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Ashani Abayasekara, Sonja de New and David Johnston

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Ashani Abayasekara: Health Economics Group, School of Public Health and Preventive Medicine, Monash University (email: ashani.abayasekara1@monash.edu); Sonja de New: Centre for Health Economics, Monash Business School, Monash University (email: sonja.denew@monash.edu); David Johnston: Centre for Health Economics, Monash Business School, Monash University (email: david.johnston@monash.edu).

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Worker Impacts from the Shutdown of a National Manufacturing Industry*

Ashani Abayasekara[†]

Sonja de New[‡]

David Johnston[§]

Abstract

As economies decarbonise and automate, entire industries within countries will disappear, raising questions about how displaced workers will fare and how policy can best support them. To provide evidence on this issue, this paper examines the economic and mental health consequences of the complete shutdown of Australia's automotive manufacturing industry. Using linked administrative data, we estimate the medium-term effects of this large-scale closure relative to comparable workers in unaffected manufacturing and construction sectors. We find substantial and persistent declines in employment and salary income among displaced workers, with limited recovery over five years. These effects are concentrated among older and lower-skilled workers, who experience higher rates of joblessness, occupational downgrading, and transition into self-employment. In contrast, younger and higher-skilled workers recover more quickly. Despite substantial disruption, we find no increase in mental healthcare use, potentially reflecting the unusually comprehensive support programs provided before and after closure.

Keywords: Job displacement, layoffs, Industrial transitions, employment, earnings, mental health

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[†] ashani.abayasekara1@monash.edu. Research Fellow, Health Economics Group, School of Public Health and Preventive Medicine, Monash University.

[‡] sonja.denew@monash.edu. Associate Professor, Centre for Health Economics, Monash Business School, Monash University.

[§] david.johnston@monash.edu. Professor, Centre for Health Economics, Monash Business School, Monash University.

1. Introduction

Technological change, globalisation, and climate policy are placing entire industries at risk of shutdown. In high-income countries with high labour costs and stringent regulations, sectors such as coal mining, thermal power generation, steel production, and automotive manufacturing face growing pressure from automation, international competition, and environmental targets (Acemoglu & Restrepo, 2019; Cooke et al., 2019; Braxton & Taska, 2023; Dupuis et al., 2024). These are typically blue-collar, capital-intensive industries that have historically employed large numbers of workers with limited formal education. The collapse of such industries entails the mass displacement of entire workforces, often concentrated in specific regions and occupations, with potentially large consequences for local labour markets, worker wellbeing, and economic inequality. Understanding these impacts is increasingly important as governments prepare for the large-scale industrial transitions required to meet economic and environmental goals.

We study the consequences of a rare but informative example: the complete shutdown of a national automotive manufacturing industry. In 2016-17, Australia's three major carmakers – Ford, General Motors, and Toyota – ceased domestic production, marking the end of a 100-year-old industry. This closure affected around 14,000 workers, many of whom were long-tenured, low-skilled, and based in concentrated blue-collar labour markets (Productivity Commission, 2014; Wallis & ACIL Allen, 2020). Analysing this event allows us to better understand how large-scale, industry-wide job displacement affects both the economic wellbeing and mental health of affected workers, helping to inform the design of targeted support programs for future large-scale displacements.

Impacts from an industry-wide shutdown may differ in important ways from the more commonly studied firm or plant closures. Government and public attention are often heightened, potentially leading to more generous layoff compensation (i.e., redundancy packages) and broader support programs, including career assistance and health and wellbeing services (Brutger & Guisinger, 2024). In addition, the collective nature of industry-wide layoffs may reduce the stigma of job loss and moderate psychological harm, especially when the closure is anticipated and widely understood (Green, 2011). On the other hand, industry-wide closures remove the possibility of re-employment in a similar firm or role, likely requiring more substantial retraining and increasing the risk of long-term scarring (Jacobson et al., 1993;

Carrington & Fallick, 2017), particularly in regions with few alternative employers. These offsetting mechanisms mean that the effects of industry closures may differ meaningfully from plant closures, both in magnitude and nature, warranting direct empirical investigation.

We use longitudinal administrative data from tax, welfare, and healthcare records, covering close to the universe of Australians and linked at the individual level. From this we construct a panel of relevant workers observed between 2010 and 2022. Our analysis focuses on blue-collar employees in the automotive manufacturing industry and compares them to matched workers in the same occupations in other manufacturing and construction industries. The empirical strategy uses a difference-in-differences event study design to trace the dynamics of outcomes over time. A key feature is that we distinguish between effects that emerge following the public announcements of closure (2013–14) and those that follow actual layoffs (2016–17), allowing us to examine how workers respond to anticipated versus realised job loss. Our analysis includes a broad set of labour market outcomes, including employment, income, occupation switching, welfare reliance, and self-employment, as well as use of mental healthcare.

We also explicitly investigate heterogeneity by skill level and age, as these factors are known to influence how workers respond to displacement. We distinguish between relatively lower-skilled workers (e.g., product assemblers) and higher-skilled workers (e.g., electricians), and between younger and older individuals. The international literature shows that older and lower-skilled workers face the greatest economic harm from displacement (Jacobson et al., 1993; Carrington & Fallick, 2017; Braxton & Taska, 2023), as they are less likely to benefit from retraining and may struggle to transfer their skills to new occupations.

We find modest effects across all outcomes in the period following the closure announcements but prior to the actual plant shutdowns. These limited early responses contrast sharply with large and persistent effects that follow the actual plant closures. On average, affected workers experienced significant declines in employment and earnings, increased welfare reliance, and greater occupation instability. These effects persisted for at least five years and were particularly pronounced for older, lower-skilled workers. For example, we estimate a 44% decline in salary income and an 11-percentage point reduction in employment for this group in the years following closure. In contrast, younger, higher-skilled workers experienced only modest disruptions.

Despite substantial and persistent economic effects, we find no evidence of increased mental healthcare use among displaced workers. This finding stands in contrast to previous studies of plant closures, which typically report worsened mental health (Kuhn et al., 2009; Marcus, 2013; Eisen et al., 2020). We suggest that this difference may be due to the extensive support programs implemented well before the closures, as well as the collective nature of the layoffs, which may have reduced stigma and facilitated coping.

This study contributes to two core strands of empirical literature. First, it adds to the extensive body of work examining the labour market consequences of job displacement.⁵ Prior studies have documented persistent reductions in earnings, employment, and job quality among workers affected by plant closures (Jacobson et al., 1993; Stevens, 1997; Eliason & Storrie, 2006; Hijzen et al., 2010; Couch & Placzek, 2010; Davis & von Wachter, 2011; Farber, 2017; Bertheau et al., 2023; Cuccu & Royuela, 2024). Our findings align with this literature in documenting enduring economic losses, but extend it by focusing on an industry-wide shutdown, which is a setting that has received far less attention. Second, we contribute to research on the mental health impacts of job loss. Previous studies show that displacement can lead to higher rates of psychological distress, antidepressant use, and suicide, particularly among blue-collar men (Hamilton et al., 1990; Kuhn et al., 2009; Marcus, 2013; Reichert & Tauchmann, 2017; Eisen et al., 2020; Venkataramani et al., 2020; Dufault et al., 2022; Blasco et al., 2024). In contrast, we find no evidence of increased mental healthcare use following the closure, raising questions about the role of collective experience and pre-emptive support in moderating these effects.

Our findings also contribute to forward-looking literatures concerned with how major industrial transitions should be managed. While our primary focus is on estimating realised outcomes from a completed industry shutdown, the evidence has clear relevance for normative and policy-oriented work on what might be considered fair or equitable transitions, including studies that examine institutional responses and the political economy of support provision (e.g., Dupuis et al., 2024). For example, Dupuis et al. (2024) argue that the “justness” of industry restructuring from the perspective of workers depends on the scale of job losses, the

⁵ Although the shutdown of Australia’s automotive assembly industry received significant media and policy attention, there is limited rigorous empirical research on the economic and health impacts for affected workers. Existing studies are primarily qualitative and focus on short-term experiences in the months following closure (Anaf et al., 2013; Wallis & ACIL Allen, 2020; Irving et al., 2022).

quality of new employment opportunities, and the nature of workers' transitions between jobs. Our paper also complements modelling-based analyses of potential employment and fiscal consequences from future closures, such as those examining the coal phase-out in Germany, Chile, and China (e.g., Heinisch et al., 2021; Feng et al., 2023; Clark & Zhang, 2022).⁶ In this context, our study provides rare empirical evidence on the realised impacts of a full industry closure – evidence that can improve the realism of modelling exercises and the policy tools used to manage future industrial transitions.

2. Automotive industry closure and support for workers

2.1 Plant closures

Australia's car manufacturing industry shut down in a staged process between October 2016 and October 2017, following public announcements by Ford, General Motors, and Toyota between May 2013 and February 2014. The closures were attributed to a combination of factors, including an overvalued Australian dollar, high production costs, declining domestic demand, and the withdrawal of government subsidies (Toscano, 2019; Spinks, 2014). Five major plants closed: four in adjoining regions in the state of Victoria and one in South Australia. Most plants scaled down operations over time, allowing for early workforce departures, while one maintained full production until closure. The specific announcement and closure dates are shown in Appendix Table A1. Alongside the main carmakers, many automotive supply chain firms were affected. In total, approximately 14,000 workers lost jobs, including those in around 50 supplier companies that closed in Victoria and South Australia (Wallis & ACIL Allen, 2020). These included a relatively high proportion of middle- and older-aged high-tenure blue-collar workers with low levels of formal education (Productivity Commission, 2014; Wallis & ACIL Allen, 2020).

Figure 1 shows Australian domestic motor vehicle production from 2010 onwards. Output slightly decreased from 2010 to 2012, then fell sharply between 2013 and 2016, before

⁶ Burke et al. (2019) assess the local unemployment effects of coal-fired power station closures in Australia, finding that a closure increases local unemployment by around 0.7 percentage points.

collapsing to near-zero following the final closures.⁷ The vertical lines mark the key phases: pre-announcement (2010–2013), post-announcement (2014–2016), and post-closure (2017 onward).

[Figure 1]

2.2 Government and industry support

In response to the announced closures, the federal and state governments, car manufacturers, and unions implemented a comprehensive set of support measures for affected workers. Most were introduced well before the final shutdowns, with major programs beginning in 2014. Support focused on three main areas: redundancy payments, employment transition assistance, and health and wellbeing services.

Redundancy payments were substantial and included both statutory entitlements and additional employer-provided benefits. Workers received payments typically ranging from AUD 80,000 to 117,000, depending on skill level and tenure. These included accrued leave (sick, annual, and long service), four weeks' pay per year of service, and additional "loyalty" bonuses (McDonald, 2013; Dowling, 2014; Michael, 2014). At plants where terminations occurred in phases, workers who left before plant closures received a pro-rata redundancy payment.

Employment transition support included pre-retrenchment training, career planning, and personalised job search assistance (Wallis & ACIL Allen, 2020). Workers were also eligible for financial support to pursue further education, occupational licensing, or other approved career transition activities. Financial counselling was also provided to assist workers in making informed decisions about managing redundancy payouts and planning their next steps. Health and wellbeing services were designed to mitigate stress and mental health impacts. These included on-site and community-based counselling, resilience training, mental health workshops, and access to gym and fitness facilities (Wallis & ACIL Allen, 2020). Services extended to workers' families and were often delivered through schools, sporting clubs, and

⁷ Research and design activities continued in most plants after the closure of the automotive assembly industry. In addition, a small number of boutique or specialist vehicle manufacturers continue to operate in Australia, but their scale of production is extremely limited and not comparable to the former industry.

local outreach centres. These centres remained open for up to 26 months after the closures and also functioned as informal hubs for social connection and peer support.

3. Data and sample

3.1. Sample construction

We use linked administrative microdata from the Australian Bureau of Statistics' (ABS') Person Level Integrated Data Asset (PLIDA) for the financial years 2010/11 to 2022/23. PLIDA integrates records from individual tax returns, welfare payments, health service use, and the Census, enabling longitudinal tracking of individual workers across a range of outcomes. Data are organised by financial year (July 1st to June 30th), reflecting the structure of tax reporting in Australia.

We construct a panel of blue-collar working-age individuals using characteristics measured in the 2011 Census. Blue-collar workers are defined based on the Australian and New Zealand Standard Classification of Occupations (ANZSCO) as technicians and trades workers (relatively higher skilled), and machinery operators, drivers, and labourers (relatively lower skilled).⁸ We include individuals aged 23–54 in 2010/11 who reported working at least 25 hours per week and had total salary income above the annual minimum-wage earnings threshold (AUD 27,355 in 2010/11). This age range is chosen to ensure that individuals had sufficient time to complete post-school education and training by the start of the sample period and were below retirement age at the time of the industry shutdown.

To form our treatment and control groups, we restrict the sample to individuals employed in three broad industry groups, defined using the Australian and New Zealand Standard Industrial Classification (ANZSIC): transport equipment manufacturing (hereafter 'automotive

⁸ We focus on these blue-collar occupations for both empirical and policy reasons. First, they account for the vast majority of roles directly affected by the automotive industry shutdown and are the most likely to experience displacement due to the closure of production facilities. Clerical, administrative, and sales workers are less prevalent in our data, while managers and professionals (e.g., mechanical engineers) tend to possess more generalised skills and higher qualifications, making them less reliant on firm- or industry-specific human capital and better insulated from displacement effects. More broadly, blue-collar workers remain at the centre of global policy debates on structural adjustment, given the labour market pressures facing manufacturing, mining, and energy sectors in high-income countries.

manufacturing'), other manufacturing (excluding automotive supply chain industries), and construction.⁹ The first group forms the treatment sample; the latter two serve as control industries. These comparison groups were selected to approximate the counterfactual employment trajectories of affected workers. They comprise similar types of physically intensive, blue-collar work, but were not exposed to industry-specific shocks over the study period. Moreover, the automotive sector has limited input-output or labour market linkages with these industries, reducing the likelihood of indirect treatment effects.

We additionally apply geographic restrictions to the sample. The treatment group consists of automotive workers residing in the five labour market regions where automotive plants were located.¹⁰ The manufacturing and construction workers in the control group are drawn from major city and inner-regional areas in the same two states, but excluding the plant regions and any directly adjoining areas. This helps minimise spillover effects. For example, if displaced automotive workers accepted jobs in nearby construction or manufacturing businesses, it could worsen employment outcomes for workers in the control industries. Although selecting regions in the same states increases the risk of such spillovers, it allows us to control for common trends in state-level industrial policy, labour demand, and macroeconomic conditions. As a robustness check, we test an alternative specification using control workers residing in a neighbouring state.

To further improve the comparability of the treatment and control groups we apply exact matching on four-digit occupation codes in 2010/11. This ensures that each treated worker is compared to a control worker with the same detailed occupation, but employed in a different industry. For instance, we compare outcomes over time of forklift drivers in automotive manufacturing to forklift drivers in beverage manufacturing who live in a region not adjacent to the automotive plant areas.¹¹ This approach improves internal validity by holding job-specific skills and knowledge constant. The trade-off is a reduction in sample size, as workers

⁹ The specific industries included in the treatment and control groups, and the automobile supply chain sub-industries omitted, are listed in Appendix Table B1.

¹⁰ We define local labour markets using Statistical Area Level 4 (SA4) regions, the largest sub-state units in the Australian Statistical Geography Standard (ASGS). SA4s are designed to reflect functional labour markets, typically centred on regional cities or metropolitan areas, with populations ranging from 100,000 to 500,000 (ABS, 2021). They are used by the ABS for regional labour force statistics and policy planning. SA4s nest within states and territories, and fully aggregate to the Greater Capital City Statistical Areas. There are 108 SA4s covering the entirety of Australia without gaps or overlaps.

¹¹ Appendix Table B2 lists the most frequent four-digit occupations among the treatment sample, for whom we find matched workers engaged in the same occupations from the control group.

in unmatched occupations are dropped.¹² Our analyses by skill level and age group further improve comparability and account for potential heterogeneity in treatment effects, as this stratification not only helps to capture meaningful differences in how workers respond to displacement, but also enhances balance between treated and control groups within each subgroup. Our final matched sample includes 8,230 workers (4,115 treated and 4,115 controls).

3.2 Outcome variables

We examine annual economic and mental health-related outcomes using linked tax, welfare, and health records. Economic outcomes include employment, log real salary income (adjusted using the CPI), business income, occupation change, and welfare receipt. Business income refers to income reported by individuals from operating a business, including as sole traders or independent contractors (e.g., freelancers, tradespeople, and delivery drivers). We use this to proxy for self-employment activity, noting that individuals may report both salary and business income in the same year. Occupation change is measured using self-reported occupations in individual tax returns, and equals one if the person's two-digit occupation code differs from their 2010/11 code. We use two-digit codes, rather than more detailed four-digit categories, to allow for small within-field transitions. For example, a worker moving from welding to sheetmetal work would remain in the same two-digit category. This means our occupation change outcome captures broader occupational shifts, often requiring significant re-training. Finally, welfare receipt includes payments from the Newstart Allowance or Jobseeker Payment, and from the Disability Support Pension.¹³

Mental health outcomes are based on two types of service use and are measured from 2011/12. The first is annual use of Medicare-subsidised psychological therapeutic services.¹⁴ These

¹² We also explored matching on additional characteristics such as age and gender. However, we found that treated and control groups were already sufficiently balanced on these dimensions when matching on occupation alone, and that including additional variables in the matching process did not significantly change the results. A key consideration in our approach was the trade-off between improving covariate balance and retaining sample size.

¹³ Unemployment benefits, offered via the Newstart Allowance scheme until March 2020 and then replaced with JobSeeker Payment, provide ongoing financial assistance to unemployed individuals actively seeking employment. The Disability Support Pension (DSP) provides income support to individuals with a permanent physical, intellectual, or psychiatric condition that prevents them from working. In practice, it may serve as an alternative to unemployment benefits for individuals with limited employability, particularly following prolonged job search. While eligibility is more restrictive than for JobSeeker, DSP uptake can increase in the context of large-scale displacement or localised labour market decline.

¹⁴ Medicare is Australia's publicly funded universal healthcare system, which provides subsidies for a broad range of medical services, including general practitioner visits, specialist care, and mental health services.

Medicare records cover the majority of formal mental health consultations in Australia, but do not include informal or employer-provided counselling services. The second is use of prescribed mental health medications, specifically antidepressants, anxiolytics, and sedatives. Since prescriptions are only available through registered medical professionals, we observe all medication use.

3.3 Summary Statistics

Table 1 presents means for selected characteristics in the baseline year (2010/11), separately for treatment and control groups, disaggregated by skill level. Within each skill group, the two groups appear broadly similar across demographic and economic indicators. For example, among lower-skilled workers, mean age differs by 0.56 years (41.50 vs 42.06), education differs by 0.01 years (11.30 vs 11.31), and log salary income differs by 0.14 (10.96 vs 10.82), equivalent to a 1.3% difference relative to the treatment group mean.¹⁵ For higher-skilled workers, the differences are similarly small. Differences in mental health service use are also modest. For instance, the shares using psychological therapy services are 1.7% and 2.6% among lower-skilled workers, and 1.7% and 2.0% among higher-skilled workers. The largest observed difference is in gender composition among lower-skilled workers, where the treatment group has a higher share of men (84.5% vs 73.3%), though both are male-dominated.

This close comparability increases the likelihood that the treatment and control groups would have followed similar labour market and mental healthcare trends in the absence of the shutdowns, supporting the credibility of our difference-in-differences design. At the same time, our empirical strategy does not require perfect balance on observables, as long as unobserved determinants of trends are uncorrelated with treatment status. In Section 4, we provide visual and statistical evidence of parallel pre-trends across outcomes.

[Table 1]

¹⁵ Everyone is employed and welfare use is close to zero in the baseline year given the sample restrictions outlined in 3.1. Similarly, it is not possible to have had an occupation change in 2010/11.

4. Methodology

We estimate the economic and health impacts of the automotive manufacturing shutdown using a difference-in-differences (DiD) event study specification, comparing outcomes over time between workers in the automotive manufacturing industry in regions affected by plant closures with similar workers in other manufacturing subindustries and the construction industry residing in comparable areas of the same two states, excluding the plant regions and their immediate surrounds, as defined in Section 3.1. Our main specification is:

$$Y_{it} = \sum_{k=11/12}^{21/22} \alpha^k \cdot (auto_i \times year_k) + \gamma_i + \gamma_t + \varepsilon_{it} \quad (1)$$

In this specification, Y_{it} denotes the outcome of interest for individual i in financial year t . The variable $auto_i$ is an indicator equal to one for individuals employed in the automotive manufacturing industry in 2010/11 in treated areas, and zero otherwise. The variable $year_k$ is a binary indicator for financial year k , ranging from 2011/12 to 2021/22, with 2010/11 omitted as the base year. The parameter α^k captures the average difference in outcomes between treated and control workers in year k , relative to their difference in 2010/11. The model includes individual fixed effects γ_i , which absorb all time-invariant differences across individuals, and year fixed effects γ_t , which control for shocks common to all individuals in a given year. The error term ε_{it} captures idiosyncratic shocks, and standard errors are clustered at the individual level.

The use of year-specific interactions accommodates the ‘fuzzy’ nature of the treatment. Closure announcements occurred at the very end of the 2012/13 financial year and in the middle of 2013/14, while actual shutdowns took place across 2016/17 and into the second quarter of 2017/18 (see Appendix Table A1). We therefore expect the initial effects to emerge in 2014/15, the first full financial year following the closure announcements, with more pronounced effects appearing after the actual shutdown, particularly from 2017/18 onward. The announcement event is expected to have induced some early leaving, especially among higher-skilled workers with strong outside options, but the largest impacts are likely to arise in the post-shutdown period when mass layoffs occurred. The financial year 2010/11 is used as the reference period rather than the year immediately preceding these events, as there is no single treatment date. Moreover, sample construction is based on 2010/11 characteristics, including employment

conditions for both treated and control groups, meaning that any coefficient estimated for this year would be mechanically constrained and potentially misleading.

The α^k estimates from Equation (1) should be interpreted as intention-to-treat effects. Treatment status is defined based on industry of employment in 2010/11 (transport equipment manufacturing) and residence near the automotive plants. While this classification implies high exposure to the closure shock, we do not observe employer identifiers. Although it is highly likely that the treatment group includes workers at the three major car manufacturers or directly affected supply chain firms, a small share may not have lost their jobs. As such, treatment status reflects exposure to, rather than certainty of, job displacement.

While treatment is defined based on pre-closure industry and location, all treated individuals were exposed to the same policy shock, the automotive industry shutdown, within a narrow time window, and the control group was never exposed to a similar treatment. This design does not involve staggered treatment adoption or variation in treatment timing across units. As such, concerns that arise in settings with staggered timing and heterogeneous treatment effects (e.g., Sun & Abraham, 2021; Callaway & Sant'Anna, 2021) do not apply here.

4.1 Matching and the parallel trends assumption

Our identification strategy relies on the standard DiD assumption that, absent the industry closure, average outcomes of treated and control workers would have followed parallel trends. To improve the validity of this assumption, we have matched exactly on detailed four-digit occupational codes. This matching approach controls for occupation-specific factors such as job skills, wage-setting institutions, and working conditions that might otherwise generate differential outcome trajectories. Additionally, treated and control groups are observably similar at baseline across demographic, economic, and health indicators (see Section 3.3). A drawback of exact matching is that it substantially reduces our effective sample size, limiting external validity by excluding occupations without exact matches. To address this concern, we perform robustness checks using a larger sample obtained through propensity score matching, and this gives very similar results (see Section 5.5).

Despite the comparability of treated and control groups, a remaining identification concern is the potential presence of industry-specific shocks differentially affecting automotive and

control-industry workers during our study period. For instance, the automotive industry experienced prolonged decline even prior to the closure announcements, possibly inducing differential pre-trends relative to more stable industries. To investigate this, we estimate models including year-by-year interaction terms, allowing for flexible deviations from parallel trends. Visual inspection of these event-study coefficients reveals no large or systematic pre-treatment differences, with the estimated pre-treatment coefficients being consistently small in magnitude. Also, formal joint F-tests of the pre-treatment interaction terms (2011/12–2013/14) fail to reject the null hypothesis of no differences for most outcomes and samples (see Appendix Table C1).

Recent literature cautions against exclusively relying on statistical pre-trends tests for validating parallel trends (Kahn-Lang & Lang, 2020; Roth, 2022; Roth et al., 2023). These tests often exhibit low statistical power, meaning failure to reject no pre-trends does not fully exclude the possibility of violations sufficiently large to bias treatment effects. Conversely, a statistically significant pre-trend does not automatically invalidate causal interpretations if post-treatment effects are considerably larger and of different timing or structure. To complement our tests, we use the approach proposed by Rambachan and Roth (2023), which relaxes the parallel trends assumption and quantifies how robust estimated effects are to plausible violations. As discussed further in Section 5.1, our primary estimates remain stable under a range of plausible scenarios.

Finally, interpreting our estimates causally requires consideration of potential general equilibrium (GE) spillover effects, such as displaced automotive workers impacting local wages, unemployment rates, or labour demand in adjacent markets or industries. Our primary control-group selection mitigates these concerns by explicitly excluding regions adjoining automotive plants. However, residual GE effects might still influence estimates. To address this explicitly, we use an alternative control group from New South Wales, geographically separated and unlikely to experience direct or indirect spillovers. Our results remain robust to this alternative specification (see Section 5.5).

5. Labour Market Effects

This section presents the estimated effects of the automotive manufacturing industry closure on labour market outcomes. We begin with an examination of pre-announcement dynamics to assess the plausibility of the identifying assumptions underpinning our DiD design. We then trace the evolution of effects through two key phases: the post-announcement, pre-closure transition period, and the post-shutdown adjustment period. The analysis focuses on five core outcomes – employment, salary income, welfare use, occupation change, and business income – disaggregated by worker skill group. We also explore heterogeneity in effects by age and skill and test the robustness of our findings to alternative treatment and control group samples.

5.1 Pre-Announcement Effects

Figures 2 and 3 present the estimated effects by financial year for lower skilled workers and higher skilled workers, respectively. The vertical dashed lines indicate the first full years of treatment for workers in all plants following the plant closure announcements (financial year 2014/15) and plant closures (financial year 2017/18). In the three years prior to the closure announcements, we observe no meaningful differences in outcomes between treated and control workers for either skill group. For lower-skilled workers (Figure 2), employment effects are 0.1 percentage points in 2011/12 and 0.2 percentage points in 2012/13.¹⁶ For higher-skilled workers (Figure 3), the corresponding estimates are 0.2 and 0.3 percentage points, respectively. Real salary income shows minor fluctuations without consistent direction or magnitude. Treated lower-skilled workers earned 2.6% more than controls in 2011/12, a statistically significant but economically modest difference that does not persist, and 0.9% less in 2012/13. For higher-skilled workers, income differences remain small and insignificant across both years. Other outcomes such as welfare use, occupation change, and business income also remain flat and statistically indistinguishable from zero during this period.

[Figure 2]

[Figure 3]

¹⁶ Estimates for the full sample and by skill level are provided in Appendix Table D1.

Economically, these pre-treatment estimates suggest that automotive industry workers were not experiencing disproportionate labour market decline relative to their matched peers in other industries prior to the closure announcements. This helps counter concerns that the treatment group may have already been on a downward trajectory due to broader structural pressures in the automotive sector. From an econometric perspective, the absence of meaningful differences in this period supports the parallel trends assumption underpinning our DiD strategy. Matching on detailed four-digit occupations and restricting to similar local labour markets appears to have produced treatment and control groups with similar trajectories prior to any treatment-related events.

We further assess the validity of the parallel trends assumption using the sensitivity analysis proposed by Rambachan and Roth (2023). This method constructs robust confidence intervals for treatment effects under bounded deviations from parallel trends, based on how much larger post-treatment violations would need to be relative to the largest pre-treatment deviation to invalidate a significant post-treatment effect. In our context, a plausible concern is that unobserved industry-specific or macroeconomic factors may have affected automotive workers differently from those in other sectors, even absent the closures. We therefore implement their recommended “bounds on relative magnitudes” – $\Delta^{RM}(\bar{M})$ – approach, which bounds the magnitude (\bar{M}) of any post-treatment violation relative to the worst-case pre-treatment deviation.¹⁷

Appendix Figures C1 and C2 show robust confidence intervals for post-treatment effects across a range of values for \bar{M} for lower- and higher-skilled workers, respectively. For most outcomes, including employment, occupation change, and business income, estimated effects remain statistically significant so long as we assume that post-treatment trend violations are no more than 1.5 times larger than the worst pre-treatment deviation ($\bar{M} = 1.5$). For log salary income and welfare use, for which we find some evidence of significant pre-trends in joint F-tests, effects are more sensitive, requiring post-treatment violations to be no larger than the maximum observed pre-trend deviation ($\bar{M} = 1$). Given that the closure period did not coincide with other

¹⁷ An alternative approach, referred to as the “smoothness restriction,” assumes that any violation of parallel trends in the post-treatment period evolves smoothly from the pre-trend—i.e., that the slope of the untreated potential outcome path changes by no more than M between adjacent periods. This method is suited to settings where violations arise from slowly evolving differences in secular trends between treatment and control groups (Rambachan & Roth, 2023). In our context, this assumption seems less appropriate, as it is unlikely that industry-specific shocks would follow a smooth trend over time.

major shocks that differentially affected the treatment group, and that the pre-treatment effects are small, particularly in comparison to the post-shutdown effects, we view our main estimates as reasonably robust to potential violations of parallel trends.

5.2 Post-Announcement Effects

Employment outcomes during the post-announcement, pre-closure period remain largely stable. The estimated employment effects for lower-skilled workers are small and statistically insignificant, apart from a 2.0 percentage point decline in 2015/16. These modest changes suggest that a small number of lower skilled workers may have taken early retirement or temporarily stepped away from work, possibly supported by financial redundancy packages, but such cases appear limited. Estimates for higher-skilled workers on the other hand are positive, albeit largely insignificant and small in magnitude (estimates of 1.4 and 1.3 percentage points in 2014/15 and 2015/16, respectively). A potential implication is that higher skilled staff held on (i.e., did not change jobs or depart the labour market) in order to qualify for full redundancy packages, the benefits of which increased with skill level and tenure. Since these observed effects appear minor, overall, the employment results indicate that treatment and control workers continued to follow broadly similar trends during this transitional period.

In contrast to the mostly stable employment patterns, we observe signs of an early behavioural response in the occupation change estimates. For lower-skilled workers, the proportion changing to a different two-digit occupation category rises steadily over the post-announcement period. In 2013/14, the effect is -0.2 percentage points and statistically insignificant, but this increases to 2.7 percentage points in 2014/15 ($p < 0.05$) and 4.7 percentage points in 2015/16 ($p < 0.01$). These shifts indicate that an increasing share of affected workers began moving into different occupations prior to the actual plant closures.¹⁸ The timing and magnitude are consistent with forward-looking behaviour, with workers moving early to avoid a saturated labour market at the point of mass redundancy.

For higher-skilled workers, the corresponding estimates are smaller and not statistically significant: 1.9 percentage points in 2013/14, 1.7 in 2014/15, and 2.5 in 2015/16. However,

¹⁸ These occupation changes reflect between-industry transitions, rather than job changes within automotive manufacturing, as the sector offered few internal promotion or role-change opportunities prior to the plant shutdowns.

these effects likely understate actual job mobility in this group. Higher-skilled workers, such as electricians, welders, and mechanics, are more likely to have found re-employment in the same occupation after leaving the automotive industry. Because our occupation change measure is based on two-digit codes, lateral job transitions within the same broad occupation group are not captured. These smaller measured effects may therefore reflect the greater portability of skills among higher-skilled workers, enabling direct transitions into similar roles in other industries, rather than a weaker behavioural response to the closure announcements.

An alternative explanation is that the structure of redundancy packages created stronger incentives for higher-skilled workers to remain employed until the official shutdown. Payouts were linked to both tenure and classification level, with larger sums available to those who stayed through to the final closure. For these workers, the economic value of waiting may have outweighed the potential costs of delayed job search or re-employment. In contrast, lower-skilled workers, who generally faced lower potential payouts and had fewer outside options, may have had stronger incentives to move early to secure new roles before local labour markets became congested.

Importantly, the early transitions were not without cost. Real salary income among employed lower-skilled workers fell by 3.7% to 8.6% between 2014/15 and 2016/17. These declines suggest that the new roles available to early movers were lower paid than their original positions, either due to occupational downgrading or a loss of tenure-linked pay. Reduced hours may also explain part of this effect, but our data do not permit an analysis of this outcome. For higher-skilled workers, earnings remained more stable through the post-announcement period, with no statistically significant declines until 2016/17 (-5.8%). This likely reflects both delayed transition and stronger bargaining power in new roles, consistent with their relatively lower rates of occupation switching.

5.3 Post-Shutdown Effects

The most substantial effects of the industry closure emerge in the years following the final shutdowns in 2016 and 2017. Among lower-skilled workers, employment falls sharply. The estimated effect in 2017/18 is -2.2 percentage points, rising to -7.4 points in 2018/19 and remaining statistically significant for several years thereafter, although the gap gradually narrows, falling to 5.4 percentage points in 2020/21 and 2.6 percentage points in 2021/22. The

binary employment measure may understate total labour supply effects if affected workers returned to jobs with reduced hours. Overall, these results point to persistent labour market displacement for a substantial share of displaced lower-skilled workers.

These patterns are less pronounced among higher-skilled workers. While employment declines slightly post-shutdown, with a peak gap of 4.6 percentage points in 2018/19, the effects are smaller in magnitude and not statistically significant in the later years of the sample. By 2021/22, the employment difference has narrowed to 1.2 percentage points. This rebound likely reflects stronger re-employment prospects among higher-skilled workers, consistent with their broader job options and more portable skills.

Among those not re-employed, a significant share of lower-skilled workers appears to have transitioned onto government income support. The treatment-control difference in welfare receipt for this group rises to 4.7 percentage points in 2019/20 and remains 2.6 points higher than the control group in 2021/22. This increase is gradual but persistent, consistent with ongoing difficulty reconnecting to the labour market. In contrast, welfare use among higher-skilled workers remains low and statistically insignificant throughout the period. These patterns suggest that the employment losses among lower-skilled workers were not the result of voluntary withdrawal or early retirement, but instead reflect constrained opportunities and prolonged joblessness.

For those who did re-enter employment, the data show clear evidence of forced reallocation across occupations, particularly among lower-skilled workers. The share of lower-skilled workers switching occupations jumps to 9.9 percentage points in 2017/18 and continues to rise, reaching 19.3 points in 2021/22. These changes are large and sustained, reflecting the absence of viable roles in the original industry and the need to retrain or pivot into unrelated fields. For higher-skilled workers, occupation switching also increases post-shutdown, though to a lesser extent: from 5.2 percentage points in 2017/18 to 9.5 points in 2021/22.

Earnings among those who remained employed fell sharply following the shutdowns, especially for lower-skilled workers. The estimated treatment effect on real salary income is –36% in 2017/18, –26% in 2019/20, and –20% in 2021/22. These large and persistent losses are consistent with transitions into lower-paying occupations, loss of tenure-based premiums, or reduced working hours. For higher-skilled workers, the earnings drop is smaller, peaking at

16% in 2017/18 and narrowing to 9.2% by 2021/22. Importantly, the income measure is conditional on having positive salary income, meaning some of the apparent rebound may reflect selective exit from employment among the lowest earners. Thus, the observed recovery in average earnings does not necessarily imply a return to pre-closure earnings capacity. Taken together with the occupation switching results, these estimates suggest that many displaced workers were re-employed in roles that offered lower earnings potential than their previous positions, with limited recovery even after five years. These findings are consistent with prior evidence that identifies workers sorting into lower paying job titles as the largest contributor to wage losses of displaced workers, compared to other mechanisms such as the allocation to lower remunerating employers and the loss of worker-firm match skills (Raposo et al., 2021).

Finally, we observe a small but sustained increase in the likelihood of having positive business income among lower-skilled workers. The proportion with business income rises by 1.7 percentage points in 2017/18 and remains approximately 1.3 points higher than the control group through 2021/22. These effects are persistent and statistically significant in 2017–2019 and 2021/22, though modest in size. Among higher-skilled workers, by contrast, the likelihood of having business income does not increase and the estimates are small and negative in most years. The timing and size of the effects suggests that business income among displaced lower-skilled workers was primarily a response to constrained employment opportunities, rather than a voluntary transition into self-employment driven by entrepreneurial opportunity.

5.4 Heterogeneity by skill and age

It is likely that workers were affected unequally by the closure of the automotive manufacturing industry. Theory and prior evidence suggest that older, lower-skilled workers face the greatest barriers to re-employment. On the supply side, this group is less likely to retrain, may face greater costs when shifting occupations, and often holds fewer formal qualifications. On the demand side, employers may favour younger or more educated candidates, whether due to perceptions of adaptability or age-based discrimination (Carlsson & Eriksson, 2019; Batinovic et al., 2023). In contrast, younger, higher-skilled workers typically have more portable skills, stronger labour market attachment, and longer time horizons to recover, and are therefore expected to be the least vulnerable. To examine these dynamics, we stratify the sample by skill

level and age, and present results for the two polar cases: older lower-skilled workers and younger higher-skilled workers (see Figure 4 and Appendix Table D2).

The results for older lower-skilled workers reveal substantial and persistent labour market disruption. Employment in this group falls by 11.5 percentage points in 2018/19 and remains 4.6 percentage points below the control group five years after the closure. These sustained losses point to prolonged joblessness and reinforce prior evidence on the re-employment difficulties faced by older displaced workers. We observe a small but consistent increase in the share of this group reporting positive business income. However, this increase, peaking at just over two percentage points, is modest relative to the overall decline in salaried employment. Conditional on being employed, real salary income falls by 44% in 2017/18 and remained nearly 30% below baseline levels by 2021/22. These large and persistent income losses likely reflect both occupational downgrading and reduced hours.

The scale of reallocation within this group is also striking. The proportion changing occupations increases steadily after the closures, reaching 21.2 percentage points by the end of the sample period. This high rate of occupational switching is consistent with forced reallocation out of the automotive sector and limited ability to transfer existing skills into equivalent roles. Combined, these results depict a group that experienced sustained exclusion from salaried employment, limited uptake of self-employment, significant earnings losses, and slow rates of recovery.

In stark contrast, the effects on younger higher-skilled workers are minimal. Employment differences are small and statistically insignificant throughout the post-closure period, with estimates fluctuating within ± 2 percentage points. Real salary income dips by 7% in 2017/18 but quickly recovers, returning to pre-closure levels within two years. This group shows a relatively modest rise in occupational switching, peaking at around six percentage points, suggesting that many were able to remain in the same occupation even when changing jobs. There is no evidence of increased welfare reliance or movement into self-employment. Taken together, these results indicate that younger higher-skilled workers successfully navigated the transition, consistent with their more generalisable skills, stronger position in the labour market, and greater attractiveness to prospective employers.

[Figure 4]

5.5 Sensitivity Analysis

We assess the robustness of our main estimates using two alternative samples. These specifications are designed to test the sensitivity of results to control group selection and matching strategy. While our main sample prioritises close comparability between treatment and control groups, by using exact occupation matching among workers from the same state, this approach results in a more restrictive sample. The alternative robustness samples include less tightly matched controls but allow for larger samples and additional geographic variation.

The first robustness check re-estimates the models using control workers from the same occupations as automotive industry workers, but located in New South Wales (NSW), a neighbouring state with no large automotive manufacturing plants. This aims to minimise potential spillovers from displaced automotive workers into the control group. Given the change in geography, we expand the treatment group to include workers in regions adjacent to the car plant locations. This sample includes 13,726 workers, evenly split across treatment and control groups.

In the second specification, we retain the original treatment and control industry groups but relax the requirement for exact occupation matching. Instead, we match individuals using propensity scores based on detailed occupation and age in 2011. This produces a larger sample of 21,800 workers (5,691 treated and 16,109 controls) and allows us to test whether results are sensitive to the matching method.

Table 2 presents the results for lower-skilled workers using both alternative specifications, alongside the estimates from the main sample.¹⁹ We report average treatment effects over the post-announcement (2014/15–2016/17) and post-closure (2017/18–2021/22) periods for ease of comparison. While some variation in magnitude and statistical significance is expected due to differences in sample composition and match quality, the results are largely consistent across samples, particularly for the post-closure results. For example, post-closure effects range from 4.8 to 5.4 percentage points for employment, from 25 to 27% for salary income, from 2.6 to 3.5 percentage points for welfare use, from 13.0 to 16.4 percentage points for occupation

¹⁹ Equivalent sensitivity estimates for higher-skilled workers are reported in Appendix Table D3. The results are similarly robust to the use of alternative samples and matching methods, with effect sizes and patterns consistent across specifications.

change, and from 0.9 to 1.6 percentage points for business income. In contrast, the smaller post-announcement effects are less consistent across specifications, especially for occupation change.

We interpret these findings as evidence that the main results are not driven by the choice of control group or matching strategy. While the same-state matched sample remains our preferred specification, the consistency of the post-closure effects across alternatives reinforces the credibility of our conclusions.

[Table 2]

6. Mental Healthcare Effects

While the preceding sections have documented large and persistent economic effects, particularly for lower-skilled workers, it remains unclear whether these disruptions translated into adverse mental health outcomes. We now turn to this question, using two outcomes derived from national administrative health records: use of psychological therapy services and prescription of mental health-related medications, including antidepressants, anxiolytics, and sedatives. These outcomes are observed annually through the Medicare Benefits Schedule (MBS) and the Pharmaceutical Benefits Scheme (PBS), and reflect all publicly subsidised services and prescriptions. While these measures provide high-quality, objective indicators of service use, they do not capture underlying mental health status directly. Rather, they reflect help-seeking behaviour conditional on perceived need, diagnosis, and access. Interpretation must also consider that our data do not include employer-provided counselling or informal supports, which may substitute for public services.

We estimate DiD models using the same specification as for economic outcomes, with results reported in Figure 5 and Appendix Table D4. For both mental health outcomes and across both skill groups, the estimates in the pre-announcement period provide support for the parallel trends assumption. In the post-announcement and post-closure periods, we observe consistently small effects on the use of psychological therapy services with no consistent pattern and only 1 of the 22 estimated effects statistically significant at the 5% level. The largest point estimate for lower-skilled workers is -1.4 percentage points in 2015/16, and for higher-skilled workers

is 1.5 percentage points in 2015/16. In contrast, we find a statistically significant decline in mental health medication use, but only for the lower-skilled group. The largest negative effect is -3.8 percentage points in 2021/22 ($p < 0.01$), with similarly negative but slightly smaller effects in all years from 2017/18.²⁰

[Figure 5]

These findings suggest that, despite experiencing substantial and prolonged economic disruption, automotive industry workers did not increase their use of publicly funded psychological therapy or mental health medications. Australia has one of the highest rates of antidepressant use in the OECD, and pharmacological treatment is common for moderate to severe depressive and anxiety disorders. Our PBS data capture nearly all prescribed mental health medications dispensed nationally. The absence of any increase in medication use therefore suggests that the closure did not lead to a rise in clinically significant depressive or anxiety disorders, at least at moderate or higher severity. While milder psychological distress may have occurred and gone unmeasured, we find no evidence of a deterioration in mental health severe enough to typically prompt medical intervention.

Our findings contrast with several previous studies that document significantly worsened mental health following job loss or plant closures. For example, Kuhn et al. (2009) find significant increases in antidepressant use among displaced male workers in Austria, and other studies using self-reported wellbeing measures report persistent psychological distress following displacement (Marcus, 2013; Reichert & Tauchmann, 2017; Schiele & Schmitz, 2016). Several explanations may account for this divergence. First, the extensive health and wellbeing support provided to workers in the lead-up to the closure may have mitigated adverse mental health effects. These services, including on-site counselling, resilience training, and post-closure outreach centres, are not recorded in our data but may have substituted for public system use or improved mental health outcomes directly.

²⁰ Appendix Figure D1 shows disaggregated estimates by age and skill group contrasting older lower-skilled workers with younger higher-skilled workers. The results are qualitatively consistent with those in Figure 5, showing no evidence of changes in mental health service use across most subgroups, except for decreased medication use among older lower-skilled workers. Full regression estimates across all subgroups are reported in Appendix Table D4.

Second, reduced financial access to services may have contributed to the flat therapy usage patterns. Psychological services often involve substantial out-of-pocket costs, which may discourage uptake in the face of income loss. However, this explanation is unlikely to account for the decline in medication use, as prescription drugs are heavily subsidised and subject to only modest co-payments. Indeed, when conditioning on having at least one general practitioner (GP) visit in a given year, we continue to find a similar decline in medication use for lower-skilled workers (Appendix Figure D2), suggesting that the effect is not driven by reduced healthcare access more broadly.

Third, the nature of the event itself may have played a protective role. The closure of an entire industry is a shared, highly visible event, which may carry less personal stigma than an individual layoff or firm-specific closure. As Green (2011) argues, the psychological burden of unemployment is often lower when job loss is perceived as common or externally imposed, rather than a personal failure. A related possibility is that the closure brought resolution after a prolonged period of uncertainty. For many automotive workers, the prospect of shutdown had loomed for years, amid declining production and public debate over industry support. Theoretical and psychological literature suggests that prolonged anticipation of negative events can be more psychologically taxing than the events themselves, as individuals are unable to prepare or adjust (Krebs, 2007; Greco & Roger, 2003). In this context, the announcement and final closure may have offered a form of closure, allowing workers to access support, plan transitions, and exit a state of chronic insecurity.

A potential econometric concern regarding the mental healthcare results is differential healthcare access across regions or occupations. However, such concerns are unlikely to explain our findings. Both treatment and control regions encompass urban and suburban areas of Victoria and South Australia, characterised by good access to GPs, who are the primary prescribers of mental health medications. Additionally, GP visits in Australia are widely available at relatively low out-of-pocket costs due to significant public subsidies. Furthermore, we find similar patterns of reduced medication use when we explicitly condition our analysis on individuals who accessed GP services. These considerations suggest that differences in healthcare access across regions or occupations are unlikely to account for our observed results.

7. Conclusion

Large-scale, industry-wide closures pose distinct challenges for affected workers. Unlike firm-level shutdowns, the collapse of an entire industry removes similar employment opportunities, often within a short window, and can overwhelm local labour markets. Understanding how workers respond and how policy can support them is increasingly important as governments prepare for major industrial transitions in response to climate, technological, and global trade pressures.

This study uses linked administrative data and a DiD event study design to examine the economic and mental health consequences of the complete shutdown of Australia's automotive manufacturing industry. We find that displaced workers experienced substantial and persistent declines in employment and earnings, with many exiting the labour force or transitioning into lower-paid jobs. A small share shifted into self-employment, though not at levels that offset broader labour market losses. These effects were concentrated among lower-skilled and older workers, who faced greater barriers to re-employment and occupational mobility. In contrast, younger higher-skilled workers experienced only modest disruptions and quickly returned to their prior earnings trajectories. The magnitude and persistence of these effects are broadly consistent with international evidence on mass layoffs and long-term scarring (e.g., Jacobson et al., 1993; Eliason & Storrie, 2006; Couch & Placzek, 2010; Farber, 2017; Raposo et al., 2021; Braxton & Taska, 2023), though the earnings losses in our setting are smaller than those observed following the UK coal industry collapse (Colmer et al., 2024).

These findings have important implications for policy design in the face of future industrial transitions. If not carefully managed, such transitions risk entrenching long-term disadvantage among vulnerable worker groups. In our case, the long lead time between closure announcements and final shutdowns enabled multiple support programs to be implemented in advance, including career planning assistance, retraining subsidies, and job search services. However, our results suggest that these interventions were insufficient to support older lower-skilled workers. International evidence suggests that few active labour market programs have consistently strong effects for older workers. Card et al.'s (2018) review of over 200 studies show that while some programs yield positive impacts, the average effects are significantly weaker for this group. This highlights an important gap in policy knowledge: we lack a clear understanding of how to effectively help older, lower-skilled workers. There is a need for new

program models and rigorous trials aimed specifically at improving economic outcomes for this population.

In contrast to the economic findings, we observe no increases in mental healthcare use. In fact, among lower skilled workers we find consistent negative effects on the use of mental health-related prescription medication. While we cannot rule out milder psychological distress, our results suggest that the closure did not result in more clinically significant mental health conditions. This contrasts with previous studies documenting adverse mental health consequences of job displacement (Kuhn et al., 2009; Marcus, 2013; Eisen et al., 2020).

The mental healthcare results highlight a promising aspect of the policy response to the shutdown. The implementation of comprehensive health and wellbeing support, including onsite counselling, resilience training, and peer-based outreach services, appears to have helped prevent deterioration in mental health outcomes despite large economic losses. Importantly, these services were delivered within the workplace or through community-based channels, reducing reliance on traditional clinical pathways. This may be especially important for older, lower-educated men, who are generally less likely to seek out mental health support through conventional channels due to stigma, low mental health literacy, and prevailing masculine norms around self-reliance (O'Brien et al., 2005; Seidler et al., 2016). Delivering support in familiar settings and alongside peers facing similar disruptions may reduce these barriers and foster greater engagement. While such programs are rare in most industrial transitions and are often treated as peripheral to employment services, our findings suggest they warrant greater prominence in future policy designs.

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Table 1: Sample means in 2010/11

	Lower skilled workers		Higher skilled workers	
	Treatment	Control	Treatment	Control
Age	41.50	42.06	40.50	39.03
Married	0.739	0.708	0.748	0.752
Male	0.845	0.733	0.976	0.982
Years of education	11.30	11.31	13.08	13.45
Log salary income	10.96	10.82	11.12	11.11
Positive business income	0.026	0.022	0.034	0.033
Psychological therapy service use	0.017	0.026	0.017	0.020
Mental health medication use	0.042	0.067	0.039	0.034
Individuals	2,684	2,684	1,431	1,431

Notes: Lower-skilled refers to machinery operators, drivers, and labourers and higher-skilled refers to technicians and trade workers. The treatment group comprises automotive manufacturing workers in regions affected by plant closures, while the control group includes similar workers in non-automotive manufacturing and construction, residing in comparable areas of the same two states, excluding the plant regions and their immediate surrounds.

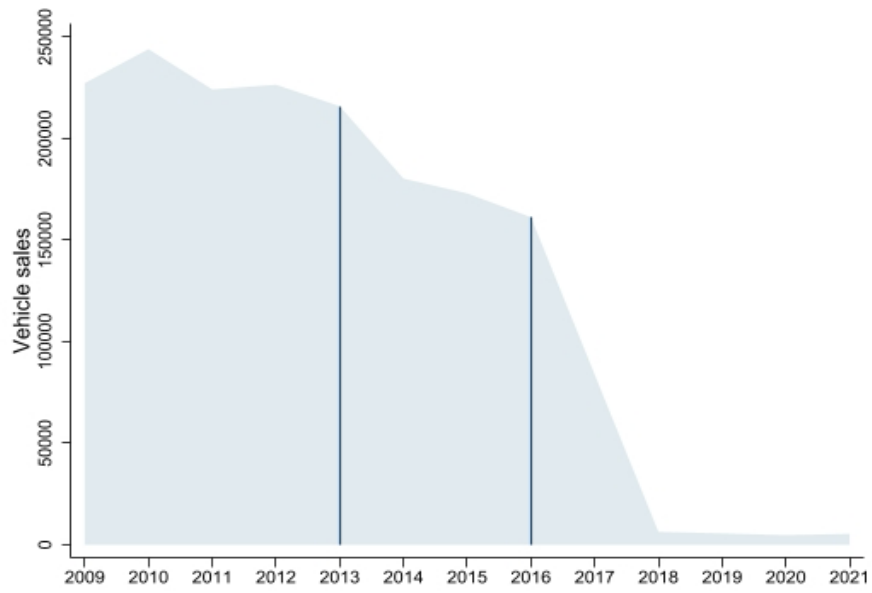
Table 2: Estimated effects on economic outcomes of lower skilled workers across different samples

	Main sample	Controls from New South Wales	Propensity score matching
	(1)	(2)	(3)
Employment			
Post-announcement	-0.013* (0.007)	-0.006 (0.006)	-0.004 (0.007)
Post-closure	-0.049*** (0.009)	-0.048*** (0.007)	-0.054*** (0.009)
Log salary income			
Post-announcement	-0.060*** (0.014)	-0.059*** (0.011)	-0.051*** (0.014)
Post-closure	-0.262*** (0.017)	-0.254*** (0.012)	-0.274*** (0.015)
Welfare use			
Post-announcement	0.002 (0.004)	0.006* (0.003)	0.001 (0.005)
Post-closure	0.026*** (0.006)	0.035*** (0.004)	0.027*** (0.005)
Occupation change			
Post-announcement	0.043*** (0.013)	0.007 (0.010)	0.019 (0.012)
Post-closure	0.160*** (0.014)	0.130*** (0.011)	0.164*** (0.014)
Business income			
Post-announcement	-0.005 (0.005)	-0.001 (0.004)	-0.003 (0.005)
Post-closure	0.013** (0.006)	0.009* (0.005)	0.016*** (0.006)

Notes: The yearly effects are aggregated into the post-announcement (2014/15–2016/17) and post-closure (2017/18–2021/2022) periods. The main sample in column 1 refers to that used in the main analysis, including treatment workers residing in car plant regions and control workers from the same states in car plant non-adjointing regions, exact matched based on occupation. The sample in column 2 includes control workers from New South Wales and treatment workers from car plant and adjoining regions, exact matched based on occupation. The sample in column 3 is based on the main sample, but treatment and control workers are matched on propensity scores based on occupation and age, as opposed to exact matching on occupation. Workers in all samples are machinery operators, drivers, and labourers. Robust standard errors, clustered at the individual level are in parentheses. Controls include individual fixed effects and year fixed effects. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

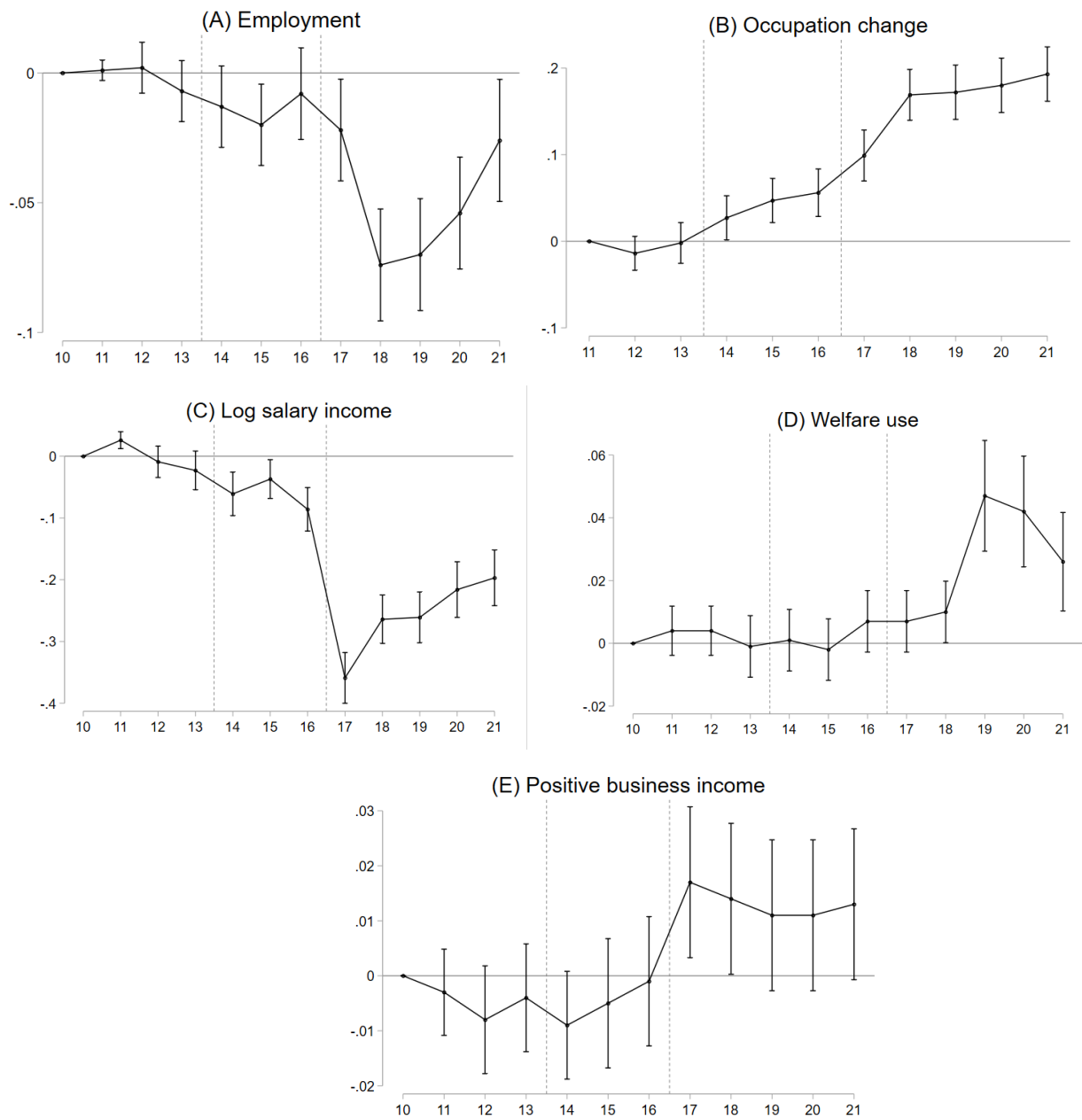
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Figure 1: Motor vehicle production in Australia, 2009–2021



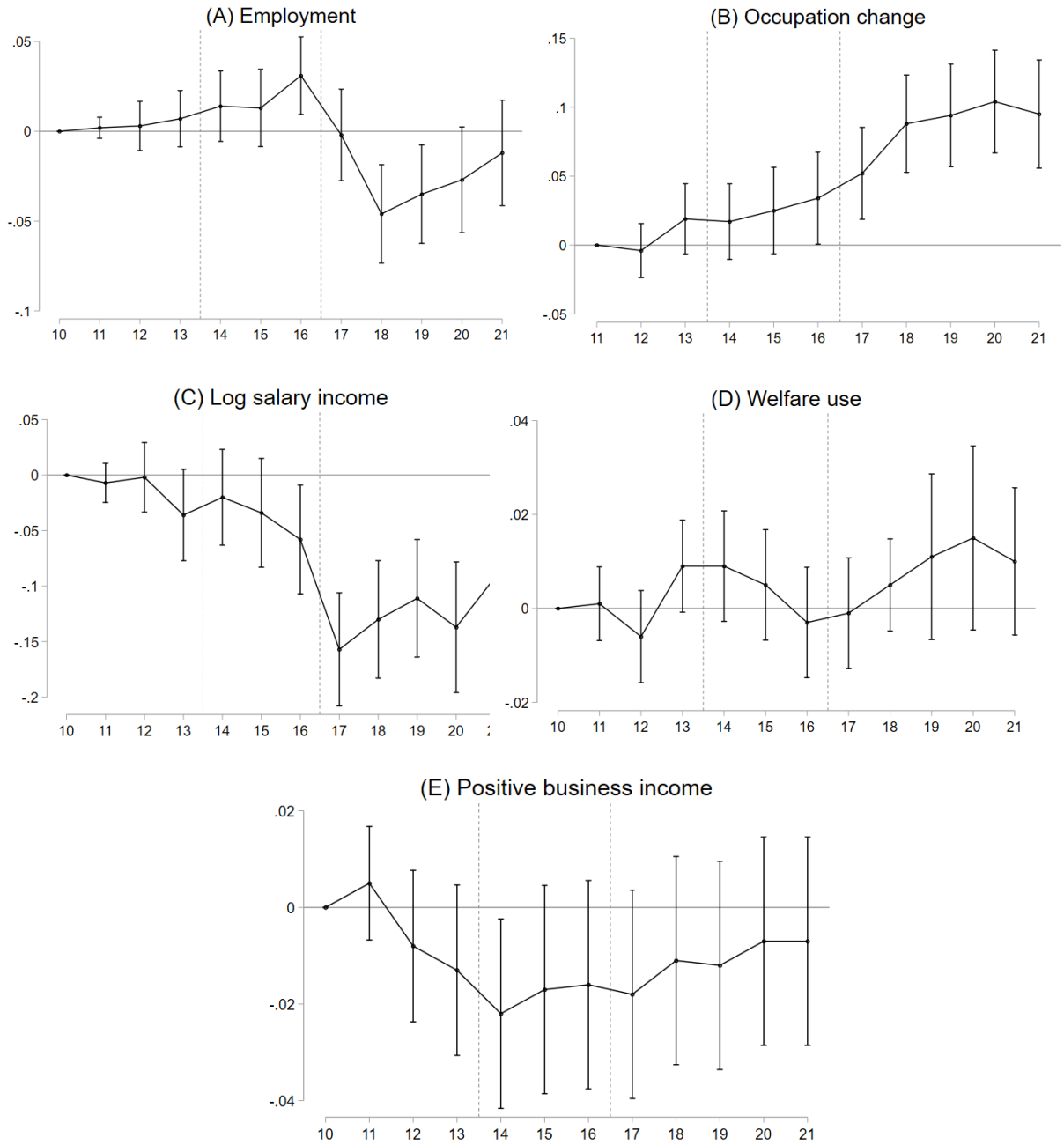
Source: International Organisation of Motor Vehicle Manufacturers, sourced from CEIC, 2021, [<https://insights.ceicdata.com/Untitled-insight/views>].

Figure 2: Estimated effects on economic outcomes for lower skilled workers



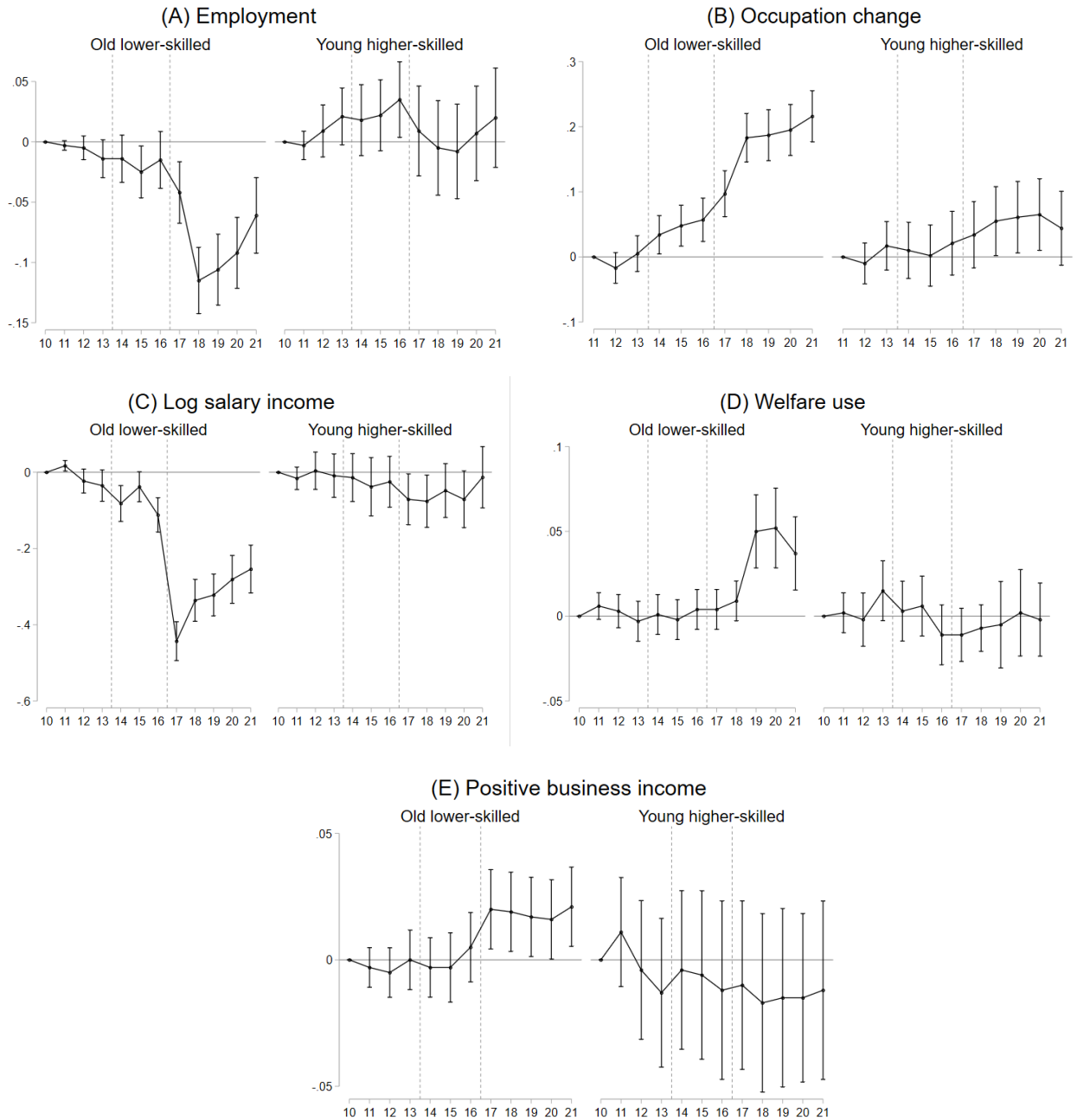
Notes: These graphs are coefficient plots of the DiD event study estimates of the economic outcomes for machinery operators, drivers, and labourers. The x-axes indicate financial years (e.g., 10 is the financial year 10/11 which runs from 1 July 2010 to 30 June 2011). The vertical dashed lines indicate the first full years of treatment for workers in all plants following the plant closure announcements (financial year 14/15) and plant closures (financial year 17/18). The vertical spikes represent the 95% confidence interval for each coefficient.

Figure 3: Estimated effects on economic outcomes for higher skilled workers



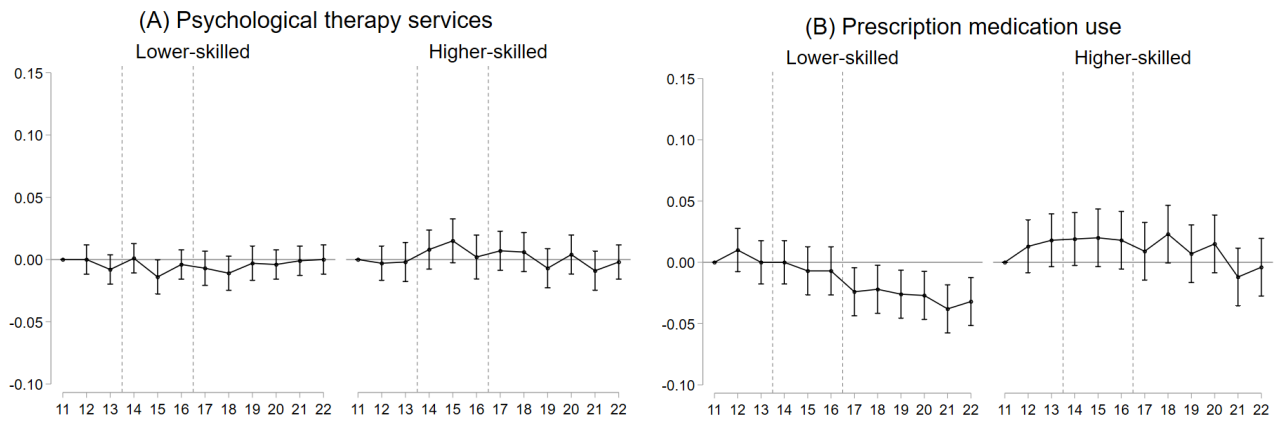
Notes: These graphs are coefficient plots of the DiD event study estimates of the economic outcomes for technicians and trade workers. The x-axes indicate financial years (e.g., 10 is the financial year 10/11 which runs from 1 July 2010 to 30 June 2011). The vertical dashed lines indicate the first full years of treatment for workers in all plants following the plant closure announcements (financial year 14/15) and plant closures (financial year 17/18). The vertical spikes represent the 95% confidence interval for each coefficient.

Figure 4: Estimated effects on economic outcomes by skill and age



Notes: These graphs are coefficient plots of the DiD event study estimates of the economic outcomes, differentiated by skill level and age. Old lower skilled refers to machinery operators, drivers and labourers aged between 40–55 in 2011. Young higher skilled refers to technicians and trade workers aged between 24–39 in 2011. The vertical spikes represent the 95% confidence interval for each coefficient. The x-axes indicate financial years. The vertical dashed lines indicate the first full years of treatment for workers in all plants following the plant closure announcements (financial year 14/15) and plant closures (financial year 17/18).

Figure 5: Estimated effects on health outcomes by skill level



Notes: These graphs are coefficient plots of the DiD event study estimates for the health outcomes, differentiated by skill level. Higher skilled refers to technicians and trade workers and lower skilled refers to machinery operators, drivers and labourers. The vertical spikes represent the 95% confidence interval for each coefficient. The x-axes indicate financial years. The vertical dashed lines indicate the first full years of treatment for workers in all plants following the plant closure announcements (financial year 14/15) and plant closures (financial year 17/18).

Appendices

Appendix A: Car plant closures

Table A1: Car plant closure announcement and closure dates

Company	Automotive facility	Announcement of closure	Date of closure
Ford	Production Plant, Broadmeadows	23 May 2013	7 October 2016
	Broadmeadows and Geelong Plants	May 2013	July 2017
GM Holden	Cruze Production, Elizabeth	December 2013	7 October 2016
	Engine Plant, Port Melbourne		29 November 2016
	Production Plant, Elizabeth	20 October 2017	
Toyota	Production Plant, Altona	February 2014	3 October 2017

Source: Information provided by Ford Australia, GM Holden, and Toyota Motor Corporation Australia, as cited in Wallis & ACIL Allen (2020).

Appendix B: Industries and occupations in the treatment and control groups

Table B1: Industry codes and descriptions

Industry code	Description
<i>A: Automotive manufacturing</i>	
23	Motor Vehicle and Motor Vehicle Part Manufacturing
	2311 Motor Vehicle Manufacturing
	2312 Motor Vehicle Body and Trailer Manufacturing
	2313 Automotive Electrical Component Manufacturing
	2319 Other Motor Vehicle Parts Manufacturing
<i>B: Other manufacturing and construction</i>	
<i>B.1: Other manufacturing</i>	
11	Food Product Manufacturing
12	Beverage and Tobacco Product Manufacturing
13	Textile, Leather, Clothing and Footwear Manufacturing
14	Wood Product Manufacturing
15	Pulp, Paper and Converted Paper Product Manufacturing
16	Printing (including the Reproduction of Recorded Media)
17	Petroleum and Coal Product Manufacturing
18	Basic Chemical and Chemical Product Manufacturing
24	Machinery and Equipment Manufacturing
25	Furniture and Other Manufacturing
<i>B.2: Construction</i>	
30	Building Construction
31	Heavy and Civil Engineering Construction
32	Construction Services
<i>C: Automobile supply chain subindustries excluded</i>	
19	Polymer Product and Rubber Product Manufacturing
20	Non-Metallic Mineral Product Manufacturing
21	Primary Metal and Metal Product Manufacturing
22	Fabricated Metal Product Manufacturing

Notes: Industry groups are based on information provided in the 2011 Census. Panel A (B) lists the industry groups of workers in the treatment (control) group. Other manufacturing refers to all manufacturing sectors except automotive manufacturing and auto supply chain subindustries. Workers in industry groups in panel C are not included in the sample.

Table B2: Frequent four-digit occupations among the treatment sample and corresponding numbers of controls

Occupation code	Description	Unmatched sample		Matched sample
		Treatment	Control	
(1)	(2)	(3)	(4)	(5)
8322	Product Assemblers	2118	810	810
7411	Storepersons	366	1147	366
3232	Metal Fitters and Machinists	334	1443	334
8393	Product Quality Controllers	316	316	316
7213	Forklift Drivers	284	1069	284
3223	Structural Steel and Welding Trades Workers	263	939	263
3212	Motor Mechanics	234	157	157
3411	Electricians	188	2798	188
3242	Vehicle Body Builders and Trimmers	158	31	31
7123	Engineering Production Workers	155	378	155
3234	Toolmakers and Engineering Patternmakers	135	195	135
8391	Metal Engineering Process Workers	110	111	110
7100	Machine and Stationary Plant Operators	93	350	93
7116	Sewing Machinists	78	302	78
8300	Factory Process Workers	74	437	74
7110	Clay, Concrete, Glass and Stone Processing Machine Operators	64	508	64
7112	Industrial Spraypainters	56	115	56
3243	Vehicle Painters	53	30	30
3941	Cabinetmakers	37	874	37
3125	Mechanical Engineering Draftspersons and Technicians	35	80	35

Notes: Occupation codes are those reported in the 2010/11 financial year. Treatment and control workers in the unmatched sample (columns 3 and 4) refer to those in the original sample before matching on occupation. The matched sample in column 5 refers to the number of workers in each group used in the analysis. This sample is derived by matching treatment and control observations with the same 4-digit occupation code, which produces a matching result that has the same number of treated and control workers in each matched occupation group by randomly dropping observations.

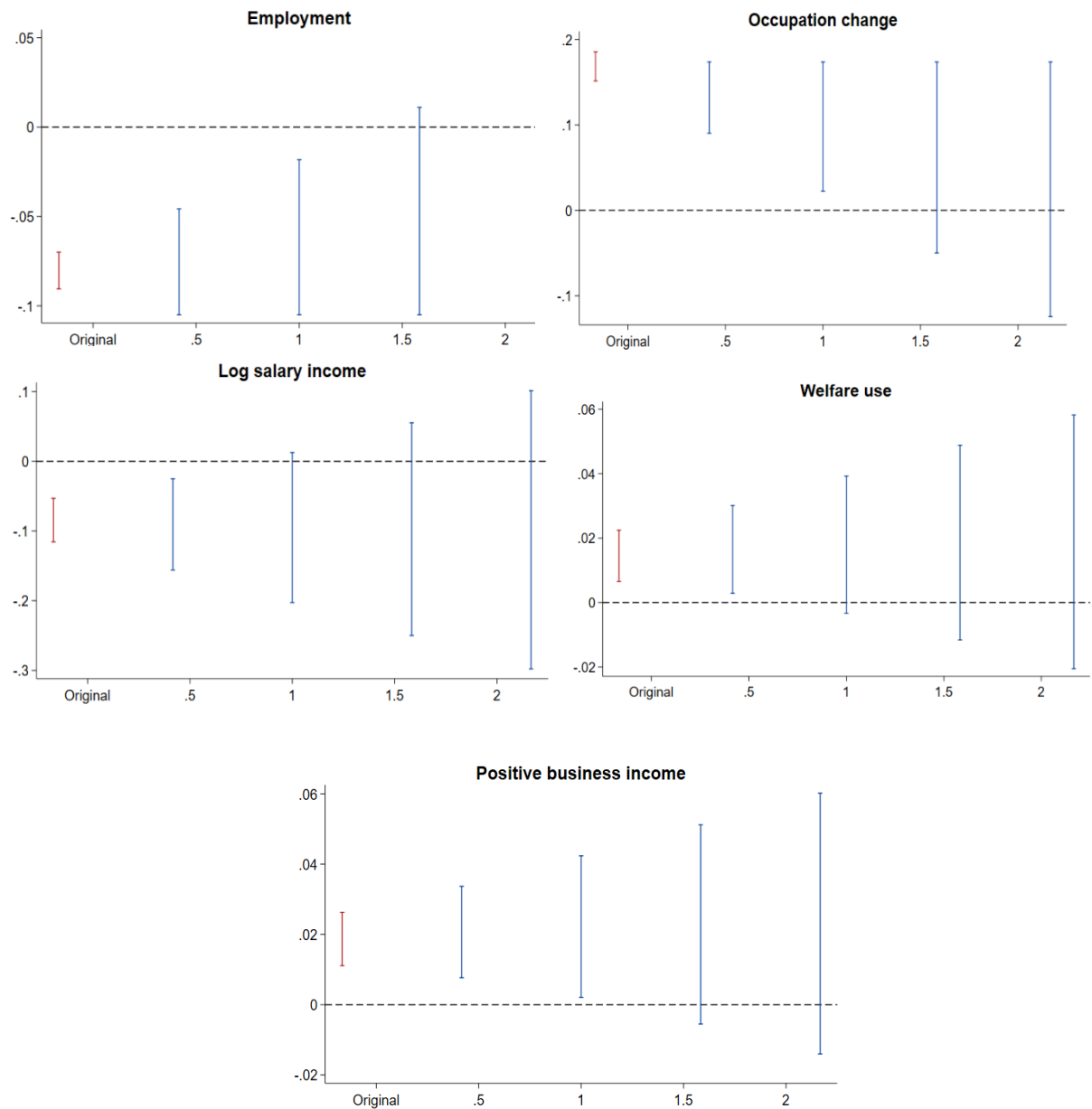
Appendix C: Testing parallel trends assumptions

Table C1: *F*-tests of joint significance of pre-trend coefficients

	Higher skilled	Lower skilled
<i>A: Economic outcomes</i>		
Employment	F(3, 2861) = 0.30 Prob > F = 0.8258	F(3, 5367) = 0.97 Prob > F = 0.4055
Welfare use	F(3, 2861) = 4.02 Prob > F = 0.0073	F(3, 5367) = 0.78 Prob > F = 0.5026
Occupation change	F(2, 2724) = 2.83 Prob > F = 0.0592	F(2, 5090) = 1.72 Prob > F = 0.1787
Salary income	F(3, 2861) = 1.33 Prob > F = 0.2621	F(3, 5367) = 7.72 Prob > F = 0.0000
Business income	F(3, 2861) = 1.62 Prob > F = 0.1816	F(3, 5367) = 1.09 Prob > F = 0.3521
<i>B: Health outcomes</i>		
Psychological service use	F(2, 10498) = 0.30 Prob > F = 0.7408	F(2, 13296) = 2.16 Prob > F = 0.1152
Mental health medication use	F(2, 12716) = 1.44 Prob > F = 0.2367	F(2, 15124) = 0.78 Prob > F = 0.4578

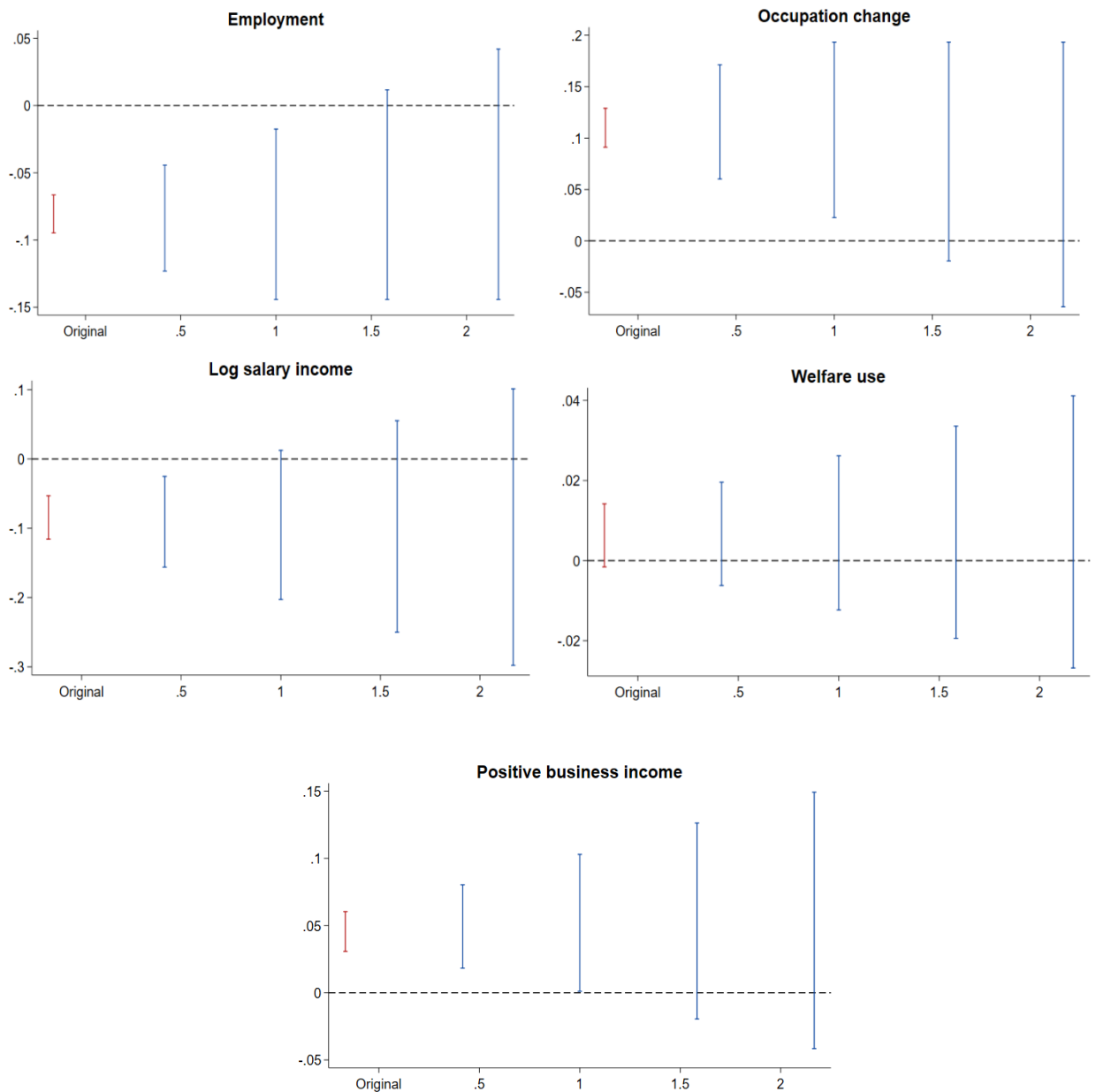
Notes: This table shows *F*-statistics and corresponding *p*-values for *F*-tests of joint significance of pre-trend coefficients, across the 2011/12–2013/14 period, for the economic and health outcomes. Higher skilled refers to technicians and trade workers and lower skilled refers to machinery operators and drivers and labourers. For most outcomes there is no significant pre-trend at the 5% level of significance, where the null hypothesis of a zero effect cannot be rejected. The exceptions are welfare use for the higher skilled and salary income for the lower skilled.

Figure C1: Robust confidence intervals for post-treatment economic effects of lower-skilled workers



Notes: These figures show 95% robust confidence intervals for the treatment effect across the post-treatment (post-announcement and post-closure) period (2014/15–2021/2022) for $\Delta^{RM}(\bar{M})$ using different values of \bar{M} . The first figure on employment implies that if we impose $\bar{M} = 1$, meaning that we restrict the post-treatment violations of parallel trends to be equal to the maximal pre-treatment violation of parallel trends, then we obtain a robust confidence interval of $[-0.105, -0.018]$ for the causal effect on the probability of lower-skilled workers being employed in the post-treatment period. Looking further to the right, we see that the “breakdown value” for a null effect is around $\bar{M} = 1.5$. Thus, our conclusion of a significant effect on employment depends on whether we are willing to restrict that the post-treatment violations of parallel trends can be no more than one and a half times the maximal pre-treatment violation. The same conclusion applies to occupation change and positive business income of both higher and lower skilled workers. For welfare use (among the lower skilled) and log salary income, the breakdown of a null effect is around $\bar{M} = 1$, meaning that a significant post-treatment effect depends on whether we are willing to restrict that the post-treatment violations of parallel trends can be no larger than the maximal pre-treatment violation.

Figure C2: Robust confidence intervals for post-treatment economic effects of higher-skilled workers



Notes: These figures show 95% robust confidence intervals for the treatment effect for technicians and trade workers across the post-treatment post-announcement and post-closure) period (2014/15–2021/2022) for $\Delta^{RM}(\bar{M})$ using different values of \bar{M} . The first figure on employment implies that if we impose $\bar{M} = 1$, meaning that we restrict the post-treatment violations of parallel trends to be equal to the maximal pretreatment violation of parallel trends, then we obtain a robust confidence interval of $[-0.144, -0.017]$ for the causal effect on the probability of higher-skilled workers being employed in the post-treatment period. Looking further to the right, we see that the ‘breakdown value’ for a null effect is around $\bar{M} = 1.5$. Thus, our conclusion of a significant effect on employment depends on whether we are willing to restrict that the post-treatment violation of parallel trends can be no more than 1.5 times the maximal pre-treatment violation. The same conclusion applies to occupation change and positive business income. For log salary income (welfare use), the ‘breakdown value’ of a null effect is around $\bar{M} = 1$ ($\bar{M} = 0.5$), meaning that a significant post-treatment effect depends on whether we are willing to restrict that the post-treatment violations of parallel trends can be no larger than (are half the value of) the maximal pre-treatment violation.

Appendix D: Estimated effects on economic and health outcomes

Table D1: Estimated effects on economic outcomes

Year	Full sample	Higher skilled	Lower skilled
	(1)	(2)	(3)
<i>A Employment</i>			
2011/12	0.002 (0.002)	0.002 (0.003)	0.001 (0.002)
2012/13	0.002 (0.004)	0.003 (0.007)	0.002 (0.005)
2013/14	-0.003 (0.005)	0.007 (0.008)	-0.007 (0.006)
2014/15	-0.003 (0.006)	0.014 (0.010)	-0.013 (0.008)
2015/16	-0.008 (0.007)	0.013 (0.011)	-0.020** (0.008)
2016/17	0.006 (0.007)	0.031*** (0.011)	-0.008 (0.009)
2017/18	-0.015* (0.008)	-0.002 (0.013)	-0.022** (0.010)
2018/19	-0.064*** (0.009)	-0.046*** (0.014)	-0.074*** (0.011)
2019/20	-0.058*** (0.009)	-0.035** (0.014)	-0.070*** (0.011)
2020/21	-0.045*** (0.009)	-0.027* (0.015)	-0.054*** (0.011)
2021/22	-0.021** (0.009)	-0.012 (0.015)	-0.026** (0.012)
Observations	98,124	34,125	63,999
<i>B Occupation change</i>			
2012/13	-0.011 (0.007)	-0.004 (0.010)	-0.014 (0.010)
2013/14	0.006 (0.009)	0.019 (0.013)	-0.002 (0.012)
2014/15	0.024** (0.010)	0.017 (0.014)	0.027** (0.013)
2015/16	0.039*** (0.010)	0.025 (0.016)	0.047*** (0.013)
2016/17	0.048*** (0.011)	0.034** (0.017)	0.056*** (0.014)
2017/18	0.082*** (0.011)	0.052*** (0.017)	0.099*** (0.015)
2018/19	0.140*** (0.012)	0.088*** (0.018)	0.169*** (0.015)
2019/20	0.144*** (0.012)	0.094*** (0.019)	0.172*** (0.016)
2020/21	0.152*** (0.012)	0.104*** (0.019)	0.180*** (0.016)
2021/22	0.158*** (0.013)	0.095*** (0.020)	0.193*** (0.016)
Observations	74,477	26,279	48,198
<i>C Salary income</i>			
2011/12	0.015*** (0.005)	-0.007 (0.009)	0.026*** (0.007)
2012/13	-0.006 (0.010)	-0.002 (0.016)	-0.009 (0.013)
2013/14	-0.027** (0.013)	-0.036* (0.021)	-0.023 (0.016)
2014/15	-0.047*** (0.014)	-0.02 (0.022)	-0.061*** (0.018)
2015/16	-0.036*** (0.014)	-0.034 (0.025)	-0.037** (0.016)
2016/17	-0.076*** (0.015)	-0.058** (0.025)	-0.086*** (0.018)
2017/18	-0.288***	-0.157***	-0.359***

	(0.016)	(0.026)	(0.021)
2018/19	-0.216***	-0.130***	-0.264***
	(0.016)	(0.027)	(0.020)
2019/20	-0.208***	-0.111***	-0.261***
	(0.017)	(0.027)	(0.021)
2020/21	-0.187***	-0.137***	-0.216***
	(0.018)	(0.030)	(0.023)
2021/22	-0.160***	-0.092***	-0.197***
	(0.018)	(0.031)	(0.023)
Observations	87,041	30,546	56,495
<i>D Welfare use</i>			
2011/12	0.003	0.001	0.004
	(0.003)	(0.004)	(0.004)
2012/13	0.000	-0.006	0.004
	(0.003)	(0.005)	(0.004)
2013/14	0.003	0.009*	-0.001
	(0.004)	(0.005)	(0.005)
2014/15	0.003	0.009	0.001
	(0.004)	(0.006)	(0.005)
2015/16	0.001	0.005	-0.002
	(0.004)	(0.006)	(0.005)
2016/17	0.003	-0.003	0.007
	(0.004)	(0.006)	(0.005)
2017/18	0.004	-0.001	0.007
	(0.004)	(0.006)	(0.005)
2018/19	0.008**	0.005	0.010*
	(0.004)	(0.005)	(0.005)
2019/20	0.035***	0.011	0.047***
	(0.007)	(0.009)	(0.009)
2020/21	0.032***	0.015	0.042***
	(0.007)	(0.010)	(0.009)
2021/22	0.021***	0.01	0.026***
	(0.006)	(0.008)	(0.008)
Observations	98,124	34,125	63,999
<i>E Positive business income</i>			
2011/12	0.000	0.005	-0.003
	(0.003)	(0.006)	(0.004)
2012/13	-0.008*	-0.008	-0.008*
	(0.004)	(0.008)	(0.005)
2013/14	-0.007	-0.013	-0.004
	(0.004)	(0.009)	(0.005)
2014/15	-0.013***	-0.022**	-0.009*
	(0.005)	(0.010)	(0.005)
2015/16	-0.009*	-0.017*	-0.005
	(0.005)	(0.011)	(0.006)
2016/17	-0.006	-0.016	-0.001
	(0.006)	(0.011)	(0.006)
2017/18	0.005	-0.018	0.017***
	(0.006)	(0.011)	(0.007)
2018/19	0.005	-0.011	0.014**
	(0.006)	(0.011)	(0.007)
2019/20	0.003	-0.012	0.011
	(0.006)	(0.011)	(0.007)
2020/21	0.004	-0.007	0.011
	(0.006)	(0.011)	(0.007)
2021/22	0.006	-0.007	0.013*
	(0.006)	(0.011)	(0.007)
Observations	98,124	34,125	63,999

Notes: The coefficients (time dummies interacted with treatment status) are compared to 2010/11, the base year, for automotive vis-à-vis construction and non-automotive manufacturing industry workers. Higher skilled refers to technicians and trade workers and lower skilled refers to machinery operators and drivers and labourers. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively. Standard errors are in parentheses. Controls include individual and year fixed effects.

Table D2: Estimated effects on economic outcomes by skill level and age

Year	Higher skilled		Lower skilled	
	24–39 years	40–55 years	24–39 years	40–55 years
<i>A Employment</i>				
2011/12	-0.003 (0.006)	0.006 (0.004)	0.010** (0.005)	-0.003 (0.002)
2012/13	0.009 (0.011)	-0.002 (0.009)	0.014 (0.010)	-0.005 (0.005)
2013/14	0.021* (0.012)	-0.008 (0.011)	0.004 (0.011)	-0.014* (0.008)
2014/15	0.018 (0.015)	0.008 (0.013)	-0.01 (0.013)	-0.014 (0.010)
2015/16	0.022 (0.015)	0.004 (0.015)	-0.011 (0.014)	-0.025** (0.011)
2016/17	0.035** (0.016)	0.027* (0.016)	0.003 (0.014)	-0.015 (0.012)
2017/18	0.009 (0.019)	-0.01 (0.019)	0.007 (0.015)	-0.042*** (0.013)
2018/19	-0.005 (0.020)	-0.080*** (0.020)	-0.013 (0.016)	-0.115*** (0.014)
2019/20	-0.008 (0.020)	-0.054** (0.021)	-0.02 (0.016)	-0.106*** (0.015)
2020/21	0.007 (0.020)	-0.051** (0.021)	-0.002 (0.016)	-0.092*** (0.015)
2021/22	0.02 (0.021)	-0.034 (0.023)	0.02 (0.016)	-0.061*** (0.016)
Observations	16,520	17,605	24,660	39,339
<i>B Occupation change</i>				
2012/13	-0.010 (0.016)	0.004 (0.013)	-0.012 (0.018)	-0.017 (0.012)
2013/14	0.017 (0.019)	0.023 (0.017)	-0.017 (0.021)	0.005 (0.014)
2014/15	0.01 (0.022)	0.027 (0.019)	0.012 (0.022)	0.034** (0.015)
2015/16	0.002 (0.024)	0.049** (0.021)	0.039* (0.023)	0.048*** (0.016)
2016/17	0.021 (0.025)	0.051** (0.022)	0.047* (0.024)	0.057*** (0.017)
2017/18	0.034 (0.026)	0.073*** (0.023)	0.092*** (0.025)	0.097*** (0.018)
2018/19	0.055** (0.027)	0.122*** (0.024)	0.139*** (0.025)	0.183*** (0.019)
2019/20	0.061** (0.028)	0.128*** (0.026)	0.142*** (0.026)	0.187*** (0.020)
2020/21	0.065** (0.028)	0.145*** (0.026)	0.150*** (0.026)	0.195*** (0.020)
2021/22	0.044 (0.029)	0.149*** (0.027)	0.154*** (0.026)	0.216*** (0.020)
Observations	12,652	13,627	18,916	29,282
<i>C Salary income</i>				
2011/12	-0.016 (0.015)	0.005 (0.010)	0.041*** (0.013)	0.017** (0.007)
2012/13	0.004 (0.025)	-0.001 (0.019)	0.014 (0.023)	-0.023 (0.016)
2013/14	-0.009 (0.029)	-0.049 (0.030)	-0.006 (0.026)	-0.035* (0.021)
2014/15	-0.014 (0.032)	-0.015 (0.029)	-0.033 (0.028)	-0.082*** (0.024)
2015/16	-0.038 (0.039)	-0.021 (0.031)	-0.04 (0.027)	-0.038* (0.020)
2016/17	-0.025 (0.034)	-0.068* (0.036)	-0.053* (0.028)	-0.112*** (0.023)
2017/18	-0.071** (0.034)	-0.209*** (0.038)	-0.247*** (0.033)	-0.443*** (0.026)
2018/19	-0.076**	-0.157***	-0.180***	-0.336***

	(0.035)	(0.041)	(0.029)	(0.028)
2019/20	-0.048	-0.147***	-0.193***	-0.322***
	(0.036)	(0.039)	(0.033)	(0.028)
2020/21	-0.071*	-0.171***	-0.151***	-0.281***
	(0.038)	(0.044)	(0.034)	(0.032)
2021/22	-0.013	-0.138***	-0.145***	-0.254***
	(0.041)	(0.045)	(0.032)	(0.032)
Observations	14,899	15,647	22,176	34,319
<i>D Welfare use</i>				
2011/12	0.002	0.000	0.001	0.006
	(0.006)	(0.005)	(0.007)	(0.004)
2012/13	-0.002	-0.009	0.004	0.003
	(0.008)	(0.007)	(0.008)	(0.005)
2013/14	0.015	0.006	0.002	-0.003
	(0.009)	(0.006)	(0.009)	(0.006)
2014/15	0.003	0.014*	-0.001	0.001
	(0.009)	(0.008)	(0.009)	(0.006)
2015/16	0.006	0.004	-0.001	-0.002
	(0.009)	(0.009)	(0.009)	(0.006)
2016/17	-0.011	0.004	0.011	0.004
	(0.009)	(0.008)	(0.009)	(0.006)
2017/18	-0.011	0.008	0.013	0.004
	(0.008)	(0.008)	(0.008)	(0.006)
2018/19	-0.007	0.016**	0.012	0.009
	(0.007)	(0.007)	(0.008)	(0.006)
2019/20	-0.005	0.025**	0.043***	0.050***
	(0.013)	(0.013)	(0.014)	(0.011)
2020/21	0.002	0.023*	0.028*	0.052***
	(0.013)	(0.014)	(0.015)	(0.012)
2021/22	-0.002	0.019	0.012	0.037***
	(0.011)	(0.012)	(0.012)	(0.011)
Observations	16520	17605	24660	39339
<i>E Positive business income</i>				
2011/12	0.011	-0.001	-0.002	-0.003
	(0.011)	(0.007)	(0.007)	(0.004)
2012/13	-0.004	-0.009	-0.013	-0.005
	(0.014)	(0.009)	(0.008)	(0.005)
2013/14	-0.013	-0.009	-0.012	0.000
	(0.015)	(0.010)	(0.009)	(0.006)
2014/15	-0.004	-0.034***	-0.019*	-0.003
	(0.016)	(0.012)	(0.010)	(0.006)
2015/16	-0.006	-0.024*	-0.01	-0.003
	(0.017)	(0.013)	(0.011)	(0.007)
2016/17	-0.012	-0.016	-0.011	0.005
	(0.018)	(0.014)	(0.012)	(0.007)
2017/18	-0.010	-0.023*	0.012	0.020**
	(0.017)	(0.014)	(0.012)	(0.008)
2018/19	-0.017	-0.002	0.004	0.019**
	(0.018)	(0.014)	(0.012)	(0.008)
2019/20	-0.015	-0.004	-0.001	0.017**
	(0.018)	(0.013)	(0.012)	(0.008)
2020/21	-0.015	0.004	0.001	0.016*
	(0.017)	(0.013)	(0.012)	(0.008)
2021/22	-0.012	0.002	-0.003	0.021***
	(0.018)	(0.013)	(0.012)	(0.008)
Observations	16,520	17,605	24,660	39,339

Notes: The coefficients (time dummies interacted with treatment status) are compared to 2010/11, the base year, for automotive vis-à-vis construction and non-automotive manufacturing industry workers. Higher skilled refers to technicians and trade workers and lower skilled refers to machinery operators and drivers and labourers. Ages are those in 2011. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively. Standard errors are in parentheses. Controls include individual and year fixed effects.

Table D3: Estimated effects on economic outcomes of higher-skilled workers across different samples

	Main sample	Controls from New South Wales	Propensity score matching
	(1)	(2)	(3)
Employment			
Post-announcement	0.019** (0.009)	0.016** (0.007)	0.014 (0.009)
Post-closure	-0.024** (0.012)	-0.008 (0.009)	-0.018 (0.013)
Log salary income			
Post-announcement	-0.037* (0.020)	0.001 (0.015)	-0.029 (0.020)
Post-closure	-0.126*** (0.023)	-0.084*** (0.017)	-0.104*** (0.025)
Welfare use			
Post-announcement	0.004 (0.005)	-0.001 (0.004)	0.000 (0.006)
Post-closure	0.008 (0.006)	0.005 (0.004)	0.006 (0.007)
Occupation change			
Post-announcement	0.025* (0.015)	0.017 (0.011)	0.038*** (0.014)
Post-closure	0.086*** (0.017)	0.042*** (0.014)	0.099*** (0.017)
Business income			
Post-announcement	-0.018* (0.009)	-0.010 (0.007)	-0.007 (0.008)
Post-closure	-0.011 (0.009)	-0.001 (0.007)	0.001 (0.010)

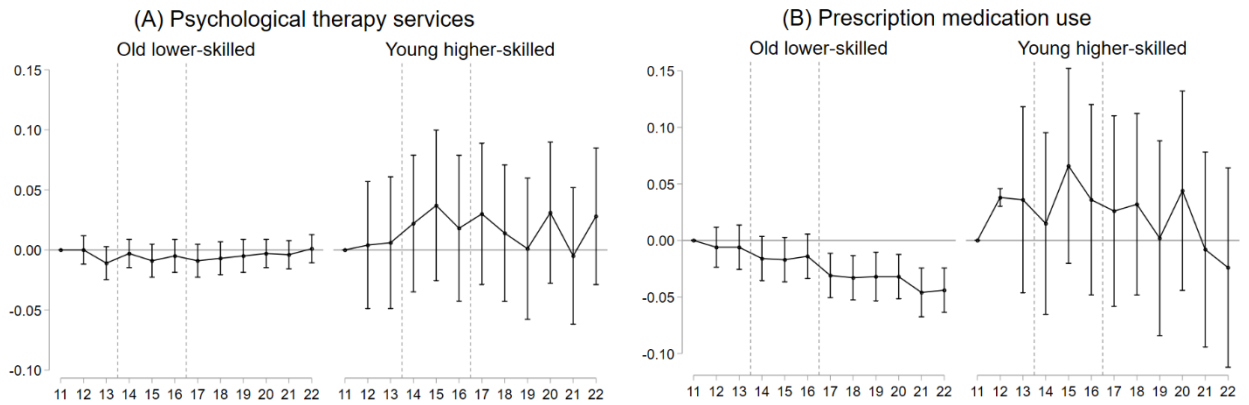
Notes: The yearly effects are aggregated into the post-announcement (2014/15–2016/17) and post-closure (2017/18–2021/2022) periods. The main sample in column 1 refers to that used in the main analysis, including treatment workers residing in car plant regions and control workers from the same states in car plant non- adjoining regions, matched based on occupation. The sample in column 2 includes control workers from New South Wales and treatment workers from car plant and adjoining regions, matched based on occupation. The sample in column 3 is based on the main sample, but treatment and control workers are matched on propensity scores based on occupation and age, as opposed to exact matching on occupation. Workers in all samples are technicians and trade workers. Robust standard errors, clustered at the individual level are in parentheses. Controls include individual fixed effects and year fixed effects. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table D4: Estimated effects on health outcomes

Year	By skill		By skill and age			
	Higher skilled	Lower skilled	Higher skilled		Lower skilled	
	(1)	(2)	24–39 years	40–55 years	24–39 years	40–55 years
<i>A Psychological therapy service use</i>						
2012/13	-0.003 (0.007)	0.000 (0.006)	0.004 (0.027)	0.000 (0.007)	0.005 (0.019)	0.000 (0.006)
2013/14	-0.002 (0.008)	-0.008 (0.006)	0.006 (0.027)	-0.001 (0.008)	-0.006 (0.021)	-0.011* (0.007)
2014/15	0.008 (0.008)	0.001 (0.006)	0.022 (0.029)	0.004 (0.008)	0.021 (0.020)	-0.003 (0.006)
2015/16	0.015* (0.009)	-0.014** (0.007)	0.037 (0.032)	0.012 (0.008)	-0.022 (0.021)	-0.009 (0.007)
2016/17	0.002 (0.009)	-0.004 (0.006)	0.018 (0.031)	0.002 (0.009)	0.003 (0.021)	-0.005 (0.007)
2017/18	0.007 (0.008)	-0.007 (0.007)	0.03 (0.030)	0.002 (0.008)	0.004 (0.022)	-0.009 (0.007)
2018/19	0.006 (0.008)	-0.011 (0.007)	0.014 (0.029)	0.008 (0.008)	-0.014 (0.021)	-0.007 (0.007)
2019/20	-0.007 (0.008)	-0.003 (0.007)	0.001 (0.030)	-0.002 (0.008)	0.015 (0.022)	-0.005 (0.007)
2020/21	0.004 (0.008)	-0.004 (0.006)	0.031 (0.030)	0.000 (0.008)	-0.008 (0.020)	-0.003 (0.006)
2021/22	-0.009 (0.008)	-0.001 (0.006)	-0.005 (0.029)	-0.007 (0.007)	0.019 (0.019)	-0.004 (0.006)
2022/23	-0.002 (0.007)	0.000 (0.006)	0.028 (0.029)	-0.009 (0.007)	0.002 (0.019)	0.001 (0.006)
Observations	40,460	68,590	9,620	30,830	14,990	53,600
<i>B Mental health medication use</i>						
2012/13	0.013 (0.011)	0.010 (0.009)	0.038 (0.040)	0.009 (0.010)	0.059** (0.028)	-0.006 (0.009)
2013/14	0.018 (0.011)	0.000 (0.009)	0.036 (0.042)	0.016 (0.010)	0.011 (0.031)	-0.006 (0.010)
2014/15	0.019* (0.011)	0.000 (0.009)	0.015 (0.041)	0.022** (0.011)	0.052* (0.031)	-0.016* (0.010)
2015/16	0.020* (0.012)	-0.007 (0.010)	0.066 (0.044)	0.013 (0.012)	0.002 (0.030)	-0.017* (0.010)
2016/17	0.018 (0.012)	-0.007 (0.010)	0.036 (0.043)	0.022* (0.012)	0.009 (0.031)	-0.014 (0.010)
2017/18	0.009 (0.012)	-0.024** (0.010)	0.026 (0.043)	0.015 (0.012)	0.003 (0.031)	-0.031*** (0.010)
2018/19	0.023** (0.012)	-0.022** (0.010)	0.032 (0.041)	0.019* (0.011)	0.012 (0.031)	-0.033*** (0.010)
2019/20	0.007 (0.012)	-0.026** (0.010)	0.002 (0.044)	0.015 (0.011)	-0.008 (0.031)	-0.032*** (0.011)
2020/21	0.015 (0.012)	-0.027*** (0.010)	0.044 (0.045)	0.005 (0.012)	-0.006 (0.032)	-0.032*** (0.010)
2021/22	-0.012 (0.012)	-0.038*** (0.010)	-0.008 (0.044)	-0.010 (0.012)	-0.014 (0.031)	-0.046*** (0.011)
2022/23	-0.004 (0.012)	-0.032*** (0.010)	-0.024 (0.045)	0.001 (0.011)	0.034 (0.032)	-0.044*** (0.010)
Observations	40,500	68,640	9,620	30,880	14,980	53,660

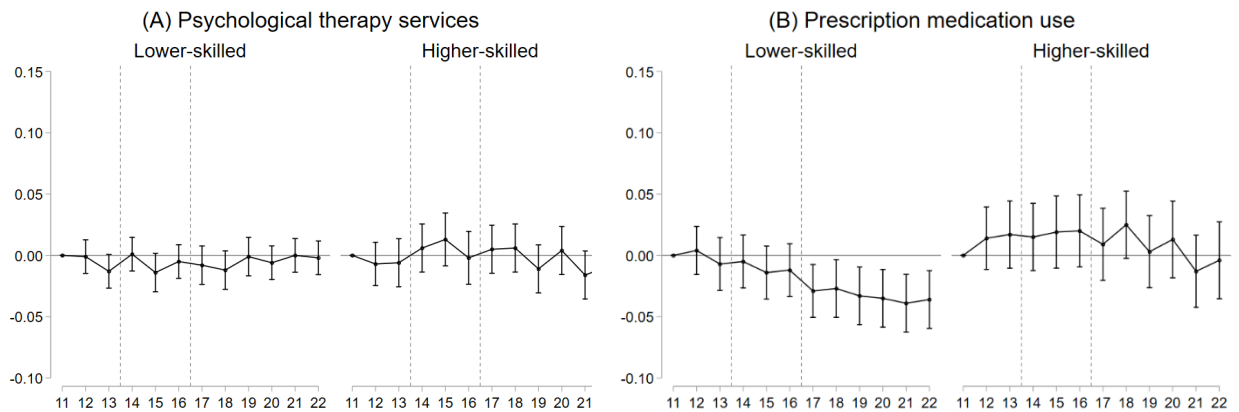
Notes: The coefficients (time dummies interacted with treatment status) are compared to 2011/12, the first year of the sample, for automotive vis-à-vis construction and non-automotive manufacturing industry workers. Columns 1 and 2 present estimates differentiated by skill level: higher-skilled refers to technicians and trade workers and lower-skilled refers to machinery operators, drivers, and labourers. Columns 3–6 present estimates differentiated by skill and age in 2011. Robust standard errors, clustered at the individual level are in parentheses. Controls include individual fixed effects and year fixed effects. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Figure D1: Estimated effects on health outcomes by skill level and age



Notes: These graphs are coefficient plots of the DiD event study estimates for the health outcomes, differentiated by skill and age. Young higher skilled refers to technicians and trade workers aged between 24–39 years in 2011 and old lower skilled refers to machinery operators and drivers and labourers aged between 40–55 years in 2011. The vertical spikes represent the 95% confidence interval for each coefficient. The x-axes indicate years. The vertical dashed lines indicate the first full years of treatment for workers in all plants following the plant closure announcements (financial year 14/15) and plant closures (financial year 17/18).

Figure D2: Estimated effects on health outcomes conditional on positive GP visits



Notes: These graphs are coefficient plots of the DiD event study estimates of the health outcomes. The sample is restricted to individuals who have visited a GP at least once. Higher-skilled refers to technicians and trade workers and lower-skilled refers to machinery operators, drivers, and labourers. The vertical spikes represent the 95% confidence interval for each coefficient. The x-axes indicate years. The vertical dashed lines indicate the first full years of treatment for workers in all plants following the plant closure announcements (financial year 14/15) and plant closures (financial year 17/18).