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# Costly Misdemeanours: Labour Market Effects of Low-Level Drink-Driving Violations

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October 29, 2026

**Abstract**

This paper estimates the causal effect of a drink-driving conviction on labour market outcomes. Using linked administrative data and variation in the timing of first convictions in a staggered difference-in-differences design, we compare individuals before and after conviction to those convicted later. We find employment falls by 6–7 percentage points and monthly earnings decline by around 2 percent. The effects are driven primarily by reduced job entry as opposed to job loss. Employment effects are largest in industries where employers are more likely to screen applicants for criminal convictions.

**JEL Codes:** K42, J24, J31, J15

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

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We thank John Abowd, Joe Altonji, Kitt Carpenter, Kevin Lang, Giovanni Mastrobuoni, and Thomas Siedler 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

Imprisonment is often regarded as the most severe consequence of criminal activity. Yet, many misdemeanours can bring about costs that extend beyond the courtroom. Drink-driving [1], a prevalent but often non-custodial offence, demonstrates this well: in the absence of incarceration, convictions result in a criminal record, with lasting consequences on labour market outcomes. This is most salient where background checks by employers are routine.

Several mechanisms may underpin these effects (Becker, 1968; Freeman, 1999). First, the stigma associated with a criminal record can create substantial barriers to hiring and limit access to employment opportunities (Uggen et al., 2014; Agan and Starr, 2017). Second, a conviction may weaken attachment to the labour market by reducing incentives to seek or retain employment (Visher et al., 2011; Sugie, 2018; Smith and Broege, 2020; Hjalmarsson et al., 2024; Ramakers et al., 2025). Lastly, causality may also operate in the opposite direction: weaker labour market prospects may increase risky behaviours, such as heavy alcohol consumption (MacDonald and Shields, 2001; Auld, 2005), thereby increasing the likelihood of alcohol-related arrests and convictions.[2] In this paper, we isolate and estimate the causal effect of a drink-driving conviction on subsequent labour market outcomes in New Zealand.

We link administrative data from the Ministry of Justice court records, Inland Revenue tax data, and the 2023 New Zealand Census to estimate the causal effect of a first-time drink-driving conviction on employment and earnings. Our empirical strategy exploits variation in the timing of conviction within a population of individuals who are all eventually convicted. We compare individuals before and after their first conviction with those convicted in a later period using a staggered difference-in-differences design (Callaway and Sant'Anna, 2021). This avoids comparing offenders with non-offenders, since time-invariant differences between the two groups could otherwise confound the relationship between conviction and labour market outcomes. In our approach, both treatment and comparison groups are drawn from the same population of eventual offenders, differing only in the timing of conviction. We complement this design with an exact matching approach, comparing convicted individuals to observationally similar members of the general public who do not receive a court conviction during the sample period.

[1] We follow the terminology used by the New Zealand Transport Agency and refer to this offence as "drink-driving" throughout the paper, consistent with usage in New Zealand and other Commonwealth jurisdictions. In the United States, the equivalent offence is typically referred to as "drunk driving" or "driving under the influence (DUI).

[2] A related literature examines the direct relationship between alcohol consumption and labour market outcomes (Berger and Leigh, 1988; Zarkin et al., 1998; MacDonald and Shields, 2001, 2004; Auld, 2005).

Drink-driving convictions in New Zealand are ideally suited for examining the informational consequences of criminal records. They are often non-custodial, involving fines or temporary licence suspensions rather than imprisonment. Thus, our estimates abstract away from incapacitation and human capital depreciation that occur during incarceration. Drink-driving offences are also well suited to this design because severity is measured using breath or blood alcohol readings, which provide an objective and continuous measure of the offence that maps onto clear legal thresholds. This distinguishes drink-driving from many other misdemeanours, where severity may be more difficult to measure consistently (Sherman et al., 2016). Furthermore, individuals may be uncertain about their precise alcohol concentration at the time of driving (Love and Larue, 2025), meaning small measured differences in alcohol concentration can translate into different legal outcomes. The availability of recorded alcohol readings allows us to construct a homogeneous analysis sample of first-time offenders with readings in a narrow range at the lower end of the conviction distribution, all receiving similar non-custodial penalties. This restriction is important: it allows us to focus on the effect of acquiring a criminal record among similar low-level offenders, avoiding comparisons between individuals who differ substantially in offence severity or punishment.

Given the prevalence of such convictions, understanding their labour market consequences is of clear importance. Linking the 2023 New Zealand Census to Ministry of Justice court charge records, we find that 15.3% of men and 5.8% of women aged 30–55 have at least one drink-driving conviction. Among those with a drink-driving conviction, 25.1% of men and 43.9% of women have only a single recorded conviction. This suggests that drink-driving convictions are relatively common among individuals with otherwise limited criminal histories. These offences largely fall into the category of *summary offences*, the New Zealand equivalent of misdemeanours, which are adjudicated without a jury and typically involve non-custodial penalties. Internationally, comparable offence classes account for the majority of criminal cases carried out in common-law jurisdictions.[3] Despite their prevalence, lower-level offences such as drink-driving have received relatively little attention in the literature, which has instead focused disproportionately on incarceration and more serious crimes (for an important exception, see Agan et al., 2023).

[3]For example, in England and Wales, summary offences account for around 90% of all criminal cases handled by magistrates' courts (Ministry of Justice, 2023). In Canada, most completed adult criminal court cases involve summary conviction offences rather than indictable crimes (Department of Justice Canada, 2023). Similarly, in Australia, the bulk of defendants finalised in criminal courts are charged with non-indictable offences (Australian Bureau of Statistics, 2023). In the United States, misdemeanours account for the vast majority of criminal filings in state courts (Stevenson and Mayson, 2018), and population-level estimates suggest that roughly one in seven Americans have been convicted of a misdemeanour at some point in their lives (Craigie et al., 2020).

In contrast to an extensive literature on deterrence and road safety (Dee, 2001; Eisenberg, 2003; Albalate, 2008; Francesconi and James, 2021; Portillo et al., 2024; Ramasco, 2026), a small body of non-economic research on the consequences of drink-driving convictions documents negligible labour market impacts. Early studies have focused on the effects of licence suspension or revocation on employment outcomes. Wells-Parker and Cosby (1988) used telephone surveys to compare 416 suspended and non-suspended DUI (driving under the influence) offenders in Mississippi, United State and found limited evidence of an effect on employment.[4] Similarly, Knoebel and Ross (1997) surveyed 579 first-time and 233 repeat-DUI offenders across several US jurisdictions with differing licence revocation regimes and reported minimal impacts on job status and income. The reliance on small, self-reported survey data, however, limits the credibility of the estimates in these studies.

More recent work has used administrative data to examine broader economic outcomes following drink-driving convictions. Using Finnish register data, Oksanen et al. (2015) show that individuals are more likely to experience debt problems, divorce, and face an income loss of approximately 20%, three years following their first offence. While this study exploits within-individual variation over time, it remains largely descriptive, lacking a formal identification strategy and failing to account for pre-existing trends.

Our study also contributes to a growing economic literature examining how criminal justice contact affects labour market outcomes through the information conveyed by a criminal record itself (Finlay, 2009; Agan and Starr, 2017). A key finding in this literature is that employers respond to information about criminal history, though effects are often heterogeneous and context-dependent. Evidence from field experiments shows that criminal records reduce callback rates for job applicants. Concurrently, policies that restrict access to information, such as "Ban-the-Box", can alter employer screening in ways that generate unintended consequences, including increased statistical discrimination against minority applicants (Agan and Starr, 2018). Subsequent research has shown considerable heterogeneity in Ban-the-Box employment effects across data sources, institutional settings, and population subgroups, yielding inconclusive evidence on aggregate employment impacts (Craigie, 2020; Doleac and Hansen, 2020; Shoag and Veuger, 2021; Rose, 2021; Burton and Wasser, 2025).

Related evidence from "clean slate" and record-remediation reforms, which conceal, clear or reduce the severity of criminal records after a period without reoffending, also points to modest earnings gains among the employed. However, the evidence on overall

[4]This null result can be partly explained by offenders still driving when suspended.

employment rates is weak (Dasgupta et al., 2025; Agan et al., 2024a,b). One interpretation is that criminal records may scar labour-market trajectories soon after contact with the criminal justice system, leading to employment disparities or movement into lower-opportunity jobs that are difficult to rectify later. Overall, the evidence shows that criminal records are economically consequential, but the effects of revealing or concealing this information are theoretically ambiguous and empirically mixed.[5]

The broader economic literature on criminal justice contact primarily focuses on incarceration or pre-trial detention. These studies often exploit quasi-experimental variation, such as random judge assignment or sentencing thresholds, to estimate causal effects on employment and earnings. The evidence is mixed – some studies find sentence length has little impact on post-release labour market outcomes (Kling, 2006), while others document more persistent employment and earnings losses following incarceration, especially for those with strong pre-arrest labour market attachment (Mueller-Smith, 2015).[6] Pre-trial detention reduces formal-sector employment and being in receipt of government benefits (Dobbie et al., 2018), with similar evidence from Chile showing that pre-trial detention lowers formal employment and monthly wages (Grau et al., 2023). More recent evidence using large administrative datasets suggests a substantial share of observed earnings losses associated with incarceration reflects incapacitation and weak pre-existing labour market attachment, rather than long-run scarring, once time spent incarcerated is accounted for (Garin et al., 2025). This literature is important but is less directly relevant to settings such as ours, where the offence does not typically result in imprisonment.

This paper makes three contributions. First, it provides the first causal evidence on the impact of drink-driving convictions on labour market outcomes, focusing on employment and earnings rather than road safety or recidivism. We identify the economic consequences of a criminal conviction by examining non-custodial drink-driving offences, exploiting new linked administrative data that combine detailed court records, including recorded breath alcohol levels, with earnings data. This setting allows us to abstract from imprisonment and variation in sentencing severity, and to trace labour market trajectories before and after conviction.

Second, we implement an identification strategy based on variation in the timing of individuals' first drink-driving conviction. Using a staggered difference-in-differences design

[5] Relatedly, Mueller-Smith and Schnepel (2021) find that avoiding a conviction has a significantly positive impact on labour market outcomes.

[6] Mueller-Smith (2015) finds that each additional year of incarceration reduces post-release employment by 3.6 percentage points and significantly lowers earnings by 0.34 log points. Among misdemeanour defendants, imprisonment reduces employment by 4.5 percentage points and earnings by 0.42 log points.

and a control group of individuals who are convicted in the future (Callaway and Sant'Anna, 2021), we estimate treatment effects within a population of individuals who eventually interact with the criminal justice system. Conditioning on eventual conviction mitigates concerns that unobserved characteristics drive both the propensity to offend and labour market attachment. We then implement an exact matching design combined with a fixed effects event-study specification, comparing convicted individuals to observationally similar individuals with no recorded court conviction during the sample period. The results are similar using both approaches, indicating that our estimates are not an artifact of the choice of control group.

To further ensure our treatment and control groups are comparable, we restrict the sample to offenders at the lower end of the breath/blood alcohol distribution. Our sample includes those with readings of 500 micrograms of alcohol per litre or less, accounting for 15% of all offenders. These individuals are closer to the legal conviction threshold, making them more comparable to drivers with readings just below the threshold. In New Zealand, a summary drink-driving offence applies for readings between 400 and 800 micrograms of alcohol per litre of breath.[7] By comparison, the mean reading among all convicted offenders in our data is 650 micrograms, indicating that the typical offender is substantially further away from the threshold.

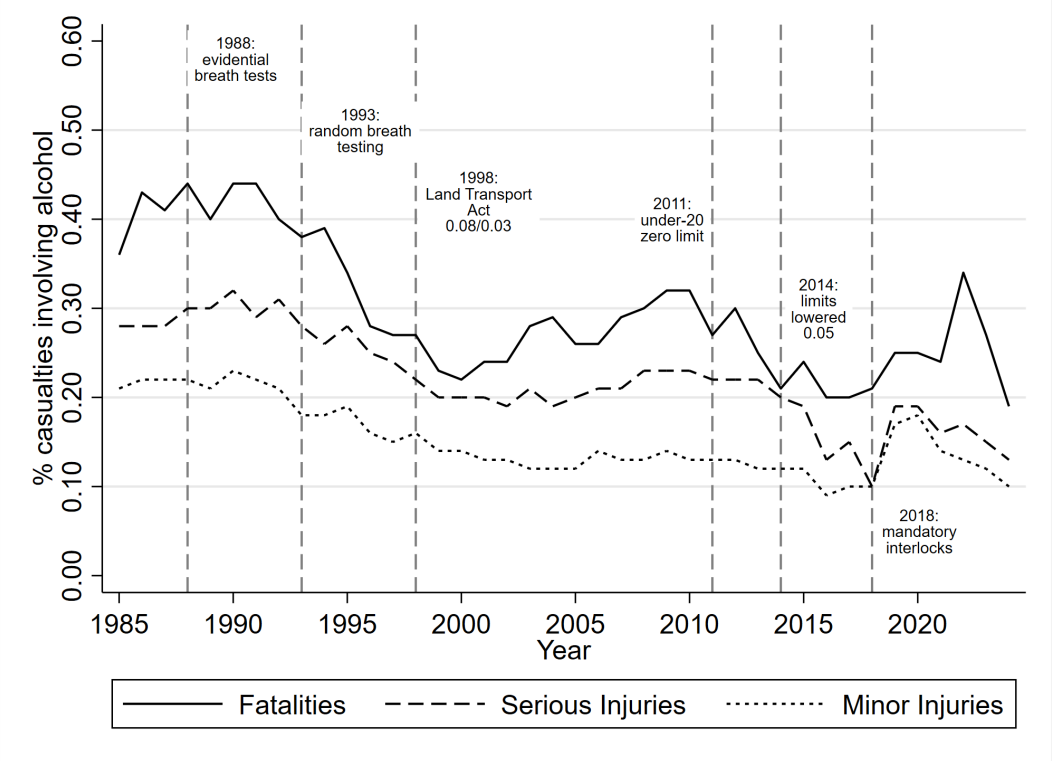
Third, we find meaningful labour market effects following conviction that persist over time. Employment falls by 6–7 percentage points on average, while monthly earnings fall by NZ$90 (around 2% at baseline). These effects are driven by both reduced entry into employment and increased job loss, with the larger effect operating through reduced job-finding among the unemployed. Further, we find that impacts on employment are larger in industries with a high propensity to screen applicants for criminal convictions. This pattern is consistent with evidence from Agan and Starr (2017), who show job applicants with criminal records face substantially lower callback rates than those without. Moreover, the effects persist beyond the typical six-month licence suspension period, suggesting that short-term driving restrictions are unlikely to explain the results. We capture the essential elements of our findings in a simple model of employer screening in a frictional labour market, which we present in the penultimate section. This framework shows that convictions reduce perceived productivity by employers, resulting in a hiring penalty that disproportionately affects job entry relative to job retention.

[7] Drivers who refuse a breath test may be required to provide a blood sample. The corresponding blood alcohol limit is 80 milligrams per 100 millilitres of blood.

# 2 Institutional Background

Figure 1 summarises the evolution of New Zealand's drink-driving regime. The figure plots the share of alcohol-related road casualties alongside selected policy changes affecting alcohol testing, legal drink-driving limits, and punishments. Over the period shown, New Zealand progressively strengthened its drink-driving enforcement through evidential and random breath testing (1988, 1993), lower legal alcohol limits (1998, 2011, 2014), and mandatory interlocks for repeat offenders (2018). Alcohol-related casualties declined substantially over this period, although drink-driving remains an important road-safety concern.[8]

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




*Source: Ministry for Cities, Environment, Regions and Transport* https://www.transport.govt.nz/statistics-and-insights/safety-annual-statistics/sheet/alcohol-and-drugs

*Note*: The figure plots the share of alcohol-related road casualties by year, alongside selected drink-driving policy changes. Vertical dashed lines indicate major changes to alcohol testing, legal thresholds, and sanctions.

The legal consequences of drink-driving are determined by the degree to which a driver's blood or breath alcohol concentration exceeds statutory limits. The Land Transport Act

[8] We use the figure to describe the institutional setting rather than as a source of identifying variation.

1998 provides the core legal framework governing alcohol-impaired driving and associated penalties, and has been amended over time to address alcohol-related road-safety risks (New Zealand Government, 2026). Two features of this framework are central to our analysis. First, alcohol concentration is measured using standardised breath or blood tests, generating an objective and continuous measure of alcohol impairment. Second, legal consequences depend on thresholds, where lower-level breaches are treated as infringement offences and higher-level breaches are prosecuted in court. The court-prosecution threshold is important as it is the point at which a drink-driving incident can generate a formal criminal conviction.

Enforcement relies on widespread roadside checks, with police authorised to stop drivers and administer alcohol tests without requiring reasonable suspicion. Four types of tests are used to assess alcohol impairment (Controller and Auditor General, 2013): (i) *passive breath tests*, an initial roadside screening using a handheld device to detect the presence of alcohol; (ii) *breath screening tests*, administered if the driver fails or refuses the passive test, where failing the screening test determines whether further evidential testing is required;[9] (iii) *evidential breath tests*, which provide a court-admissible measure of breath alcohol concentration; and (iv) *evidential blood tests*, conducted by a health professional if breath testing is refused or failed. Courts typically prioritise evidential breath or blood results when determining whether an offence has occurred and the relevant penalty.

At the time of its introduction, the Land Transport Act set the legal blood alcohol concentration (BAC) limit at 30 mg per 100 ml of blood, or 150 micrograms ($\mu$g) per litre of breath, for drivers under 20, and 80 mg per 100 ml of blood, or 400 $\mu$g per litre of breath, for drivers aged 20 and over.[10] Exceeding these limits resulted in court-imposed penalties, including fines, licence suspension, or imprisonment. In 1999, the legal alcohol purchasing age was reduced from 20 to 18 (Kypri et al., 2006), increasing access to, and consumption of, alcohol among young adults (Boes and Stillman, 2013; Dasgupta et al., 2022).

In 2011, a zero-alcohol policy was introduced for drivers under 20, replacing the previous youth limit of 30 mg per 100 ml of blood, or 150 $\mu$g per litre of breath (New Zealand Government, 2026). Under the new regime, driving with any measurable alcohol became unlawful for drivers under 20. However, the penalties were tiered. Low-level breaches be-

[9] 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, such as an evidential test.

[10] For comparison, US jurisdictions typically express blood alcohol content (BAC) as a percentage. New Zealand's adult (20+) court-prosecution threshold of 80 mg per 100 ml of blood corresponds to a BAC of 0.08%, matching the standard limit in most US states. The lower infringement-only threshold of 50 mg per 100 ml of blood (250 g per litre of breath) corresponds to 0.05%, a lower threshold adopted by a small number of states. Our main analysis sample — 80–100 mg per 100 ml of blood — corresponds to a BAC range of approximately 0.08–0.10%.

low 30 mg per 100 ml of blood, or 150 $\mu$g per litre of breath, were treated as infringement offences and were not processed in court, typically resulting in a fine of around NZ$200 and 50 demerit points.[11] By contrast, young drivers with readings at or above this threshold were prosecuted in court and faced more severe penalties, including fines of up to NZ$2,250, licence disqualification of three months or more, or imprisonment of up to three months. Additional measures introduced in 2012, such as alcohol interlocks and zero-alcohol licences, further restricted repeat offenders.

In 2014, the limit for drivers aged 20 and over was reduced to 50 mg per 100 ml of blood, or 250 $\mu$g per litre of breath. Drivers with alcohol readings between 51 and 80 mg per 100 ml of blood, or 251–400 $\mu$g per litre of breath, commit an infringement offence, while those exceeding 80 mg per 100 ml of blood, or 400 $\mu$g per litre of breath, are prosecuted in court. Penalties for these offences include substantial fines of up to NZ$20,000, licence disqualification or suspension, typically six months or more, and, in severe cases, imprisonment of up to 10 years.[12] The severity of sentencing depends on the level of intoxication, prior offences, and whether the incident resulted in injury or death.

# 3 Data and Methods

## 3.1 Sample Selection and Descriptives

Our study uses administrative data from the Statistics New Zealand's (Stats NZ) Integrated Data Infrastructure (IDI), a centralised repository of administrative microdata on individuals and households drawn from multiple government agencies (e.g., Ministry of Justice and Inland Revenue). Individual are linked across datasets using a unique anonymised identifier, enabling longitudinal analysis of criminal justice and labour market outcomes.

We draw on the Ministry of Justice (MoJ) court charges dataset, which contains records of all criminal charges processed in New Zealand courts since 1992. This includes information on offence dates, court hearings, sentencing outcomes, offence types, conviction status, and court location and type (e.g., district court, high court). As the court data contains limited demographic information, we link these records to the Stats NZ Personal Details dataset, which provides detailed demographic characteristics (such as sex, ethnicity, date of birth, and date of death) constructed from all data sources within the IDI. We also link a separate MoJ

[11] Drivers who accumulate 100 or more demerit points within a two-year period face an automatic three-month licence suspension.

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

dataset containing recorded alcohol levels for drink-driving offences.

Given that New Zealand's BAC limits vary by age (with stricter thresholds for drivers under 20), we restrict our analysis to individuals aged 20 or over at the time of the offence to ensure a consistent legal standard. Our analysis sample comprises men and women aged 21–55, whose first conviction occurred between January 2007 and December 2019. To define a homogeneous population of first-time offenders and isolate the effects of a single, low-level violation, we impose the following restrictions:[13]

(i) We focus on individuals for whom drink driving is their first *and* only recorded offence, with a measured alcohol level between 400 and 500 $\mu$g per litre of breath (or 80–100 mg per 100 ml of blood). This restriction excludes repeat offenders and ensures that the estimated effects are attributable to a single, relatively low-level violation.

(ii) The individual receives a conviction and a standard sentence consisting of a fine between NZ$400–500 and a 180-day driver licence suspension. This is the most common sentencing outcome for the drink-driving offense described above, ensuring comparability of legal consequences across the sample.

(iii) The individual must not experience an accident in the same month as the drink-driving violation (based on Accident Compensation Claims data). This restriction reduces the likelihood that health shocks, rather than the conviction itself, confound post-conviction labour market outcomes.

(iv) The individual must be born in New Zealand, ensuring that prior convictions are fully observed in the data and that we correctly identify first-time offenders.

We link our sample to monthly tax records from Inland Revenue to construct employment and earnings outcomes before and after conviction, where conviction is defined as the date of the final court hearing. The final sample consists of $1,824$ individuals, shown in the first column of Table 1.

Table 1 compares our sample characteristics (measured one month prior to the offence) to those of the general population, comprising New Zealand-born individuals aged 20–55 in the 2013 Census. Our drink-driving sample is younger, more likely to be male, and more ethnically diverse compared to the general population. While employment rates are higher in our sample, average earnings are lower, reflecting a greater concentration in lower-wage

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

Table 1: Descriptive statistics

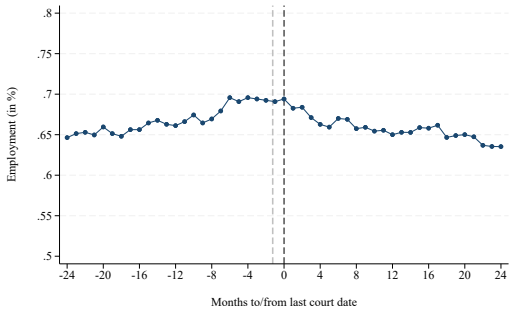
| | Drink-driving sample[a] | 2013 Census[b] |
|---|---|---|
| Date | May 2013 | March 2013 |
| Age | 32.55 | 38.20 |
| Male | 0.556 | 0.474 |
| European only | 0.560 | 0.732 |
| Employed | 0.691 | 0.566 |
| Labour income (in $NZD) | 3946.16 | 4521.95 |
| Labour income relative to industry mean | 1.09 | 1.14 |
| *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 | 0.850 | |
| Alcohol level (relative to legal limit)[c] | 1.174 | |
| Fine (in $NZ) | 428.71 | |
| Driver license suspension (in days) | 180 | |
| Processing duration[d] | 38.05 | |
| Unique individuals | 1,824 | 684,927 |

*Note:* Source is IDI and authors' calculations. [a] Measured one month before offense date. [b] Includes all NZ born individuals aged between 20 and 55 on 2013 Census date. [c] The legal limit is 400 micrograms (mcg) of alcohol per litre of breath or 80 milligrams (mg) per 100ml of blood. [d] Measured as the difference in days between last court date and offence date. Labour income includes monthly earnings received from wages and salaries.

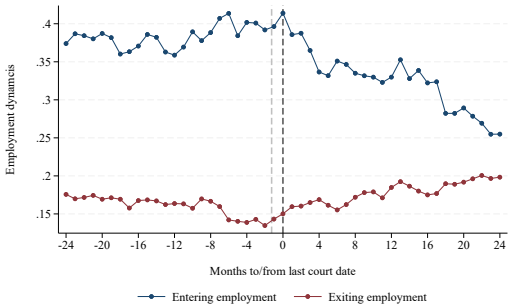
industries such as manufacturing, construction, trade and retail. These differences indicate that our sample represents a distinct subpopulation rather than a random cross-section of New Zealanders.

Figure 2 presents raw means of employment, employment transitions, and earnings in the 24 months before and after the first conviction. Panel (a) shows a modest increase in employment prior to conviction, followed by a decline thereafter. As these are unadjusted averages, this pattern should not be interpreted as causal. Rather, it reflects underlying labour market flows – panel (b) shows that entry into employment exceeds exits prior to conviction, followed by a decline in entry and a slight increase in exits after conviction. Panel (c) shows that earnings continue to trend upward for those who remain employed, a pattern that could lead to an underestimate of the negative effects of conviction. This underscores the importance of our research design, which compares treated (convicted) individuals with individuals

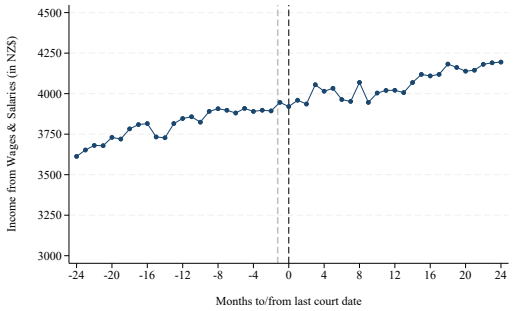
Figure 2: Economic outcomes before and after the first drink-driving conviction, raw averages




(a) Employment




(b) Labour Market Transitions




(c) Earnings

*Notes:* These figures show economic outcomes 24 months before and after the first drink-driving conviction. The black dashed line refers to the last court date and the gray dashed line to the the average offence date relative to the last court date. Entering employment is conditional on being non-employed at $t - 24$. Exiting employment is conditional on being employed at $t - 24$. Source: IDI and authors calculations.

who are not-yet-convicted.

## 3.2 Research Design

Our baseline empirical strategy exploits variation in the timing of individuals' first drink-driving conviction. We compare labour market outcomes before and after conviction across individuals who are all eventually convicted, but at different points in time. This design reduces concerns that fixed differences between offenders and non-offenders drive the results, as all individuals in the sample ultimately experience treatment.

Because first convictions occur at different points between January 2007 and December 2019, treatment timing is staggered across individuals. A natural benchmark in this setting

is the two-way fixed effects (TWFE) specification:

$$Y_{it} = \alpha + \gamma \, Conviction_{it} + \mu_i + \lambda_t + \upsilon_{it}, \tag{1}$$

where $Y_{it}$ denotes the labour market outcome of individual $i$ at time $t$. The variable $Conviction_{it}$ is a binary indicator equal to 1 if the individual has been convicted by time $t$ and 0 otherwise. $\mu_i$ and $\lambda_t$ represent individual and time fixed effects, respectively.

Under homogenous treatment effects, $\gamma$ can be interpreted as the average effect of conviction on a labour market outcome. However, when treatment effects vary across cohorts or time, comparisons involving already-treated individuals can generate negative weights in the TWFE estimator, biasing the estimated treatment effects (e.g., 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), which excludes already-treated units from the control group and estimates group-time average treatment effects.

The group-time average treatment effect on the treated (ATT) is given by:

$$ATT(g,t) = E[Y_t - Y_{g-1} | G = g] - E[Y_t - Y_{g-1} | D_t = 0, G \neq g] \quad \forall t \geq g, \tag{2}$$

where $g$ denotes the cohort of individuals who are first convicted in the same month, $Y_{g-1}$ is the outcome in the period immediately prior to treatment, and $D_t = 0$ indicates that an individual remains untreated at time $t$. Intuitively, Equation (2) compares the change in outcomes for cohort $g$ to the change for individuals not yet treated at time $t$.

We summarise these effects by averaging the ATT over post-treatment periods:

$$\theta_s(g) = \frac{1}{T - g + 1} \sum_{t=g}^{T} ATT(g,t), \tag{3}$$

and aggregate across cohorts to obtain the overall treatment effect:

$$\theta_S = \sum_{g} \theta_s(g) P(G = g), \tag{4}$$

where $P(G = g)$ denotes the share of individuals first treated in period $g$.

Three assumptions must be satisfied for our estimate to be interpreted causally. First, we assume labour market trajectories would have evolved similarly over time for treated and

not-yet-treated individuals in the absence of treatment (the parallel trends assumption). In our staggered setting, this means individuals convicted earlier and later would have followed similar labour market trajectories had neither group been convicted. The timing of a conviction may be correlated with unobserved shocks that affect labour market outcomes, such as deteriorating mental health or other adverse life events that could increase both the likelihood of drink-driving and the likelihood of job loss.

Second, we assume that individuals do not change their behaviour (e.g., job-seeking in anticipation of punishment or job loss) in advance of a conviction (the no anticipation assumption). This is important because the offence date can precede the final court date by a considerable period, and it is the conviction – not the offence – that determines an individual's criminal record status and is therefore most relevant for employer screening. We therefore define treatment as the final court date and examine labour market outcomes before and after this time point.

Third, we assume one individual's conviction does not directly affect another individual's labour market outcomes (the stable unit treatment value assumption). This is plausible given that convictions are individually assigned and experienced, and drink-driving is unlikely to generate obvious spillovers that could indirectly affect someone else's employment or earnings.[14]

The absence of differential pre-conviction labour market trends in Figure 2 provides suggestive evidence in support of the parallel trends and no anticipation assumptions. We also examine pre-trends in health-related outcomes by comparing the mental and physical health of individuals convicted earlier in the sample to those convicted later, over the 24 months preceding conviction (Figure A.1 in the Online Appendix). Panel (a) examines prescriptions for nervous-system-related medications, while Panel (b) examines accident-related hospital admissions.[15] The estimated differences are economically small, statistically insignificant, and show no evidence of health deterioration in the months leading up to conviction. Taken together, the absence of differential pre-conviction trends in both labour market and health outcomes provides support for the identifying assumptions underlying our staggered adoption design.

As an additional robustness exercise, we implement an exact matching strategy that complements our main staggered adoption design. While the main approach has the advantage of

[14] While spillovers among drink-driving offenders are possible, we are unable to assess such effects with the available data.

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

comparing only individuals who eventually commit drink-driving offences–thereby controlling for underlying characteristics that lead to both conviction and labour market outcomes, and reducing concerns about systematic differences between offenders and non-offenders–its validity depends on later-convicted individuals providing an appropriate counterfactual for those convicted earlier.

If the timing of convictions is related to shocks that independently affect labour market trajectories (e.g., job loss precipitating risky behaviour that leads to earlier conviction), this could violate the parallel trends assumption. To assess whether our findings are sensitive to such timing-related shocks, we estimate a fixed effects event-study specification using an exactly matched sample that balances treated and control individuals on pre-conviction characteristics.[16] Under this alternative strategy, we compare convicted individuals to a control pool of observationally similar New Zealand-born individuals residing in the country between 2007 and 2019 with no recorded court conviction.

Each convicted individual is exactly matched to all available controls sharing the same ethnicity category (European versus non-European), sex, birth cohort (defined by birth year and birth quarter), and employment status 24 months prior to conviction. Conditional on employment, controls are also matched based on their monthly labour income being within approximately NZ$250 of the convicted individual's earnings at the same point in time. Each control is assigned the conviction date of their matched treated individual as a pseudo-event date, so that treated and control individuals are compared over the same calendar period.

Using this procedure, we identify exact matches for 1,818 of the 1,824 convicted individuals (99.7%) in our estimation sample. Table A.2 reports covariate balance before and after matching. Prior to matching, the non-convicted population differs substantially from the convicted sample: non-convicted individuals are less likely to be male, more likely to be European, less likely to be employed, and have lower labour income. After matching, these differences are effectively eliminated. The matched control group has the same birth cohort, sex, ethnicity, and employment-status distribution as the convicted sample, while average pre-event labour income is nearly identical across groups.

Control individuals can be matched to multiple treated individuals, in which case they are weighted within each matched set so that they receive the same weight as the treated individual. The weight assigned to a control individual therefore reflects the sum of that

[16]This approach follows a growing literature that combines matching with an event-study or difference-in-differences design in settings where treatment is not randomly assigned and treatment timing may be related to individual circumstances or characteristics. Examples include studies of workplace violence, domestic violence, and parental death (Adams-Prassl et al., 2024; Adams et al., 2024; Jensen and Zhang, 2026).

individual's contributions across all matched sets in which they appear. Using the matched sample, we estimate the following fixed effects event-study specification:

$$Y_{it} = \xi_i + \sum_{k \in S} \delta_k 1(t = t_i^* + k) + \sum_{k \in S} \beta_k 1(t = t_i^* + k) \times D_i + \phi_q + \varepsilon_{it}, \quad (5)$$

where $Y_{it}$ denotes the labour market outcome of individual $i$ in period $t$, $\xi_i$ captures individual fixed effects, $D_i$ is an indicator for convicted individuals, and $t_i^*$ denotes the conviction date. The coefficient of interest, $\beta_k$, captures the difference in outcomes between convicted individuals and their matched controls at event time $k$, relative to the reference period (the period immediately prior to conviction, $t = -1$). The specification also includes quarterly fixed effects, $\phi_q$, which absorb aggregate macroeconomic and seasonal shocks common to both groups.

# 4 Results

## 4.1 Baseline Estimates

Table 2 reports average labour market effects over the two years before and after the first drink-driving conviction. We find no evidence of differential pre-conviction trends in labour market outcomes–providing empirical support for the validity of the identification strategy.[17]

Following conviction, there is a 6.8 percentage point decline in the likelihood of being employed, representing a 9.8% reduction relative to the month prior to conviction. This reduction reflects both a decrease in employment entry (8.2 percentage points, or 20.7%) and an increase in employment exit (2.2 percentage points, or 15.4%), indicating a net contraction in labour market participation. Following conviction, average monthly labour income decreases by NZ$90, equivalent to a 2.3% reduction from pre-conviction earnings.

Figure 3 presents the corresponding event-study estimates. Panel (a) shows a persistent decline in employment following conviction, reaching approximately 15 percentage points two years after the event. Panels (b)-(c) indicate that this pattern is driven primarily by a reduction in employment entry, with comparatively modest increases in employment exit. Panel (d) shows a statistically significant, but economically small, decline in labour income among those employed.

[17] As a robustness check, we conduct a placebo test (Appendix Table B.1) by artificially shifting the conviction dates 24 months earlier. We find no significant effects on employment, job entry, or labour income. Although the coefficient on employment exit is statistically significant, it has the opposite sign to our main results. These findings confirm that our post-conviction effects reflect genuine impacts rather than pre-existing labour market trends.

Table 2: Average effects of a drink-driving conviction on employment and earnings

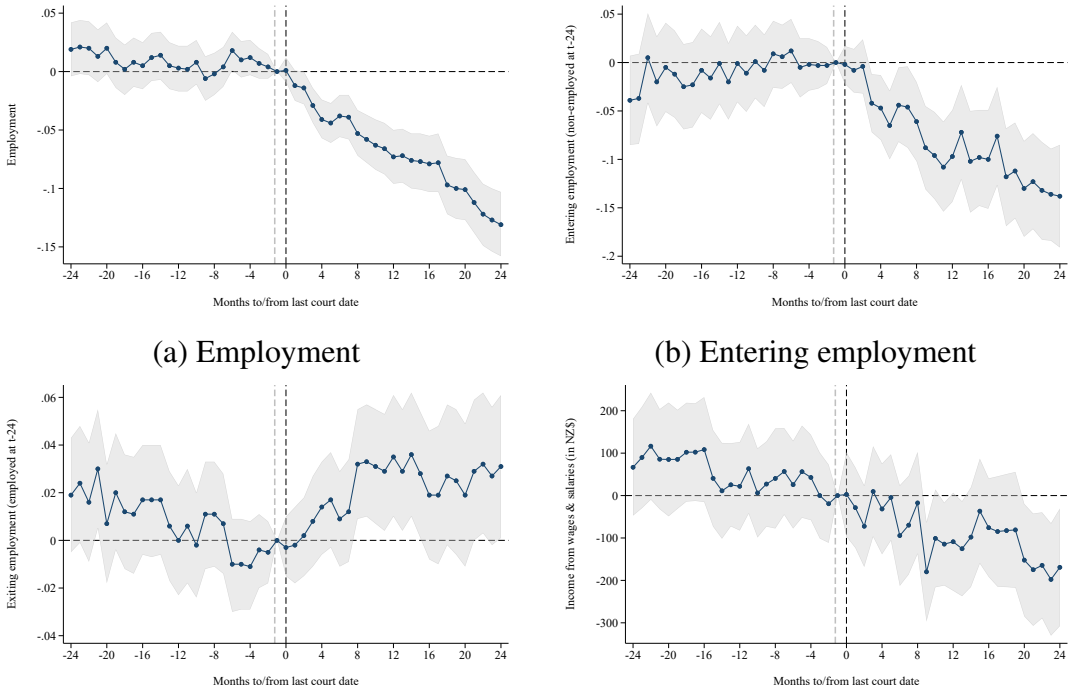
| | months to/from last court date | |
|---|---|---|
| | $-24 \leq t \leq -2$ | $0 \leq t \leq +24$ |
| Employment | 0.009 | -0.068*** |
| | (0.008) | (0.009) |
| Entering employment | -0.009 | -0.082*** |
| | (0.015) | (0.015) |
| Exiting employment | 0.008 | 0.022** |
| | (0.008) | (0.009) |
| Labour income | 53.933 | -90.109** |
| | (41.093) | (40.853) |
| Unique individuals | 1,824 | |

*Note:* Entering employment is conditional on being non-employed at $t-24$. Exiting employment is conditional on being employed at $t-24$. Event-study analysis using Callaway and Sant'Anna's (2021) staggered difference-in-difference estimator. Time window refers to 24 months before the first drink-driving conviction and up to 24 months after. Reference time point is $t = -1$. Numbers in parenthesis are standard errors. *** p<0.01, ** p<0.05, * p<0.1. Labour income includes monthly earnings received from wages and salaries. Source: IDI and authors calculations.

These patterns suggest that the estimated effects are not entirely driven by the temporary loss of a driving license. If employment effects were primarily driven by the inability to commute or perform driving-related tasks during the suspension period, we would expect a sharp increase in job exits among the previously employed, followed by attenuation once the typical six-month suspension expires. Instead, the effects persist beyond the suspension period. Furthermore, the results are predominantly driven by reduced entry into employment rather than increased exits. This pattern is consistent with the presence of barriers to hiring following conviction–such as employer screening or stigma–rather than substantial displacement from existing jobs.

These results suggest that the primary channel through which a drink-driving conviction affects labour market outcomes is by reduced entry into employment. For individuals already employed before conviction, the effects on job exits and earnings are somewhat limited. This indicates that the adverse impacts are concentrated among those attempting to enter (or re-enter) the workforce. To investigate this mechanism more directly, we exploit employer identifiers and five-digit Australia and New Zealand Standard Industrial Classification (ANZSIC) codes. This allows us to distinguish whether post-conviction employment is characterised by: i. remaining with the same employer; ii. moving to another employer

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




(a) Employment

(b) Entering employment

(c) Exiting employment

(d) Labour income

*Notes:* Entering employment is conditional on being non-employed at $t-24$. Exiting employment is conditional on being employed at $t-24$. Event-study analysis using Callaway and Sant'Anna's (2021) staggered difference-in-difference estimator. Time window refers to 24 months before the first drink-driving conviction and up to 24 months after. Reference time point is $t = -1$. Numbers in parentheses are standard errors. *** p<0.01, ** p<0.05, * p<0.1. Labour income includes monthly earnings received from wages and salaries. Source: IDI and authors calculations.

within the same narrowly defined industry; iii. moving to a different industry; or iv. entering employment from non-employment. The full decomposition is reported in Online Appendix B.1.

Appendix Table B.2 shows no detectable effect on retention with existing employers or transitions within narrowly defined industries. Instead, employment losses arise entirely from two sources: reduced entry into employment from non-employment (44% of the total decline) and reduced transitions to jobs in different industries (the remaining 56%). This pattern is consistent with employer screening, rather than direct productivity losses, as the primary channel through which convictions affect employment and reflects the different information available to employers across hiring contexts.

Existing employers directly observe worker performance, while those recruiting from

closely related industries can infer productivity from observable skills and employment histories. Neither group therefore relies heavily on criminal records when making employment decisions. Employers evaluating applicants from outside the firm or from different industries face greater uncertainty and depend more on screening signals such as criminal records. The conceptual framework in subsection 4.5 formalises these dynamics, positing that criminal records affect hiring through employer screening but become less important as employers observe worker performance directly.

The earnings decomposition points to the same mechanism. Wage penalties are concentrated among workers who change industries or re-enter employment, while workers who remain with the same employer or move within the same five-digit industry face essentially no post-conviction penalty (Appendix Table B.3. Together, the employment and earnings decompositions point to the same conclusion: convictions primarily act as a barrier to finding new employment, rather than reducing pay or productivity within existing jobs.

## 4.2 Robustness Checks

**Exact Matching** — As a robustness exercise, we implement the exact matching design combined with the fixed effects event-study specification (Equation 5) described in subsection 3.2. Appendix Table C.1 reports average before and after conviction estimates, and Appendix Figure C.1 presents the dynamic estimates from this approach alongside our baseline staggered adoption design estimates.

Overall, we observe very similar patterns across both identification strategies, showing that conviction leads to persistent declines in employment and earnings, driven primarily by reduced entry into employment alongside comparatively modest increases in employment exits. Although the exact matching estimates are smaller in magnitude for employment and entry into employment, the dynamic responses broadly mirror those obtained from the staggered adoption design, including the absence of pronounced pre-treatment trends. The exception is average pre-conviction entry into employment, which is statistically significant, as shown in Appendix Table C.1. However, Appendix Figure C.1 shows that this difference is concentrated in the earlier period of the event window and largely closes in the months immediately preceding conviction.

**Analysis Period** — As shown in Figure 1, New Zealand's drink-driving regime changed during our analysis period, which covers individuals convicted between January 2007 and December 2019. In addition, the early part of the sample coincides with the global finan-

cial crisis, which significantly affected labour market conditions in New Zealand (Hall and McDermott, 2016; Razzak, 2016). These changes raise a potential concern for the staggered design, where later-convicted individuals may not provide an ideal counterfactual for earlier-convicted individuals, since the two groups faced different legal settings, macroeconomic conditions, or hiring environments.

To assess the sensitivity of our results to these changes, we re-estimate the Callaway and Sant'Anna specification (Callaway and Sant'Anna, 2021) on four restricted samples defined around key institutional changes: convictions after 2012 (following the introduction of alcohol interlocks and zero-alcohol licences for repeat offenders), convictions before and after December 2014 (the reduction in the adult alcohol limit), and convictions before 2018 (prior to interlocks becoming mandatory). We continue to restrict the sample to drivers aged 20 and over, and to readings above the adult court-prosecution threshold (400 and 500 $\mu$g per litre of breath or 80–100 mg per 100 ml of blood).

Appendix Table C.2 shows that the results for each restricted sample are highly consistent with the baseline estimates. Across all four samples, conviction is associated with a statistically significant decline in employment, ranging from 4.9 to 7.9 percentage points. The reduction in employment is again driven primarily by lower entry into employment, with estimated effects between 8.4 and 10.3 percentage points, while employment exits increase modestly by 2.2 to 3.2 percentage points. Estimates for wages and salaries are negative in all four samples, though statistically significant for only the pre-2018 specification. We also find little evidence of differential pre-conviction trends across the restricted samples. These results suggest that the main findings are not driven by comparisons between cohorts exposed to different legal regimes or macroeconomic conditions.

**Subsequent convictions** — We also verify that the results are not driven by individuals who receive a subsequent conviction after the first drink-driving conviction. We re-estimate the baseline specification after excluding individuals with a second conviction within 24 months. This yields very similar effects on employment and employment entry (Table C.3), suggesting that the main estimates capture the consequences of the initial conviction rather than subsequent criminal justice contact.

## 4.3 Additional Outcomes

Appendix Table C.4 examines the impact of a drink-driving conviction on additional outcomes. Conditional on being employed at $t - 24$ and $t$, we find no evidence that convicted

individuals are more likely to switch employers or experience weaker wage progression. We also find no evidence that conviction induces greater enrolment in college or other higher education programs. However, we find that convicted individuals are more likely to work in low-paying industries, defined as industries whose mean earnings are in the bottom quartile of the overall industry labour income distribution. Together, these findings suggest that drink-driving convictions do not substantially disrupt existing employment relationships among incumbent workers, but may reduce access to higher-paying sectors and jobs.

Next, we consider broader *annual* income measures, which may better reflect an individual's economic circumstances beyond monthly labour income alone, as reported in the first two columns of Appendix Table C.5.[18] We find a substantial and persistent decline in total annual earnings in the four years following conviction, indicating that the economic consequences of a drink-driving conviction extend beyond standard employment income. We also examine self-employment outcomes, defined as a binary indicator equal to 1 if an individual reports self-employment income exceeding NZ$5,000 in a given financial year, 0 otherwise.[19] While we do not observe significant changes in the likelihood of being self-employed following conviction, we find suggestive evidence of lower self-employment income, although this impact is only statistically significant at the 10 percent level.

## 4.4 Heterogeneity Analysis

**Ethnicity** — The first two columns in Table 3 examines heterogeneous effects by ethnicity.[20] The results show that drink-driving convictions are associated with adverse labour market outcomes for both Europeans and Non-Europeans, though point estimates are generally larger in magnitude for Non-Europeans. For Europeans, employment declines by 4.9 percentage points (7%), driven primarily by reduced entry into employment, with a small and imprecisely estimated increase in exit rates. For Non-Europeans, the decline in employment is larger – 9.1 percentage points (13.3%) – reflecting both reduced entry and increased

[18] Inland Revenue provides information on income from self-employment and from investments only at the financial year level. We adjust conviction dates accordingly. Total annual earnings combine multiple income sources from labour (wages and salaries), self-employment, investment incomes, income support payments, tax credits, student allowances, paid parental leave, ACC income support, and other taxable income. Accident Compensation Corporation (ACC) income support refers to weekly compensation payments provided to people who cannot work or experience reduced earnings because of an injury covered by ACC.

[19] Findings are robust to alternative threshold definitions.

[20] New Zealand's ethnicity classification includes several broad groups: European, Māori (the indigenous population), Asian, Pacific peoples, and Middle Eastern/Latin American/African (MELAA). Respondents may identify with more than one group. In the 2023 Census, 68% of the population identified as European, 18% as Māori, 17% as Asian, 9% as Pacific peoples, and 2% as MELAA (percentages sum to more than 100% due to multiple responses allowed).

exit from employment. Earnings decline significantly for Non-Europeans, whereas the corresponding estimate for Europeans is smaller and not statistically significant.

Denning et al. (2025) showed that judicial outcomes can vary by defendant characteristics, even for similar drink-driving cases. That is, two people who commit the same offence (for example, same level of alcohol in their system) might end up with different judicial outcomes depending on their characteristics. An alternative explanation, which is more plausible in the context of our heterogeneous labour market effects, is that a conviction conveys the same information to employers – that the applicant has a criminal record – but this information is processed differently depending on the applicant's characteristics. Regardless of which explanation is driving the heterogeneous results, Figure C.2 shows that post-conviction labour market trajectories are broadly similar across ethnicities, and we cannot reject equality of effects between the two groups. We therefore interpret the estimates as suggestive of larger penalties for Non-Europeans, while noting that the differences are not statistically distinguishable.

**Gender** — Turning to gender, we find that convictions are associated with adverse labour market outcomes for both men and women (Table 3). While we observe similar declines in employment, the underlying margins differ. For men, the decline is driven primarily by reduced entry into employment; for women, this decline reflects both reduced entry into employment and a higher likelihood of exit. Men also experience a statistically significant decrease in labour income, while the estimate for women is smaller and not statistically significant. As with ethnicity, these differences should be interpreted with caution – Figure C.3 indicates that the dynamic effects for men and women are not statistically different from one another.

**Education Enrolment** — To further assess the role of human capital in moderating the labour market consequences of conviction, we split the sample according to whether individuals had previously enrolled in university or other higher education prior to conviction (see Appendix Table C.6). Given that tertiary enrolment data was only available from 1994 onwards, the analysis is restricted to offenders aged below 30 at the time of the offence. The estimates suggest individuals with prior tertiary enrolment experienced smaller declines in post-conviction employment (8.1 percentage points) compared to those not previously enrolled (11.8 percentage points), though they experienced a larger decline in entry into employment (10.0 percentage points) relative to those not previously enrolled (8.8 percentage points). These comparisons should be interpreted with caution as we observe statistically significant pre-treatment ATT estimates in two specifications for individuals without prior

enrolment, indicating potential deviations from parallel pre-trends in this subgroup. Taken together, these findings provide only limited evidence that prior tertiary enrolment substantially mitigates the labour market consequences of conviction.

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

| | European | | Non-European | | | Male | | Female | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | months to/from last court date | | | | | | | | | |
| | $-24 \leq t \leq -2$ | $0 \leq t \leq +24$ | $-24 \leq t \leq -2$ | $0 \leq t \leq +24$ | | $-24 \leq t \leq -2$ | $0 \leq t \leq +24$ | $-24 \leq t \leq -2$ | $0 \leq t \leq +24$ | |
| | (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) |
| Labour 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 | | |

*Note:* Entering employment is conditional on being non-employed at $t - 24$. Exiting employment is conditional on being employed at $t - 24$. Event study analysis using Callaway and Sant'Anna's (2021) staggered difference-in-differences estimator. Time window refers to 24 months before the first drink-driving conviction and up to 24 months after. Reference time point is $t = -1$. Numbers in parentheses are standard errors. *** p<0.01, ** p<0.05, * p<0.1. Source: IDI and authors calculations.

## 4.5 Employer Screening

We provide further evidence that employer screening is the primary mechanism driving these effects. We first review descriptive evidence of employer screening. Second, we present reduced-form evidence on firms' hiring behaviour. Third, we formalise this evidence in a simple model in which criminal records convey information to employers, abstracting from incapacitation and human capital effects.

**Descriptive evidence** — A 2019 study using a survey of 237 firms from New Zealand's Canterbury region finds qualitatively similar evidence of employer screening (Gilbert et al., 2019). The surveyed firms were selected from sectors with positions requiring skills that generally matched those of justice-involved individuals. Notably, 51% of surveyed firms conduct criminal background checks.[21] The authors find that employer willingness to hire differs significantly by offence severity: minor offences like driving under the influence (DUI) show are associated with only modestly lower employability relative to candidates without records, while violent crimes substantially reduce hiring prospects. However, the survey sample was concentrated in small businesses within selected sectors, potentially limiting generalisability of the findings.

While crime type is not a strong predictor of reoffending (Bushway, 2024), surveys on employers suggest that the severity of prior offences play a crucial role in employment outcomes experienced by justice-involved individuals. U.S.-based empirical research also shows that employers' reluctance differs by offence severity (Uggen et al., 2014), and these differences are often reflected in policy guidelines as well. For instance, the Equal Employment Opportunity Commission (EEOC) recommends that firms consider the nature and seriousness of the offence, along with job characteristics and time elapsed since the conviction, while making hiring decisions.[22]

In addition to the above existing survey-based evidence, we examine how criminal convictions can affect employment prospects using linked data from the 2013 New Zealand Census, MoJ court charges register, and Inland Revenue tax records. Our sample comprises individuals who were unemployed and actively searching for a job in March 2013–the Census survey month. In particular, we look at those who had not received wage or salary income

[21] In the fiscal year 2023-24, the New Zealand Ministry of Justice processed over 641,000 requests for criminal conviction history checks (Ministry of Justice, 2024). These requests may come from various sources other than employers, including individuals, public agencies, and international consulates (for immigration purposes).

[22] Information retrieved from the EEOC "Arrest and Conviction Records: Resources for Job Seekers, Workers and Employers" report, which is available at: https://www.eeoc.gov/.

between December 2012 and March 2013, thereby excluding people who experienced short-term unemployment spells.

The individuals in our sample are classified into three mutually exclusive groups based on their most serious sentence. Specifically: (a) those with no prior convictions; (b) those whose most serious sentence was imprisonment or home detention ('severe offences'); and (c) those whose most serious sentence comprised other community-based or monetary penalties ('minor offences').

Consistent with the Clean Slate legislation, we limit our sample in categories (b) and (c) to those who received a conviction within the previous seven years (Dasgupta et al., 2025). This is to make sure that all the justice-involved individuals in our sample have visible crime records that can be accessed by the firms during background checks. We further disaggregate the third category (c) of minor offences into traffic violations and into non-traffic offences to examine whether the likelihood of finding a job for candidates with less severe sentences can further vary by the offence type.

Table 4: Employment probability of job seekers by conviction history

| | Employed at: | | |
|---|---|---|---|
| | 09-2013 (6 months later) | 03-2014 (12 months later) | 03-2015 (24 months later) |
| No conviction in past seven years | *reference* | *reference* | *reference* |
| Imprisonment/Home detention | -0.024*** | -0.028*** | -0.054*** |
| | (0.008) | (0.009) | (0.010) |
| Other community/Monetary | -0.022*** | -0.035*** | -0.047*** |
| | (0.006) | (0.007) | (0.007) |
| Other community/Monetary & Traffic violation | -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 |

*Note:* The sample is drawn from the 2013 New Zealand Census and includes individuals who reported being unemployed in March 2013 and actively seeking work in the previous four weeks. Census records are linked to MoJ Court Charges data to classify individuals by the most serious sentence received within the past seven years (Clean Slate threshold): (i) no prior conviction, (ii) imprisonment or home detention, and (iii) other community-based or monetary sentences. The latter category is further divided by whether the offence involved a traffic violation. These groups are mutually exclusive. The sample is additionally linked to IR tax records and restricted to individuals with no wage and salary (W&S) income between December 2012 and March 2013 to exclude short-term unemployment. Employment outcomes are defined as indicators for receiving W&S income of at least NZD 100 in September 2013, March 2014, and April 2014. Linear probability models control 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. Numbers in parenthesis are standard errors. *** p<0.01, ** p<0.05, * p<0.1. Source: IDI and authors calculations.

We measure employment outcomes using binary indicators for receipt of monthly labour

income (in wages and salaries) of at least NZ$100 at 6, 12, and 24 months after the Census (i.e., September 2013, March 2014, and March 2015, respectively). We estimate linear probability models controlling for age, education, ethnicity, nativity, sex, household income and size, and prior labour market attachment (months employed since March 2012). The results are presented in Table 4.

Based on Table 4 above, individuals with any conviction history experience significantly lower employment probabilities compared to those with no convictions in the past. Convictions that led to custodial sentences (like imprisonment or home detention) have the strongest negative effect, with reductions in the probability of being employed ranging from 2.4 to 5.4 percentage points across the three time periods post-Census. Non-traffic convictions with less severe sentences such as community work or monetary penalties appear to have comparable magnitudes, with effects varying between 2.2 and 4.7 percentage points. Meanwhile, traffic violations that led to community sentences or monetary fines show somewhat smaller but statistically significant declines in the employment probability in the range of 1.3-1.7 percentage points. In general, our results are aligned with the survey-based findings discussed above. Importantly, all estimates are statistically significant and relatively stable across the observation periods, suggesting persistent employment penalties associated with criminal justice involvement, regardless of offense severity.

**Screening intensity and driving requirements** — To further examine the extent that screening plays in the formulation of new employment matches we categorise industries by the extent that criminal records are screened during the recruitment process. We do not have direct measures of screening behaviour however we reviewed each ANZSIC 2006 division-subdivision and assigned it to low-, medium-, or high-screening likelihood based on industry characteristics. Features of industries that make screening more likely include formal regulation, safety-sensitive tasks, responsibility for vulnerable populations, public trust, or unsupervised access to clients. Appendix Table B.4 presents the results. It shows that, following conviction, employment through new matches falls by 1.9 percentage points in industries with a low probability to screen and by 4.8 percentage points for medium to high screening industries. This evidence is consistent with the conviction acting as a negative signal to employers at the hiring stage, and this signal is obviously more salient and apparent in those industries that screen.

Relatedly, we also examine whether the effects are concentrated in industries where driving is a key part of the job. If the results are a function of workers not being able to perform key (driving parts) of their job, then one would expect the effects to be concentrated in

those industries. In contrast to this hypothesis, Appendix Table B.5 shows that employment through new matches falls by 4.9 percentage points in low-driving-requirement industries, compared with 1.9 percentage points in high-driving industries. This evidence then provides little support that the decline in employment is being driven primarily licence loss.

**Conceptual Framework** — To interpret our empirical findings, we outline a stylized model of employer screening in a frictional labour market in which a criminal conviction acts as a negative signal of worker productivity and reliability.[23]

*Setup*. Consider a labour 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 $\varepsilon_i$ captures unobserved worker heterogeneity.[24] 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, firms observe the worker's on-the-job performance over time. Consequently, hiring decisions rely primarily on pre-match information, whereas retention decisions increasingly rely on employer-specific information accumulated during the employment relationship.

We assume that a conviction acts as a negative signal of expected productivity or job-related reliability. In particular, for newly hired workers,

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

where $\delta > 0$ captures stigma, expected absenteeism, reputational concerns, or other costs associated with a criminal record.

*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

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

[23] The framework combines a standard search-and-matching environment (Mortensen and Pissarides, 1999) with statistical discrimination and employer learning (Phelps, 1972; Arrow, 1973; Altonji and Pierret, 2001; Lange, 2007), while incorporating Becker (1968)'s insight that criminal sanctions affect economic outcomes by altering the costs faced by workers and employers. See also Pager (2003), Agan and Starr (2018), and Hjalmarsson et al. (2024).

[24] 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.

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:

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

Since a conviction lowers expected productivity by $\delta$, fewer workers satisfy the hiring condition (10) when $R_i = 1$. Convicted workers therefore face lower hiring probabilities and reduced entry into employment. A formal proof is provided in Online Appendix D.1.

*Employer learning and incumbent workers.* Once a match is formed, employers gradually replace imperfect pre-hiring information with direct observations of worker performance. Let $p_i$ denote an employer-specific performance signal observed after hiring. Conditional on $p_i$, the criminal record contains relatively little additional information about expected productivity. We therefore assume

$$\mathbb{E}[y_i \mid s_i, p_i, R_i = 1] = \mathbb{E}[y_i \mid s_i, p_i] - \rho\delta, \tag{11}$$

where $\rho \in [0,1]$ measures the weight employers continue to place on the criminal record once worker-specific performance information has accumulated. When employer learning is strong, $\rho$ is close to zero. The surplus associated with an existing match is therefore

$$J_i^I = (1-\beta)\left(\mathbb{E}[y_i \mid s_i, p_i, R_i] - b\right). \tag{12}$$

Because $\rho\delta < \delta$, the effect of a conviction on expected productivity is attenuated for incumbent workers relative to new hires. Consequently, hiring decisions depend heavily on criminal records, whereas retention decisions depend primarily on realised job performance. Criminal records therefore have substantially weaker effects on existing employment relationships than on the formation of new matches.

*Wages.* From the wage equation (8), a conviction lowers wages through its effect on expected

productivity:

$$\frac{\partial w_i}{\partial R_i} = -\beta\delta. \tag{13}$$

However, wage effects among employed workers are attenuated for two reasons. First, hiring selection implies that only convicted workers with sufficiently high expected productivity satisfy condition (10). Second, employer learning reduces the informational content of criminal records for incumbent workers over time through Equation (11). Together, these forces compress wage differences conditional on employment. Appendix D.1 formalizes this result.

*Implications*. The model yields three main predictions. First, employment declines following a conviction because employer screening lowers the probability that a new match satisfies the hiring condition (10), as implied by Equations (6) and (9). Second, the effect operates primarily through reduced entry into employment rather than increased separations. This is because criminal records are used primarily during hiring, whereas after a match forms employers increasingly rely on direct observations of worker performance. As employer learning replaces pre-hiring signals (Equation (11)), the informational value of a criminal record diminishes, reducing its influence on retention decisions. Third, conditional on employment, wage effects are comparatively modest. This follows from the wage-setting Equation (8) together with the positive selection induced by the hiring threshold in Equation (10), which raises average productivity among employed convicted workers (see Equation (13)).

These predictions align closely with our empirical findings, which show a substantial decline in employment driven primarily by reduced entry into employment, comparatively small increases in separations, and relatively modest effects on labour income.[25]

*Monopsonistic labour markets.* In Online Appendix D.2, we formally extend the baseline framework to allow for employer wage-setting power arising from frictions in worker mobility and job search.

In this environment, a criminal conviction affects labour market outcomes through three channels. First, it reduces expected productivity, tightening the hiring condition and lowering employment entry (the screening channel). Second, because convicted workers who are hired are positively selected, average match quality among employed workers is higher, which helps explain why separations remain limited even in the presence of sizeable employment effects. Third, convictions may weaken workers' outside options and mobility across firms,

[25]The framework can be extended to allow the conviction penalty to vary across demographic groups. For example, if $\delta$ differs by ethnicity or gender, the model predicts larger employment losses for groups facing stronger perceived penalties, consistent with the heterogeneous effects documented in our analysis. This is the case, for example, when $\delta_{Non\text{-}European} > \delta_{European}$.

increasing monopsony power and allowing firms to capture a larger share of match surplus.

These mechanisms jointly shape wage determination. In particular, wages reflect (i) productivity-based screening through lower expected match quality, (ii) selection into employment which raises observed productivity among employed workers, and (iii) monopsony markdowns arising from weaker outside options. As a result, wage effects may differ from employment effects not only in magnitude but also in interpretation, since they embed both composition effects and changes in employer wage-setting power.

A key implication of this extension is a sharper conceptual distinction between margins of adjustment. Employment effects remain primarily driven by screening at the hiring stage, whereas wage effects may additionally reflect employer market power over workers with criminal records. This distinction also affects the interpretation of our estimates: while the combination of large employment effects and more modest wage responses is consistent with screening-based models, the presence of monopsony power implies that wage effects may arise even in the absence of productivity losses. Consequently, the relatively small wage effects we estimate can be interpreted as a lower bound on the true extent of labour-market stigma associated with a conviction.

# 5 Conclusion

In this paper we find that decreased employment following a drink-driving conviction is primarily driven by a lower likelihood of entering employment. This indicates that such a conviction is negatively updated by employers and operates as a barrier to job access. By contrast, for individuals already employed, we find only small effects on job retention and labour income, suggesting that the main margin of adjustment is labour market entry rather than within-job outcomes.

Heterogeneity analysis reveals systematic differences in labour market responses by ethnicity and gender. Non-Europeans experience larger declines in employment and earnings than Europeans. This difference is driven by both lower entry into employment and higher exit rates. While these patterns are not statistically distinguishable, they are suggestive of heterogeneous penalties that may reflect differences in how misdemeanor convictions are processed in employer screening and hiring decisions. Men and women experience comparable overall employment declines, but the underlying margins differ. Reduced entry is the primary channel for men. For women both reduced entry and increased exit contribute.

These empirical patterns are consistent with the mechanism emphasised in our conceptual framework, in which a misdemeanor conviction lowers expected match value. It reduces

the probability that a match is formed in a frictional labour market. Given that a conviction primarily affects employers' hiring decisions rather than the productivity of incumbent workers, the model predicts that employment entry should be the key adjustment margin, with comparatively smaller effects on separations and wages.

Policy implications follow directly from this mechanism. Since the main cost of a drink-driving conviction operates through reduced employment entry, with disproportionate effects for non-European individuals and, through higher exit rates, for women, these findings point to several areas where further research and policy evaluation may be warranted.

First, examining whether a clearer distinction between low-level and more serious criminal offending in DUI enforcement—through non-conviction or diversionary pathways for borderline misdemeanor cases—affects labour market outcomes would be valuable. Although current thresholds already differentiate sanctions by alcohol levels in most countries, future research could investigate whether a greater emphasis on non-conviction resolutions where public safety risk is limited reduces employment penalties while maintaining deterrence. This possibility aligns with recent evidence finding that avoiding formal prosecution can mitigate re-offending and prevent longer-term harms (Agan et al., 2023). However, our findings suggest that such approaches alone would likely be insufficient to address the full scope of employment barriers.

Second, examining modifications to existing Clean Slate schemes that could allow eligible individuals to withhold low-level convictions from employers may be particularly relevant for minor traffic offences. Our results suggest that the main effects arise through reduced re-entry into employment rather than lower earnings among incumbent workers, suggesting that shorter waiting periods or fewer exceptions may help reduce the long-term employment consequences of these convictions.

Third, fair-chance hiring approaches that limit initial access to criminal record information during hiring may affect employment opportunities for individuals with low-level criminal records. However, existing research suggests that limiting access to criminal-record information can also encourage employers to rely more heavily on other observable characteristics when making hiring decisions. Alternative approaches such as targeted hiring incentives or time-limited record disclosure may therefore complement efforts to reduce the consequences of prosecution.

More broadly, the evidence from this study indicates that even relatively minor misdemeanor convictions can generate persistent labour-market effects through employer screening rather than through incapacitation or human capital loss. These effects arise because con-

viction records continue to shape hiring decisions well beyond the period of formal sanction. Our findings indicate that approaches focused solely on reducing prosecution of marginal cases, while valuable, would leave already-convicted individuals exposed to persistent barriers, underscoring the potential relevance of complementary approaches that facilitate labour-market reintegration and their empirical evaluation.

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# Online Appendix

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

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

**A. Additional Tables and Figures**
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**B. Additional Baseline Results**
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**C. Further Results on Robustness**
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**D. Model: Formal Results and Extension**
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# A Additional Tables and Figures

Table A.1: Funnel table on sample preparation

| Individuals | Sample type |
|---|---|
| 263,118 | Individuals aged 20-54 with a conviction in 2007-2019 |
| 60,405 | Individuals aged 20-54 with their first conviction in 2007-2019 (no prior court appearances) |
| 30,903 | Offense includes breath/blood alcohol recording |
| 26,697 | Charged for a single offense |
| 26,253 | Offense code is 'A518' or 'A323' (breath or blood above the legal limit) |
| 3,972 | Alcohol level is between 400-500 for breath and 80-100 for blood |
| 1,824 | Received a fine between $400-$500 and DL suspension of 180 days |

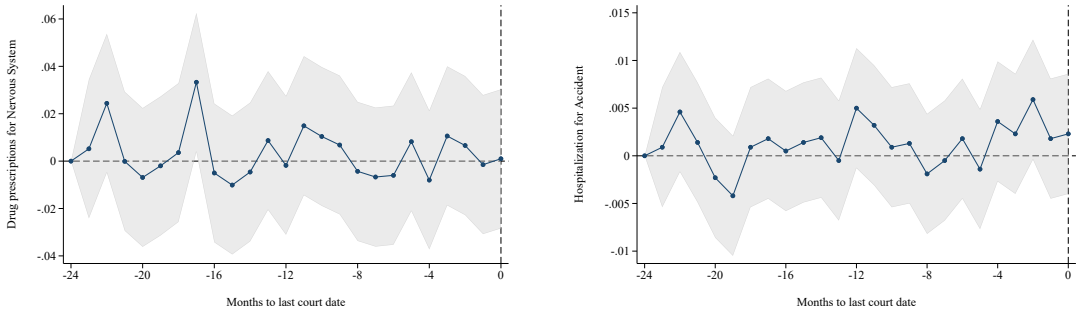
*Source:* IDI and authors calculations.

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

| | 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 |
| Labour income (in $NZD) | 3612.74 | 3286.34*** | 3583.86 | 3583.77 |
| Unique individuals | 1,824 | 427,560 | 1,818 | 1,818 |

*Note:* This table reports covariate balance before and after exact matching. Treated individuals are first-time drink-driving offenders with breath alcohol levels between 400 and 500 mcg. Controls are NZ-born individuals with no criminal convictions, matched exactly on sex, ethnicity (European vs non-European), birth year-quarter, employment status 24 months before conviction, and earnings within ±NZ$250 if employed. Employment and labour income measured 24 month before actual/assigned offense date. Statistical significance tested using (weighted) OLS regressions. *** $p<0.01$, ** $p<0.05$, * $p<0.1$ Source is IDI and authors' calculations.

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




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

*Notes:* These figures present health outcomes over the 24 months preceding the first drink-driving conviction. Health outcomes are measured using: (a) the Ministry of Health Pharmaceutical Collection to identify prescriptions for nervous-system-related medications (including analgesics, antidepressants, antipsychotics, anxiolytics, stimulants/ADHD treatments, and treatments for substance dependence), and (b) the Ministry of Health public and private hospital events dataset to identify hospital admissions flagged as accident-related. We construct a binary indicator for early conviction, taking the value of 1 if the last court date occurred before 2015, and 0 otherwise. Using fixed-effects linear regression models, the graphs display the interaction effects between the early-conviction indicator and time relative to the last court date, with $t = -24$ serving as the reference period. Source: IDI and authors calculations.

# B Additional Baseline Results

Table B.1: Regression results on placebo regression

| | 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 | |

*Note:* Last court date is moved 24 months earlier. Includes only individuals who are 20 years or older at the newly assigned pseudo court date. Entering employment is conditional on being non-employed at $t-24$. Exiting employment is conditional on being employed at $t-24$. Event study analysis using Callaway and Sant'Anna's (2021) staggered difference-in-difference estimator. Time window refers to 24 months before the last pseudo court date of the first drink-driving conviction and up to 24 months after. Reference time point is $t=-1$. Numbers in parenthesis are standard errors. *** p<0.01, ** p<0.05, * p<0.1. Source: IDI and authors calculations.

## B.1 Decomposition

**Labour 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 labour 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.

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 ob-

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

| State | Description | months to/from last court date $-24 \leq t \leq -2$ | months to/from last court date $0 \leq t \leq +24$ |
|---|---|---|---|
| 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 | |

*Note:* Event study analysis using Callaway and Sant'Anna's (2021) staggered difference-in-difference estimator. Time window refers to 24 months before the first drink-driving conviction and up to 24 months after. Reference time point is $t = -1$. Numbers in () are standard errors. *** p<0.01, ** p<0.05, * p<0.1. Source: IDI and authors calculations.

servable 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 labour 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.

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

| State<br>Description | months to/from last court date $-24 \leq t \leq -2$ | months to/from last court date $0 \leq t \leq +24$ |
|---|---|---|
| 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 | |

*Note:* Event study analysis using Callaway and Sant'Anna's (2021) staggered difference-in-difference estimator. Time window refers to 24 months before the first drink-driving conviction and up to 24 months after. Reference time point is $t = -1$. Numbers in () are standard errors. *** p<0.01, ** p<0.05, * p<0.1. Source: IDI and authors calculations.

**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]

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 labour-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 B.4 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).

Table B.4: Decomposing employment effects by industry screening intensity (averages)

| 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 | |

*Note:* Event study analysis using Callaway and Sant'Anna's (2021) staggered difference-in-difference estimator. Time window refers to 24 months before the first drink-driving conviction and up to 24 months after. Reference time point is $t = -1$. Numbers in () are standard errors. *** p<0.01, ** p<0.05, * p<0.1. Source: IDI and authors calculations.

**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 licence.

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

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.5 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.

Table B.5: Decomposing employment effects by industry driving requirements (averages)

| 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 | |

*Note:* Event study analysis using Callaway and Sant'Anna's (2021) staggered difference-in-difference estimator. Time window refers to 24 months before the first drink-driving conviction and up to 24 months after. Reference time point is $t = -1$. Numbers in parenthesis are standard errors. *** p<0.01, ** p<0.05, * p<0.1. Source: IDI and authors calculations.

# C Further Results on Robustness

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




(a) Employment

(b) Entering employment

(c) Exiting employment

(d) Wages & Salaries

*Notes:* These figures show economic outcomes 24 months before and after the first drink-driving conviction. The blue regression line refers to the coefficients using Callaway and Sant'Anna's (2021) staggered difference-in-difference estimator. The maroon regression line refers to exact matching. The black dashed line refers to the last court date and the gray dashed line to the the average offense date relative to the last court date. Entering employment is conditional on being non-employed at $t-24$. Exiting employment is conditional on being employed at $t-24$. Source: IDI and authors calculations. Source: Authors calculations and IDI.

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

| | With controls<br>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) |
| Labour income | 42.761 | -107.673*** |
| | (40.058) | (38.558) |
| Unique individuals | 3,636 | |

*Note:* Entering employment is conditional on being non-employed at $t-24$. Exiting employment is conditional on being employed at $t-24$. Event study analysis using Callaway and Sant'Anna's (2021) staggered difference-in-difference estimator (column I) and exact matching (column II), where controls are NZ-born individuals with no criminal convictions, matched exactly on sex, ethnicity (European vs non-European), birth year-quarter, employment status 24 months before conviction, and earnings within ±NZ$250 if employed.Time window refers to 24 months before the first drink-driving conviction and up to 24 months after. Reference time point is $t = -1$. Numbers in parenthesis are standard errors. *** p<0.01, ** p<0.05, * p<0.1. Source: IDI and authors calculations.

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

| | 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 | |

*Note:* Entering employment is conditional on being non-employed at $t-24$. Exiting employment is conditional on being employed at $t-24$. Event study analysis using Callaway and Sant'Anna's (2021) staggered difference-in-difference estimator. Time window refers to 24 months before the first drink-driving conviction and up to 24 months after. Reference time point is $t = -1$. Numbers in parenthesis are standard errors. *** p<0.01, ** p<0.05, * p<0.1. Source: 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 | |
|---|---|---|
| | $-24 \leq t \leq -2$ | $0 \leq t \leq +24$ |
| 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 | |

*Note:* Entering employment is conditional on being non-employed at $t-24$. Exiting employment is conditional on being employed at $t-24$. Event study analysis using Callaway and Sant'Anna's (2021) staggered difference-in-difference estimator. Time window refers to 24 months before the first drink-driving conviction and up to 24 months after. Reference time point is $t = -1$. Numbers in () are standard errors. *** p<0.01, ** p<0.05, * p<0.1. Source: IDI and authors calculations.

Table C.4: Regression results—alternative outcomes

| | 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 | |

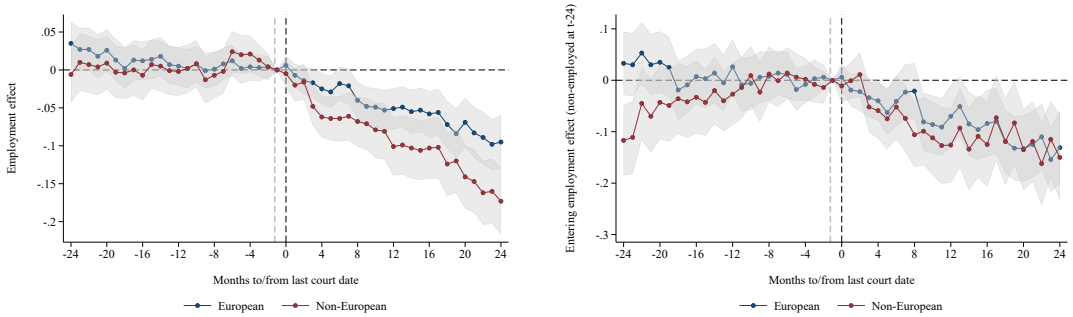
*Note:* Same employer is conditional on being employed at $t - 24$ and $t$ and takes the value of 1 if it is the sample employer and 0 else. Wage change is conditional on being employed at $t - 24$ and $t$ and calculated the absolute difference. Low paying industry is conditional on being employed at $t$ and takes the value of 1 if the mean industry wage at that date belongs to the bottom quartile and 0 else. Using the ANZSIC 2006 Industry classification at the 5 digit level. Enrolled in tertiary education is a binary marker taking the value of 1 if the person is enrolled at $t$ in a university, polytechnic, private training establishments, colleges of education, other tertiary education providers, or Wānanange and 0 else. Event study analysis using Callaway and Sant'Anna's (2021) staggered difference-in-difference estimator. Time window refers to 24 months before the first drink-driving conviction and up to 24 months after.. Reference time point is $t = -1$. Numbers in parenthesis are standard errors. *** p<0.01, ** p<0.05, * p<0.1. Source: IDI and authors calculations.

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

| | 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*** | -858.7.6 | -6714.319*** | -1736.942** | -7241.413*** | 100.751 | -5212.879*** |
| | (4999.660) | (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 | |

*Note:* Total earnings is measured at the financial year level spanning from April of the previous year until March. Income sources include wages & salaries, self employment, investments, income support payments, tax credits, student allowances, paid parental leave, ACC income, other income. Being self-employed is a binary indicator taking the value of 1 if the person received negative income or positive income exceeding NZ$5000 in the respective financial year or 0 else (findings are robust to other threshold definitions). Income from self employment only includes individuals with negative income or positive income exceeding NZ$5000 from self-employment in the respective financial year. Event study analysis using Callaway and Sant'Anna's (2021) staggered difference-in-difference estimator. Time window refers to 4 year before the first drink-driving conviction and up to 4 years after. Reference time point is $t = -1$. Numbers in parenthesis are standard errors. $^{***}$ p<0.01, $^{**}$ p<0.05, $^{*}$ p<0.1. Source: IDI and authors calculations.

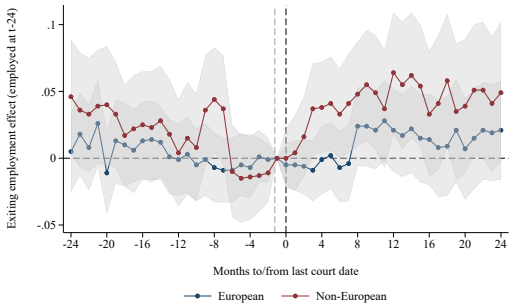
Figure C.2: Economic outcomes before and after the first drink-driving conviction, event study regression by ethnicity



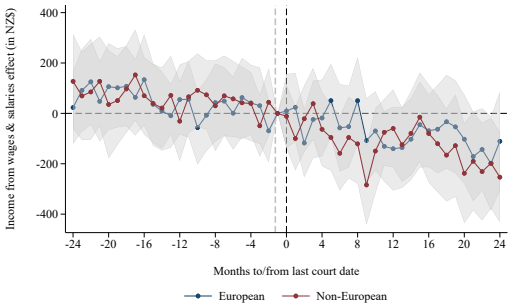

(a) Employment




(b) Entering employment




(c) Exiting employment




(d) Wages & Salaries

*Notes:* These figures show economic outcomes 24 months before and after the first drink-driving conviction for Europeans and non-Europeans. The black dashed line refers to the last court date and the gray dashed line to the the average offense date relative to the last court date. Entering employment is conditional on being non-employed at $t-24$. Exiting employment is conditional on being employed at $t-24$. Source: IDI and authors calculations. Source: Authors calculations and IDI.

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




(a) Employment

(b) Entering employment

(c) Exiting employment

(d) Wages & Salaries

*Notes:* These figures show economic outcomes 24 months before and after the first drink-driving conviction for male and female. The black dashed line refers to the last court date and the gray dashed line to the the average offense date relative to the last court date. Entering employment is conditional on being non-employed at $t-24$. Exiting employment is conditional on being employed at $t-24$. Source: IDI and authors calculations. Source: Authors calculations and IDI.

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

| | 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 | |

*Note:* Sample includes people who were at the offence date aged below 30. Entering employment is conditional on being non-employed at $t-24$. Exiting employment is conditional on being employed at $t-24$. Event study analysis using Callaway and Sant'Anna's (2021) staggered difference-in-difference estimator. Time window refers to 24 months before the first drink-driving conviction and up to 24 months after. Reference time point is $t=-1$. Numbers in parenthesis are standard errors. † Individuals without any enrolment at a university or polytechnics or enrolled not prior to the conviction date. ‡ Individuals enrolled at a university or polytechnics prior to the conviction date. *** p<0.01, ** p<0.05, * p<0.1. Source: IDI and authors calculations.

# D Model: Formal Results and Extension

## D.1 Proofs

This proposition formalizes that criminal records reduce employment primarily by lowering hiring probabilities, as they increase the productivity threshold required for a match to form.

**Proposition 1** (Hiring probability and criminal records)**.** *Let worker productivity be $y_i = \bar{y} + \varepsilon_i$, where $\varepsilon_i \sim F(0, \sigma^2)$ and $F$ is strictly increasing. Suppose firms hire worker $i$ if and only if expected match surplus is nonnegative: $J_i^H = (1-\beta)\left(\mathbb{E}[y_i \mid s_i, R_i] - b\right) \geq 0$. Assume that a criminal record lowers expected productivity at the hiring stage: $\mathbb{E}[y_i \mid s_i, R_i = 1] = \mathbb{E}[y_i \mid s_i] - \delta,\ \delta > 0$. Then there exists a hiring threshold $\tilde{y}(R_i)$ such that workers are hired only if*

$$y_i \geq \tilde{y}(R_i),$$

*with*

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

*Consequently, hiring probabilities satisfy*

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

*and*

$$\frac{\partial P(\textit{hire}_i)}{\partial \delta} < 0.$$

*Proof.* A match forms whenever expected surplus is nonnegative:

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

Since $(1-\beta) > 0$, this condition is equivalent to

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

Using $y_i = \bar{y} + \varepsilon_i$, this defines a productivity threshold $\tilde{y}(R_i)$ such that workers are hired only if

$$y_i \geq \tilde{y}(R_i).$$

Because a criminal record lowers expected productivity by $\delta$, firms require a higher productivity realization for convicted workers: $\tilde{y}(1) > \tilde{y}(0)$. Hiring probabilities are therefore

$$P(\text{hire}_i \mid R_i) = P(y_i \geq \tilde{y}(R_i)) = 1 - F(\tilde{y}(R_i) - \bar{y}).$$

Since $F$ is strictly increasing and $\tilde{y}(1) > \tilde{y}(0)$,

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

Moreover, an increase in $\delta$ raises the hiring threshold, implying

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

□

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

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

*where $\Omega_i$ denotes the information available to firms.*
*For newly hired workers,*

$$\Omega_i = (s_i, R_i),$$

*while for incumbent workers,*

$$\Omega_i = (s_i, p_i, R_i),$$

*where $p_i$ is a performance signal observed after hiring.*

*Assume employer learning attenuates the informational content of criminal records:*

$$\mathbb{E}[y_i \mid s_i, p_i, R_i = 1] = \mathbb{E}[y_i \mid s_i, p_i] - \rho\delta,$$

*where $\rho \in [0, 1]$.*

*Then wage effects among employed workers are attenuated for two reasons:*

1. *Convicted workers who remain employed are positively selected due to the higher hiring threshold;*
2. *Employer learning reduces the informational role of criminal records over time when $\rho < 1$.*

*Proof.* Wages are increasing in expected productivity:

$$w_i = (1 - \beta)b + \beta\mathbb{E}[y_i \mid \Omega_i].$$

At the hiring stage, convicted workers face a higher hiring threshold:

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

Among employed workers, this truncates the lower tail of the productivity distribution for workers with criminal records. Since

$$\tilde{y}(1) > \tilde{y}(0),$$

the distribution of $y_i$ among employed workers with $R_i = 1$ first-order stochastically dominates that for workers with $R_i = 0$. Consequently,

$$\mathbb{E}[y_i \mid y_i \geq \tilde{y}(1)] \geq \mathbb{E}[y_i \mid y_i \geq \tilde{y}(0)].$$

This positive selection offsets part of the direct wage penalty associated with criminal records. In addition, after hiring firms observe performance signals $p_i$. Employer learning reduces the informational role of the criminal record from $\delta$ to $\rho\delta$, where $\rho < 1$. Therefore, the productivity penalty associated with criminal records is attenuated for incumbent workers:

$$\rho\delta < \delta.$$

Together, positive selection into employment and employer learning imply that wage effects conditional on employment are smaller than the corresponding effects on hiring probabilities. □

A higher hiring threshold induces positive selection into employment, while employer learning reduces the informational content of criminal records over time. Both mechanisms attenuate wage effects among employed workers relative to the larger effects observed for employment entry.

## D.2 Employer Screening and Monopsony Power

This appendix extends the conceptual framework in the main text to allow for employer wage-setting power (see subsection 4.5). The purpose is to examine how the interaction between criminal records and monopsonistic labour markets affects employment and wages.

*Setup*. As in the baseline model, worker productivity is

$$y_i = \bar{y} + \varepsilon_i,$$

where $\varepsilon_i \sim F(0, \sigma^2)$. Employers observe a signal $s_i$ together with a criminal record indicator $R_i \in \{0, 1\}$. For newly hired workers, expected productivity satisfies

$$\mathbb{E}[y_i|s_i, R_i = 1] = \mathbb{E}[y_i|s_i] - \delta,$$

where $\delta > 0$ captures the productivity penalty or stigma associated with a criminal record.

Unlike the baseline model, firms now have wage-setting power. Following the monopsony literature, wages are determined by

$$w_i = \mathbb{E}[y_i|s_i, R_i] - \mu_i, \tag{14}$$

where $\mu_i \geq 0$ denotes the monopsony markdown. The markdown reflects the firm's ability to pay wages below expected productivity because workers face search frictions and imperfect outside options.

We allow criminal records to affect monopsony power by weakening workers' outside opportunities. Specifically,

$$\mu_i = \mu_0 + \phi R_i, \qquad \phi \geq 0. \tag{15}$$

When $\phi > 0$, firms exercise greater wage-setting power over convicted workers because those workers face fewer outside employment opportunities.

*Hiring*. The value of hiring worker $i$ is

$$J_i^H = \mathbb{E}[y_i|s_i, R_i] - w_i. \tag{16}$$

Substituting (14) into (16) gives

$$J_i^H = \mu_i.$$

Thus, hiring occurs whenever expected productivity exceeds the reservation productivity level required by the firm. As in the baseline model, a criminal record lowers expected productivity through the parameter $\delta$, reducing the set of workers who satisfy the hiring condition. Consequently, criminal records continue to reduce employment primarily through lower hiring probabilities.

*Employer Learning and Job Separations*. Once workers are employed, firms observe additional information about job performance. Let $p_i$ denote an observed performance signal. Expected productivity

for incumbent workers satisfies

$$\mathbb{E}[y_i|s_i, p_i, R_i = 1] = \mathbb{E}[y_i|s_i, p_i] - \rho\delta,$$

where $\rho \in [0, 1]$ measures the remaining informational content of the criminal record after employer learning. The surplus from an existing employment relationship is therefore

$$J_i^I = \mathbb{E}[y_i|s_i, p_i, R_i] - w_i.$$

Because $\rho\delta < \delta$, employer learning attenuates the effect of the criminal record on match surplus once a worker has been hired. Thus, even in a monopsonistic labour market, criminal records are predicted to affect employment primarily through hiring rather than through job separations.

*Wages*. Combining (14) and (15) yields

$$w_i = \mathbb{E}[y_i|s_i, R_i] - \mu_0 - \phi R_i. \tag{17}$$

Differentiating (17) with respect to criminal record status gives

$$\frac{\partial w_i}{\partial R_i} = -\delta - \phi. \tag{18}$$

Equation (18) shows that wage effects arise through two distinct channels. The first is the productivity-screening channel captured by $\delta$. The second is the monopsony channel captured by $\phi$, reflecting weaker outside opportunities and greater employer wage-setting power.

At the same time, positive selection into employment attenuates observed wage effects because convicted workers who remain employed are drawn disproportionately from the upper tail of the productivity distribution.

*Implications*. The monopsony extension preserves the main predictions of the baseline framework while refining the interpretation of wage effects. First, employment declines following conviction because employer screening lowers hiring probabilities. As in the baseline model, employment effects are driven primarily by reduced entry into employment rather than by increased separations.

Second, employer learning reduces the informational role of criminal records for incumbent workers, implying relatively modest effects on job retention. Third, wages may decline for two reasons: lower expected productivity and greater employer wage-setting power over workers with criminal records. Finally, the distinction between employment and wage effects becomes informative about underlying mechanisms. Employment losses identify the screening effect of criminal records because monopsony power alone does not reduce hiring probabilities. By contrast, wage losses reflect both screening and monopsony channels. Consequently, modest wage effects alongside substantial employment losses suggest that employer screening is the dominant mechanism in our setting.