb{E}[y_i \mid s_i, R_i = 0] - \delta \geq b. \tag{11}$$

Relative to an otherwise identical applicant without a criminal record, the convicted applicant must therefore have a sufficiently favorable pre-hire signal to offset the perceived penalty δ. Provided there is positive density around the hiring threshold, increasing δ re-

duces the measure of convicted applicants satisfying the hiring condition and hence lowers their probability of forming a new employment match. A formal derivation is provided in Online Appendix D.1.

Employer learning and incumbent workers. Once a match is formed, the employer begins to observe worker performance directly. Let $p_{i\tau}$ denote the employer-specific performance information accumulated after τ periods of tenure. We represent the declining informational importance of the criminal record by

$$\mathbb{E}[y_i \mid s_i, p_{i\tau}, R_i = 1] = \mathbb{E}[y_i \mid s_i, p_{i\tau}, R_i = 0] - \rho_\tau \delta, \tag{12}$$

where

$$0 \leq \rho_\tau \leq 1, \qquad \frac{\partial \rho_\tau}{\partial \tau} \leq 0.$$

Thus, ρ_τ measures the weight that the employer continues to place on the criminal record after observing the worker's performance for τ periods. Under sufficiently informative employer learning, $\rho_\tau \to 0$ as tenure increases.

The surplus from retaining an incumbent worker is therefore

$$J_{i\tau}^I = (1 - \beta)\left(\mathbb{E}[y_i \mid s_i, p_{i\tau}, R_i] - b\right). \tag{13}$$

Because the perceived penalty associated with the criminal record falls from δ at hiring to $\rho_\tau \delta$ within an existing match, the effect of the record on the employer's continuation decision becomes weaker as worker-specific information accumulates. The model therefore predicts a larger effect on the formation of new matches than on the destruction of existing matches.

Wages. For a given pre-hire signal s_i, Equation (8) implies that the criminal record changes the wage offer by

$$\begin{aligned} w_i(1) - w_i(0) &= \beta\left[\mathbb{E}[y_i \mid s_i, R_i = 1] - \mathbb{E}[y_i \mid s_i, R_i = 0]\right] \\ &= -\beta\delta. \end{aligned} \tag{14}$$

Thus, holding the applicant's other information fixed, the perceived penalty associated with a criminal record is partly reflected in the wage offer.

Observed wage differences among employed workers need not equal $-\beta\delta$, however. Two forces attenuate the comparison. First, the hiring condition in Equation (11) positively selects convicted workers into employment: among convicted applicants, only those with sufficiently favorable pre-hire signals form matches. Second, once a match is formed, employer learning reduces the perceived penalty from δ to $\rho_\tau \delta$. Consequently, the model can generate sizable effects on employment entry alongside substantially smaller wage differ-

ences among workers who are observed employed. Online Appendix D.1 formalizes these results.

Empirical Implications. The framework yields three predictions that map directly into our empirical analysis. First, a criminal record reduces the formation of new employment matches by lowering employers' assessments of applicants at the hiring stage. Second, the effect on existing matches is smaller because incumbent employers observe worker performance directly, reducing the informational weight placed on the criminal record. Third, wage differences conditional on employment may be considerably smaller than employment effects because hiring positively selects convicted applicants and employer learning attenuates the perceived record penalty within surviving matches.

These predictions are consistent with the central patterns in the data. Employment falls substantially following conviction, the decline operates primarily through reduced entry and the formation of new matches rather than separations from existing employers, and labor-income effects are comparatively modest. These patterns are not uniquely diagnostic of employer screening: search or mobility frictions may also reduce the formation of new matches, and the resulting periods of non-employment may have longer-run consequences through lost labor market experience. The additional evidence that new-match losses are larger in industries where criminal-record screening is more likely, but not in industries where driving is more important for work, provides further support for the screening interpretation. The model is therefore intended as a parsimonious rationalization of these patterns and of the employer-screening channel in particular, rather than as a structural interpretation of the estimated coefficients or an exhaustive account of the mechanisms through which conviction may affect labor market outcomes.

Employer wage-setting power. The baseline framework abstracts from employer wage-setting power. Online Appendix D.2 extends the model to allow search frictions to generate monopsonistic wage setting. This extension introduces an additional channel through which conviction may affect wages: if a criminal record reduces workers' outside options or mobility across employers, firms may capture a larger share of match surplus. The distinction is useful for interpreting the empirical results. Employment effects continue to arise from screening at the formation of new matches, whereas wage effects may reflect both employers' beliefs about match value and changes in workers' bargaining positions. We therefore interpret the comparatively modest wage response more cautiously than the employment and transition results.

6 Conclusion

This paper studies the labor market consequences of acquiring a relatively low-level criminal conviction carrying a non-custodial sanction. Using linked administrative data from New Zealand and variation in the timing of first drink-driving convictions, we find substantial

and persistent reductions in subsequent employment. The decline is driven primarily by lower entry into employment, with considerably smaller effects on exits from existing jobs and more modest effects on labor income. The employment penalty also persists beyond the typical period of driver's license suspension. Thus, even a relatively minor conviction can generate labor market consequences that extend well beyond the direct effects of the formal sanction.

The richness of the linked employer–employee data allows us to go beyond these average effects and identify where in workers' labor market trajectories the employment losses emerge. They are concentrated among workers seeking to enter employment or form matches with employers in different industries, while we find little change in retention by existing employers or mobility within narrowly defined industries. This concentration in new-match formation does not by itself identify the underlying mechanism, but several additional findings support an important role for employer screening. Reductions in new matches are larger in industries where criminal-record screening is more likely, but not in industries where driving is more important for work. Our conceptual framework rationalizes these patterns by showing why criminal-record information may matter most when prospective employers have limited information about applicants and become less important once employers observe worker performance directly. At the same time, the evidence leaves scope for search, mobility, and other worker-side channels to contribute to the employment response.

The effects are present across demographic groups, although their underlying margins differ. Point estimates indicate larger employment and earnings penalties for Non-European workers than for European workers, while men and women experience similar overall employment declines but different combinations of entry and exit responses. Because many of these differences are not statistically distinguishable across groups, this evidence is suggestive rather than conclusive of heterogeneous labor market penalties.

Our findings also speak to policies governing low-level criminal records. Diversion and other non-conviction pathways may prevent relatively minor offenses from creating persistent barriers to employment, consistent with evidence that avoiding formal prosecution can have important subsequent consequences (Agan et al., 2023). For individuals who already have a conviction, record-remediation policies such as New Zealand's Clean Slate scheme may reduce the extent to which older convictions remain relevant to employers (Dasgupta et al., 2025). Fair-chance hiring policies provide another possible approach, although the mixed evidence on Ban-the-Box cautions against assuming that restricting criminal-history information necessarily improves employment outcomes for all affected workers (Craigie, 2020; Doleac and Hansen, 2020; Shoag and Veuger, 2021; Rose, 2021; Burton and Wasser, 2025). Our estimates do not identify the welfare effects of any particular policy, but they indicate that interventions aimed at the formation of new employment matches are especially relevant to the margin on which the employment penalty emerges.

In sum, our results show that the economic consequences of a criminal conviction need

not operate primarily through incarceration or the destruction of existing jobs. For the relatively low-level offenses studied here, the formal sanction is temporary and incarceration is absent, yet the employment consequences persist and are concentrated at the formation of new employment relationships. Employer screening provides a natural explanation for this pattern and is supported by the cross-industry evidence, although other frictions in forming new matches may also contribute. A relatively minor criminal conviction can therefore affect workers well beyond the period of formal punishment by restricting subsequent access to employment opportunities. Understanding how these consequences arise is important for assessing the full economic cost of low-level criminal justice contact and for designing policies that balance the objectives of deterrence, employer information, and labor market reintegration.

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Data Access Disclaimer

Disclaimer for output produced from the IDI: The results in this paper are not official statistics. They have been created for research purposes from the Integrated Data Infrastructure (IDI), which is carefully managed by Stats NZ. For more information about the IDI please visit https://www.stats.govt.nz/integrated-data.

Disclaimer for Inland Revenue tax data: The results are based in part on tax data supplied by Inland Revenue to Stats NZ under the Tax Administration Act 1994 for statistical purposes. Any discussion of the 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.

Disclaimer for Census 2018 data: Access to the data used in this study was provided by Stats NZ under conditions designed to give effect to the security and confidentiality provisions of the Data and Statistics Act 2022. The results presented in this study are the work of the authors, not Stats NZ or individual data suppliers.

Online Appendix

Costly Misdemeanors: Labor Market Effects of Low-Level Drink-Driving Violations

Kabir Dasgupta, Marco Francesconi, Jonathan James, Linda Kirkpatrick, and Alexander Plum

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D. Conceptual Framework: Formal Results and Extension
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A Additional Tables and Figures

Individuals	Sample restriction
263,118	Individuals aged 20–55 with a conviction in 2007–2019
60,405	Individuals aged 20–55 with their first conviction in 2007–2019 (no prior court appearances)
30,903	Offense includes a recorded breath or blood alcohol level
26,697	Charged with a single offense
26,253	Offense code is 'A518' or 'A323' (breath or blood alcohol above the legal limit)
3,972	Alcohol level is between 400–500 μg per liter of breath or 80–100 mg per 100 ml of blood
1,824	Received a fine between NZ$400–500, a 180-day driver license suspension and having no hospitalization for an accident

Notes: The restrictions are applied cumulatively, and each row reports the number of individuals remaining after the stated restriction.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table A.1: Funnel table on sample preparation

	Drink-driving sample (unmatched)[b]	Non-convicted (unmatched)	Drink-driving sample[b] (matched)[b]	Non-convicted (matched)
Birthdate	1980q1	1979q2***	1980q1	1980q1
Male	0.556	0.449***	0.555	0.555
European only	0.560	0.680***	0.561	0.561
Employed	0.646	0.260***	0.645	0.645
Labor income (in $NZ)	3612.74	3286.34***	3583.86	3583.77
Unique individuals	1,824	427,560	1,818	1,818

Notes: The table reports covariate balance before and after exact matching. Treated individuals are first-time drink-driving offenders with breath alcohol concentrations between 400 and 500 μg per liter. Potential controls are New Zealand-born individuals with no recorded court conviction during the sample period. Controls are matched exactly on sex, ethnicity (European or non-European), birth year-quarter, and employment status 24 months before conviction; among employed individuals, monthly labor income must additionally lie within NZ$250 of the treated individual's income. Employment and labor income are measured 24 months before the actual or assigned conviction date. Differences in means are tested using OLS before matching and weighted OLS after matching. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table A.2: Covariate balance before and after exact matching

Classification	Subdivisions	%	Coding rationale	Industry groups (ANZSIC subdivision codes)
Low	32	37.2	Routine criminal-record screening is expected to be relatively uncommon. Roles are generally less regulated and less likely to involve formal licensing, vulnerable populations, public trust, cash handling, or unsupervised access to clients or property.	Agriculture, forestry, and fishing (A01–A05); manufacturing excluding safety-sensitive production (C11–C16, C19–C25); selected wholesale trade (F33–F34, F37–F38); retail trade (G41–G43); accommodation and food services (H44–H45); media and creative activities (J54–J57, R90).
Medium	41	47.7	Criminal-record checks are plausible for some roles, but are unlikely to be routine across the subdivision. Common features include safety-sensitive work, site-access requirements, client or property access, and selected regulated occupations.	Mining (B06–B10); safety-sensitive manufacturing (C17–C18); utilities (D26–D29); construction (E30–E32); vehicle and grocery wholesale trade (F35–F36); automotive and fuel retail (G39–G40); transport and logistics (I46–I48, I50–I53); information services (J58–J60); rental and real estate services (L66–L67); professional and administrative services (M69–M70, N72–N73); tertiary and adult education (P81–P82); heritage and recreation (R89, R91); other services (S94–S96).
High	13	15.1	Screening is expected to be comparatively common because work often involves formal regulation or licensing, vulnerable populations, public safety or trust, financial probity, or substantial responsibility for clients and property.	Air and space transport (I49); finance and insurance (K62–K64); public administration, defence, and public safety (O75–O77); school education (P80); health care and social assistance (Q84–Q87); gambling (R92).

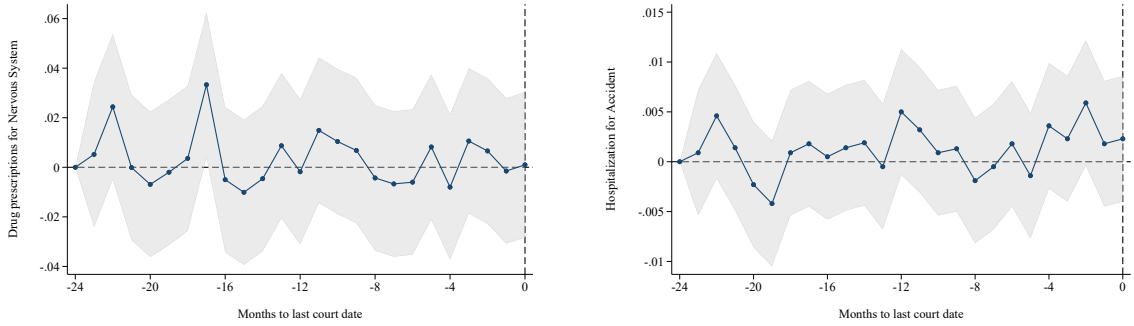
Notes: Authors' classification of the 86 ANZSIC 2006 subdivisions. Screening intensity is a theory-informed assessment of the likelihood that employers use criminal-record information during recruitment; it does not measure whether a particular employer undertook a check. Medium- and high-screening subdivisions are combined in the empirical analysis because only 13 subdivisions are classified as high screening.
Source: Authors' coding using ANZSIC 2006 (Revision 2.0) subdivision definitions.

Table A.3: Industry-level coding of expected criminal-record screening intensity

Classification	Subdivisions	%	Coding rationale	Industry groups (ANZSIC subdivision codes)
Low	51	59.3	Work is generally factory-, office-, venue-, facility-, or household-based. Where transport is central, it is commonly water-, rail-, or air-based rather than dependent on road driving.	Aquaculture and fishing (A02, A04); manufacturing (C11–C25); fuel and other retail trade (G40–G42); accommodation and food services (H44–H45); rail, water, air transport, and storage (I47–I49, I53); media and information services (J54–J57, J59–J60); finance and insurance (K62–K64); professional and administrative services (M69–M70, N72); public administration and defence (O75–O76); education (P80–P82); health care and residential services (Q84–Q86); arts and recreation (R90–R92); personal and household services (S95–S96).
High	35	40.7	Work commonly involves road transport, delivery, field work, travel between sites, vehicle operation, or mobile service provision.	Agriculture and forestry (A01, A03, A05); mining (B06–B10); utilities (D26–D29); construction (E30–E32); wholesale trade (F33–F38); automotive and non-store retail (G39, G43); transport, courier, and transport-support services (I46, I50–I52); telecommunications (J58); rental, property, and support services (L66–L67, N73); public safety (O77); social assistance (Q87); heritage activities (R89); repair and maintenance (S94).

Notes: Authors' classification of the 86 ANZSIC 2006 subdivisions. The measure captures the expected relevance of driving *for work*, rather than commuting or an individual worker's actual licence status.
Source: Authors' coding using ANZSIC 2006 (Revision 2.0) subdivision definitions.

Table A.4: Industry-level coding of expected driving-for-work relevance



(a) Drug prescriptions for Nervous System (b) Hospitalization for Accident

Notes: The panels report differences in pre-conviction health-outcome dynamics between individuals convicted before 2015 and those convicted in 2015 or later. Estimates are from fixed-effects linear regressions in which the early-conviction indicator is interacted with event month; $t = -24$ is the omitted event month, and shaded areas show 95% confidence intervals. Nervous-system-related medications include analgesics, antidepressants, antipsychotics, anxiolytics, stimulants and ADHD treatments, and treatments for substance dependence. Accident-related hospital admissions are identified from the Ministry of Health public and private hospital-events data.

Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Figure A.1: Health outcomes between early and late convicted, event study regression

B Additional Baseline Results

	months to/from last court date	
	$-24 \leq t \leq -2$	$0 \leq t \leq +24$
Employment	0.014*	-0.000
	(0.009)	(0.010)
Entering employment	-0.025*	0.003
	(0.015)	(0.017)
Exiting employment	-0.006	-0.022**
	(0.010)	(0.010)
Wages & Salaries	59.967	-68.405
	(49.721)	(53.603)
Unique individuals	1,446	

Notes: The placebo conviction date is defined by shifting each individual's final court date 24 months earlier. The sample is restricted to individuals aged at least 20 at the placebo date. Entries are averages of the event-study coefficients over the pre-placebo period ($-24 \leq t \leq -2$) and the post-placebo period ($0 \leq t \leq 24$). Estimates use the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator; the omitted event month is $t = -1$. Entering employment is estimated among individuals who were non-employed at $t = -24$; exiting employment is estimated among individuals who were employed at $t = -24$. Wages and salaries are monthly amounts among employed individuals. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table B.1: Regression results on placebo regression

B.1 Decomposition

Labor market transitions — To investigate mechanisms underlying employment losses, we exploit New Zealand's Employer Monthly Schedule (EMS) tax data that also includes unique employer identifiers and Australian and New Zealand Standard Industrial Classification (ANZSIC) codes. Five-digit ANZSIC codes represent detailed industry categories (e.g., specific retail types), while one- or two-digit codes represent broader groupings (e.g., manufacturing, services). This enables us to distinguish whether post-conviction transitions involve staying with the same employer, moving within the same narrowly defined industry, or moving across broader industry boundaries.

Using EMS employer identifiers and ANZSIC codes, we construct six labor market transition states between month $t-24$ and month t. State 0: not employed at t regardless of employment status at $t-24$ (mirroring our baseline findings). State 1: same employer at both time points $t-24$ and t. State 2: changed employers between $t-24$ and t but remained in the identical five-digit ANZSIC industry. State 3: changed employers between $t-24$ and t, and switched to different five-digit but same one-digit ANZSIC industry. State 4: changed employers between $t-24$ and t, and switched to a different one-digit ANZSIC industry, reflecting cross-industry mobility. State 5: not employed at $t-24$, but employed at t. These states are mutually exclusive and collectively exhaustive among the employed, representing entry into employment. By construction, these six states are mutually exclusive and collectively exhaustive, so that each individual belongs to exactly one state in each month

Overall, Table B.2 reveals that the 6.8 percentage-point employment decline is not driven by job separation from existing employers. We find no statistically significant effects on the probability of someone remaining with the same employer (State 1) or transitioning to new employers within the identical five-digit industry (State 2), indicating convictions do not increase dismissal or reduce retention among incumbent workers. Instead, employment losses concentrate in reduced mobility across broader industry boundaries and reduced entry from non-employment. Convictions reduce transitions to new employers within the same one-digit but different five-digit industry by 1.8 percentage points (State 3, 26% of total effect), transitions to different one-digit industries by 1.9 percentage points (State 4, 28%), and entry from non-employment by 3.0 percentage points (State 5, 44%). These three states almost fully account for the 6.8 percentage-point increase in the probability of non-employment in month t, as revealed by State 0.

Our results point to employer screening, rather than productivity declines, as the primary channel through which drink-driving convictions impair employment. Existing employers and those hiring within narrowly defined industries possess direct information about worker productivity through observable skills and experience, rendering them less sensitive to criminal records. Conversely, employers hiring across broader industry boundaries face greater information asymmetries, amplifying their reliance on criminal record screening

during recruitment. This pattern is consistent with labor market signaling theory and inconsistent with human capital depreciation or employer-initiated dismissal.

State	Description	months to/from last court date $-24 \leq t \leq -2$	$0 \leq t \leq +24$
0	Not employed in month t	-0.009	0.068***
		(0.008)	(0.009)
1	Employed in month t and with the same employer as in month $t-24$	-0.003	0.000
		(0.008)	(0.009)
2	Employed in month t, with a different employer than in month $t-24$,	-0.003	-0.001
	but in the *identical* 5-digit ANZSIC industry	(0.005)	(0.005)
3	Employed in month t, with a different employer than in month $t-24$,	0.004	-0.018**
	in a different 5-digit ANZSIC industry but the same 1-digit ANZSIC industry	(0.006)	(0.007)
4	Employed in month t, with a different employer than in month $t-24$,	0.013*	-0.019**
	and in a different 1-digit ANZSIC industry	(0.007)	(0.008)
5	Employed in month t but not employed in month $t-24$ (entry into employment)	-0.001	-0.030***
		(0.008)	(0.009)
	Unique individuals	1,824	

Notes: The six labor-market states are mutually exclusive and collectively exhaustive. Employment status, employer, and industry in month t are compared with their values in month $t=-24$; industries follow the five-digit and one-digit ANZSIC 2006 classifications shown in the table. Entries are averages of the event-study coefficients over the pre-conviction period ($-24 \leq t \leq -2$) and the post-conviction period ($0 \leq t \leq 24$). Estimates use the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator; the omitted event month is $t=-1$. Standard errors are reported in parentheses. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.10$.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table B.2: Decomposing the employment effects (averages)

To examine the earnings decline, we perform a complementary decomposition using broader transition categories given limited sample sizes in the more granular groups. We distinguish between (i) individuals who remain with the same employer or move to a different employer within the identical five-digit ANZSIC industry, and (ii) individuals who transition to a different five-digit industry (either within or across one-digit ANZSIC groups) or enter employment from non-employment.

Table B.3 shows no significant earnings effect for workers remaining with the same employer or moving within identical industries. We see statistically insignificant decline of NZ$29.5 per month, indicating that convictions do not notably reduce earnings when productivity remains directly observable to employers. By contrast, earnings decrease substantially (NZ$204 per month) for workers seeking undergoing broader job reallocation or re-entry into employment. These findings reinforce that drink-driving convictions impair labor market outcomes primarily by restricting access to new employment matches where employers lack direct productivity information, rather than through wage cuts or productivity declines within existing employment relationships.

Industry screening intensity — To examine whether employment losses from job reallocation concentrate in industries where criminal-record screening is more likely, we constructed an industry screening classification. We reviewed each ANZSIC 2006 division-subdivision and assigned it to low-, medium-, or high-screening categories based on industry characteristics making screening more likely–formal regulation, safety-sensitive tasks, responsibility for vulnerable populations, public trust, or unsupervised access to clients.[i]

[i]The classification should be interpreted a theory-informed measure of likely screening intensity rather than a direct measure of employers' screening practices.

State Description	months to/from last court date $-24 \leq t \leq -2$	 $0 \leq t \leq +24$
Employed at same employer or identical 5-digit industry	101.691	-29.500
	(74.158)	(64.316)
Employed in different industry or entering employment	12.130	-203.804***
	(56.633)	(72.078)
Unique individuals	1,824	

Notes: The two categories aggregate the labor-market states defined in Table B.2. labor income is monthly income from wages and salaries among individuals employed in the indicated category. Entries are averages of the event-study coefficients over the pre-conviction period ($-24 \leq t \leq -2$) and the post-conviction period ($0 \leq t \leq 24$). Estimates use the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator; the omitted event month is $t = -1$. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table B.3: Decomposing the earnings effects (averages)

High-screening industries include finance, public administration, education, and health. Medium-screening industries include mining, utilities, construction, and transport. Low-screening industries comprise primary production, manufacturing, retail, and accommodation. Of 86 ANZSIC subdivisions, 32 are low screening, 41 medium, and 13 high screening. We combine medium- and high-screening industries given few high-screening subdivisions.

We reconstruct labor-market states from Table B.2, keeping States 0–2 unchanged. We replace States 3–5 with two new states defined by screening intensity: State 3 denotes employment in low-screening industries following job reallocation or entry; State 4 is defined analogously for medium/high-screening industries.

Table 5 shows no differential pre-conviction trends. Convictions reduce employment in low-screening industries by 1.9 percentage points and in medium/high-screening industries by 4.8 percentage points–nearly 70% of the total decline from the pre-conviction level.

By ethnicity, Europeans experience a 4.4 percentage-point decline in medium/high-screening industries (90% of their total decline estimated in Table 3), while non-Europeans experience significant declines in both low-screening (3.4 percentage points) and medium/high-screening industries (5.5 percentage points, 60% of total decrease in Table 3). While medium/high-screening industries account for substantial employment losses in both groups, non-Europeans experience additional significant losses in low-screening industries.

By gender, men's decline concentrates in medium/high-screening industries (6.0 percentage points, 94% of total estimated reduction in Table 3), while women experience significant declines in both low-screening (3.5 percentage points) and medium/high-screening industries (3.1 percentage points, accounting for 44% of total reduction in Table 3).

Driving requirements — We next examine whether employment losses from job reallocation and entry concentrate in industries where driving is central to work. We reviewed each ANZSIC 2006 division-subdivision and classified it according to whether driving is likely to be an important feature of work. This coding captures driving *for work* rather

than commuting and is not a direct measure of whether a particular worker was required to hold a driver's license.

Industries were classified as high driving requirement where work commonly involves road transport, delivery, field work, site-to-site travel, vehicle operation, or mobile service provision–including agriculture, mining, utilities, construction, wholesale trade, road transport, postal services, and repair. Low driving requirement industries include those where work is factory-, office-, or site-based, or where transport is primarily water-, rail-, or air-based–such as manufacturing, retail, accommodation, finance, education, health care, and creative activities. Of 86 ANZSIC subdivisions, 35 have high and 51 have low driving requirements.

As in the screening analysis, we reconstruct States 3–5 from Table B.2, keeping States 0–2 unchanged. New State 3 denotes employment in low-driving-requirement industries following job reallocation or entry; State 4 is defined analogously for high-driving-requirement industries.

Table B.4 shows employment declines are substantially larger in low-driving-requirement industries. Following conviction, employment in these industries falls by 4.9 percentage points compared with 1.9 percentage points in high-driving-requirement industries. These sum to the 6.8 percentage-point increase in non-employment from Table B.2, with 72% attributable to low-driving-requirement industries.

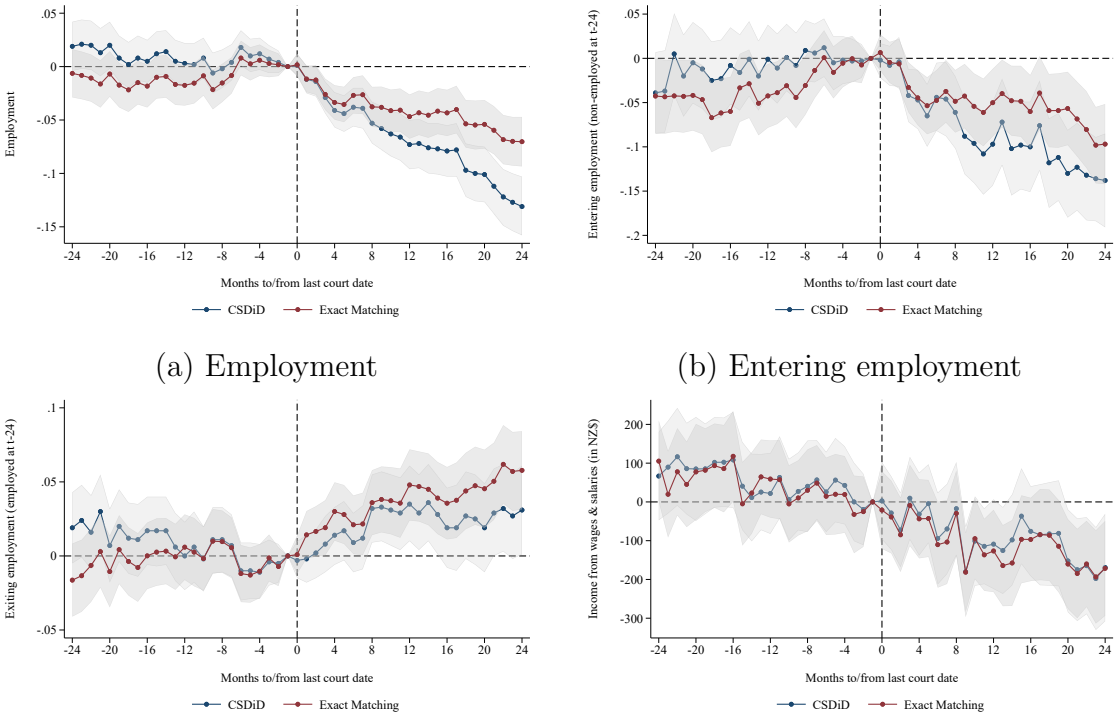
This pattern provides little support for driving restrictions as the primary mechanism. If temporary inability to drive were central, the largest decline would occur in high-driving-requirement industries. Instead, the decline is more than twice as large in low-driving-requirement industries, consistent with employment penalties primarily reflecting hiring barriers following criminal record acquisition.

Driving requirements	months to/from last court date	
	$-24 \leq t \leq -2$	$0 \leq t \leq +24$
Low	0.017*	-0.049***
	(0.009)	(0.010)
High	-0.001	-0.019**
	(0.007)	(0.008)
Unique individuals	1,824	

Notes: Each outcome equals one when an individual is employed in month t following entry from non-employment or a move to a different five-digit ANZSIC 2006 industry, and the new industry belongs to the indicated driving-requirement category. The classification captures the importance of driving for work rather than commuting and is defined at the ANZSIC division-subdivision level; it is not a direct measure of whether an individual job requires a driver's license. Entries are averages of the event-study coefficients over the pre-conviction period ($-24 \leq t \leq -2$) and the post-conviction period ($0 \leq t \leq 24$). Estimates use the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator; the omitted event month is $t = -1$. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table B.4: Decomposing employment effects by industry driving requirements (averages)

C Further Results on Robustness



(a) Employment

(b) Entering employment

(c) Exiting employment

(d) Wages & Salaries

Notes: The blue series reports estimates from the Callaway and Sant'Anna (2021) staggered difference-in-differences design, with not-yet-convicted individuals as the comparison group. The maroon series reports estimates from the exact-matching event-study design in Equation (5). In both designs, the omitted event month is $t = -1$, and shaded areas show 95% confidence intervals. Entering employment is estimated among individuals who were non-employed at $t = -24$; exiting employment is estimated among individuals who were employed at $t = -24$. Wages and salaries are monthly amounts among employed individuals. The black dashed line marks the final court date ($t = 0$), and the grey dashed line marks the average offense date relative to the final court date.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Figure C.1: Economic outcomes before and after the first drink-driving conviction, event study regression—CSDiD and exact matching

	With controls months to/from last court date	
	$-24 \leq t \leq -2$	$0 \leq t \leq +24$
Employment	-0.010	-0.041***
	(0.008)	(0.008)
Entering employment	-0.034**	-0.049***
	(0.014)	(0.014)
Exiting employment	-0.002	0.037***
	(0.008)	(0.009)
labor income	42.761	-107.673***
	(40.058)	(38.558)
Unique individuals	3,636	

Notes: Entries are averages of the interaction coefficients from the fixed-effects event-study specification in Equation (5) over the pre-conviction period ($-24 \leq t \leq -2$) and the post-conviction period ($0 \leq t \leq 24$). Convicted individuals are matched to New Zealand-born individuals with no recorded court conviction during the sample period. Matching is exact on sex, ethnicity (European or non-European), birth year-quarter, and employment status 24 months before conviction; among employed individuals, monthly labor income must additionally lie within NZ$250 of the treated individual's income. The regression uses the matching weights and includes individual and calendar-quarter fixed effects; the omitted event month is $t = -1$. Entering employment is estimated among individuals who were non-employed at $t = -24$; exiting employment is estimated among individuals who were employed at $t = -24$. labor income is monthly income from wages and salaries among employed individuals. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table C.1: Regression results (averages) for exact matching

	Post Dec 2014		Pre Dec 2014		Pre 2018		Post 2012	
	months to/from last court date							
	$-24 \leq t \leq -2$	$0 \leq t \leq +24$	$-24 \leq t \leq -2$	$0 \leq t \leq +24$	$-24 \leq t \leq -2$	$0 \leq t \leq +24$	$-24 \leq t \leq -2$	$0 \leq t \leq +24$
Employment	0.012	-0.072***	-0.007	-0.049***	0.007	-0.064***	0.013	-0.079***
	(0.013)	(0.017)	(0.010)	(0.012)	(0.008)	(0.010)	(0.011)	(0.014)
Entering employment	0.017	-0.088**	-0.016	-0.084***	-0.014	-0.088***	0.006	-0.103***
	(0.023)	(0.035)	(0.020)	(0.020)	(0.016)	(0.016)	(0.021)	(0.026)
Exiting employment	-0.011	0.029*	0.015	0.023*	0.008	0.022**	-0.008	0.032**
	(0.015)	(0.016)	(0.010)	(0.013)	(0.009)	(0.010)	(0.012)	(0.014)
Wages & Salaries	37.737	-38.824	23.557	-72.384	60.095	-87.689**	70.526	-66.067
	(66.028)	(75.999)	(53.557)	(54.738)	(45.570)	(43.542)	(57.552)	(61.157)
Unique individuals	711		1,101		1,530		939	

Notes: Each pair of columns restricts the sample according to the timing of the conviction shown in the column heading. Entries are averages of the event-study coefficients over the pre-conviction period ($-24 \leq t \leq -2$) and the post-conviction period ($0 \leq t \leq 24$). Estimates use the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator; the omitted event month is $t = -1$. Entering employment is estimated among individuals who were non-employed at $t = -24$; exiting employment is estimated among individuals who were employed at $t = -24$. Wages and salaries are monthly amounts among employed individuals. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table C.2: Regression results for different time period (averages)

	months to/from last court date	
	$-24 \leq t \leq -2$	$0 \leq t \leq +24$
Employment	0.005	-0.066***
	(0.008)	(0.009)
Entering employment	-0.009	-0.082***
	(0.016)	(0.017)
Exiting employment	0.012	0.023**
	(0.008)	(0.009)
Wages & Salaries	60.549	-60.244
	(43.875)	(42.658)
Unique individuals	1,566	

Notes: The sample excludes individuals who received another conviction within 24 months after their first drink-driving conviction. Entries are averages of the event-study coefficients over the pre-conviction period ($-24 \leq t \leq -2$) and the post-conviction period ($0 \leq t \leq 24$). Estimates use the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator; the omitted event month is $t = -1$. Entering employment is estimated among individuals who were non-employed at $t = -24$; exiting employment is estimated among individuals who were employed at $t = -24$. Wages and salaries are monthly amounts among employed individuals. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table C.3: Regression results for individuals without a second conviction within 24 months (averages)

	months to/from last court date	
	$-24 \leq t \leq -2$	$0 \leq t \leq +24$
Same employer	-0.011	0.008
	(0.013)	(0.016)
Wage change	60.426	35.849
	(42.556)	(47.631)
Low paying industry	0.012	0.022**
	(0.008)	(0.009)
Enrolled in tertiary education	0.010*	-0.009
	(0.006)	(0.007)
Unique individuals	1,824	

Notes: "Same employer" is estimated among individuals employed at both $t = -24$ and t and equals one when the employer is unchanged. "Wage change" is the absolute change in monthly wage and salary income between $t = -24$ and t, among individuals employed in both months. "Low-paying industry" is estimated among individuals employed at t and equals one when mean earnings in the individual's five-digit ANZSIC 2006 industry are in the bottom quartile of the industry earnings distribution in that month. "Enrolled in tertiary education" equals one when the individual is enrolled at a university, polytechnic, private training establishment, college of education, wānanga, or other tertiary education provider in month t. Entries are averages of the event-study coefficients over the pre-conviction period ($-24 \leq t \leq -2$) and the post-conviction period ($0 \leq t \leq 24$). Estimates use the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator; the omitted event month is $t = -1$. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table C.4: Regression results—alternative outcomes

	Total sample		European		Non-European		Male		Female	
	Years to/from last court date									
	-4 to -2	0 to +4	-4 to -2	0 to +4	-4 to -2	0 to +4	-4 to -2	0 to +4	-4 to -2	0 to +4
Total income	-886.107*	-6505.147***	-961.472	-6417.746***	-858.766	-6714.319***	-1736.942**	-7241.413***	100.751	-5212.879***
	(499.660)	(678.068)	(751.751)	(979.249)	(623.813)	(914.611)	(800.269)	(1054.951)	(534.534)	(812.061)
Being self-employed	0.008	0.006	0.004	0.008	0.013**	0.002	0.011	-0.001	0.004	0.015
	(0.006)	(0.008)	(0.008)	(0.012)	(0.007)	(0.009)	(0.009)	(0.012)	(0.007)	(0.010)
Income from self-employment	-6050.900	-8120.156*	-7264.426*	-8431.804*	2035.026	-3651.483	-5752.297	-4653.427	-4377.172	-17595.53*
	(3965.865)	(4656.602)	(4322.561)	(5087.395)	(8350.924)	(9070.015)	(4805.623)	(5282.474)	(5658.365)	(9100.328)
Unique individuals	1,824		1,023		801		1,011		813	

Notes: Outcomes are measured by financial year, running from April to March. Total income comprises wages and salaries, self-employment and investment income, income-support payments, tax credits, student allowances, paid parental leave, ACC income support, and other taxable income. "Self-employed" equals one when annual self-employment income is negative or exceeds NZ$5,000. Income from self-employment is reported for individuals meeting the same criterion. Entries are averages of the event-study coefficients over the pre-conviction period ($-4 \leq t \leq -2$) and the post-conviction period ($0 \leq t \leq 4$). Estimates use the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator; the omitted financial year is $t = -1$. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table C.5: Regression results on annual earnings and income from self-employment



(a) Employment

(b) Entering employment

(c) Exiting employment

(d) Wages & Salaries

Notes: The panels report event-study estimates separately for European and non-European individuals using the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator. The omitted event month is $t = -1$, and shaded areas show 95% confidence intervals. Entering employment is estimated among individuals who were non-employed at $t = -24$; exiting employment is estimated among individuals who were employed at $t = -24$. Wages and salaries are monthly amounts among employed individuals. The black dashed line marks the final court date ($t = 0$), and the grey dashed line marks the average offense date relative to the final court date.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Figure C.2: Economic outcomes before and after the first drink-driving conviction, event study regression by ethnicity

	Aged <30		Aged <30 with no prior enrolment†		Aged <30 with enrolment‡	
	months to/from last court date					
	$-24 \leq t \leq -2$	$0 \leq t \leq +24$	$-24 \leq t \leq -2$	$0 \leq t \leq +24$	$-24 \leq t \leq -2$	$0 \leq t \leq +24$
Employment	0.031***	-0.092***	0.050**	-0.118***	0.021	-0.081***
	(0.012)	(0.014)	(0.020)	(0.024)	(0.014)	(0.017)
Entering employment	0.004	-0.097***	0.052	-0.088***	-0.030	-0.100***
	(0.022)	(0.025)	(0.038)	(0.038)	(0.029)	(0.033)
Exiting employment	-0.002	0.032**	-0.003	0.076***	0.002	0.016
	(0.013)	(0.015)	(0.025)	(0.027)	(0.016)	(0.018)
Wages & Salaries	104.413**	-176.129***	167.253**	-252.557***	68.407	-144.774**
	(51.777)	(56.120)	(81.340)	(97.345)	(66.514)	(69.469)
Unique individuals	951		306		645	

Notes: The sample is restricted to individuals younger than 30 on the offense date. † The "no prior enrolment" group comprises individuals with no university or polytechnic enrolment before conviction. ‡ The "prior enrolment" group comprises individuals enrolled at a university or polytechnic before conviction. Entries are averages of the event-study coefficients over the pre-conviction period ($-24 \leq t \leq -2$) and the post-conviction period ($0 \leq t \leq 24$). Estimates use the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator; the omitted event month is $t = -1$. Entering employment is estimated among individuals who were non-employed at $t = -24$; exiting employment is estimated among individuals who were employed at $t = -24$. Wages and salaries are monthly amounts among employed individuals. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Table C.6: Regression results and enrollment in tertiary education (averages)



(a) Employment

(b) Entering employment

(c) Exiting employment

(d) Wages & Salaries

Notes: The panels report event-study estimates separately for men and women using the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator. The omitted event month is $t = -1$, and shaded areas show 95% confidence intervals. Entering employment is estimated among individuals who were non-employed at $t = -24$; exiting employment is estimated among individuals who were employed at $t = -24$. Wages and salaries are monthly amounts among employed individuals. The black dashed line marks the final court date ($t = 0$), and the grey dashed line marks the average offense date relative to the final court date.

Source: Statistics New Zealand's Integrated Data Infrastructure (IDI) and authors' calculations.

Figure C.3: Economic outcomes before and after the first drink-driving conviction, event study regression by sex

D Conceptual Framework: Formal Results and Extension

D.1 Proofs

The first result formalizes how a criminal record can reduce the probability of forming a new employment match by raising the quality of the pre-hire signal required for hiring.

Proposition 1 (Hiring probability and criminal records)**.** *Suppose firms hire worker i if and only if expected match surplus is nonnegative:*

$$J_i^H = (1-\beta)\left(\mathbb{E}[y_i \mid s_i, R_i] - b\right) \geq 0,$$

where $\beta \in (0,1)$ and b denotes the worker's outside option. Let

$$m(s_i) \equiv \mathbb{E}[y_i \mid s_i, R_i = 0]$$

be continuous and strictly increasing in the pre-hire signal s_i. Suppose that a criminal record lowers the employer's assessment of expected match value by $\delta > 0$:

$$\mathbb{E}[y_i \mid s_i, R_i = 1] = m(s_i) - \delta.$$

Then there exists a signal threshold $\tilde{s}(R_i)$ such that worker i is hired if and only if

$$s_i \geq \tilde{s}(R_i),$$

with

$$\tilde{s}(1) > \tilde{s}(0).$$

If the distribution of s_i has positive density around the hiring threshold, then

$$P(\text{hire}_i \mid R_i = 1) < P(\text{hire}_i \mid R_i = 0),$$

holding the distribution of pre-hire signals fixed, and

$$\frac{\partial P(\text{hire}_i \mid R_i = 1)}{\partial \delta} < 0.$$

Proof. Because $1 - \beta > 0$, a match forms if and only if

$$\mathbb{E}[y_i \mid s_i, R_i] \geq b.$$

For an applicant without a criminal record, the hiring condition is

$$m(s_i) \geq b.$$

Since $m(\cdot)$ is continuous and strictly increasing, there exists a unique threshold

$$\tilde{s}(0) = m^{-1}(b)$$

such that the worker is hired if and only if

$$s_i \geq \tilde{s}(0).$$

For an applicant with a criminal record, the hiring condition becomes

$$m(s_i) - \delta \geq b,$$

or equivalently

$$m(s_i) \geq b + \delta.$$

The corresponding threshold is therefore

$$\tilde{s}(1) = m^{-1}(b + \delta).$$

Because $\delta > 0$ and $m(\cdot)$ is strictly increasing,

$$\tilde{s}(1) > \tilde{s}(0).$$

Let $G(s)$ denote the distribution function of the pre-hire signal. Holding this distribution fixed across criminal-record status, hiring probabilities are

$$P(\text{hire}_i \mid R_i = r) = 1 - G(\tilde{s}(r)) \,.$$

It follows immediately that

$$P(\text{hire}_i \mid R_i = 1) < P(\text{hire}_i \mid R_i = 0).$$

Finally,

$$\frac{\partial \tilde{s}(1)}{\partial \delta} = \frac{1}{m'(\tilde{s}(1))} > 0.$$

If $g(\cdot)$ denotes the density of s_i, then

$$\frac{\partial P(\text{hire}_i \mid R_i = 1)}{\partial \delta} = -g(\tilde{s}(1)) \frac{\partial \tilde{s}(1)}{\partial \delta} < 0$$

whenever $g(\tilde{s}(1)) > 0$. □

The second result characterizes the implications of this hiring threshold for selection into employment and shows how employer learning attenuates the informational role of the criminal record within continuing matches.

Proposition 2 (Wages, selection, and employer learning)**.** *Suppose wages are determined through Nash bargaining:*

$$w_{i\tau} = (1 - \beta)b + \beta \mathbb{E}[y_i \mid \Omega_{i\tau}],$$

where $\Omega_{i\tau}$ denotes the information available to the employer.

For prospective workers,

$$\Omega_{i0} = (s_i, R_i),$$

whereas for incumbent workers,

$$\Omega_{i\tau} = (s_i, p_{i\tau}, R_i), \qquad \tau > 0,$$

where $p_{i\tau}$ denotes employer-specific information accumulated during the match. Suppose that

$$\mathbb{E}[y_i \mid s_i, R_i = 1] = \mathbb{E}[y_i \mid s_i, R_i = 0] - \delta, \qquad \delta > 0,$$

and that employer learning attenuates the perceived penalty associated with the criminal record:

$$\mathbb{E}[y_i \mid s_i, p_{i\tau}, R_i = 1] = \mathbb{E}[y_i \mid s_i, p_{i\tau}, R_i = 0] - \rho_\tau \delta,$$

where

$$0 \leq \rho_\tau \leq 1, \qquad \frac{\partial \rho_\tau}{\partial \tau} \leq 0.$$

Then:

1. *holding pre-hire information s_i fixed, the wage-offer difference associated with a criminal record at hiring is*

$$w_{i0}(1) - w_{i0}(0) = -\beta\delta;$$

2. *among convicted applicants, the higher hiring threshold positively selects workers with more favorable pre-hire signals into employment;*

3. *holding s_i and $p_{i\tau}$ fixed, the wage difference associated with the criminal record for an incumbent worker is*

$$w_{i\tau}(1) - w_{i\tau}(0) = -\beta\rho_\tau\delta,$$

whose absolute magnitude weakly declines with tenure whenever ρ_τ weakly declines with τ.

Proof. At the hiring stage, Nash bargaining implies

$$w_{i0}(R_i) = (1-\beta)b + \beta\mathbb{E}[y_i \mid s_i, R_i].$$

Hence, holding s_i fixed,

$$\begin{aligned} w_{i0}(1) - w_{i0}(0) &= \beta\left[\mathbb{E}[y_i \mid s_i, R_i = 1] - \mathbb{E}[y_i \mid s_i, R_i = 0]\right] \\ &= -\beta\delta. \end{aligned}$$

From Proposition 1, applicants with a criminal record face the higher signal threshold

$$\tilde{s}(1) > \tilde{s}(0).$$

Consequently, among applicants with $R_i = 1$, employment selects those with signals satisfying

$$s_i \geq \tilde{s}(1).$$

Because expected productivity $m(s_i)$ is increasing in s_i, hired convicted applicants are positively selected on the information used by employers relative to the population of convicted applicants. This selection can attenuate observed wage differences conditional on employment relative to the fixed-signal wage difference $-\beta\delta$.

For incumbent workers,

$$w_{i\tau}(R_i) = (1-\beta)b + \beta\mathbb{E}[y_i \mid s_i, p_{i\tau}, R_i].$$

Therefore, holding s_i and $p_{i\tau}$ fixed,

$$\begin{aligned} w_{i\tau}(1) - w_{i\tau}(0) &= \beta\left[\mathbb{E}[y_i \mid s_i, p_{i\tau}, R_i = 1] - \mathbb{E}[y_i \mid s_i, p_{i\tau}, R_i = 0]\right] \\ &= -\beta\rho_\tau\delta. \end{aligned}$$

Since $0 \leq \rho_\tau \leq 1$,

$$|w_{i\tau}(1) - w_{i\tau}(0)| = \beta\rho_\tau\delta \leq \beta\delta.$$

Moreover, if ρ_τ weakly decreases with tenure,

$$\frac{\partial}{\partial\tau}|w_{i\tau}(1) - w_{i\tau}(0)| = \beta\delta\frac{\partial\rho_\tau}{\partial\tau} \leq 0.$$

Thus, employer learning weakly attenuates the wage difference associated with the criminal record as information about worker performance accumulates. □

A higher hiring threshold induces positive selection into employment, while employer learning reduces the informational content of criminal records over time. The two mechanisms clarify why large effects on employment entry can coexist with comparatively modest wage differences among employed workers. The higher hiring threshold selects convicted applicants with more favorable pre-hire signals into employment, while employer learning progressively reduces the weight placed on the criminal record within continuing matches. The model therefore does not require wage effects conditional on employment to mirror the magnitude or persistence of the effects on new-match formation.

D.2 Extension: Employer Screening and Monopsony Power

This part extends the conceptual framework in Section 5 to allow employers to exercise wage-setting power. The purpose is to distinguish two channels through which a criminal record may affect labor market outcomes. First, criminal-record information may reduce employers' assessments of the value of prospective matches, thereby reducing hiring. Second, by weakening workers' outside employment opportunities, a criminal record may increase employers' wage-setting power over workers who are hired.

Setup. — As in the baseline framework, employers observe a pre-hire signal s_i together with a criminal-record indicator $R_i \in \{0, 1\}$. Let

$$m(s_i) \equiv \mathbb{E}[y_i \mid s_i, R_i = 0],$$

where $m(\cdot)$ is increasing in s_i. A criminal record lowers the employer's assessment of expected match value by $\delta > 0$:

$$\mathbb{E}[y_i \mid s_i, R_i = 1] = m(s_i) - \delta. \tag{D.1}$$

As in the baseline model, δ is a reduced-form perceived penalty associated with the criminal record and need not represent a decline in the worker's underlying productivity.

Unlike the baseline framework, firms now have wage-setting power. We represent the wage paid to a newly hired worker as

$$w_i(R_i) = \mathbb{E}[y_i \mid s_i, R_i] - \mu_i, \tag{D.2}$$

where $\mu_i \geq 0$ denotes the firm's monopsony markdown. The markdown captures the firm's ability to pay the worker less than expected match value because search frictions limit the worker's outside employment opportunities.

We allow the criminal record to increase this markdown:

$$\mu_i = \mu_0 + \phi R_i, \qquad \phi \geq 0. \tag{D.3}$$

When $\phi > 0$, a criminal record weakens the worker's outside opportunities and thereby increases the firm's wage-setting power.

Hiring. — To preserve the distinction between match formation and surplus division, suppose a new match forms only if its expected value exceeds the worker's outside option b and a hiring cost $c \geq 0$:

$$\mathbb{E}[y_i \mid s_i, R_i] - b - c \geq 0. \tag{D.4}$$

The monopsony markdown determines how the surplus from a viable match is divided but does not itself determine whether the match is technologically and economically viable.

For an applicant without a criminal record, the hiring condition is

$$m(s_i) \geq b + c.$$

For an applicant with a criminal record, it becomes

$$m(s_i) - \delta \geq b + c,$$

or

$$m(s_i) \geq b + c + \delta.$$

Hence, exactly as in the baseline framework, a criminal record raises the pre-hire signal required for a match to form. If $m(\cdot)$ is strictly increasing, the corresponding hiring thresholds satisfy

$$\tilde{s}(1) = m^{-1}(b + c + \delta) > m^{-1}(b + c) = \tilde{s}(0).$$

Thus, holding the distribution of pre-hire signals fixed, criminal-record screening reduces the probability of forming a new employment match.

Importantly, the monopsony parameter ϕ does not enter this hiring condition. In this stylized extension, δ governs the screening margin, whereas ϕ governs the division of surplus conditional on a match being formed. This separation allows the employment and wage channels to be distinguished transparently.

Employer Learning and Existing Matches. — Once a match is formed, the employer observes worker-specific performance information $p_{i\tau}$ accumulated over tenure τ. As in the baseline framework, suppose

$$\mathbb{E}[y_i \mid s_i, p_{i\tau}, R_i = 1] = \mathbb{E}[y_i \mid s_i, p_{i\tau}, R_i = 0] - \rho_\tau \delta, \tag{D.5}$$

where

$$0 \leq \rho_\tau \leq 1, \qquad \frac{\partial \rho_\tau}{\partial \tau} \leq 0.$$

The informational penalty associated with the criminal record therefore declines as the employer accumulates direct information about worker performance. Under sufficiently informative employer learning, $\rho_\tau \to 0$ as tenure increases.

Consequently, the screening component of the record becomes less important for the continuation of existing matches than for the formation of new matches. This prediction is unchanged by the presence of monopsonistic wage setting.

Wages. — Combining Equations (D.1)–(D.3), the wage offered to a newly hired worker can be written as

$$w_i(R_i) = \mathbb{E}[y_i \mid s_i, R_i] - \mu_0 - \phi R_i.$$

Holding the applicant's pre-hire signal s_i fixed, the wage-offer difference associated with a criminal record is therefore

$$\begin{aligned} w_i(1) - w_i(0) &= [\mathbb{E}[y_i \mid s_i, R_i = 1] - \mathbb{E}[y_i \mid s_i, R_i = 0]] - \phi \\ &= -\delta - \phi. \end{aligned} \tag{D.6}$$

The wage effect therefore contains two conceptually distinct components. The first, $-\delta$, reflects the employer's lower assessment of expected match value associated with the criminal record. The second, $-\phi$, captures the additional wage markdown arising from weaker outside opportunities.

Observed wage differences conditional on employment need not equal $-\delta - \phi$. As in the baseline model, convicted applicants face a higher hiring threshold and are therefore positively selected on the pre-hire information used by employers. In addition, employer learning attenuates the screening component of the wage difference within continuing matches. Accordingly, observed wage differences among employed workers combine screening, selec-

tion, employer learning, and potentially monopsonistic wage setting.

Implications. — The extension clarifies the interpretation of the employment and wage responses without changing the central prediction of the baseline framework. Criminal-record screening reduces the formation of new matches through the perceived penalty δ, while employer learning reduces the informational importance of the record within existing matches. Monopsony introduces an additional wage channel: if conviction weakens workers' outside opportunities, firms may impose a larger markdown on workers with criminal records.

The distinction is useful for interpreting our empirical findings, but it should not be viewed as separately identifying the structural parameters δ and ϕ. In the stylized formulation above, employment effects operate through the screening margin, whereas observed wage effects may reflect screening, endogenous selection into employment, employer learning, and monopsonistic wage setting. The comparatively modest wage response in our data therefore does not by itself establish the absence of monopsony power. Rather, the concentration of employment losses in the formation of new matches provides the more direct empirical motivation for the screening mechanism emphasized in the main text.