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Job displacement and local employment density

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Abstract

Past research finds evidence that workers' labour market outcomes are enhanced if they live in areas with greater job opportunities and employment density. Using two alternative measures of the employment density and job opportunities faced by workers in the local labour market in which they were displaced, this paper analyses their effects on the subsequent migration decisions and labour market outcomes of workers who involuntarily lose their jobs as part of a firm closure or mass layoff event. Our analysis finds only limited support for the spatial mismatch hypothesis. The results imply that workers displaced from jobs in areas with greater employment density or job opportunities are more likely to emigrate, are less likely to be re-employed following layoff and have lower subsequent earnings, although earnings are higher conditional on being employed. However, if employed, workers displaced in areas with more opportunities are less likely to have moved area, but more likely to have changed industry, and have a more similar job to that from which they were displaced.

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Displaced workers; unemployment duration; local labour markets

Summary haiku

When a job is lost

do other nearby options

offset the damage?

Table of Contents

Tables and Figures

1 Introduction

A well-functioning economy reallocates inputs from lower to higher productive uses over time. An important part of the reallocation process involves the labour market: workers may lose their jobs and experience substantial income loss while seeking new employment or become discouraged from participating in the labour market. As most individuals' and families' incomes derive from labour market earnings, the resilience of workers' employment and earnings prospects in the face of adverse shocks to labour markets is crucial for the wellbeing of families and the communities they live in. While transfers can help smooth negative income shocks, the risk of persistent negative impacts of job loss (labour scarring) is likely to increase the longer a worker spends in unemployment (due to depreciation of skills while out of work, and a negative signalling effect of ongoing unemployment, etc).

A potentially important driver of labour market resilience is how the structure of the local labour market (LLM) affects the employment and earnings prospects of workers. A large international research literature has analysed the effects of LLM conditions on such labour market outcomes, focused on the spatial mismatch hypothesis that workers with inferior access to jobs tend to have worse labour market outcomes. Although much of this literature has concluded that local opportunities are important for workers' labour market prospects, it is difficult to identify causal effects separately from other endogenous factors (such as correlated unobserved worker and neighbourhood characteristics) associated with workers who are seeking re-employment.

In recent US research, Andersson et al. (2018) deal with the endogeneity issue by analysing workers who unexpectedly lose their jobs as part of a mass layoff. Their results suggest that the duration of joblessness is lower for workers who reside near thicker labour markets, even after accounting for the fact that labour markets with more jobs also have more workers competing for those jobs. These findings seem particularly pertinent to the New Zealand labour market where a significant proportion of workers change jobs in any given year, and where the relatively small economy creates thin labour markets, particularly outside of major urban areas.

In this paper we adopt the Andersson et al. (2018) approach and analyse how the LLM structure affects the re-employment and subsequent earnings prospects of workers who have been involuntarily displaced from their jobs. For this, we focus on alternative measures of labour market characteristics, including employment density as used by Máre and Graham (2013), and local job prospects that depend also on a worker's industry, demographic and human capital characteristics. We use these measures to examine whether workers in more dense labour markets (or those with better job prospects) face better employment and earnings opportunities: that is, whether such workers when displaced have better subsequent re-employment and earnings outcomes than workers displaced in sparser (less-dense) labour markets or with worse local job prospects.

We first construct a sample of worker layoff events, associated either with a firm-closure (in which all workers leave the firm) or a mass layoff (in which a substantial majority of workers leave the firm) over the period from January 2005 to June 2020. We follow the literature in viewing such closure and mass layoff events as being arguably exogenous to the workers involved.[1] We follow this sample of worker layoffs, from 24 months prior to layoff to at least 36 months following layoff.

We begin by analysing whether the local labour market conditions at the time a worker is laid-off affect whether they remain in New Zealand or migrate overseas. For workers who do not migrate, we then analyse whether the LLMs affect their domestic migration decision, and their post-layoff labour market outcomes. As our primary interest is on the effects of local labour market conditions on subsequent outcomes, rather than on estimating the effect of displacement on subsequent outcomes *per se*, our focus is on estimating how the outcomes of displaced workers vary with the LLM conditions they face.[2]

The main labour market outcomes we analyse are the probability that workers are re-employed, as measured by whether they receive any PAYE-withheld earnings in a month; the 'quality' of their re-employment, as measured by their monthly earnings if employed; and their unconditional monthly earnings (i.e. including zero earnings for non-employed workers) which combines the previous two extensive (employment) and intensive (earnings) margin measures. In addition, as summary measures of workers' subsequent labour market outcomes, we also consider the duration of their first spell of unemployment before being re-employed and their subsequent first re-employment spell duration. Finally, conditional on being employed, we analyse whether the LLM conditions affect whether workers are employed in a different location or different industry, and how similar or different their subsequent jobs are compared to jobs they were laid-off from.

[1] See Andersson et al. (2018). Such layoffs are a subset of involuntary displacements, and arguably more exogenous (to the workers) than other displacement events. For example, individually displaced workers may have been more selectively chosen on the basis of their characteristics for redundancy by a firm than workers made redundant as part of a mass layoff.

[2] Estimating the impact of displacement requires more careful consideration of what the counterfactual outcomes would be if a worker was not laid-off. This requires non-trivial assumptions to identify appropriate counterfactual outcomes. For example, a common matching-on-observables approach selects workers who were not laid-off with similar observed sociodemographic characteristics and labour market outcomes to workers who were laid-off, and then compares the actual and matched-comparison outcomes: the validity of this approach requires there are no unobserved factors related to the layoff decision.

Our analysis finds only limited support for the spatial mismatch hypothesis. The results imply that workers displaced from jobs in areas with greater employment density or job opportunities are more likely to emigrate, are less likely to be re-employed following layoff and have lower subsequent earnings, although earnings are higher conditional on being employed. If employed, workers displaced in areas with more opportunities are less likely to have moved area, more likely to have changed industry, and have a more similar job to that from which they were displaced, relative to those in areas with fewer opportunities.

The remainder of the paper proceeds as follows. In the next section we review the literature on the local labour market and job displacement effects on labour market outcomes. In section 3 we discuss the estimation framework. Section 4 describes the data to be used in the analysis. The analytical results are presented in section 5 and the paper concludes with a discussion of the implications in section 6.

2 Review of relevant studies

In this section, we review the relevant international and New Zealand literatures on the effects of local labour market density on employment and earnings prospects of workers, paying particular attention to outcomes for workers who are involuntarily displaced. The focus of our analysis is on the subset of workers who are 'permanently' displaced as a result of a firm closure or mass-layoff event. Throughout the discussion in the paper, we use the terms "displacement", "redundancy", and "layoff" interchangeably.

There is an extensive economics literature that examines the effect of local employment density and access to local jobs on worker outcomes. Two of the main streams relevant to our research relate to spatial mismatch, and spatial job search and matching. Kain (1964, 1968) identified the lack of nearby jobs as a key contributor to high unemployment for black workers in US central cities. Kain argued that inner city black workers were disadvantaged by the suburbanisation of jobs. He claimed that a mismatch arose because discrimination in housing and mortgage markets reduces their ability to move closer to suburban jobs, and employment discrimination in suburban areas, together with weak information and transport network connections with the suburbs, limit their employment options. This argument was subsequently referred to as the 'spatial mismatch hypothesis'.

In the six decades since Kain's initial study, there has been ongoing research and debate about the nature and importance of spatial mismatch as a source of spatial and racial/ socio-economic inequality. Review articles by Kain (1992) and Ihlanfeldt & Sjoquist (1998) conclude that there is, overall, moderate support for the spatial mismatch hypothesis.[3] The reviews do, however, highlight the variation across studies in methodological approaches and measurement choices, which makes an overall evaluation challenging. Some more recent studies have aimed to distinguish the various theoretical mechanisms that could contribute to apparent spatial mismatch (Gobillon et al., 2007) or delineate the contributions of differing research approaches (Houston, 2005). In doing so, spatial mismatch is framed within a broader framework of spatial inequality (Galster & Sharkey, 2017) and spatial job search, and less explicitly linked to the specific issue of poor employment outcomes for African American workers, or to constraints related to residential location (housing market discrimination, access to vehicles and public transport, etc) that were at the heart of Kain's spatial mismatch hypothesis.

Gobillon et al (2007) identify seven mechanisms as having the potential to explain how distance to job opportunities could be harmful.[4] Of these, five are not directly linked to ethnicity. Instead, they capture features of spatial job search. Workers in less dense labour markets face higher costs of job search, and lower benefits of job search, either of which may reduce their likelihood of securing a job. Job search costs are higher because information about distant jobs is more costly to acquire, and there may be higher travel costs associated with job search. The benefits of job search are lower in less dense labour markets because commuting is costly and the proportion of potential jobs openings that offer sufficiently high wages to compensate for this is small.

Models of spatial job search consider a wider range of options and choices than are captured by aspatial job search. In standard aspatial job search models, workers choose a reservation wage level (Lippman & McCall, 1976), and possibly an intensity of search, and accept wage offers that exceed their reservation wage. For spatial job search, each job offer has an associated commuting cost, and possibly a migration cost if a distant wage offer is sufficiently high. Job seekers will search more intensively for nearby jobs because of the higher costs and lower expected benefits of distant job search. When deciding whether to apply for or accept a job offer, they will take account of not only the wage but also the costs of moving or commuting (Herzog et al., 1993; Manning & Petrongolo, 2017; Rouwendal, 2021).

The links between local job density and worker outcomes have been analysed extensively in the context of spatial job search and matching models. Within the urban economics literature,

[3] This is in contrast with the earlier conclusion of Jencks & Mayer (1990, p219) that support is "so mixed no prudent policy analyst should rely on it", or of Ellwood (1986) that "race, not space, remains the key explanatory variable [for Black teenage unemployment]". See also the survey by Holzer (1991).

[4] These mechanisms are: 1) workers may refuse jobs with long commutes; 2) search efficiency may decrease with distance; 3) search effort may decrease with distance; 4) search costs limit search areas; 5) employers may (statistically) discriminate against workers from stigmatised areas; 6) employers may prefer nearby workers because commuting reduces productivity; and 7) white customers may discriminate against minority workers (paraphrase of Gobillon et al., 2007, s3).

Marshall (1920) provided an early account of the links between employment density and the ease with which unemployed workers find employment. He notes that "men seeking employment naturally go to places where there are many employers who need such skill as theirs and where therefore it is likely to find a good market." Duranton & Puga (2004) formalise this idea when discussing the 'matching' advantages of thick urban labour markets. They outline models of how dense urban labour markets can increase not only the probability that workers find a job, but also the quality of the job match that they make.[5]

Whereas Kain's initial spatial mismatch studies focused primarily on the relationship between local employment density and non-employment, the breadth of subsequent studies reflects the range of inter-related outcomes of spatial job search and matching. The local density and accessibility of jobs has been found to affect not only employment but also earnings (Card et al., 2023), location change (Herzog et al., 1993), commuting (Rouwendal, 1999), and the duration of unemployment (Andersson et al., 2018; Rogers, 1997). High local job accessibility is associated with better labour market outcomes – higher earnings, lower unemployment durations, lower commutes, and less residential re-location. It is also found to improve the quality of job matches. The outcomes of job search in dense labour markets include not only higher earnings, but also a higher likelihood of finding a job in an occupation that better suits workers' qualifications (Abel & Deitz, 2015), and enables workers to avoid changing occupation or industry (Neffke et al., 2018).

The effects of local job density on job search and labour market outcomes are not uniform for all workers. The spatial mismatch literature highlights ethnic differences in the link between job density and employment rates, with low job accessibility affecting minority groups most acutely. Job accessibility is also a more significant factor in labour market outcomes for workers with limited access to cars (Ihlanfeldt, 1993). For example, younger workers have lower costs of residential mobility, so their search horizons are larger and their outcomes are less dependent on local labour market conditions. Gathmann et al (2020) find that young workers are sufficiently mobile to enable them to avoid the adverse effects of a local employment decline. Similarly, Ihlanfeldt (1992) finds that the employment chances of teen workers do not depend on local job accessibility. Effects may also differ by skill, with more specialised highly educated workers benefiting more from the higher density of specialised jobs.

The strength of employment density effects on labour market outcomes may vary with the level of density. Mills (2000) finds the variation in density is not relevant for labour market

[5] Subsequent literature has examined the implications of spatial search and matching behaviour on urban form and the contribution to agglomeration effects (Berliant et al., 2006; Helsley & Strange, 1990; Zenou, 2009).

outcomes in non-metropolitan areas. Ihlanfeldt (1992, pp78-79) documents that, for teens, the effect of density on employment is significant only in metropolitan areas with populations larger than 800,000. Di Addario (2011) reports diminishing benefits of density for employment probabilities, with positive effects only for local labour markets smaller than 1.9 million residents.

A range of measures have been used in the literature to capture local labour market conditions. A commonly-used measure is the average density of total employment in a local area, possibly inversely weighted by distance from a worker's residence, or by mode-specific travel times, to capture 'job accessibility'. Measures of industrial specialisation or variety have been used to capture the diversity of job types in the local area (Di Addario, 2011; Neffke et al., 2018).

To reflect the fact that not all jobs are relevant for any given worker, the measure of density can be calculated for a subset of jobs, reflecting labour market segmentation by skill, industry, or occupation. Neffke et al (2018) examine whether labour market outcomes of displaced workers are affected by the prevalence of jobs in the same industry as they were displaced from, or of jobs in industries that use related skills. Andersson et al (2018) measure job accessibility for low income workers based on the location of lower-income jobs only, adjusted for commuting time by expected mode of travel.

Studying outcomes for involuntarily displaced workers is a helpful approach for disentangling the impacts of density from the dynamics of voluntary job changes. There is an established literature on the impacts of displacement, both in New Zealand (Dixon & Maré, 2013; Dixon & Stillman, 2009; Hyslop, 2019; Hyslop & Townsend, 2019), and internationally (Addison & Portugal, 1989; Jacobson et al., 1993; Kletzer, 1998; Quintini & Venn, 2013). These consistently show substantial post-displacement declines in wages and in the probability of employment. Only a subset of these studies have examined how outcomes for displaced workers are affected by local labour market conditions (e.g.: Andersson et al., 2018; Neffke et al., 2018), as we do in this study. Grimes & Young (2011) provide a case study of two specific plant closures in New Zealand in the 1980s, which occurred in markedly different labour markets. Their study presents a strong illustrative case for the proximity of alternative jobs being an important determinant of whether workers stay in a region (since migration is an alternative to finding a local job). They examine the (arguably exogenous) closure of two freezing works – one in a relatively isolated town (Patea) and one in a town (Whakatu) close to a larger labour market, where employment outcomes were more positive.

In a recent NZ study examining all job ends (not just displacements) Coleman and Zheng (2020) find that Auckland has a higher proportion of annual job changes within the same industry and region (17% compared with 6%-9% in small cities and minor urban areas); and also that 24% of job changes are within-Auckland changes of industry. Also looking at all job ends, Stroombergen et al. (2021) estimate positive effects of higher job accessibility on employment rates, with stronger effects in Auckland than in Wellington (and in Napier-Hastings). They also find insignificant job accessibility effects on non-employment durations. Because neither of these studies focuses on displacement events, it is hard to disentangle the effects of local labour market conditions from voluntary search behaviour.

Studies vary not only by the measures used to capture local labour market conditions, but also by the range of worker outcomes that are examined. As noted above, local labour market density and job accessibility can affect job search, and can therefore potentially affect worker mobility, employment probabilities, unemployment durations, and when employed, wage levels and job quality.

Andersson et al. (2018) focus on the duration of joblessness following displacement, and find that job accessibility significantly reduces joblessness durations, especially for workers with lower pre-displacement earnings, for non-white non-Hispanic workers, for women and for older workers. Neffke et al. (2018) consider a broader range of outcomes, finding that a high prevalence of jobs in the same industry from which workers were displaced not only reduces non-employment durations, but also reduces the likelihood of changing industry or changing region. The prevalence of employment in skill-related industries increases the likelihood of changing industries, and leads to lower post-displacement earnings, though not to lower employment or wage rates.

The effect of local labour market density can also vary by pre-displacement job characteristics. Workers with longer job tenure, with industry-specific skills (especially if in declining industries), or who are older tend to be most adversely affected by displacement. There is less evidence on how local labour market density affects outcomes for different groups of displaced workers. Andersson et al (2018) finds that accessibility reduces the duration of joblessness most strongly for older workers, as well as for Black and Hispanic workers.

3 Estimation Framework

We follow an “Event Study” approach (Freyaldenhoven et al., 2021), in which the month of layoff defines the “event” date, and we analyse outcomes relative to this date. As we do not include observations of non-displaced workers, we are not identifying the impact of displacement.

Instead, we are most interested in the effects of employment density or other measures of job prospects on the outcomes of displaced workers, and thus estimate these effects relative to the mean outcomes of the displaced workers.

We estimate the relative effect of local labour market conditions on an outcome of interest y (such as migrating, being re-employed or monthly earnings), where our analyses involve modelling either worker-level observation data, or worker-month level panel data. We estimate outcomes using data from 2 years prior to layoff (t = –24) to at least 3 years after layoff (t >= 36).[6] Location is the key variable which identifies the local labour market characteristics that are the focus of our study. For worker-level analyses, such as whether the worker emigrated, the duration of their initial (layoff) unemployment spell, or the duration of their first re-employment spell following layoff, the estimating equation takes the form:

$$y_i = llmc_i\delta + X_i\beta + u_i \qquad (1)$$

where y_i is an outcome for displaced worker-*i*, $llmc_i$ is a measure of the worker's local labour market conditions at the time of layoff, and X_i is a vector of observable characteristics that include worker demographics and location, and also job or firm characteristics such as industry or firm quality at the time of layoff. The coefficient δ captures the effect of the local labour market conditions on the outcome, and is our primary focus.

For worker-month panel data analyses, the estimating equation is summarised in equation (2):

$$y_{it} = \tau_t + llmc_i\delta_0 + llmc_i 1(t > 0)\delta_1 + X_i\beta + u_{it} \qquad (2)$$

where y_{it} is the outcome for displaced worker-*i* in month-t, which is measured relative to the worker's month of layoff, $llmc_i$ represents the local labour market conditions at time of layoff, and X_i is a vector of observable characteristics measured at the time of layoff. The first term (τ_t) non-parametrically captures mean outcomes by month: average pre-displacement outcomes are captured by τ_t $(t \leq 0)$, while average post-displacement outcomes are captured by τ_t $(t > 0)$. In the panel data context, we allow the effects of the local labour market conditions to vary before and after layoff, and the coefficient δ_1, capturing the relative post-layoff effect, is the main parameter of interest. For parsimony, in our main analyses, we restrict the estimated local labour market conditions' effects to be constant over the period before layoff (δ_0), and after-layoff (δ_1), rather than allowing unrestricted month-specific effects (δ_t).

[6] That is, we observe at least 3 years post-layoff for all workers, and 4 years for most workers: workers laid-off between July 2018 and June 2019 are observed for less than 4 years post-layoff.

4 Data

The main source of data used for estimation is the Fabling-Maré labour dataset (Fabling & Maré, 2015) available in the Statistics New Zealand Integrated Data Infrastructure (IDI).[7] This dataset provides a longitudinal monthly panel of jobs, including measures of earnings and work intensity. A job is defined as a unique combination of worker and enterprise (i.e. a 'permanent enterprise' or *pent*, as defined in Fabling (2011)). We use data covering the period from January 2004 until June 2022. These data are linked to workers' residential addresses, using a table of cleaned residential addresses derived from the *full address* table in the IDI.[8] From these data, we identify job displacements, measure worker outcomes, and create measures of local labour market conditions. In this section, we document the definitions we use and the restrictions we impose to create a novel dataset to support our analysis.

4.1 Identifying displacements

We aim to identify involuntary separations, for which the role of local labour market conditions is a more important influence on subsequent worker outcomes relative to voluntary separations, which will reflect workers' career choices and prior on-the-job search behaviour. Previous New Zealand studies of displaced worker outcomes have used either administrative data to identify involuntary job loss due to firm closures or mass layoffs (Dixon & Stillman, 2009), or workers' self-reported job displacements (Dixon & Maré, 2013; Hyslop & Townsend, 2019). We refine the approach to identifying displacement from administrative data. Involuntary separations are not explicitly identified in administrative data, so we impose some restrictions on observed job ends. For a job end to be treated as a displacement, we require a sufficient number of stable employees to leave a firm at the same time. This requires restrictions in the scope of employees, of jobs, and of firms.

We focus on employees aged 20-64 years old and exclude jobs that are observed in only one calendar month. To be considered as displaced workers, employees must have been in their job for at least one year prior to their job ending (possibly with one-month gaps in employment during that year). We exclude workers with very high or low annual earnings in the year prior to job end (below $24,000 or above $240,000 in 2020 dollars) on the basis that these may be atypical jobs. We refer to the resulting subset of jobs as 'in-scope' jobs.

[7] The user-generated table is available in the *IDI_Adhoc* database. The version that we use is based on the *IDI_clean_202210* archive.
[8] See Fabling and Maré (2020) for documentation of the approach to deriving cleaned residential addresses.

In-scope firms are those that have at least 5 in-scope jobs in the job-end month, and in which the employer never files EMS (employer monthly schedule) payroll returns for more than one enterprise. This latter restriction ensures that observed job-ends are not a result of statistical reallocation of workers between enterprises.

To distinguish involuntary separations from cases where multiple workers in a firm have concurrent voluntary job ends, we require that a high proportion of in-scope jobs end in the same month. For firms with 5 to 9 in-scope employees, we require that 100 percent of in-scope jobs end in the same month (referred to as '5/100'). For firms with 10 or more in-scope jobs in the displacement month, we require that at least 75 percent of in-scope jobs end in the same month ('10/75').[9] For a job-end to be treated as a displacement, we require that the employee does not return to the same firm within 2 years. We also exclude events where at last half of displaced workers move together to the same new employer within 2 months of the displacement ("mass transfers"). On average, displaced workers are part of a displacement event affecting 64 in-scope employees (median of 13).

Table 1 summarises the numbers of jobs and displacements. These requirements for a job end to be treated as a displacement are restrictive. Between 2005 and 2020, the average number of filled jobs in New Zealand was around 1.91m.[10] Average employment of 20–64-year-old workers (excluding those in one month jobs) in firms with 5 or more employees was around 1.15m. For this subset, there were around 45,000 job ends each month, 2,900 of which were in-scope jobs (with one year of tenure, excluding very high or low prior earnings, and excluding mass transfers and workers who return to the firm). Only around 130 per month satisfy our definition of displacement (5/100 or 10/75). [11] This implies that identified displacements are about 0.14 percent of employment annually – much lower than survey-based estimates of displacement rates in New Zealand of around 2 percent (Dixon & Maré, 2013; Hyslop & Townsend, 2019), or the 2 to 7 percent rates reported in cross-country studies (OECD, 2017). Our definition is deliberately selective, so as to increase the likelihood that the displacements that we identify are genuinely involuntary, but comes at the expense of incomplete coverage of genuine displacements.

[9] Andersson et al (2018) restrict their analysis to employers with at least 25 employees, and use an employment drop of at least 30% over a year to indicate involuntary job loss.

[10] Filled Job counts are from Statistics New Zealand's Linked Employer-Employee Data (LEED).

[11] Table 1 summarises the annual variation in the number of monthly job ends and displacements, comparing magnitudes with LEED data. LEED separation rates are measured at the plant level rather than at the enterprise level, and thus include within-enterprise movements that are excluded from our measures.

4.2 Local job prospects

We measure local employment density in two related ways, the first capturing the density of total employment, and the second providing a worker-specific measure that reflects the prevalence of the sort of jobs that the worker is most likely to work in.

Overall employment density is calculated as a spatially weighted sum of employment, similar to the effective density measure used in Maré & Graham (2013). For each displaced worker, we calculate the density of employment around the meshblock where they were most likely to have resided in the year prior to displacement.

$$Dens_{mt} = \sum_{k,d_{mk}<50} (g(d_{mk}) * E_{kt}) \tag{3}$$

where E_{kt} is employment in meshblock *k* in month *t,* and d_{mk} is the distance in km between the residential meshblock (*m*) and potential employment meshblock (*k*) within 50km. The distance decay function, $g(d_{mk}) = \exp(-0.1 \max(0, d_{mk} - 5))$, is chosen to reflect the observed spatial decay of commuting distances (Stroombergen et al., 2021). [12]

An analogous heterogeneous (varying across workers) measure of local job prospects, which we refer to as 'job opportunities', is calculated based on the density of jobs similar to the job from which each worker was displaced. To calculate this, jobs are classified as one of 114 distinct job types, defined by industry, firm skill intensity, and firm pay premium.[13] For each displacement-job type, the density of local jobs is calculated by weighting similar jobs more highly – in addition to the distance decay relationship. Similarity is calculated based on the intensity of inter-job transitions observed for voluntary job transitions.[14] The similarity between job types is calculated as a function of the correlation (ρ_{jh}) between columns of the symmetric matrix (*F*+*F*'), where the elements of *F* (F_{jh}) are the number of transitions from job-type *j* to job-type *h* $\left(s_{jh} = \frac{1+\rho_{jh}}{2}\right)$. By this measure, job-types are similar if they have similar sources of inflows and similar destinations of outflows. Separate transitions matrices are calculated for each of 12 groups of workers, defined by qualification, sex, and age.[15] There is thus a separate

[12] Where *m*=*k*, the distance is approximated by the mean distance between two independent points within a circle having the same area as the residential meshblock: $d_{mm} = \frac{128}{45\pi^{1.5}} Area^{0.5}$ (Apsimon, 1958). Employment at a distance of 12km has a weight of $g(12) = 0.5$. The weight falls to 0.1 at 28km, and is only 0.01 at the 50km limit over which we measure local labour market conditions.

[13] 19 1-digit ANZSIC industries; 3 firm pay premium groups, based on firm fixed effects estimates from a two-way worker-firm fixed effects regression (top quartile within industry; bottom quartile within industry; other); 2 skill-intensity groups, capturing whether firms are relatively intensive users of the worker's skill level (19*3*2=114 distinct job types).

[14] We treat as voluntary transitions all job changes observed between April 1999 and June 2022, excluding displaced workers and mass transfers. This will include some displacements that fall outside our strict definition. Appendix Table A1 illustrates the pattern of transitions for one dimension of job-type – industry. The table shows relative risk ratios for all voluntary transitions.

[15] This includes 3 qualification groups (no qualification; degree or above; and other); 2 gender groups; and 2 age groups (younger than 40, and 40 and above).

value for the correlation-based similarity weight (s_{ijh}) for each worker type (*i*), and each pair of job-types (*jh*).

The index of job opportunities for a worker of type *i*, residing in meshblock *m*, who is displaced from a job of type *j*, is calculated as:

$$Job\_opportunities_{ijmt} = \sum_{k,d_{mk}<50} \left(\frac{\sum_h (s_{ijh} * E_{hkt})}{g(d_{mk})} \right) \tag{4}$$

where E_{hkt} is employment of job type *h*, in meshblock *k*, month *t*. Jobs of the same type as the displacement job have a weight of $s_{ijh} = 1$.

4.2.1 Defining local labour markets

Since employment density and job prospects are defined for a 50km circle around each residential meshblock centroid, the local labour market is meshblock-specific. For the analysis and summary of results, we group displacements based on functional labour markets. Table 2 shows the prevalence of displacement events by functional urban area (FUA), as defined by Statistics New Zealand, and the proportion of layoffs, compared with 2018 population proportions.

Our main analysis examines the relationship between local employment density and worker outcomes using variation between as well as within each FUA. We also analyse within-FUA variation separately for the three largest metropolitan areas of Auckland, Wellington and Christchurch; and pool Hamilton and Tauranga, other large North Island regional centres (population of around 30,000 to 100,000), other large South Island regional centres, other regional centres (5,000 to 30,000), and locations outside urban areas.

4.3 Worker outcomes

Displaced workers in less dense labour markets may face more limited options for re-employment, and may be more likely to move, within New Zealand or overseas, or to accept a job that pays less and differs from the job they have left. We therefore examine a range of outcomes for displaced workers, to identify potentially different margins for adjustment.

We first examine whether the local labour market conditions affect a worker's decision to move overseas following a layoff event. Following this, for non-emigrant workers, we analyse several 'unconditional' post-displacement outcomes, and other conditional-on-employment job-quality outcomes. The unconditional outcomes include whether a worker moves geographically to a different FUA within New Zealand; the duration of the initial spell of unemployment following displacement, and for those who find work, the duration of their first re-employment spell; the likelihood of employment each month; and their monthly earnings (including zero earnings in months of unemployment). Conditional on a worker being employed in a month, the job- or match-quality measures include log(monthly earnings); whether the worker is re-employed in the same area, or the same industry from which they were displaced, and a richer measure of whether they are re-employed in a ‘similar’ job.[16]

To ensure that we observe workers’ employment, earnings, and residential location for at least 24 months before and 36 months after the displacement, we focus on job ends from January 2005 to June 2019.

4.4 Descriptive statistics and labour market trends

To provide some context for our analytical estimates of the effects of local labour market conditions on workers’ post-layoff outcomes, we first discuss the sample characteristics and describe the trends in employment and earnings around the month of displacement.

4.4.1 Sample descriptive statistics

Table 3 summarises the descriptive statistics for the sample of workers used in the analysis. The first column summarises the characteristics of the full analysis sample; the second column relates to the sample of non-emigrants, which is used for analysing domestic outcomes; and the last four columns relate to quartile subsamples of non-emigrant workers’ log(employment density). Our full analysis sample in column 1 consists of 21,384 workers displaced from jobs as part of a firm closure or mass layoff between January 2006 and June 2019, with about 70% of the displacements occurring after the Global Financial Crisis (GFC) (i.e. displaced from 2010 onwards).

About 36% of the displaced workers are female, the average age at layoff is about 41 years, and the average observed tenure in the job is 4.2 years, with 6% of laid-off workers’ tenure left-censored (i.e. they were in the job at the start of the EMS observation period in April 1999). The ethnic breakdown of the sample is 57% European-only, 8% Māori-only, 7% European and Māori, 7% Pacific-only, 15% Asian-only, and the remaining 8% of other ethnicities;[17] and nearly one-third of workers were born outside of New Zealand. We group education qualifications into three groups: Low-qualifications include workers with no qualifications or school level qualifications (level 0-3); Medium-qualifications include those with post-school

[16] This exploits the same similarity (s_{ijh}) metric as used in the job opportunities measure (equation (4)).
[17] Employees can report identifying with more than one ethnicity. Responses are grouped into distinct combinations, so that someone identifying as both Māori and European will be classified as “Māori-European” and will not be included in the Māori or European groups. The exception is in Table 8, where subgroups are defined based on total responses, meaning that employees identifying with multiple ethnicities will be included in more than one subgroup.

qualifications (level 4-6); and High-qualifications include those with Bachelor degrees and above (level 7+).[18] Between 25 and 44 percent of workers are classified into each of these groups.

The two local labour market conditions indexes are measured on a logarithmic scale: the average log(employment density) is 11.0 and average log(job opportunities) is 10.6.[19] For subsequent interpretation of results, it is helpful to understand the degree of variation in these measures: one measure is the standard deviation, which equals 1.5 for each measure, and the variation in means across the quartile subsamples provides another, as shown in Table 3.

Alternatively, it is helpful to compare the differences in the averages across areas (see Table 2): e.g. the average log(employment density) (log(job opportunities)) in the Auckland urban area is 12.3 (11.8), compared to 11.4-11.6 (10.9-11.2) in Wellington and Christchurch urban areas, 10.8 (10.3) in Hamilton and Tauranga, 9.9-10.2 (9.5-9.8) in other main North Island and South Island urban areas, and 8.9 (8.5) in other urban areas. Therefore, the local labour market density is approximately 60-90 log-points greater for a displaced worker in Auckland, compared to that for a worker in Wellington or Christchurch, and about 150 log-points more than in Hamilton and Tauranga. Variation in local labour market conditions is dominated by differences between urban areas. Between-area variance accounts for over 85 percent of the cross-sectional variance. When estimating the impact of local labour market conditions on post-displacement outcomes, we focus on within-area variation. Differences between areas may reflect many factors in addition to differences in density, whereas density differences are a more salient factor when examining within-area differences in outcomes. We do, however, report some estimates from 'simple' specifications that reflect both within- and between-area variation, which do not differ greatly from specifications that control for between area (and other) differences, particularly for the main labour market outcomes of interest.

Ten percent of displaced workers are estimated to have emigrated following layoff.[20] Comparing the characteristics of the full sample (column 1 of table 3) with those for the non-emigrant sample (column 2), migrants are slightly more likely to be male, younger, Asian or with miscellaneous ethnicity, and also born overseas. Otherwise, these samples appear broadly similar. Differences in socio-demographic characteristics across the four log(employment

[18] For workers with missing qualifications, we impute their highest qualification using an order logit model estimated using sex, age, ethnicity, country-of-birth, percentile of the estimated worker earnings fixed effect, and the industry and size of their first employer as control variables.

[19] These indexes are very highly correlated, with a simple correlation coefficient of more than 0.99.

[20] Departures occur, on average, 15 months after layoff. About one-eighth of emigrants are estimated to leave immediately, although some of this may be due to errors in classification as more than half of these workers are measured to be overseas in any month prior to layoff. In addition, we estimate that about a quarter of emigrating workers leave in the next 6 months, one-third between 6-months and 2-years of layoff, and the remaining (28%) leave after 2 years. Also, as our measure is one of persistent (i.e. a spell overseas of at least three months), rather than permanent, emigration, "emigrating" workers can and do return to New Zealand following an emigration spell: e.g. of those we measure to emigrate 1-6 months after layoff, 20% are in New Zealand over months 7-24, and 30% after 2-years.

density) quartile samples (columns 3–6) appear to largely reflect regional differences, with the higher quartile samples being more urban.[21] Pre-layoff average tenure is slightly longer for workers in lower quartiles, while average earnings are higher in higher quartiles (reflecting urban wage premia).

The domestic outcomes among non-emigrants are shown in the second column of table 3. About 19% move to another area within New Zealand (after 21 months on average). In the month following layoff, 59% of non-emigrant displaced workers are employed. This is partly related to whether the worker is observed to hold multiple jobs prior to layoff. For example, 15% of workers have multiple jobs in the month prior to layoff; and the employment rate in the month following layoff for these workers is 95%, compared to 53% for workers with a single job.[22] The average duration of unemployment following layoff is 6.3 months: this includes zero months for workers who have no months of unemployment, and the maximum observed duration (36-48 months) for the 9% of workers are not re-employed during the follow-up period. Among the 91% of workers who do find re-employment, the average duration in their first spell is 29 months, and 48% are continuing in that spell at the end of the observation period (36-48 months after layoff). Across the four log(employment density) quartile samples, workers in higher density areas are less likely to migrate, are less likely to be employed immediately following layoff (e.g. 52% of workers in quartile-4 are employed in the month following layoff, compared to 64% in quartile-1), have longer unemployment durations and lower re-employment rates; but, conditional on being re-employed, have similar re-employment durations.

Finally, we briefly summarise patterns of the incidence of changes in the location or industry a worker is employed in, and the job similarity, compared to their layoff job (not shown in Table 3). About 4% of employed workers are in a different area 6 months after layoff, 8% 18 months after, and 10% 30 months after; these rates compare to about 6% 18 months prior to layoff (i.e. 6 months prior to the 12-month selection period). In contrast, much larger fractions of employed workers change 2-digit industry, and this appears to be more directly related to layoff: 56% 6 months after layoff, 59% 18 months after and 62% 30 months after layoff. The similarity of their current to layoff-job also declines following layoff: the index is 84% 6 months, 83% 18 months and 82% 30 months after layoff. By comparison, although most workers are in the same

[21] Note that Auckland urban area workers account for about 37% of the non-emigrant sample, but all of the (top) quartile-4 sample, about 40% of the quartile-3 sample, 14% of quartile-2, and only 1% of the (bottom) quartile-1 sample. Similarly, Wellington workers are mainly in quartiles 2 (19%) and 3 (17%), compared to 9% overall; and Christchurch workers are also mainly in quartiles 2 (12%) and 3 (43%), compared to 14% overall.

[22] Also, among the 3% of workers measured without a job in the month prior to layoff, the employment rate is 38% in the month after layoff.

job 18 months prior to layoff, 11% are employed in a different industry and the similarity index is 92% relative to their layoff job.[23]

4.4.2 Trends in labour market outcomes

We next describe the patterns of workers' post-layoff labour market outcomes, focussing on monthly employment, log(monthly earnings) (conditional on being employed), and unconditional monthly earnings. Figure 1 plots the trend patterns of the employment rate (panel a), average log(monthly earnings) conditional on being employed (panel b), and the average unconditional earnings (including zeros for displaced workers in months they are not employed) (panel c). Each of these figures include the "raw" trends; and for the employment rate and log(earnings), we also include both estimated 'counterfactual' and 'adjusted' trends. Because we select on being employed for at least one year, there is a mechanical decline in the employment rate away from the selection months. We estimate this mechanical ('counterfactual') pattern from the increase that is observed in months -12 to -24, and project this symmetrically as a drop-off in employment from month 0.[24] Similarly, for conditional earnings outcomes, there is approximately a linear trend increase in log(earnings) prior to displacement, that we estimate over months -24 to -1, and project forward. The 'adjusted' trends are then estimated as the difference between the raw and counterfactual estimates for employment and log(earnings). The declining post-layoff employment rate together with increasing log(earnings) confounds a simple characterisation of the unconditional earnings trends: because of this, we don't attempt to provide a counterfactual for that outcome.

Figure 1(a) shows a sharp drop in the raw employment rate (solid line) in the month after layoff to 59%.[25] This is followed by a recovery over the following 6 months before steadily flattening out to about 77%, and then gradually declining to 74-75% after 3-4 years. However, given the employment rate's expected counterfactual (dotted line) decline irrespective of layoff (estimated to be about 8% after 12 months and 11% after 3 years), the adjusted employment rate rises throughout the post-layoff period. Conversely, the estimated adjusted drop in the employment rate (dashed line) is about 14 percentage points (ppts) compared to the raw 26 ppts drop after 3-4 years.

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[23] A graphical summary of outcomes is included as Figure A1, which shows the implied pattern of outcomes for a group of 1,000 displaced workers.

[24] In particular, we estimate a log-linear employment-rate trend over months -24 to -12, and symmetrically apply this estimated trend to the post-displacement period.

[25] There is a noticeable (3%) drop in the employment rate in the month prior to layoff. We don't have a clear explanation for this but suspect it may reflect that some workers are eligible for redundancy payments even if they leave a firm before its final month, and subsequently receive a final payment after they have left the firm. In addition, Figure 1(b) shows average log(earnings) fall about 3% over the final 3 months prior to layoff. However, Figure 1(c) shows no apparent drop in average unconditional earnings over this period.

Figure 1(b) shows the analogous graphs for log(earnings). Adjusting for the counterfactual expected post-layoff increase in earnings, the adjusted decline in earnings is greater than the raw decline.[26] For example, compared to earnings in the month prior to layoff, average earnings are about 11% lower 6 months after layoff and about the same as pre-layoff after 3 years; in contrast, adjusting for the counterfactual growth in the absence of layoff, earnings are 13% lower after 6 months and 15% lower after 3 years.

Figure 1(c) shows the combined effects on workers' earnings of both employment and conditional earnings losses following layoffs. Relative to the month prior to layoff, raw average monthly earnings are less than half in the month after layoff ($2,600 compared to $5,300), before rising gradually to nearly $4,000, which still represents a greater than 25% loss of earnings.

Second, to describe how these outcomes vary by labour market, Figure 2 displays trends in each outcome for five urban area groupings: Auckland, Hamilton and Tauranga, Wellington, Christchurch, and other urban areas.[27] We plot the raw patterns for each outcome (i.e. we do not adjust the employment rate or log(earnings) for the estimated counterfactual trends described above). But, to account for relative earnings (level) differences across areas, in panel (b) we plot the difference between average log(earnings) in a month and the average log(earnings) over the year prior to layoff (i.e. over months -12 to -1), and in panel (c) we plot the ratio of average earnings in a month to the average earnings over the year prior to layoff.

In contrast to the predictions of the spatial mismatch hypothesis, the employment rate patterns in Figure 2(a) show that the re-employment rates of displaced workers in the larger urban areas (especially Auckland and Wellington) are lower than in smaller areas with lower employment density. The log(earnings) patterns across areas in Figure 2(b) are less clear, but don't obviously show higher relative earnings for workers displaced in larger urban areas. Finally, Figure 2(c) plots the combined extensive (employment) and intensive (conditional earnings) margin effects, which shows a similar but weaker pattern to that seen for employment in Figure 2(a), with the relative earnings being lower in larger urban areas.

[26] In Figure 1(b), we have anchored the counterfactual earnings trend to average log(earnings) in the month prior to layoff. While this affects the level of the adjusted log(earnings), it doesn't affect the relative trend over time. There is a substantial spike in earnings in the layoff month, which reflects final payments including any leave payouts and severance or redundancy pay.

[27] We present analogous employment, log(monthly earnings) and monthly earnings trends for other subgroups of workers by industry growth (Figure A2), sex and age (Figure A3), highest qualification (Figure A4), and ethnicity (Figure A5).

5 Results

We now report estimation results of the local labour market contribution to post-displacement outcomes of workers. We begin, in the next subsection, by analysing the effect on the decision to emigrate, defined as having any persistent spells overseas. Following this, we turn to the effects on domestically measured outcomes for the subpopulation of workers who do not emigrate. These outcomes include whether workers migrate domestically following displacement; their labour market outcomes measured by the lengths of initial unemployment spell following layoff and subsequent first re-employment spell for those who find employment; their monthly employment and earnings. Also, for workers who are employed, we analyse alternative possible dimensions of job quality relative to their layoff-job: whether they have changed location or industry, and how similar their current job is.

5.1 International emigration

We first analyse whether local labour market characteristics affect displaced workers' decisions to migrate internationally. For this analysis, we focus on whether workers have any persistent spells overseas, which we define as being overseas for a continuous period of at least three months at some point following their layoff event.[28] Table 4 presents two sets of results for overseas migration: both a simple model with no covariate controls and a model with controls,[29] and for two alternative measures of the local labour market conditions, log(employment density) and log(job opportunities). The first two columns summarise the results for the binary emigration outcome, estimated using logit models. The simple model estimates for the two LLM conditions are almost identical and imply that a 100 log-point increase in either employment density or job opportunities is associated with 0.15 higher odds of migration (about 1.8 ppts higher probability of migrating).[30] When covariate controls are included, the estimated effects are somewhat higher: a 100 log-point increase in log(employment density) increases migration odds by 0.21 (2.1 ppts higher probability), and the same increase in log(job opportunities) increases migration by 0.19 ppts (1.9 ppts higher probability). These positive effects are counter

[28] There is a trade-off in the choice of overseas spell length used to define emigration, with shorter lengths resulting in higher incidences of measured emigration and consequent effects on the size of the non-migration sample to be used for subsequent analysis. The choice of three months allows for relatively moderate non-migration spells overseas, which we believe provides a reasonable compromise and the results appear to be robust to variations in length (e.g. 6 or 12 months).
[29] The control variables include indicator variables for overseas-born, female, ethnicity (Māori only, Pacific only, Asian only, European and Māori, and miscellaneous ethnicity responses), highest qualification (low, medium and high), functional urban areas (FUA), and whether the layoff-job length of tenure is censored (i.e. job started before April 1999); and quadratics in age and layoff-job tenure.
[30] The standard deviations of log(employment density) and log(job opportunities) are about 1.5 (150 log points), and the Interquartile ranges are about 2.2 (220 log points).

to the hypothesis that workers displaced in areas with more employment opportunities are less likely to emigrate.

For our subsequent analyses, we exclude workers who have any persistent spells overseas, and focus on workers who are based predominantly in New Zealand throughout the post-layoff period. This avoids any possible confounding effects associated with not observing overseas activities: e.g. those who go overseas to work for a period and then return to New Zealand. However, our approach of estimating the effects on emigration separately before domestic outcomes does ignore the possibility that a worker may treat emigrating versus taking a job in New Zealand as alternative job search options, and thus ignores the possible selectivity of remaining in New Zealand.

5.2 Domestic migration

For workers who don’t have any spells overseas lasting at least three months, we begin by analysing whether local labour market characteristics affect displaced workers’ decisions to migrate domestically within New Zealand. For this outcome, we again consider whether the worker ever subsequently lives in an urban area (FUA) different from the one they lived in in the layoff month.

We present the results in the third and fourth columns of Table 4, which are analogous to the overseas emigration outcomes in the first two columns. The simple model estimates for the binary “migration” outcomes suggest that being laid-off from a job in a LLM with greater employment density or log opportunities lowers the likelihood of migrating. However, when control variables are included in the models, the estimates are statistically insignificantly positive and small.

5.3 Layoff unemployment and re-employment durations

Next, we consider two worker-level summary measures of unemployment and employment experiences after layoff. First, we analyse the duration of their first spell of unemployment following layoff, which can be zero months for workers who either move immediately into another job, or who have multiple jobs at the time of their measured layoff. Second, for displaced workers who find employment following their layoff, we analyse the duration of their first re-employment spell. We use proportional hazard duration models for each outcome, and allow for right censoring of both (i.e. workers who do not find re-employment, and workers whose first re-employment spell does not end within 48 months of layoff).

Table 5 summarises the results. First, we estimate that greater employment density or job opportunities tends to lower the probability of unemployment exit in the simple duration models, but the effect is small and statistically insignificant when control variables are included. Second, we find no effect of local area employment density or job opportunities on employment spell exits rates among those displaced workers who find employment after their layoff.

5.4 Worker re-employment and earnings outcomes

In this subsection we provide a more detailed analysis of displaced workers' subsequent labour market experiences. For this, we analyse their monthly employment and earnings using panel data on these outcomes over the period before and after layoff.

The regression specification we use to examine the effects on displaced workers employment and earnings is an extension of equation (2) that includes controls for possible trends in the employment and log(monthly earnings) outcomes respectively.[31] In particular, the regressions are of the form:

$$y_{it} = \tau_t + llmc_i\delta_0 + llmc_i 1(t > 0)\delta_1 + X_i\beta + c(trend_t) + u_{it} \qquad (5)$$

where $llmc_i$ is either the log(employment density) or log(job opportunities) in the local labour market in which the worker was displaced, X_i is a vector of demographic and human capital characteristics of worker-*i* measured at the time of layoff, and $c(trend_t)$ is a control function for expected secular employment or earnings trends in the absence of the displacement event.

The sample selection requirement that workers were employed for at least 12 months prior to layoff implies the employment rate will be close to 100% over the 12 months prior to layoff (we allow 1-month gaps in employment). Given this selection requirement, we expect that the employment rate would fall even in the absence of the workers' being laid-off – e.g. over the 12-months leading up to this selection period (months -24, …, -12) the employment rate rose steadily from about 90% to 100%. Based on this pattern, and evidence from Hyslop and Townsend (2019), in line with the trends described in Figure 1(a), we use $c(trend_t)$ to symmetrically control for the trend in the employment rate following layoff as a log-linear function from the layoff month using the 12-month period from month -24 to month -12.[32]

[31] As we discuss above, this isn't necessary given our focus is on the relative effects of local labour market conditions on displaced workers' outcomes rather than the displacement effects *per se*, and doesn't change the estimates of interest.
[32] To do this, we restrict the time effects (τ_t). We include dummy variables for each post-layoff month, a single dummy variable for months -12 to -1 (to control for workers with intermittent months with no earnings), and a log-linear time trend that is estimated from months -24 to -13, which is symmetrically applied to the post-layoff months. An alternative approach would be to ignore the trend, and include monthly dummy variables across the entire sample period. Our estimated local labour effects of interest are not materially affected by this choice, and only differ to the extent that the estimated parametric trend differs from the (non-parametric) *trend* associated with monthly dummy variables over the pre-layoff period.

For workers' conditional earnings, average log(earnings) over the 2-years prior to layoff show an approximately linear trend. In line with the counterfactual trend shown in Figure 1(b), to control for secular earnings growth in the absence of the layoff events, we estimate and extrapolate this trend forward over the post-layoff period.[33] When modelling unconditional earnings we do not attempt to control for secular trends because the pre-displacement trends in employment and earnings growth are both positive, while the secular post-displacement trends are negative for employment and continuing positive for earnings growth, which is relatively complicated to model.

Table 6 summarises the estimated $llmc_i$ effects on monthly employment in panel (A), log(monthly earnings) in panel (B), and unconditional monthly earnings in panel (C) for the full sample of non-emigrant displaced workers. We report post-layoff $llmc_i$ coefficients from four separate regression specifications for each of the three outcomes: first a 'simple' regression that controls only for monthly time effects and the trend functions described above; second including observable control variables; third, also interacting the controls with a post-layoff indicator to allow the effects of the control variables to vary before and after layoff; and finally controlling for constant unobserved characteristics of workers by including worker fixed effects. The reported coefficients have been scaled to represent percentage effects for both the employment and log(earnings) outcomes;[34] while the coefficients for the unconditional earnings regressions represent monthly $-value effects.

The estimated post-layoff employment density or job opportunities effects on the employment margin, reported in panel (A), are negative. The estimates across all specifications are remarkably consistent and imply that a 100 log-point increase in employment density or job opportunities in a local labour market reduces workers' post-layoff employment rate by about 0.8 ppts. This is counter to the hypothesis that re-employment opportunities would be stronger for workers displaced in areas with greater employment density or job opportunities, but is consistent with the employment rate patterns across urban areas shown in Figure 2, which are based on between-area density variation.

The analogous estimates for conditional log(monthly earnings) are reported in panel (B) of Table 6. The post-layoff effects of employment density and job opportunities on conditional earnings are consistently positive. The post-layoff estimates imply a 100 log-point increase in

[33] That is, we include dummy variables for each post-layoff month, as well as a layoff-month dummy variable to control for the spike in earnings in that month, and a linear time trend that is estimated from months -24 to -1, which we extrapolate forward. Again, the alternative of simply ignoring the trend in earnings and including dummy variables for each month, does not materially affect the estimated local labour market conditions effects of interest.
[34] That is, the monthly employment binary variable is 0-100 and log(earnings) has been multiplied by 100: strictly speaking, for log(earnings), the coefficients are 100*log-point effects, which provides a reasonable approximation to percentage effects for small values.

either employment density or job opportunities has a positive effect on earnings of about 0.8% in the simple and with-control regressions. However, when we allow the effects of the controls to vary pre- and post-layoff in the final two columns, the estimated effect drops substantially (to 0.15 – 0.2%). This suggests there are relatively more positive (or less negative) displacement effects associated with some characteristics that are positively correlated with a worker's employment density – e.g. if lower skilled workers, who experience relatively lower conditional earnings losses, tend to live in more dense areas.

The estimated post-layoff effects of local labour market conditions on the employment probability and conditional earnings appear to be offsetting. Partly for this reason, in panel (C) we report estimates for unconditional earnings (estimated in $-levels), which combine both the extensive (employment) and intensive (earnings) margins. The estimates suggest the negative employment effects tend to dominate the positive effects on conditional earnings: a 100 log-point increase in employment density or job opportunities is estimated to result in $30-$40 lower post-layoff earnings in the first two specifications, and $70-$80 in the latter two specifications.

5.5 Change in location and industry, and job-similarity if employed

The final set of outcomes we examine are whether, conditional on being employed, a worker is employed in a different urban area from where they were laid-off, a different 2-digit industry, or how similar their current job is to their layoff job. The similarity measure is an index that ranges from 0 to 100% (i.e. zero to complete job similarity). We again report the effects of local labour market conditions, based on each of the log(employment density) and log(job opportunities) measures.

Table 7 summarises the results from these analyses, again presenting the post-layoff interaction $llmc_i$ coefficients for each of the outcomes: employed in a different area[35] in panel (A), different industry in panel (B), and the job similarity in panel (C). The results in panel (A) imply that a 100 log-point increase in the employment density or job opportunities associated with the area in which a worker is displaced reduces the post-layoff probability that they will be employed in a different area by about 0.6 ppts (-0.56 to -0.65 across the specifications presented). This is consistent with the notion that more dense labour market areas offer greater employment opportunities, reducing the need for workers to move elsewhere.

On the other hand, the results in panel (B) show that workers displaced in more dense labour market areas are more likely to change the industry in which they are employed. For

[35] Being employed in a different area is identified based on residential location.

example, allowing for the effects of observable factors to differ pre- and post-layoff, the results imply that a 100 log-point increase in employment density or job opportunities increases the post-layoff probability of an industry change by 0.5 – 0.66 ppts. This pattern may reflect that more dense areas provide greater occupational (or other job-characteristic) similarity across industries, or that they have greater industrial diversity. The final outcome in panel (C), the measure of similarity between a worker's current and layoff jobs, support this notion: a 100 log-point increase in employment density or job opportunities results in the current job being 0.15 – 0.4 ppts more similar to a worker's layoff job. Compared to the average post-layoff job similarity of about 82%, these estimates imply relative effects of about 0.2-0.5%.

5.6 Effects across subsamples

We now document how the effects of local labour market conditions on the suite of outcomes considered above vary across sub-populations defined by several dimensions, including time, geographic area, industry growth, whether the worker was born overseas, and worker demographics (sex, age, qualifications and ethnicity).[36] We summarise these patterns in Table 8, which reports the estimated log(employment density) effects from the most general specification for each outcome considered in Table 4 – Table 7.[37] The table is organised with different outcomes across columns, and different sub-population across rows. The first row repeats the full sample results from before.

The results are broadly consistent across the sub-samples. Given that, we will briefly discuss the more systematic differences across groups for some outcomes. First, it appears that the overseas migration full sample results are largely driven by workers displaced in Auckland and Wellington. In addition, the local labour market effects on emigration are stronger for workers displaced from mid- to higher-growth industries, younger (<40 years) workers, those with higher qualifications, and Europeans. The only (weakly) statistically significant estimate of labour market conditions on domestic migration is for displaced Māori workers, which implies those laid-off in a less dense area are less likely to move.

For monthly employment, log(earnings) and unconditional earnings outcomes, the density effects are more strongly positive (or less negative) for workers laid-off post-GFC. The results by growth industries indicate that the employment density effects for workers displaced in higher growth industries are broadly more positive (or less negative) for employment and earnings.

[36] Ethnicity is measured here based on total responses (people identifying with more than one ethnicity will be included in more than one row.

[37] The baseline employment rate, average log(earnings) and average unconditional earnings trends for each of the non-regional subgroups are presented in appendix Figure A2 – Figure A5.

However, results by workers' education level show more negative density effects on employment for higher qualified workers, which translates into greater monthly earnings loss.

In addition, we have also conducted some sensitivity testing to exclude the period following the onset of the COVID-19 pandemic. To do this we excluded layoffs that occurred after March 2017, which provides a 36-month follow-up period that ends by February 2020, and re-estimated the employment, log(earnings), and unconditional monthly earnings models. Doing this, the results are qualitatively similar and quantitatively somewhat larger than those presented here.

One consistent pattern is that within larger urban areas (Auckland, Hamilton, Tauranga, Wellington, Christchurch), displaced workers who are re-employed are more likely to find employment in jobs similar to the job from which they were displaced. In contrast, young workers, women, and workers displaced from slower growing industries are more likely to be re-employed in dissimilar jobs if they are in more dense labour markets.

6 Concluding discussion

Our analysis finds limited support for the hypothesis that living in local areas with greater employment density or job opportunities results in relatively better outcomes for involuntarily displaced workers. First, the results shows that workers displaced in more dense local areas are more likely to emigrate. This pattern is concentrated for workers in Auckland and Wellington, those in mid- to high growth industries, young workers, those with higher qualifications, and of European ethnicity. Although this appears somewhat counter-intuitive, it is perhaps consistent with emigration being viewed positively as a more viable option by relatively advantaged displaced workers. In contrast to this emigration result, we find no evidence of local area density effects on the propensity of displaced workers to migrate domestically.

Second, among non-emigrants, we find that monthly re-employment rates are lower for workers who were laid-off in more dense areas.[38] However, conditional on being employed, such workers have higher earnings, although not sufficiently to outweigh the negative employment effects in terms of their unconditional earnings. These results generally apply broadly across the analysis subsamples.

Third, conditional on being employed, we also find that workers displaced in more dense local areas are less likely to be employed in a different area, more likely to employed in a different industry, and have a job that is more similar to the job from which they were laid-off.

[38] However, we find no density effects on the duration either the workers' layoff unemployment spell or their first re-employment spell.

Collectively, these results support the notion that thicker local labour markets provide more suitable re-employment opportunities, albeit not necessarily in the same industry. However, the patterns are variable across subsamples. For example, the density effects on industry changes are negative in the three largest urban areas (Auckland, Wellington and Christchurch); also, subsamples where the positive industry-change effect is relatively strong (workers displaced from low-growing industries, females, young workers, higher qualifications, and Pacific and Asian workers), the job-similarity effects are generally negative, and the corresponding employment and earnings effects are more negative.

One of the novel contributions of this study is the use of job similarity measures based on the transition patterns of voluntary job changers. This provides a more meaningful measure of whether displaced workers find re-employment in the sort of jobs that they might have moved to in the absence of displacement, than is captured by the more commonly used 'industry change' measure. An employee-specific measure of local labour market opportunities based on job similarity does not, however, appear to capture much variation beyond what is captured by the density of total employment. The two measures are highly correlated, and provide the same insights when used interchangeably for estimation.

The spatial mismatch literature has led to a range of policy prescriptions to move unemployed workers closer to jobs (Katz et al., 2001), to move jobs closer to unemployed workers (Kolko & Neumark, 2010; Mason et al., 2023), and to improve transport connections between job-rich and high-unemployment areas (Holzer et al., 2003). Our findings that local labour market density does not improve outcomes for displaced workers weakens the rationale for such policies as means of increasing the resilience of workers to displacement events. A range of other supply-side (retraining, job search assistance) policy measures may provide more effective means of achieving greater resilience for displaced workers in New Zealand (Evans-Klock et al., 1999).

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Tables and Figures

Table 1: Number of displacements – comparison with LEED data

	LEED		Admin Data				
June year	Filled jobs (mean)	Separations (monthly)	Employees (mean)	Job Ends (monthly)	In-scope Job Ends (monthly)	Layoffs (monthly)	Layoffs (Annual)
2006	1,744,853	99,226	1,005,316	43,613	2,382	110	1,323
2007	1,787,925	100,798	1,031,094	44,930	2,602	104	1,251
2008	1,827,943	102,307	1,058,745	45,919	2,755	113	1,359
2009	1,815,935	96,473	1,057,863	42,071	2,542	164	1,971
2010	1,782,440	93,737	1,048,533	38,692	2,357	137	1,641
2011	1,792,270	91,723	1,065,880	40,835	2,452	133	1,599
2012	1,812,805	91,463	1,080,965	40,992	2,685	96	1,152
2013	1,832,823	87,321	1,095,503	41,091	2,665	122	1,458
2014	1,875,778	90,098	1,133,032	42,715	2,786	131	1,572
2015	1,928,260	94,318	1,177,875	44,702	2,869	153	1,839
2016	1,977,410	97,566	1,222,839	47,263	3,255	173	2,079
2017	2,039,385	103,986	1,274,464	50,284	3,409	138	1,659
2018	2,105,498	117,377	1,325,829	53,131	3,512	123	1,473
2019	2,154,478	111,288	1,364,717	53,773	3,609	129	1,548
2020	2,188,493	107,824	1,394,382	51,124	3,600	173	2,079
TOTAL							24,003 *

Notes: LEED data measure separations at the plant (PBN) level. Admin data are restricted to firms with at least 5 in-scope employees (age 20-64, $24k-$240k prior earnings), that are never a joint EMS-filer; and where the job end is not a part of a mass transfer.

* This table summarises layoffs during June years and thus omits two quarters (2005q1 and 2005q2). The total number of layoffs from January 2005 to June 2020 is 24,426. All counts have been randomly rounded in accordance with Statistics New Zealand's confidentialisation requirements.

Table 2: Displacements – by functional urban area grouping

Functional Urban Area	Number of layoffs	% of layoffs	% of 2018 Census population	local labour market conditions Mean (st.dev.)	
				Log(Density)	Log(Opps)
Auckland	8,118	38%	33%	12.28	11.84
				(0.58)	(0.58)
Hamilton & Tauranga	1,356	6%	8%	10.75	10.31
				(0.38)	(0.39)
Wellington	1,953	9%	9%	11.38	10.93
				(0.52)	(0.52)
Christchurch	2,952	14%	10%	11.60	11.15
				(0.49)	(0.49)
Other NI Main	2,640	12%	13%	9.91	9.47
				(0.45)	(0.46)
Other SI Main	1,011	5%	6%	10.19	9.75
				(0.41)	(0.40)
Other Urban	1,833	9%	11%	8.92	8.48
				(0.57)	(0.57)
Outside Urban	1,521	7%	12%	7.91	7.47
				(1.13)	(1.12)
TOTAL	21,384	100%	100%	11.02	10.58
				(1.50)	(1.50)

Note: Functional urban areas defined by Statistics New Zealand (2021). All counts have been randomly rounded in accordance with Statistics New Zealand confidentialisation requirements.

Table 3: Summary sample statistics

	Full sample	Non-emigrants	Employment density quartile (non-emigrants) 1	2	3	4
No. workers	21,384	19,236	5,088	4,890	4,794	4,464
Female	0.36	0.37	0.35	0.36	0.39	0.38
Age	40.7	41.3	42.8	41.3	40.7	40.2
	(12.0)	(11.9)	(12.0)	(12.0)	(11.8)	(11.6)
Quals: low	0.44	0.45	0.48	0.47	0.46	0.40
medium	0.30	0.30	0.36	0.33	0.27	0.24
high	0.25	0.24	0.15	0.20	0.27	0.36
European-only	0.57	0.58	0.66	0.66	0.60	0.39
Māori-only	0.08	0.08	0.13	0.09	0.05	0.05
Pacific-only	0.07	0.07	0.01	0.05	0.07	0.15
Asian-only	0.15	0.14	0.05	0.07	0.16	0.29
Euro & Māori	0.07	0.07	0.10	0.07	0.05	0.04
Other ethnicity	0.08	0.07	0.06	0.07	0.07	0.09
Overseas born	0.32	0.30	0.15	0.23	0.35	0.51
Tenure (years)	4.18	4.30	4.43	4.32	4.18	4.26
	(3.5)	(3.6)	(3.6)	(3.7)	(3.5)	(3.6)
Tenure censored	0.06	0.06	0.06	0.06	0.07	0.06
Local labour market conditions:						
log(employment density)	11.02	10.99	8.92	10.77	11.90	12.59
	(1.50)	(1.51)	(0.99)	(0.44)	(0.19)	(0.19)
log(job opportunities)	10.58	10.55	8.49	10.33	11.46	12.15
	(1.50)	(1.51)	(0.99)	(0.44)	(0.21)	(0.19)
Outcomes:						
Emigrate (overseas)	0.10	0	0	0	0	0
Migrate domestically	0.19	0.19	0.26	0.20	0.15	0.12
Empl (month=-1)	0.97	0.97	0.98	0.97	0.97	0.97
log(monthly earnings)	8.41	8.41	8.36	8.42	8.44	8.45
	(1.50)	(1.50)	(1.42)	(1.52)	(1.52)	(1.54)
Monthly Earnings (2020$)	5,316	5,319	4,953	5,301	5,473	5,591
	(4,957)	(5,001)	(3,696)	(5,889)	(5,443)	(4,706)
Empl (month=1)	0.57	0.59	0.64	0.61	0.57	0.52
Unemployment duration (months)	7.15	6.26	5.35	5.86	6.31	7.70
	(15.0)	(14.0)	(13.1)	(13.5)	(13.9)	(15.2)
Unempl censored	0.10	0.09	0.07	0.08	0.08	0.11
Re-employment duration (months)	26.9	28.6	29.7	29.1	28.6	27.0
	(18.2)	(18.0)	(18.0)	(18.0)	(18.1)	(17.9)
Re-emp censored	0.44	0.48	0.50	0.49	0.49	0.46

Notes: standard deviations are in parentheses. Age, tenure and density are measured in the month of layoff. The log(monthly earnings) and monthly earnings statistics pertain to the month prior to layoff (month=-1). The unemployment spell relates to that following layoff (including zero months); and the re-employment spell relates to the first employment spell following layoff. Monthly earnings are CPI-adjusted and expressed in 2020-values.

Table 4: Migration outcomes

	Migrated overseas		Migrated domestically (non-emigrants)	
	Simple	Controls	Simple	Controls
Log(employment density)	0.150***	0.213***	-0.230***	0.039
	(0.017)	(0.049)	(0.012)	(0.035)
No. workers	21,384	21,291	19,236	19,236
Pseudo R-sq	0.006	0.079	0.021	0.083
logL	-6926.6	-6408.8	-9,081.4	-8,502.3
Chi-square	87.42	1103.8	383.0	1,541.4
Log(job opportunities)	0.148***	0.193***	-0.232***	0.029
	(0.017)	(0.049)	(0.012)	(0.035)
No. workers	21,384	21,291	19,236	19,236
Pseudo R-sq	0.006	0.079	0.021	0.083
logL	-6,928.1	-6,410.4	-9,078.8	-8,502.5
Chi-square	84.45	1,100.6	388.2	1,540.9

Notes: The migration outcomes are binary measures, but scaled 0-100, so the coefficients represent the effect on log(odds). Estimates are from logit models. Controls include indicator variables for Overseas born, Female, ethnicity (Māori only, Pacific only, Asian only, European and Māori, and miscellaneous ethnicity responses), highest qualification (low, medium and high), functional urban areas (FUA), and whether the layoff-job length of tenure is censored (i.e. job started before April 1999); and quadratics in age and layoff-job tenure. The number of observations in the second column is lower than in the first because some outcomes are completely determined, and so are dropped from estimation.

Table 5: Layoff unemployment and re-employment job durations

	Exit from unemployment (hazard)		Job end (hazard)	
	Simple	Controls	Simple	Controls
Log(employment density)	-0.031***	-0.019	0.010	-0.005
	(0.005)	(0.014)	(0.007)	(0.020)
No. workers	19,236	19,236	17,598	17,598
Pseudo R-sq	0.000	0.001	0.000	0.004
logL	-164,692.2	-164,541.2	-78,380.6	-78,076.5
Chi-square	40.14	342.1	1.672	610.0
Log(job opportunities)	-0.030***	-0.015	0.010	-0.006
	(0.005)	(0.014)	(0.007)	(0.020)
No. workers	19,236	19,236	17,598	17,598
Pseudo R-sq	0.000	0.001	0.000	0.004
logL	-164,693.7	-164,541.5	-78,380.5	-78,076.5
Chi-square	37.11	341.4	1.898	610.0

Notes: Sample is restricted to workers who don't emigrate overseas; the unemployment duration sample includes the 59% of workers who have zero months of unemployment following layoff; the re-employment duration sample also excludes workers with ongoing (right censored) layoff unemployment spells. In the first two columns, the effect on the layoff unemployment spell duration is estimated using a proportional hazards duration model; in the third and fourth columns, the first re-employment spell duration is estimated using a proportional hazards duration model. Controls include indicator variables for Overseas born, Female, ethnicity (Māori only, Pacific only, Asian only, European and Māori, and miscellaneous ethnicity responses), highest qualification (low, medium and high), functional urban areas (FUA), and whether the layoff-job length of tenure is censored (i.e. job started before April 1999); and quadratics in age and layoff-job tenure.

Table 6: Employment and earnings – non-emigrant sample

	Simple	Controls	Post*Controls	Worker FE
		(A) Monthly employment		
Log(density)	-0.808***	-0.808***	-0.805***	-0.812***
*post-layoff	(0.043)	(0.043)	(0.046)	(0.036)
R-sq	0.081	0.097	0.101	0.132
Log(opportunities)	-0.776***	-0.776***	-0.790***	-0.798***
*post-layoff	(0.043)	(0.043)	(0.046)	(0.036)
R-sq	0.080	0.097	0.101	0.132
		(B) log(monthly earnings)		
Log(density)	0.784***	0.846***	0.179*	0.154*
*post-layoff	(0.078)	(0.072)	(0.077)	(0.061)
R-sq	0.015	0.148	0.155	0.042
Log(opportunities)	0.794***	0.858***	0.196*	0.156*
*post-layoff	(0.078)	(0.072)	(0.077)	(0.061)
R-sq	0.014	0.148	0.155	0.042
		(C) Monthly earnings ($)		
Log(density)	-39.88***	-39.19***	-74.09***	-76.04***
*post-layoff	(4.77)	(4.60)	(4.92)	(4.01)
R-sq	0.055	0.119	0.126	0.095
Log(opportunities)	-33.39***	-32.70***	-71.46***	-73.40***
*post-layoff	(4.77)	(4.61)	(4.92)	(4.02)
R-sq	0.054	0.119	0.126	0.095

Notes: The outcome variables are as follows: in panel (A) is an employment-indicator variable (scaled 0-100) for whether a worker has any monthly earnings; in panel (B), the log(monthly earnings) (x100) for employed workers; and panel (C), the monthly earnings for all workers (including zeros for those not employed). Each regression in panel (A) and (C) is based on 1,392,396 monthly observations for 19,236 workers experiencing involuntary layoffs; regressions in panel (B) are based on 1,149,891 monthly observations on employed workers. Controls include indicator variables for Overseas born, Female, ethnicity (Māori only, Pacific only, Asian only, European and Māori, and miscellaneous ethnicity responses), highest qualification (low, medium and high), functional urban areas (FUA), and whether the layoff-job length of tenure is censored (i.e. job started before April 1999); and quadratics in age and layoff-job tenure.

Table 7: Employed in different FUA, different industry, or similar job

	Controls	Post*controls	Worker FE
(A) Employed in different FUA from layoff			
Log(density)	-0.647***	-0.572***	-0.562***
*post-layoff	(0.031)	(0.033)	(0.026)
R-sq	0.056	0.058	0.037
Log(opportunities)	-0.654***	-0.579***	-0.569***
*post-layoff	(0.031)	(0.033)	(0.026)
R-sq	0.056	0.058	0.037
(B) Employed in different industry from layoff-job			
Log(density)	0.0284	0.502***	0.552***
*post-layoff	(0.050)	(0.054)	(0.040)
R-sq	0.331	0.334	0.470
Log(opportunities)	0.130**	0.618***	0.661***
*post-layoff	(0.050)	(0.054)	(0.040)
R-sq	0.331	0.334	0.470
(C) Similarity of current-job to layoff-job			
Log(density)	0.349***	0.147***	0.188***
*post-layoff	(0.021)	(0.022)	(0.015)
R-sq	0.251	0.253	0.409
Log(opportunities)	0.404***	0.200***	0.243***
*post-layoff	(0.021)	(0.022)	(0.015)
R-sq	0.251	0.254	0.409

Notes: The outcome variables are as follows: in panel (A) is an indicator variable for whether a worker was employed in a different area (scaled 0-100); in panel (B), is an indicator variable for whether a worker was employed in a different industry (scaled 0-100); and panel (C), is the measure of how similar a worker's current job is compared to their layoff job. Each regression in panels (A) and (B) is based on 1,149,891 monthly observations, and regressions in panel (C) are based on 980,055 monthly observations on employed workers. Controls include indicator variables for Overseas born, Female, ethnicity (Māori only, Pacific only, Asian only, European and Māori, and miscellaneous ethnicity responses), highest qualification (low, medium and high), functional urban areas (FUA), and whether the layoff-job length of tenure is censored (i.e. job started before April 1999); and quadratics in age and layoff-job tenure.

Table 8: Urban density post-layoff effects on outcomes – subsamples

Sample	Overseas migration	Domestic migration	Layoff- Un duration	Re-employ duration	Employed	Log(monthly earnings)	Monthly earnings	Change emp FUA	Change emp industry	Job similarity
Full	0.213***	0.039	-0.019	-0.005	-0.812***	0.154*	-76.04***	-0.562***	0.552***	0.188***
	(0.049)	(0.035)	(0.014)	(0.020)	(0.036)	(0.061)	(4.01)	(0.026)	(0.040)	(0.015)
Post-GFC	0.212***	0.013	-0.020	-0.013	-0.506***	0.755***	-16.49***	-0.516***	-0.022	0.291***
	(0.060)	(0.041)	(0.017)	(0.024)	(0.042)	(0.070)	(4.73)	(0.031)	(0.048)	(0.018)
Auckland	0.355***	-0.003	-0.026	-0.040	0.182	1.951***	18.14	0.0687	-1.741***	0.156*
	(0.078)	(0.063)	(0.022)	(0.031)	(0.152)	(0.253)	(16.88)	(0.081)	(0.165)	(0.061)
Hamilton &	-0.045	-0.062	0.012	-0.084	2.823***	4.491***	271.1***	0.441	0.334	0.214
Tauranga	(0.326)	(0.242)	(0.104)	(0.149)	(0.545)	(0.894)	(56.40)	(0.464)	(0.610)	(0.230)
Wellington	0.744***	0.228	-0.057	0.181*	-2.934***	-3.128***	-441.4***	-1.100***	-4.648***	0.465***
	(0.208)	(0.142)	(0.051)	(0.077)	(0.339)	(0.565)	(38.84)	(0.218)	(0.359)	(0.132)
Christchurch	0.041	-0.028	0.019	0.009	0.266	2.239***	103.8***	-0.619***	-1.066***	2.106***
	(0.155)	(0.114)	(0.041)	(0.065)	(0.269)	(0.447)	(28.69)	(0.187)	(0.316)	(0.117)
Other NI	-0.057	0.020	0.106	-0.040	2.723***	2.517***	307.3***	2.674***	3.654***	-3.308***
main urban	(0.238)	(0.177)	(0.073)	(0.108)	(0.310)	(0.533)	(32.69)	(0.264)	(0.359)	(0.135)
Other SI	1.095	-0.063	-0.136	0.606**	-4.474***	-4.592***	-256.8***	-2.450***	-3.069***	-0.260
main urban	(0.598)	(0.280)	(0.108)	(0.202)	(0.528)	(0.930)	(60.95)	(0.426)	(0.629)	(0.239)
Other	-1.178**	0.027	0.044	0.127	1.919***	1.703***	227.5***	-1.108***	-4.751***	-0.195
urban	(0.437)	(0.334)	(0.143)	(0.212)	(0.288)	(0.516)	(37.54)	(0.275)	(0.327)	(0.133)
Outside	0.045	0.096	-0.022	-0.020	-0.146	0.454	-69.10***	-0.182	-0.124	-0.457***
urban	(0.092)	(0.055)	(0.026)	(0.036)	(0.172)	(0.298)	(18.09)	(0.165)	(0.190)	(0.077)
Lower-Quartile	0.079	0.048	-0.057**	0.041	-1.636***	-0.613***	-188.2***	-0.606***	2.863***	-0.339***
growth Ind	(0.078)	(0.055)	(0.021)	(0.032)	(0.056)	(0.097)	(6.53)	(0.041)	(0.062)	(0.024)
Inter- Quartile	0.342***	0.074	0.026	-0.016	-0.904***	0.689***	12.44	-0.641***	-1.522***	0.482***
growth Ind	(0.102)	(0.068)	(0.026)	(0.039)	(0.062)	(0.106)	(7.18)	(0.044)	(0.070)	(0.027)
Upper- Quartile	0.268**	-0.026	0.003	-0.048	0.476***	0.493***	-32.85***	-0.479***	0.093	0.449***
growth Ind	(0.084)	(0.064	(0.026)	(0.036)	(0.074)	(0.121)	(7.46)	(0.053)	(0.082)	(0.031)

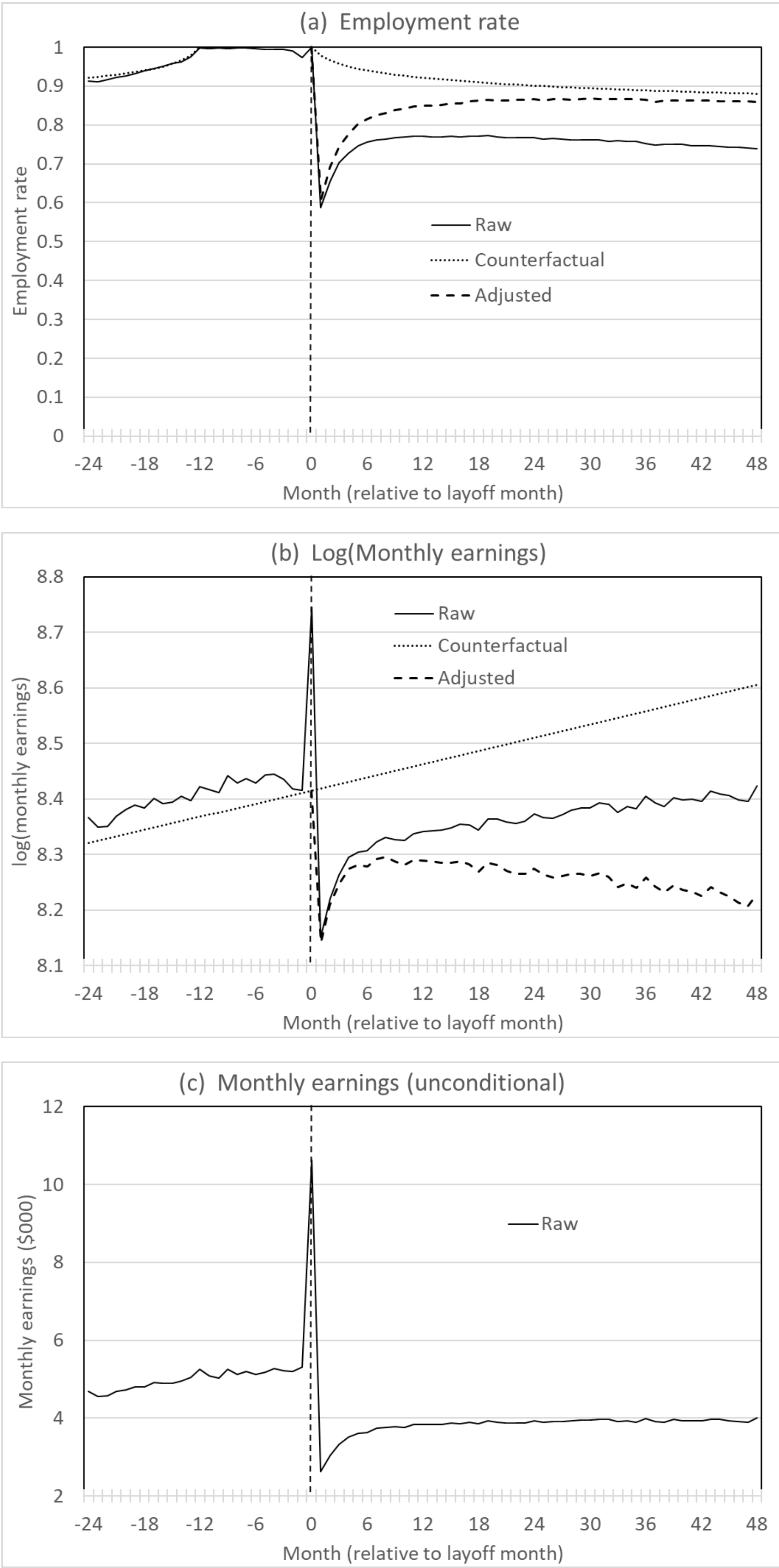
Table continues

Table 8 continued

Sample	Overseas migration	Domestic migration	Layoff-Un duration	Re-employ duration	Employed	Log(monthly earnings)	Monthly earnings	Change emp FUA	Change emp industry	Job similarity
Full	0.213***	0.039	-0.019	-0.005	-0.812***	0.154*	-76.04***	-0.562***	0.552***	0.188***
	(0.049)	(0.035)	(0.014)	(0.020)	(0.036)	(0.061)	(4.01)	(0.026)	(0.040)	(0.015)
Overseas	0.222**	0.032	-0.057*	0.027	-1.489***	-0.390**	-113.8***	-0.617***	1.897***	-0.062
born	(0.080)	(0.078)	(0.029)	(0.045)	(0.076)	(0.126)	(8.25)	(0.050)	(0.085)	(0.032)
Males	0.202***	0.026	-0.020	-0.016	-0.720***	0.124	-45.28***	-0.571***	0.128**	0.504***
	(0.059)	(0.043)	(0.017(	(0.025)	(0.044)	(0.071)	(5.34)	(0.033)	(0.049)	(0.019)
Females	0.237*	0.046	-0.022	0.008	-0.997***	0.302**	-130.7***	-0.517***	1.359***	-0.408***
	(0.092)	(0.062)	(0.024)	(0.034)	(0.063)	(0.112)	(5.79)	(0.042)	(0.069)	(0.026)
Young	0.293***	0.014	-0.013	-0.020	-0.838***	0.402***	-58.19***	-0.952***	1.247***	-0.091***
(<40)	(0.065)	(0.051)	(0.022)	(0.030)	(0.056)	(0.095)	(5.49)	(0.045)	(0.063)	(0.023)
Older	0.093	0.063	-0.016	0.004	-0.683***	0.053	-83.76***	-0.268***	0.044	0.399***
(≥40)	(0.077)	(0.049)	(0.018)	(0.027)	(0.047)	(0.078)	(5.71)	(0.030)	(0.052)	(0.020)
Low	0.143	0.036	-0.004	0.001	-0.534***	0.268**	-74.25***	-0.702***	0.558***	0.119***
quals	(0.076)	(0.052)	(0.020)	(0.029)	(0.052)	(0.088)	(4.63)	(0.036)	(0.058)	(0.021)
Medium	0.159	0.063	-0.0001	-0.060	-0.783***	-0.064	-27.80***	-0.356***	-0.419***	0.405***
quals	(0.085)	(0.061)	(0.025)	(0.035)	(0.062)	(0.101)	(7.11)	(0.047)	(0.070)	(0.028)
High	0.399***	0.037	-0.078	0.085	-1.610***	0.231	-171.9***	-0.524***	2.424***	0.011
quals	(0.108)	(0.080)	(0.033)	(0.049)	(0.084)	(0.145)	(11.48)	(0.062)	(0.091)	(0.035)
European	0.243***	0.023	-0.007	-0.012	-0.475***	0.482***	-52.96***	-0.566***	-0.065	0.362***
	(0.060)	(0.039)	(0.016)	(0.023)	(0.041)	(0.070)	(4.96)	(0.031)	(0.047)	(0.018)
Māori	0.171	0.201*	0.005	-0.047	-0.757***	0.076	-78.70***	-0.004	0.016	0.220***
	(0.122)	(0.080)	(0.033)	(0.043)	(0.084)	(0.141)	(7.60)	(0.064)	(0.090)	(0.034)
Pacifica	-0.076	-0.325	-0.055	0.021	-2.068***	-1.261***	-173.7***	-2.163***	4.363***	-0.196**
	(0.234)	(0.220)	(0.067)	(0.102)	(0.166)	(0.279)	(15.07)	(0.087)	(0.176)	(0.064)
Asian	0.226	-0.050	-0.113*	-0.008	-2.369***	-1.070***	-184.8***	-0.880***	3.653***	-0.762***
	(0.142	(0.142)	(0.050)	(0.079)	(0.114)	(0.185)	(10.98)	(0.074)	(0.124)	(0.047)

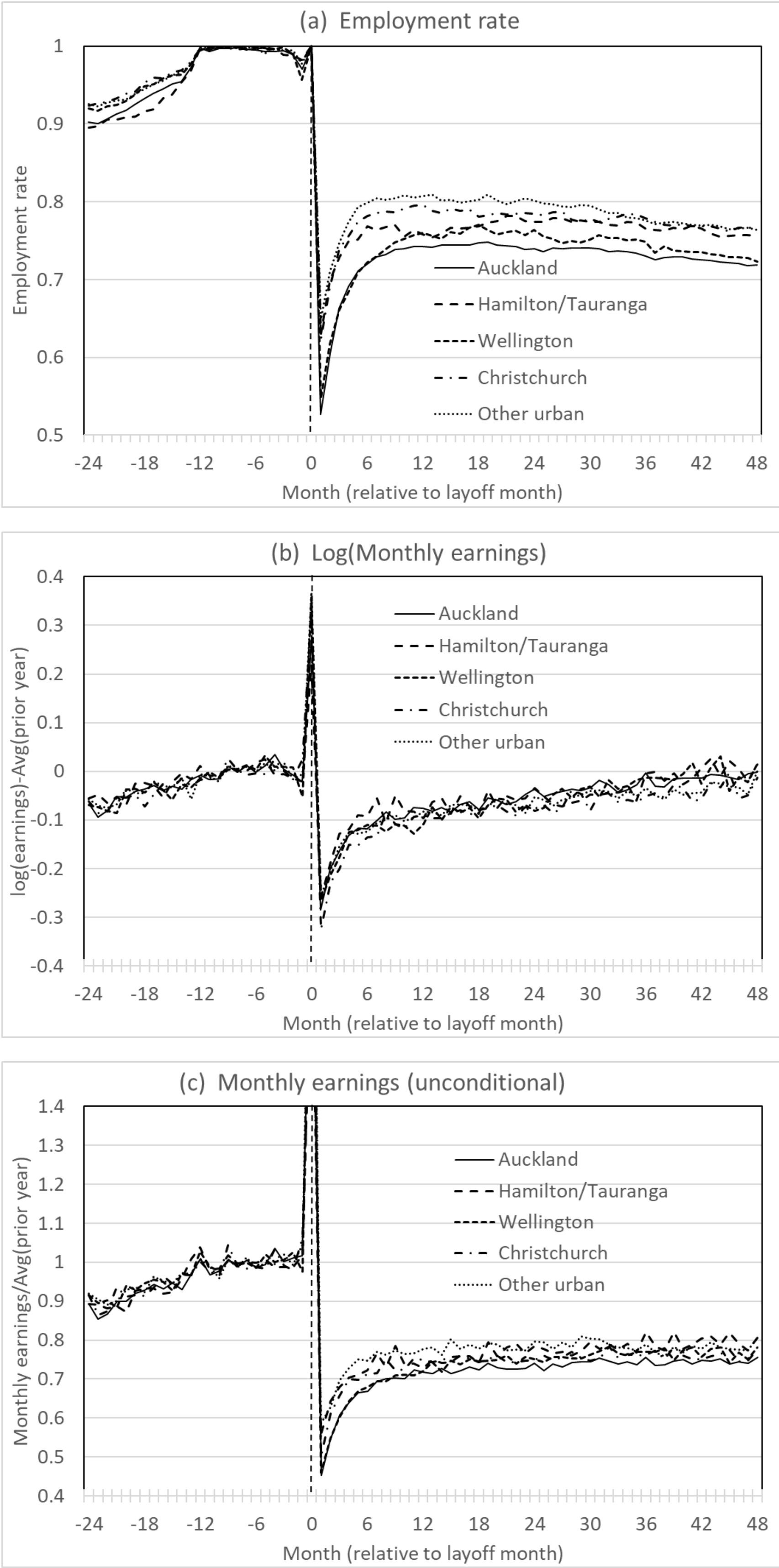
Notes: All reported estimates are the coefficients on log(employment density) based on the most general specification for each outcome, as reported in Table 4 – Table 7: i.e. specifications with controls for the worker-level outcomes, and with controls and worker fixed effects in the worker-month level outcomes.

Figure 1: Post-displacement outcomes and the effect of displacement



Notes: for employment in panel (a) the counterfactual is a symmetric log-linear trend post-displacement, estimated from months -24 to -12; for log(earnings) in panel (b), the counterfactual is a linear trend post-displacement, estimated from months -24 to -2.

Figure 2: Post-displacement outcomes by urban area



Notes: for each area, panel (a) plots the monthly employment rates; panel (b) plots the deviation between average monthly log(earnings) and average log(earnings) over the year prior to layoff (months -12 to -1); and panel (c) plots the ratio of average monthly earnings to average earnings over the year prior to layoff (months -12 to -1).

Appendix

Table A1: Industry mobility conditional on re-employment (relative risk)

	No job	From industry	To industry | job A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S
Agriculture, Forestry and Fishing	0.950	A	6.3	**1.1**	0.8	0.4	0.6	0.4	0.4	0.4	0.6	0.2	0.2	0.8	0.3	0.8	0.3	0.2	0.2	0.6	0.3
Mining	1.000	B	**1.1**	62.1	**1.1**	2.2	2.7	0.9	0.3	0.2	**1.8**	0.2	0.3	0.8	0.6	0.9	0.5	0.2	0.1	0.4	0.6
Manufacturing	0.971	C	0.7	**1.2**	3.3	0.9	0.9	**1.4**	0.7	0.5	0.7	0.5	0.4	0.7	0.6	**1.1**	0.5	0.4	0.3	0.5	0.7
Electricity, Gas, Water and Waste Services	0.960	D	0.4	2.3	0.9	24.9	1.9	**1.0**	0.4	0.2	2.4	0.9	0.7	0.9	0.9	**1.5**	**1.1**	0.4	0.3	0.4	0.5
Construction	0.917	E	0.5	2.7	0.8	1.8	5.6	0.6	0.4	0.2	0.8	0.3	0.3	0.9	0.6	0.9	0.6	0.3	0.1	0.5	0.5
Wholesale Trade	0.947	F	0.4	1.0	**1.4**	**1.1**	0.7	4.5	**1.2**	0.3	**1.0**	**1.2**	0.8	**1.0**	1.0	1.0	0.6	0.5	0.3	0.6	**1.1**
Retail Trade	0.981	G	0.3	0.4	0.7	0.5	0.4	**1.3**	3.9	0.8	0.5	**1.1**	1.0	1.0	0.7	0.7	0.7	0.6	0.6	0.9	**1.0**
Accommodation and Food Services	1.018	H	0.4	0.2	0.5	0.2	0.2	0.4	**1.0**	4.8	0.3	0.7	0.5	0.8	0.4	0.7	0.5	0.6	0.5	**1.4**	0.5
Transport, Postal and Warehousing	0.900	I	0.6	**1.7**	0.7	2.3	0.8	0.9	0.5	0.2	9.8	0.3	0.4	0.8	0.4	**1.0**	0.6	0.3	0.2	0.5	0.5
Information Media and Telecommunications	1.129	J	0.2	0.2	0.6	0.8	0.4	**1.2**	0.8	0.5	0.4	17.7	2.1	**1.1**	2.6	**1.1**	**1.6**	**1.2**	0.5	**1.4**	0.7
Financial and Insurance Services	1.053	K	0.2	0.4	0.4	0.8	0.3	0.8	0.7	0.3	0.4	**1.7**	21.0	**1.7**	1.8	**1.0**	**1.5**	0.8	0.5	0.9	0.8
Rental, Hiring and Real Estate Services	1.019	L	0.8	0.7	0.6	0.8	0.9	0.9	0.9	0.7	0.8	**1.2**	**1.7**	10.5	**1.1**	1.0	0.8	0.7	0.5	**1.1**	0.9
Professional, Scientific and Technical Services	1.007	M	0.2	0.8	0.6	0.9	0.5	0.9	0.5	0.3	0.3	2.6	1.9	**1.1**	6.1	0.8	**1.6**	**1.1**	0.5	0.8	0.6
Administrative and Support Services	1.007	N	0.6	0.9	**1.3**	1.8	1.0	**1.1**	0.6	0.6	**1.1**	**1.2**	**1.2**	0.9	0.8	2.4	**1.3**	0.7	0.7	0.8	0.5
Public Administration and Safety	1.055	O	0.3	0.6	0.4	**1.3**	0.6	0.5	0.5	0.3	0.6	**1.2**	**1.4**	0.8	**1.7**	0.9	8.6	**1.5**	0.8	**1.8**	0.9
Education and Training	1.152	P	0.2	0.3	0.4	0.4	0.5	0.4	0.4	0.4	0.3	**1.2**	0.8	0.6	**1.3**	0.7	**1.6**	10.1	2.0	**1.5**	**1.1**
Health Care and Social Assistance	1.078	Q	0.2	0.2	0.2	0.2	0.1	0.3	0.4	0.3	0.2	0.4	0.5	0.5	0.5	0.6	0.8	2.3	7.6	0.6	0.8
Arts and Recreation Services	1.141	R	0.6	0.4	0.5	0.5	0.5	0.6	0.8	**1.3**	0.6	**1.2**	0.8	**1.1**	0.9	0.9	1.9	**1.5**	0.7	11.2	0.8
Other Services	0.980	S	0.3	0.7	0.7	0.6	0.5	**1.0**	**1.0**	0.4	0.6	0.7	0.9	0.9	0.6	0.6	0.9	**1.1**	0.8	0.8	9.5
Share of employment	59.4%*		9.1%	0.2%	10.9%	0.6%	9.1%	5.3%	9.7%	10.8%	4.6%	0.9%	1.5%	1.8%	6.8%	8.7%	3.6%	3.0%	7.9%	1.7%	3.8%

Note: Table entries are relative risk ratios (RRR) based on all industry transitions. This is the probability of moving to the (column) industry, relative to the probability of moving to that industry if a job were chosen randomly. The 'No job' column shows the relative risk of non-employment. Shaded cells indicate RRR>1. Shading in pink indicates the highest 10% of RRRs. The final row shows industry shares of destination jobs. *The 'No job' column entry in the final row is as a percentage of all origin jobs.

Figure A1: Graphical summary of the patterns of outcomes for displaced workers

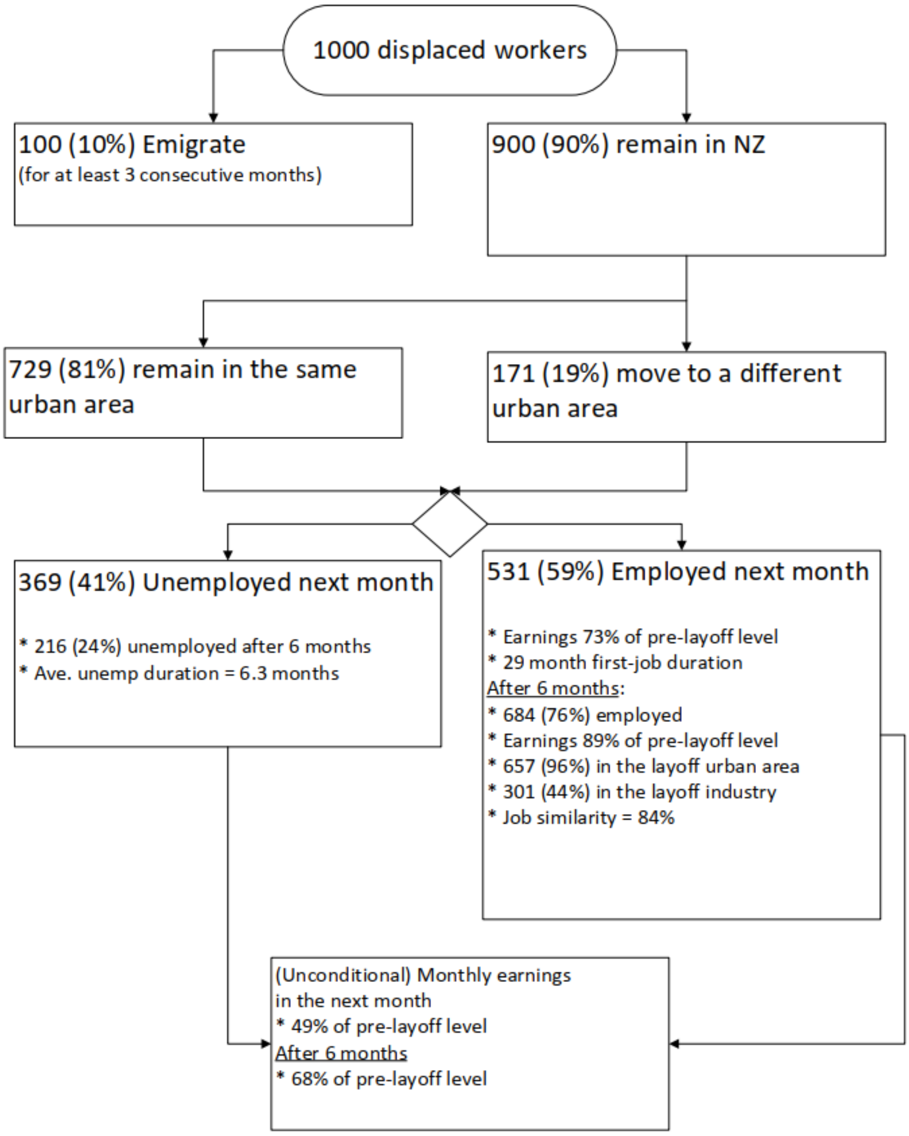
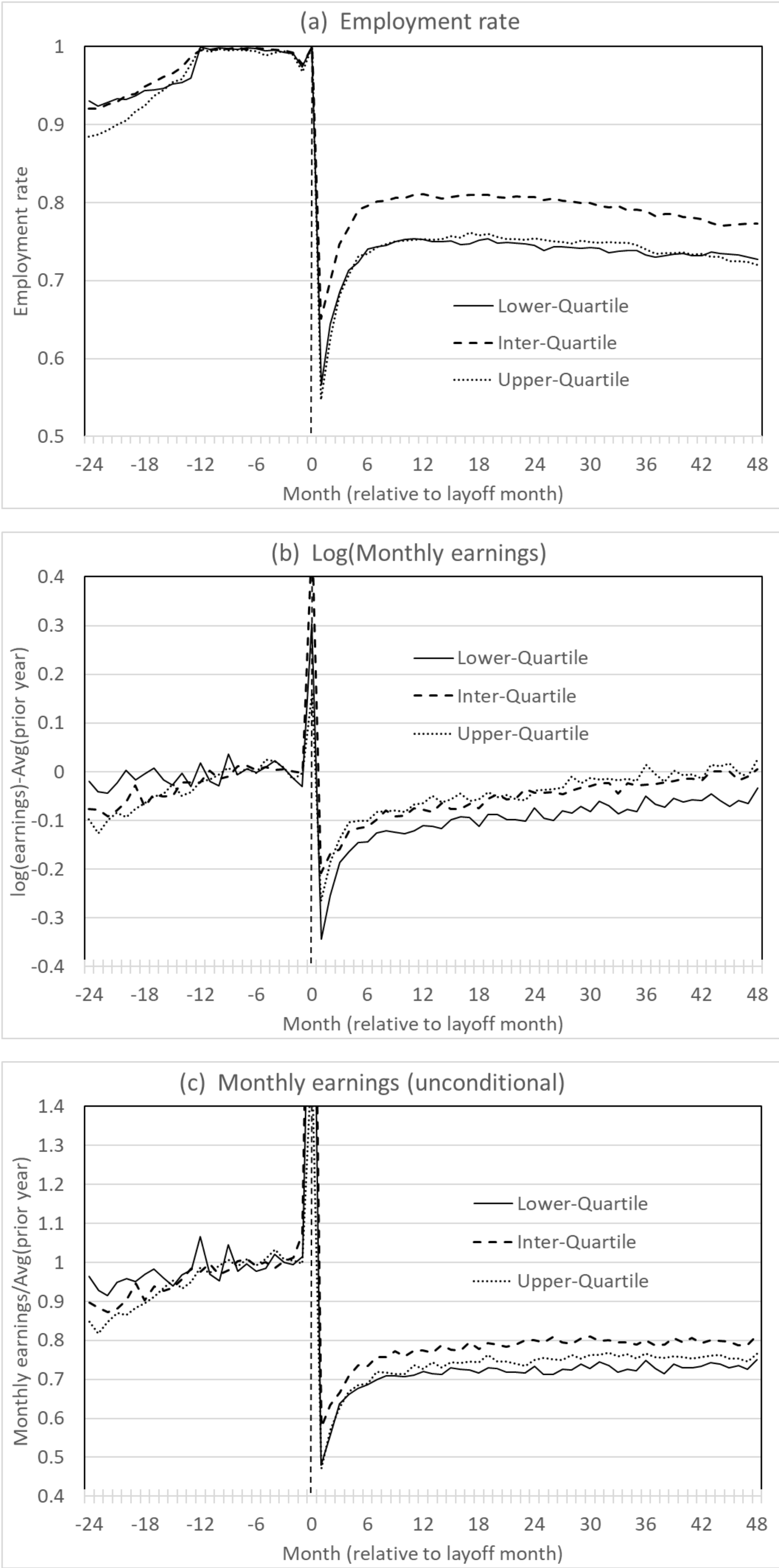
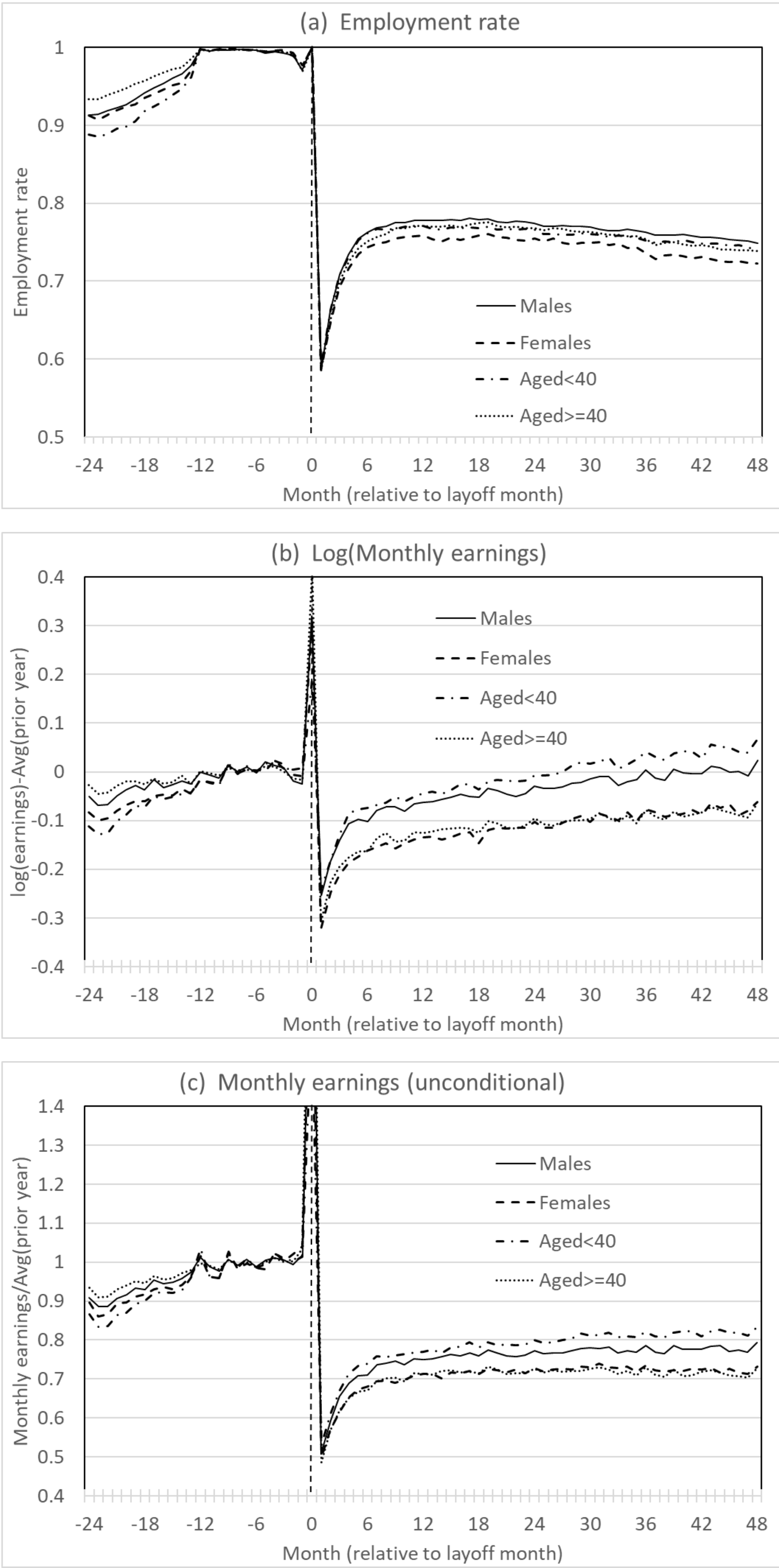


Figure A2: Post-displacement outcomes by industry growth (quartile)



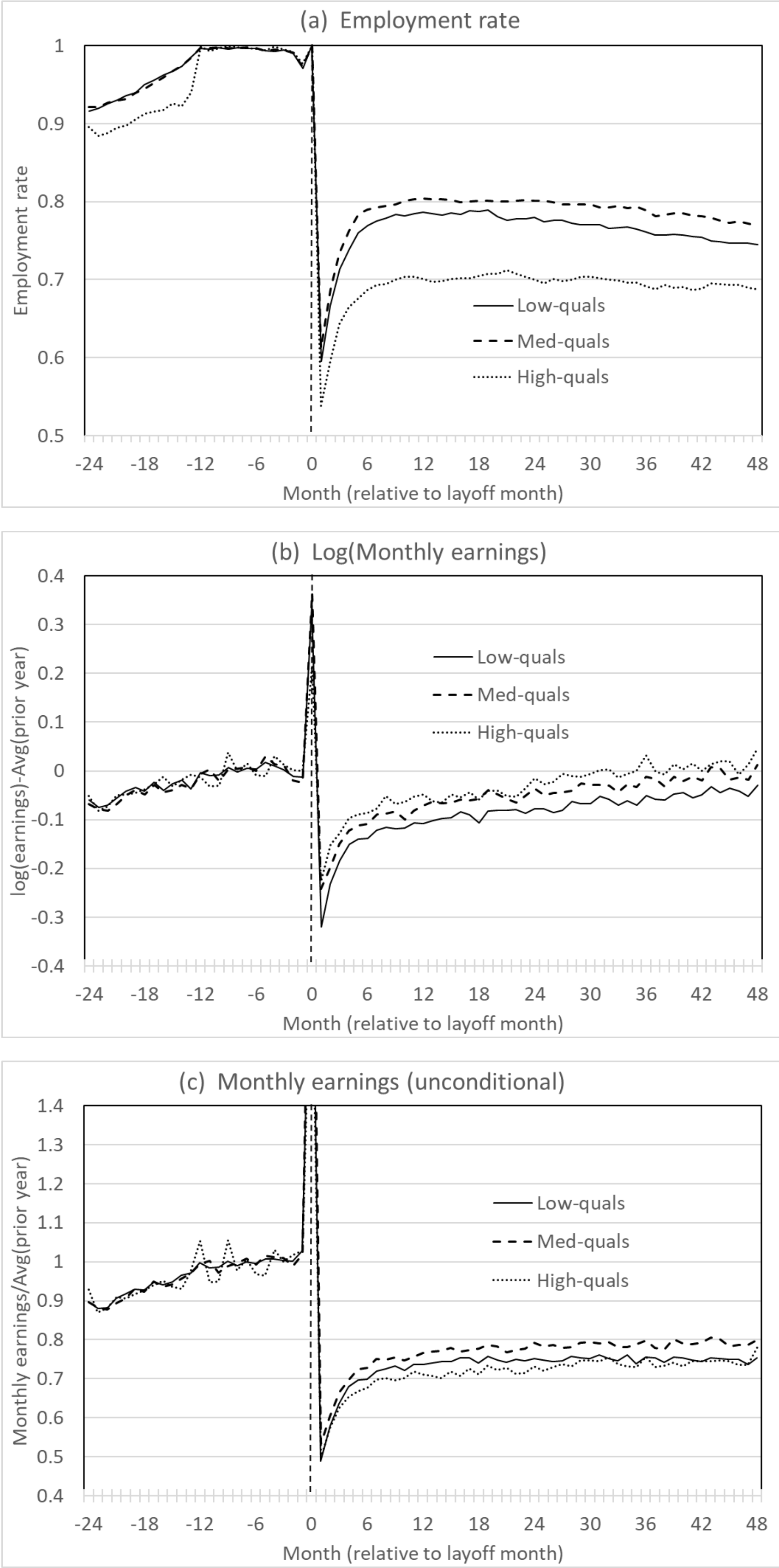
Notes: see notes to Figure 2.

Figure A3: Post-displacement outcomes by Sex and Age



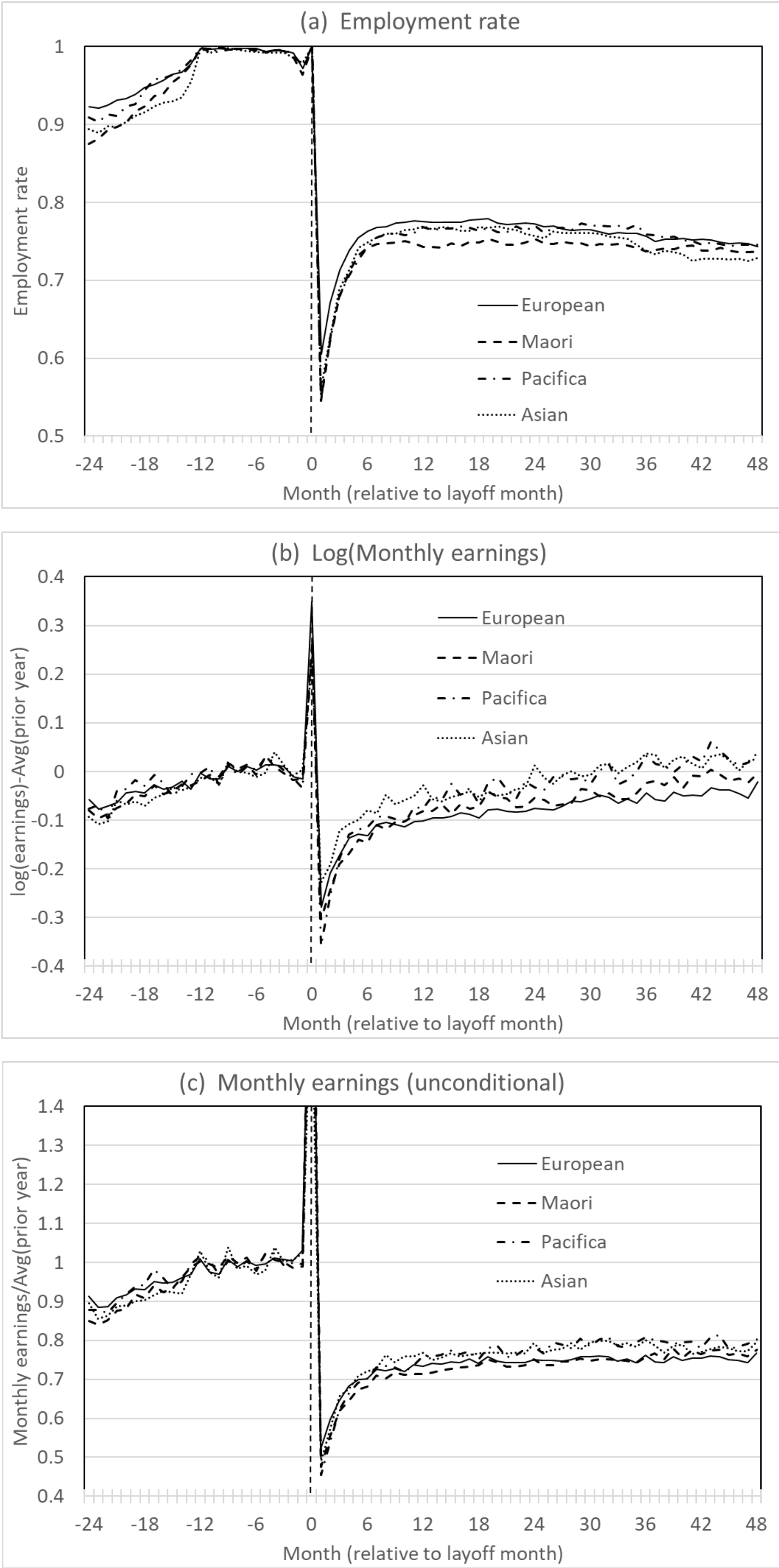
Notes: see notes to Figure 2.

Figure A4: Post-displacement outcomes by qualifications



Notes: see notes to Figure 2. Qualifications are grouped as follows: Low includes no qualifications and school level qualifications (level 0-3); Medium includes post-school qualifications (level 4-6); and High includes Bachelor degrees and above (level 7+).

Figure A5: Post-displacement outcomes by ethnicity



Notes: see notes to Figure 2. Ethnic groupings are based on total ethnicity responses, so are not mutually exclusive subgroups.