The Impact of Firm Closure on Workers' Future Labour Market Outcomes

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Access to the data used in this study was provided by Statistics NZ under conditions designed to give effect to the security and confidentiality provisions of the Statistics Act 1975. Only people authorised by the Statistics Act 1975 are allowed to see data about a particular person or firm. The tables in this paper contain information about groups of people so that the confidentiality of individuals is protected.

The results are based in part on tax data supplied by Inland Revenue to Statistics NZ under the Tax Administration Act. These tax data must be used only for statistical purposes, and no individual information is published or disclosed in any other form, or provided back to Inland Revenue for administrative or regulatory purposes. Any discussion of data limitations or weaknesses is in the context of using the Linked Employer-Employee Dataset (LEED) for statistical purposes, and is not related to the ability of the data to support Inland Revenue's core operational requirements. Careful consideration has been given to the privacy, security and confidentiality issues associated with using tax data in this project. Any person who had access to the unit record data has certified that they have been shown, have read and have understood Section 81 (Privacy and Confidentiality) of the Tax Administration Act. A full discussion can be found in the LEED Project Privacy Impact Assessment paper, available on the Statistics NZ Website.

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Abstract

This paper examines the impact of job loss due to firm closure on workers' employment, earnings, and benefit receipt. It uses data from Statistics New Zealand's Linked Employer-Employee Data (LEED). LEED is an administrative dataset in which the exit of a firm identifier does not always represent a complete firm closure. After selecting all continuing establishments with at least five employees that ceased to operate in LEED between April 2001 and March 2004, information on the flows of clusters of workers from these establishments to other firms was used to classify them according to the likelihood that a complete closure took place. Other exits were classified as partial closures or restructurings.

The impact of job displacement was estimated by comparing the changes in labour market outcomes for workers who experienced a closure or restructuring, with the changes in outcomes for a control group of workers who were employed at firms that did not experience these events. We allow the estimated impacts of displacement to vary according to the type of event experienced. We also assess whether the effects of job displacement differ for workers with different personal and job characteristics, and whether there are identifiable impacts on people who worked at affected firms in the year prior to the closure or restructuring.

We find that job loss due to firm closure has persistent impacts on the subsample of workers who were most likely to have experienced a complete firm closure. The employment rate for these workers is 17 percent lower one year after the firm closed than those for comparable workers at non-closing firms, and remain 12 percent lower four years after the closure. Similarly, monthly earnings are 22 percent lower one year after the closure and 16 percent lower four years after the closure. The benefit receipt rate is 45 percent higher one year after the closure, although this increase is from a very low base. We find smaller but still significant negative impacts for workers whose firm event was classified as a partial closure. Displacement has relatively little impact, on average, on the outcomes of workers who were most likely to have experienced a firm restructuring.

Employees at small and medium-sized establishments that closed experienced greater employment and earnings losses than those at closing firms with 50 or more employees. Employees with at least two years of job tenure also sustained larger losses than those with shorter tenure. Although overseas studies have found that mature workers in the 55 years and over age bracket tend to be harder hit by redundancy than prime-age workers, in this study the impacts on prime-aged and older employees were of a similar magnitude, while larger impacts were found for younger employees.

This is a revised version of a paper first published in 2008 (Dixon and Stillman, 2008). The original paper has been extended to include analyses of the impact of job displacement on benefit receipt rates, and the variation in displacement impacts by geographical location. The possible effects of external migration and transitions to self-employment on our main estimates of the employment impacts of displacement, are also considered in a new section of the paper.

Keywords

Job displacement, employment and earnings losses, New Zealand.

Contents

1. Introduction 5
2. Data and sample creation 7
The Linked Employer-Employee Data 7
Identifying firm closures 8
Defining the analysis samples of displaced workers 10
Defining the control samples of non-displaced workers 11
3. Sample characteristics: closing or restructuring firms and affected workers 12
Firm characteristics 12
Worker characteristics 13
4. Results 15
Event study of the effect of displacement on labour market outcomes 15
Regression model of the impact of displacement 16
Main estimates of the impact of displacement on employment and earnings 19
Variations in the impact of displacement 22
Impacts on workers who left dying firms prior to closure 25
5. Summary and conclusions 28
References 31
Appendix 1 59
How firms are identified and tracked in the Business Frame, the Longitudinal Business Frame, and LEED 59
Appendix 2 62
Additional tables 62

1. Introduction

Restructuring is a natural part of the life cycle of businesses as they innovate, expand or contract, respond to changes in market conditions or technologies, or change the business model under which they operate. On a macroeconomic level, the restructuring or closure of businesses is part of the process whereby less productive economic activities are replaced by more productive ones. Although restructuring often leads to benefits for the economy as a whole, and for particular firms and workers, it can also impose costs on employees who lose their jobs.

This paper uses data from Statistics New Zealand's Linked Employer-Employee Data (LEED) to examine the impact of involuntary job loss (or 'displacement') caused by the closure or restructuring of firms on the employment, earnings, and benefit receipt rates of affected workers. LEED is an administrative dataset that contains monthly earnings data for every employer-employee match in New Zealand from April 1999 to the present. The study focuses on workers who were employed at firms that ceased to operate in LEED between April 2001 and March 2004. It estimates the impact of the closure or restructuring event on the outcomes of the affected workers during the following four years. This is the first study to examine the impacts of involuntary job loss on New Zealand workers in a microeconomic framework.

The international literature on displacement indicates that involuntary job loss can have significant effects on individual and household wellbeing over an extended period (Browning and Crossley, 2004; Stephens, 2001; Ruhm, 1991; Stephens and Charles, 2004; Jacobson et al, 1993). Kletzer (1998) surveys the job displacement literature and reports that annual labour earnings typically fall by 25 percent to 40 percent in the year of displacement. A number of researchers have found that while post-displacement unemployment declines over time, the wages and earnings of displaced workers are both still significantly below expected levels up to 10 years after a job loss (Ruhm, 1991; Stevens, 1997; Eliason and Storrie, 2006). The potential long-run impact of economic shocks is further illustrated in Stephens (2001), which finds that household consumption is significantly reduced even 10 years following the job displacement of the main income earner.

While three recent papers used matched employer-employee administrative data to examine the impact of involuntary job loss on workers' future outcomes (Eliason and Storrie, 2006; Carneiro and Portugal, 2006; Huttunen et al, 2006), the majority of previous studies have relied on survey data. Both types of data have strengths and weaknesses (Kletzer, 1998). In surveys, job displacement is typically self-reported by workers and therefore can be inaccurately measured or inconsistently defined, and a relevant comparison group to measure impacts against is often lacking. Surveys also typically only capture a small number of displaced workers, making it difficult to examine whether displacement has heterogeneous effects. Studies using administrative data typically have access to data for the entire population and are better able to construct comparison groups, using the records of workers who did not experience a job displacement. However, accurately identifying firm closures can be a challenge, as administrative databases often do not contain much information on the nature of business restructuring events, and firm identifiers in these databases can change for administrative as well as real world reasons.

This study aims to overcome some of the weaknesses in previous papers. First, we examine the flows of workers from firms that apparently closed in LEED to other firms during the closing firms' final months of existence. We look for evidence of groups of employees transferring directly to other establishments, suggesting that a partial

closure, ownership change, or restructuring may have occurred, rather than a complete close-down in which all employees were made redundant and dispersed. We use this information to classify our sample of firms and close-down events into several groups, according to whether they were likely to be complete closures, closures with some re-employment, or some other type of business reorganisation. We then compare the effects of job displacement across these different categories of events.

Second, the impact of displacement on future employment and earnings is measured by comparing the changes in outcomes experienced by workers employed at firms that closed or restructured, with the changes in outcomes experienced by a 'control' group of workers who were employed in the same month at continuing firms. Comparing post-displacement outcomes with those of a properly-defined comparison group is vital because the employment rates of any group of individuals who are all currently working will naturally be lower in the future. This point is missed in studies that estimate impact models on just the group of displaced workers, or on these workers pooled with a sample of other workers who are not matched at the displacement month.

Third, because we have a fairly large sample of displaced workers, we can examine whether impacts differ for workers with different characteristics, such as length of tenure, age, or gender, or for workers at different types of firms. Workers with longer tenures have more firm-specific human capital and therefore displacement is likely to have a greater impact on them. Previous research has found that displacement has larger impacts on older workers, with many choosing to retire rather than find a new job (Chan and Stevens, 2002).

Our main results focus on the impact of displacement on workers who were employed at closing or restructuring firms in their final month of operation. The advantage of focusing on these workers is that they are most likely to be involuntary job leavers, who lost their job regardless of their individual characteristics and motivations. It is more difficult to tell whether employees who left closing firms in the months leading up to the closure left voluntarily or were made redundant. If employees are aware that their firm is struggling, the more motivated or skilled individuals might leave before the firm closes. Alternatively, firms that are struggling may stagger redundancies and lay off less valuable workers at an earlier stage than more valuable ones. In either case, early departure is a selective process, and workers who leave prior to the firm closing may differ in their circumstances and attributes from workers who remain until the final month of operation, influencing estimates of the impact of displacement. In a separate analysis, we examine whether firm closures and restructurings have impacts on individuals who worked at these firms in the year prior to the event, but left before the final month.

Section 2 describes the data used in this study and the construction of the firm and worker samples. Section 3 describes the characteristics of the firms and workers in the analytical samples. Our main results on the impacts of job displacement are presented in section 4. Section 5 concludes.

We find that displacement had persistent impacts on workers employed at the firms that were most likely to have experienced a complete closure. We estimate that the employment rate of these workers was 17 percent lower, on average, one year after the firm closed than that of comparable workers at non-closing firms, and remained 12 percent lower four years after the closure event. Their monthly earnings were 22 percent lower one year after the closure, and 16 percent lower four years after the closure. In addition, their benefit income receipt rate was 45 percent higher one year after the closure than the benefit receipt rate of comparable workers at non-closing

firms. However, this group comprised only 9 percent of the entire sample of workers affected by the closure and restructuring events in our initial sample of establishment exits from LEED. We find smaller but still significant average impacts for workers whose firm event was classified as a partial closure (an additional 3 percent of workers). Displacement had relatively little impact, on average, on the outcomes of workers who were more likely to have experienced a firm restructuring. Within the subgroup of workers who were most likely to have experienced a complete firm closure, we also find evidence of substantial variations in impacts by age, tenure, level of earnings, and size of firm.

2. Data and sample creation

The Linked Employer-Employee Data

The analysis in this paper uses Statistics New Zealand's Linked Employer-Employee Data (LEED), which utilises information from tax and statistical sources to construct a record of all paid jobs. Since April 1999, all employers in New Zealand have been required to file a monthly record with Inland Revenue called an employer monthly schedule (EMS). This lists all paid employees at that firm during the month, the earnings they received during the month, and the amount of pay-as-you-earn (PAYE) tax that was deducted at source. This paper uses data covering the 96 months from April 1999 to March 2007.

Employees are identified by unique confidentialised identifiers that are consistent over time, enabling workers to be tracked longitudinally and across the firms for which they work. LEED's administrative records contain some basic demographic information on individuals, including sex, age, and location of residence. The core data in LEED can be used to create a number of additional variables, such as the number of jobs held by each employee in a particular month, and each individual's pattern of employment over the 96-month period.

LEED contains information on the identities of the establishment and enterprise that each employee worked for in each month. Information on the attributes of firms, including their industry and geographical location, is also available. Using the employer-employee links in LEED, a variety of other firm-level variables can be derived, such as the firm's payroll, size, and mean earnings per employee in each month.

Income support payments (ie benefits) paid by the government are also recorded in LEED. LEED has information on all of the main income-tested working-age benefits, such as the Unemployment Benefit, Sickness Benefit, Invalid's Benefit, Domestic Purposes Benefit, Widow's Benefit, Emergency Benefit, Independent Youth Benefit, and Transitional Retirement Benefit.

One weakness of LEED is that there is no information on hours worked. As a result, we cannot accurately distinguish impacts of displacement on hourly wages from impacts on the number of hours worked per month, for individuals who remain employed. Another limitation is that there is no information on external migration or any other form of attrition. In our analysis, individuals who leave the country or die are classified as non-employed. However, we also consider whether it is likely that job displacement leads to increased attrition from our sample.

Identifying firm closures

The unit of analysis when identifying firm closures is the establishment or geographical business unit, identified in LEED by a permanent business number (PBN).[1] A PBN is an establishment number that has undergone a range of checks and repair processes, which are designed to ensure that establishments are tracked longitudinally and a consistent ID is maintained through time, for each unique establishment. Appendix 1 gives details of how this is done.

We began by identifying all PBNs that permanently ceased to employ workers in LEED during a three-year period from April 2001 to March 2004, which we refer to as 'firms that exited from LEED'. In a business demography sense, these firms 'died'. This selection period provides at least two years of employment history before the firm closed or restructured for all workers at firms in the sample, and at least three years of post-closure data on outcomes.

We restricted our analysis to PBNs that employed staff every month for at least two years prior to the firm's exit from LEED and had at least five employees in every month of this window, except for the six months prior to the closure or restructuring. The continuity restrictions were imposed to focus the analysis on 'permanent' firms, as opposed to firms with shorter and in some cases temporary employment histories. We did not impose a size restriction in the six months prior to the closure or restructuring (besides the requirement of employing someone), in order to allow establishments to decline gradually in size over this period. In March 2004, the population of firms that met these size and continuity criteria employed 75 percent of all employees.

We identified approximately 3,570 firms meeting the size and continuity restrictions that exited from LEED (ie, they permanently ceased to employ staff) between April 2001 and March 2004.[2] Approximately 61,430 establishments met the continuity and size conditions at some time between April 2001 and March 2004, indicating that around 5.8 percent of these continuing firms ceased to employ workers in LEED during our selection period.

In an economic sense, a business operation closes down if it ceases to produce goods or services and its factors of production are dispersed. Establishments in LEED were initially identified as having ceased in a particular month if the firm's PBN was not associated with any employees in any future month. However, the establishment-number repair rules that are implemented by Statistics NZ in the construction of LEED are intended to link establishments longitudinally only when there is a reasonably high level of certainty that the establishments being linked are the same. When a merger, acquisition, ownership change, outsourcing of functions, or relocation occurs, a new PBN number may be assigned because the continuity rules have not been met. This means that PBNs may cease to be used in LEED in situations that do not necessarily result in job loss for the employees who are involved. For this reason, we used additional information to better distinguish firm exits involving complete close-downs from other types of business change.

[1] We focus on establishment closures partly because they are likely to be more common than enterprise closures, and partly because Statistics NZ has developed a longitudinal establishment identifier (the PBN). For reasons that are explained in appendix 1, there are likely to be fewer 'false' establishment deaths recorded in LEED than 'false' enterprise deaths.

[2] All sample sizes given in this paper were randomly rounded for confidentiality reasons.

First, we examined whether groups of employees who were working for the firm in the six months leading to its exit stopped working at the firm in a particular month, and immediately started working at another firm.[3] The transfer of a substantial group of employees directly to another firm suggests that some sort of re-assignment of staff has occurred, as might take place in a business restructuring or ownership change where the new owner retains staff. Establishment-level group transfers were defined as situations where a minimum of three employees in an establishment with less than 20 employees, five employees in an establishment with 20 to 49 employees, and 10 percent of employees in an establishment with 50 or more employees, transferred as a group to another establishment in the same or the next month. Enterprise-level group transfers were defined as situations where a minimum of five employees in an enterprise with 20 or more employees, and a minimum of 10 percent of employees in an enterprise with 50 or more employees, transferred as a group to another enterprise in the same or the next month.

Second, we distinguish between exiting firms in which both the establishment and its enterprise ceased in the same month, and those belonging to multiple-establishment enterprises (multis) whose parent enterprise continued to operate after the firm closed. Branch closures within ongoing enterprises cause special issues for the identification of genuine closures in LEED because the true branch location of each employee within a multiple-branch enterprise is not recorded in tax returns. Statistics NZ allocates the employees of multis to their branches using information on the expected employment level of each branch and the home addresses of the employees. There is potential for error in this probabilistic allocation process, which means that some of the employees who were associated with a branch close-down in the LEED dataset may have actually worked elsewhere, and not experienced a job loss.

Using this information the establishment exits recorded in LEED were classified into four groups:[4]

(1) Firm closures with no group transfers. Both the establishment and enterprise identifiers ceased to operate in the same month, and there were no transfers of groups of employees to other establishment or enterprise numbers during the final six months. Most of these firms were single-unit entities in which the enterprise and establishment were one and the same. A few were 'multis' in which all units ceased at the same time.

(2) Firm closures with some employee group transfers. These differ from the first group only in that up to one-third of the employees were transferred directly to jobs elsewhere. This group may include: ownership changes in which only a minority of employees were re-employed; closures in which a fraction of workers were immediately re-employed by the new owner or as a group by another firm; and other types of restructuring.

(3) Branch closures: closures of establishments that were part of multi-unit enterprises, in which other parts of the enterprise kept operating. This group excludes branch closures in which group transfers were recorded.

[3] The strategy of analysing worker flows to distinguish between genuine firm closures and other types of change, including changes in administrative identifiers, was developed by researchers in the US Longitudinal Employer-Household Dynamics Program (HEHD) (see, for example Benedetto *et al*, 2004).

[4] This classification is a provisional one, based on the limited evidence available. We cannot confirm that the events we classified as closures or probable restructurings actually were.

(4) Probable restructurings. This group includes all other PBN exits that occurred in LEED during the study period.

These four groups of closures and restructurings differ in the nature of the employment situation faced by the workers. Workers in the first closure group are most likely to have lost their jobs without the option of immediate re-employment elsewhere. (That is, no other firm offered employment to a group of them.) In the second closure group, a closure may have occurred, but a group of the firm's employees were either not made redundant or were immediately re-employed under a different firm identity. In the branch closure group, the affected employees could have been offered jobs at other branches of the enterprise.[5] The 'restructuring' group is likely to include workers who faced a range of different employment situations: redundancy; technical redundancy (in which there is a change in the employer-employee relationship but workers are reconfirmed in their positions); redeployment to a new position in a restructured organisation; or no change in their employment relationship or position. We do not attempt to divide the workers in this broad final group into 'displaced' and 'not-displaced' categories because this outcome is likely to be endogenously determined.[6]

Note that our total sample of firms that ceased in LEED does not include all types of restructuring. Most notably, firms that partially downsized but did not change their firm identifier will not be included.

Defining the analysis samples of displaced workers

Our main sample of displaced workers is defined as all individuals who were working at one of the exiting firms in the month that it closed or restructured, were aged 25 to 64 in that month and had at least two months of job tenure at the firm. The age range is chosen to exclude individuals still enrolled in educational institutions, who quite often work in seasonal jobs. Because LEED only measures the receipt of earnings by calendar month, it is not possible to identify the date that a firm ceased within a month, therefore it is useful to restrict the sample to individuals who were also working at the firm in the month prior to the closure or restructuring. We identified approximately 39,650 workers who met the age and tenure restrictions.

A number of individuals were displaced more than once in the sample period. In these cases, we include each event that meets our sample restrictions as a separate observation, and therefore we examine outcomes after the termination of particular jobs.[7]

To investigate the impacts of closure on workers who leave a closing firm before its closure, we also created a secondary sample comprising the individuals who were working at an establishment that exited during our selection period, but left this firm 1–12 months prior to the month that it exited. In order to have at least two years of

[5] In addition, the employees may have been inaccurately assigned to a particular branch in the construction of LEED.

[6] That is, in a restructuring where only some workers are selected for job loss, the attributes of the workers who lose their jobs are probably not independent of the selection decision, and therefore their outcomes after displacement are not independent of the selection decision.

[7] None of our analyses account for the fact that we have multiple observations for each individual when calculating standard errors. However, since we are examining a full population sample of all individuals, it is not clear whether any clustering correction is needed, and given our large sample sizes, this type of correction will not have a qualitative impact on the results in practice.

employment history for all individuals, we only selected those who worked at an exiting firm until at least April 2001. We also applied the same age and tenure restrictions as for our main sample of displaced workers (that is, workers had to be aged 25 to 64 in the month they left the firm, and had to have at least two months' tenure). Using these criteria, we selected approximately 34,250 workers who left a firm 1–12 months prior to its closure or restructuring from our sample of PBN exits. The characteristics of these workers are measured relative to the month they left the firm, not the month the firm ceased.

Defining the control samples of non-displaced workers

Because workers who experience displacement represent a fixed cohort of individuals who are employed at a particular time, the impact of displacement on future outcomes can only be measured by comparing the observed changes in outcomes for these workers with the changes in outcomes of a 'control' group of non-displaced workers. We took a very simple approach to creating this comparison group. For each month between April 2001 and March 2004, we created a 0.5 percent sample of all firms that met the size and continuity criteria used in selecting the analysis sample of exiting establishments; were alive in that month (called the reference month); and were alive in at least one future month.[8] Pooling these samples resulted in a control sample of approximately 8,010 firm observations.[9]

Our main analysis sample of non-displaced workers includes all individuals who were working at one of these firms in its reference month (ie the month that the firm was sampled), were aged 25 to 64 in that month, and had at least two months' tenure at the firm. Using this process we created a control group sample of approximately 136,990 workers. The characteristics of the control group are defined relative to the reference month, which serves as the counterfactual exit month. For example, tenure is measured only up to the reference month, even if the worker continued in the same job after that month. As with the displaced worker sample, individuals can have multiple observations in the control group sample if their firm was selected in more than one reference month or they moved to another sample firm. We allow these individuals to have multiple records and treat them as separate counterfactual observations.

For our analysis of the impact of displacement on early leavers, we also selected approximately 63,070 individuals who were working at any firm in our control sample until at least April 2001, but left this firm 1–12 months prior to the reference month, and met the same age and tenure restrictions as the other samples. The characteristics of these workers are also measured relative to the month that they left the firm. This sample is used as the control group when we estimate the impact of job displacement on workers who left a firm in the year before it closed or restructured.

[8] We considered using a matching approach to more tightly define the control group as firms with the same characteristics as the firms that ceased, but decided that including individual fixed effects in our regression models was sufficient for controlling for heterogeneity in who is employed at closing firms.

[9] Three percent of control group firms ceased to operate in the period after their selection into the control group. Of these, 35 percent exited in the following six months, 27 percent exited 7–12 months after selection, 29 percent exited 13–24 months after selection and 9 percent exited 25–36 months after selection. We believe the proportion of control group firms that ceased soon after selection for the analysis is likely to be too small to lead to bias in the control group estimates.

Unfortunately, LEED does not contain any information on employees' type of employment contract, which means it is not possible to distinguish workers who were employed on a temporary basis from those that had permanent employment relationships. Both the displaced worker and the control group samples are likely to contain a number of individuals whose jobs were temporary. These employees may not have experienced a redundancy in the conventional sense when their firm closed, since they would have already anticipated the need to move to other work in the near future. Although we cannot separate temporary and permanent employees in the analysis and treat them differently, we assume that they are represented in the study and control group samples in similar proportions.

3. Sample characteristics: closing or restructuring firms and affected workers

The characteristics of firms and workers in the displacement and control group samples are described in this section. To the extent that there are significant differences between the workers who are displaced from their jobs through firm closures and those who are not, estimates of the impact of displacement will need to take those pre-existing differences into account. We also present some indicators of job change that are useful for understanding the differences in the labour market outcomes of workers in the four closure groups that are shown later in the paper.

By identifying how firms and workers that experienced different types of firm closure event differed from firms and workers that did not, we may gain also some insights into the question of which types of employees are most vulnerable to involuntary job loss.

Firm characteristics

Table 1 shows the distribution of event type for our sample of firms that ceased to employ people in LEED between 2002 and 2004, based on the classification criteria described above. Twenty-four percent of the firm exits were classified as 'closures', just under 5 percent were classified as 'closures with transfers', and just over 5 percent were classified as 'branch closures'. The remaining 65.3 percent of the firm exits recorded in LEED were either branch closures in which some employee group transfers were recorded, or firm exits in which both the establishment and enterprise ceased to operate in LEED but more than one-third of employees were transferred in groups to other firms during the final six months of operation. As discussed above, the majority of events in this category are likely to represent ownership changes or business restructurings, rather than complete close-downs in which all employees were dispersed.

Table 2 describes the size, structure, industry and sample year of firms in the control and analysis samples. The majority of firm characteristics are measured six months before the firm closed or restructured, or six months prior to the matching month in the case of firms in the comparison sample. However, whether the firm was part of a multi-establishment enterprise is measured in the closure month (or the matching month) to maintain consistency with how the different closure types are defined.

Firms that exited from LEED were substantially smaller, on average, than firms in the comparison sample. The mean size was 20.3 employees, compared with 28.3 for the comparison sample. Among the firms that exited, 44 percent belonged to an enterprise with multiple establishments (multis), compared with 37 percent of the control group firms. On average, exiting firms were also more likely to be part of smaller enterprises than firms that did not cease. Exiting firms were more likely than continuing firms to be

operating in the agriculture, forestry and fishing, finance, and the business services industries. The firm exits in our sample were fairly evenly spread across the three selection years, 2002 to 2004.

Table 2 also compares the characteristics of firms with different types of business change. There are large differences across these groups. Firms in the 'closure – no transfers' group were about half the size of firms in the other groups, with the exception of branch closures. Ninety-two percent of these firms had less than 20 employees (at six months prior to closure). Only 4.5 percent were part of multis. The firms in this subsample were fairly well spread across industries but most numerous in retail trade, hospitality, and business services.

In contrast, the firms we classified as experiencing a 'probable restructuring' had an average of 24.4 employees. Fifty-seven percent belonged to a multi. Although the industrial distribution of these firms was not radically different from that of the first group, they were less concentrated in the retail trade, hospitality, and business services industries.

Note that we did not attempt to identify all firm closures in the economy, but rather focused on a subset that met certain size and continuity criteria – a sample in which we can identify displaced workers as typically defined. For this reason, and because the profiles of our closure groups may have been influenced by the methods that were used to distinguish closures from other types of business change, we would caution readers from assuming that the profile of 'closing' firms shown in table 2 is typical of all firms that close in the labour market as a whole.

Worker characteristics

Table 3 presents information on the number of workers who were employed at our samples of exiting and control group firms and met the age and tenure restrictions for selection. The first column describes workers who left their firms in the month of the closure, and the second column describes workers who left 1–12 months previously. The sample of workers who left exiting firms in the month of closure comprises about 3,500 people from firms with complete closures (ie those in the 'closure – no transfers' group), 1,100 from firms with closures involving some transfers, 780 from branch closures, and 34,300 from firms that experienced some other type of business change. The sample of workers who left closing firms during the year leading to the closure comprises about 5,300 people from the complete closures group, 2,700 from the closures with transfers group, 800 from branch closures, and 25,400 from firms with other events.

Table 4 summarises the characteristics of the workers who were employed at exiting firms until their last month. The characteristics of workers in the control group sample are also shown for comparative purposes. We examine the age, gender, work history, benefit receipt history, job tenure, mean earnings, firm size distribution, and industrial distribution of these individuals.

The entire sample of workers at exiting firms had broadly similar characteristics to the control sample workers. There were small differences in gender mix, mean age, and mean number of months employed in the two years prior to the closure or restructuring. The mean monthly earnings of the displaced workers during the two years before

displacement were 3 percent lower than those of the comparison sample. [10] They had, on average, five months less job tenure at the time of displacement. They were also more likely to be working at very small firms.

More substantial differences are apparent if we compare workers by the type of closure or restructuring event. Workers in firms with complete closures were more likely to be female than the control group workers, and were younger. They had less recent employment experience and significantly lower average monthly earnings. Their average monthly earnings in the two years leading to the closure were about 21 percent lower than those of the control group workers. They were approximately twice as likely to have received income from one of the main working-age benefits in an average month during the two years leading to the closure.

Workers in firms with probable restructurings and workers at branch closures were more similar to the control group in their months of recent work experience and mean earnings. Those in the 'closure-some transfers' group were generally between the first and fourth groups in terms of their level of prior employment experience and earnings.

There are large differences in the firm size and industry mix of the workers in the different business change groups, which is unsurprising given the large differences found across firms in table 2. Workers in the 'closure – no transfers' and 'branch closure' groups were predominantly employed at very small firms. A higher proportion of workers in the 'closure with transfers' and 'probable restructuring' groups were employed at medium sized or large firms. Workers at 'closures with transfers' were over-represented in the education industry and workers at 'branch closures' were over-represented in small establishments in the retail trade industry.

Table 4 also shows two indicators of job separation. The first is the ratio of average earnings in the final month of employment at the closing or reference firm, to average earnings (conditional on employment) during the previous 12 months, for each group. This ratio should be above one if workers are genuinely leaving jobs in the month their firm closes, because they are likely to receive any unused annual leave in their final pay. They may also receive redundancy compensation. We find that the ratio is 1.01 for the control group, indicating that the earnings of control group workers in the section month were typical of their average earnings in previous months. The ratio is slightly higher for workers who were involved in branch closures at 1.05, somewhat higher for workers in the probable restructuring group at 1.12, and substantially higher for workers in the 'complete closure' and 'closure with transfers' groups (1.36 and 1.24, respectively). The variation in this ratio across the four groups suggests that workers in the two firm closure groups were more likely to have genuinely separated from a job (without an offer of re-employment) in the month of firm closure than workers in groups 3 and 4.

The second measure is the percentage of workers who had ceased to work for their pre-displacement enterprise two months after the exit of the establishment (or in the case of the control group, two months after the selection month). By definition, 100 percent of workers in the two firm closure groups met this criterion, because we defined these groups as firm closures in which both the establishment and enterprise numbers ceased in LEED in the same month. In contrast, just under half of workers in the 'probable restructuring' and only 16 percent of workers in the 'branch closure' groups recorded a change in their parent enterprise two months after the exit of their firm from

[10] To minimise the impact of large outliers on the calculation of mean earnings, the monthly earnings variable was capped at $150,000 per month in March 2007 values.

LEED. This suggests that a high proportion of workers in both groups either moved to a different branch of the same enterprise after the closure of their firm, or were reallocated to a different branch of the same enterprise in the construction of employment counts in LEED.

The characteristics of the workers who left exiting firms 1–12 months before the firm's final month of existence are shown in table 5. Early leavers in both the displaced worker and control groups had substantially lower employment rates and earnings during the two years prior to their exit than workers who did not leave early, implying a lower level of labour market attachment. The early leavers also had substantially higher rates of benefit receipt in the two years prior to job exit, and shorter job tenure, If the four subgroups of early leavers from closing firms are compared, the differences across these groups are generally smaller than is the case for workers who did not leave early. In addition, job leavers in all subgroups tended to have higher monthly earnings, a similar or higher level of recent work experience, and similar or longer tenure in their jobs, than the control group job leavers.

The results in this section have identified some significant differences between the displaced and control group samples, prior to the job loss event. Estimates of the impact of displacement must take these into account. The indicators of job loss discussed in this section show patterns consistent with the hypothesis that workers in the 'branch closure' and 'probable restructuring' groups were much less likely to have left a job at the time of their firm's exit from LEED than workers in the 'complete closure' and 'closure – some transfers' groups, providing some support for the classification.

4. Results

Estimates of the impacts of displacement are presented in this section. We begin by summarising the raw differences between the employment rates and earnings of displaced workers and control group workers, before and after the firm event. The regression model used to estimate the impact of job loss is then presented. The main set of results captures the average impact of job displacement on the labour market outcomes of workers who stayed at an exiting firm until its closure. We extend this basic analysis by estimating the impact of displacement for subgroups of the study population, broken down by demographic and job characteristics. Finally, we explore the impact of firm closure on workers who left an exiting firm 1–12 months before the closure.

Event study of the effect of displacement on labour market outcomes

We begin our analysis of the effects of displacement by describing the longitudinal employment and earnings patterns of workers employed in firms that exited from LEED, before and after the closure of the firm, and comparing them to those of the control group workers. Figures 1–4 graph the employment rates, mean monthly earnings, mean monthly earnings conditional on being employed, and benefit receipt rates, of our main samples of displaced and control group workers. Note that our sample selection strategy ensures that 100 percent of workers were employed in the month of displacement. Our requirement that each worker have a minimum of two months job tenure ensures that the vast majority were also employed in the preceding month.

There is a distinctive pattern of increasing employment rates in the months leading up to the displacement (or the selection month, in the case of the control group) and decreasing employment rates in the months following the event. This occurs because we are examining a fixed cohort of individuals who were all employed in a particular month of selection, and as we move away from that month (in either direction), the

effects of job churn mean that increasing numbers of these individuals were out of employment.

The employment rate of workers in the complete closure group fell by 51 percent (to 49 percentage points) in the month immediately following displacement. Twelve months after displacement the average employment rate of this group was 30 percent lower than in the pre-displacement month. Workers in the 'closures with some transfers' group also experienced substantial short-run employment effects, including an employment rate decline of 28 percent in the month immediately after displacement. After 12 months, their average employment rate was 20 percent lower than in the selection month. The employment rate reductions of workers in the other two groups were much smaller. These groups did not record a sharp fall in employment in the month immediately after the event. After 12 months, the average employment rate of workers in these groups was 10–12 percent lower than in the pre-displacement month, which is similar to what is found for the control group of non-displaced workers.

Figures 2 and 3 graph the mean real monthly earnings of displaced and control group workers, before and after the firm closure or reference month. Workers in firms classified as complete closures, and to a lesser extent workers in the 'closures with some transfers' group, experienced reductions in their average monthly earnings immediately after the exit of the firm. This was not the case for workers in branch closures and the 'other restructuring' group. The earnings reductions could be due to reduced hours of work, reduced wage rates, or changes in the composition of the sample of workers who were employed. An interesting feature of the graphs is that the mean earnings of workers in firms that closed have spikes in the month coinciding with the firm's exit. These spikes are likely to be caused by the payout of unused annual leave or redundancy payments.

Benefit receipt rates (defined as the percentage who received any benefit income during the month) are graphed in figure 4. Workers at firms with complete closures and workers in the 'closure with some transfers' group have sharply increased levels of benefit receipt in the months immediately after displacement.

While it is possible to use these figures to make inferences about the impact of displacement by comparing the employment rates or earnings of the displaced workers to those of the control group in the months following displacement, this approach would not account for the fact that workers at exiting firms are have different characteristics than those at other firms. Therefore, we next turn to a regression analysis that allows us to examine the impact of displacement on future labour market outcomes, controlling for differences in both the observable and unobservable characteristics of individuals who are displaced.

Regression model of the impact of displacement

We specify a regression model that allows us to examine labour market outcomes for displaced workers relative to outcomes for the counterfactual sample measured over the same time period. Ignoring individual and time subscripts, the base regression specification we use is:

$$Y = \alpha + \tau * Displaced + \sum_{L} \left\{ TimeAfter(L) * \left[\gamma(L) + \delta(L) * Displaced \right] \right\} + X\beta + \varepsilon \qquad (1)$$

where Y is the dependent variable (capturing whether an individual is employed, their mean monthly earnings, or whether they receive benefit income), *Displaced* is a dummy variable equal to 1 if the individual has been displaced at the time of closure and equal

to 0 if they are drawn from the control sample, *TimeAfter(L)* is a dummy variable that equals 1 if the observation is *L* months after displacement, *X* is a vector of variables to control for other factors influencing either the outcome or the likelihood that an individual has been displaced, and *ε* is an error term to capture unobserved effects. Including the *TimeAfter(L)* dummy variables allows there to be a general declining pattern of employment over time for the fixed cohort of employed individuals in both the displaced firms and the control group. We focus on the coefficients $\delta(L)$, which represent the differences in employment rates or earnings between the displaced workers and the control group *L* months after displacement.[11]

This regression model is estimated on a pooled sample of displaced and non-displaced workers with one observation for each month that an individual is in the sample. We have data for the 24 months leading up to the displacement month and for 36 post-displacement months, for every worker in the sample. During this period, the sample is balanced because there is no attrition from LEED (individuals are recorded as not working if they have left New Zealand or died). We also have data on additional post-displacement months for some individuals, depending on the timing of their selection into the sample. To extend the period covered by our estimates, we use information on outcomes in months 37–48, which is still available for approximately two-thirds of sample members by month 48.

We estimate linear regression models even though employment and benefit receipt are discrete outcomes, because of the relative ease of obtaining estimated probability effects associated with the various factors of interest, which is the natural scale to use for interpreting the results (at least in terms of the magnitudes of the estimated effects).

In figure 5 and table A1 in appendix 2, we compare a number of specifications of this basic regression model. For this comparison, we use the data for workers in complete closures (ie the first subsample of displaced workers) and the control group. The $\delta(L)$ coefficients from each specification are presented. We begin by estimating the base regression model described in (1), including in *X* only control variables for the month and year in which each observation is measured to account for any period-specific or seasonal effects. The results for this base model are plotted in figure 5. Selected monthly estimates are also shown in the second column of table A1. Each point that is graphed represents the reduction in the average employment rate of group 1 workers that is attributed by the model to the impact of displacement. As a reference point, we also graph the raw difference between the employment rates of the displaced and control group workers in the months after displacement, which corresponds to the distance between the lines plotted in figure 1.

Because workers in this group had somewhat lower employment rates prior to displacement than the workers in the control group, the base model regression estimates of the impact of displacement are smaller than the raw difference between the employment rates of the displaced and non-displaced groups, by a constant amount in all months. The intuition for this adjustment is that taking into account their pre-displacement employment history, the post-displacement outcomes of the displaced workers are not as bad as the raw gap between the displaced and control group employment rates would imply.

[11] Note that because our sample covers the entire population, standard errors presented in this paper do not have their typical interpretation as a measure of sampling variation. It is more useful to interpret them in a Bayesian framework as representing the parameter variability if 'new' populations are examined.

The regression model is then extended by adding controls for the following characteristics of both the displaced and non-displaced workers: gender; a quadratic in the worker's age measured at each month; a quadratic in tenure at the time of displacement; an indicator variable for whether the job spell is left censored; log of 'real' monthly earnings at the event job in the month prior to displacement; the firm size and wage bill of the closing firm six months prior to the closure or restructuring; an indicator variable for whether the firm was part of a multi-unit enterprise; the industry and region of the firm six months prior to the closure or restructuring; and the month and year during which the displacement took place. Results are shown in the third column of table A1. Controlling for differences in these characteristics makes only a small difference to the estimates, suggesting that controlling for differences in employment rates prior to displacement, as is already done in the base model, sufficiently accounts for the important differences between the displaced workers and the control group. Because the estimates from the base and extended models are so similar, we do not graph the extended model estimates in figure 5.

It is possible that individuals working at firms that are at risk of closure have different unobservable characteristics than other workers and that these characteristics are correlated with their general propensity to be employed. If this is the case, then the estimates obtained from models estimated using 'ordinary least squares' will be biased. For example, if individuals who are less motivated about work are more likely to work for unstable firms, then displacement may seem to have negative impacts on future labour market outcomes even when it does not. To deal with this concern, we next estimate equation (1) including a separate intercept for each individual in the sample. In other words, the error term ε_{it} is partitioned into two components, α_i, which is an individual intercept, and μ_{it}, which is a normal random error term. This fixed effects regression model controls for all time-invariant characteristics of individuals that may be correlated with both the propensity to be employed at a closing firm and their future labour market outcomes.[12]

The estimated coefficients from the fixed-effects specification are shown in the fourth column of table A1. These coefficients can be interpreted as showing the impact of displacement on the *change* in employment rates for displaced individuals relative to the *change* in employment rates for non-displaced individuals, over the same time-frame. In practice, our estimates of the effect of displacement on future employment rates for displaced workers controlling for individual fixed effects are quite similar to those obtained in the base model. This suggests that controlling for differences in employment rates prior to displacement for displaced workers and the control sample, as is done in the base model, adequately controls for unobserved differences between these groups that are correlated with future employment outcomes.

If some workers at firms that are struggling anticipate a closure, or if a closure is announced to workers before the final month, there may be changes in the composition of the workers who remain at the firm until its final month of operation. For example, some workers may see the writing on the wall and leave before the firm gets the chance to close. On the other hand, workers who are eligible for redundancy pay may decide to stay until the end, when in other circumstances they would have left earlier. There may also be impacts on pre-closure earnings, for example if the firm cuts its hours of production. To avoid the possibility that pre-closure processes alter the employment rates and earnings of displaced workers for a period before the closure of the firm, and

[12] All non-time varying covariates (eg all covariates besides the quadratic in age and the controls for current month and year) are now dropped from the model, since these are absorbed into the individual intercept term.

therefore distort our estimates of the average pre-displacement differences between the control and displacement groups, we extend the fixed-effects regression model by including additional control variables for the six months prior to the closure month. These additional controls mean that the adjustment for differences between the displaced worker and control group is now estimated using the data for the period 7–24 months prior to the closure.

The estimates from this model are shown in figure 5 as the bold line labeled 'final model' and in the fifth column of table A1. The average gap between the employment rates of the displaced and control group samples is slightly larger in the period 7–24 months before the closure event than during the full 24 months, and therefore the estimated coefficients for the post-displacement impact are slightly smaller than in the previous specifications.

Finally, we estimate an alternative model of the impact of displacement, in which the dependent variable is the log of the non-employment rate rather than the employment rate. This model is motivated by the observation that the difference between the employment rate of workers displaced in complete closures and the control group, as shown in figure 1, may be a proportional rather than a fixed one. (A proportional relationship would exist if the employment rate of displaced workers was 90 percent of the control group employment rate in each month, rather than a constant distance of 10 percentage points lower.) A log-linear model is used to estimate a proportional relationship between the dependent variable and the explanatory variables.

The estimates obtained from the log-linear model of the non-employment rate are shown in the final column of table A1 and plotted as the dashed line in figure 5. The key difference from our previous estimates is that under this specification, the adverse impact of displacement diminishes at a faster rate, declining to 7 percentage points by month 48 (compared with 10 percentage points in the previous specification). We do not pursue this specification because it is more complex to estimate and yields results that are only moderately different from those of the linear model. However, we present it to make the point that all impact estimates are to some degree dependent on the model and specification chosen, and greater weight should be attached to the overall patterns of difference and change than the exact size of the estimates.

Main estimates of the impact of displacement on labour market outcomes

Our main estimates of the impact of being displaced on workers who remained at closing and restructuring firms until their final month of operation are presented in this section. We use the final linear model described above for these estimates and for all further regressions in this paper. In each table, we present a selection of the estimated $\delta(L)$ coefficients from this model along with their associated standard errors, focusing on the effects 1–6, 12, 24, 36 and 48 months after the displacement. Each coefficient indicates the change in the employment rate, earnings or benefit receipt rate of displaced workers at month *L* relative to the period 7–24 months before displacement, and relative to the same change for non-displaced workers over the same time period.

The impact of displacement on employment is shown in table 6. The first column of table 6 gives the mean employment rates of non-displaced workers at specific points in time after the reference month, against which the magnitude of the impact of displacement should be judged. The second column gives estimates of the average employment impact for the entire study sample of displaced workers, and the remaining columns give estimates for each of the four displaced worker subsamples. For the latter

results, we estimate four separate regressions using the data for the workers in each event group and the entire control group sample.

The results in column 2, giving the average effect of displacement for workers across all firms that exited from LEED, show relatively small adverse employment impacts. Displaced workers are estimated to have 5 percentage point lower employment rates in the first month after the firm exit, and 3 percentage point lower employment rates 12 months after. Relative to the control group's employment rate, these impacts represented a 3–5 percent employment rate reduction. After an initial recovery, the employment rates of the total sample of displaced workers remain 3–4 percent below those of the control group.

Disaggregating the sample of displaced workers by type of event reveals substantial negative effects for some groups and insignificant effects for other groups. Displacement has the largest impact on workers who were involved in a closure without transfers. We estimate a 45 percentage point (or 46 percent) employment rate reduction in the first month after closure, a 21 percentage point (or 22 percent) reduction six months after closure, and a 16 percentage point (or 17 percent) reduction 12 months after closure. The estimated impact declines to just under 10 percentage points (or 12 percent) after four years.

Smaller but still substantial effects are estimated for workers involved in closures with some transfers. Displacement is estimated to reduce the employment rate of this group by 21 percentage points (or 22 percent) in the month after closure, and 6 percentage points (or 7 percent) 12 months after closure. However, after four years, the estimated impact is insignificant.

The estimates for workers in branch closures and other restructurings indicate that their employment rates were also negatively affected on average, but those impacts (under 3 percent for branch closures and under 2 percent for probable restructurings) are small. In all likelihood, the small size of the impacts reflects the mixed composition of these groups, which include both employees who lost their jobs and employees who remained employed or were immediately re-employed at a different branch or at a restructured firm.

The impact of displacement on earnings is examined in tables 7 and 8. Table 7 presents estimates of the impact on total monthly earnings, in March 2007 dollars, using data for all sample members including those who had zero earnings. The regressions in table 7 capture the total effect of displacement on employment, hours worked and wages. Our estimates of the earnings reductions are in dollars and their standard errors are presented in the upper section of the table. In the lower section, the earnings losses are expressed as a percentage of the mean earnings of the control group.

Workers in the 'closure – no transfers' subsample experienced the largest total monthly earnings reductions – approximately $1,840 in the first month after displacement, falling to $950 six months after displacement, and $830 12 months after displacement. These reductions represent 46 percent, 24 percent and 22 percent of the control group's mean earnings at each point in time. After four years, the estimated reduction in the total monthly earnings of this group of workers was $560, or 16 percent of the control group's mean earnings. At each month, the percentage impacts on total earnings were generally larger than the percentage impacts on employment rates, suggesting that job loss has an adverse impact on hours worked or wage rates, as well as on employment. However, because the composition of employees is also changing, it is not possible to conclusively measure whether displacement has impacts on hours worked or wages.

The estimated earnings losses of workers in the closures with some transfers group were also moderately large. The total monthly earnings of this group are estimated to have been 11 percent lower after six months, 13 percent lower after 12 months, and 6 percent lower after 24 months. The estimates of the impacts of displacement on the total monthly earnings of workers in the branch closures and probable restructurings groups are generally small, and are positive rather than negative in some months. Allowing for measurement and modeling error, we lack strong evidence that these latter groups experienced significant earnings reductions.

Table 8 presents estimates of the impact of displacement on the real monthly earnings of those who were employed in each month, using log earnings as the dependent variable. Examining this outcome allows us to consider whether displacement is directly affecting wages or hours of work beyond its impacts on employment. Note, though, that it is difficult to get a clean estimate of this impact because the composition of employment is changing over time. For example, when examining the impacts for workers in the 'closure – no transfers' closure subsample, we find a 22 percent reduction in employment rates six months after displacement. If this employment impact is occurring, on average, for workers who would have had to take a larger earnings decrease to remain employed, then our estimate of the direct impact of displacement on earnings is understated when we focus on earnings conditional on employment. Conversely, if these were workers who, on average, would have had smaller wage losses, then our estimate of the direct impact of displacement on earnings is overstated. Because the employment impacts are smaller in the medium run than in the short run, this compositional effect (often referred to as selection bias) will be smaller in the medium run, unless the pattern of selection of workers into employment changes over time.

The estimates are shown in log points in the upper section of table 8 and as percentage impacts in the lower section of the table. They show that workers in the complete closure subsample who were working in the month after closure experienced a 30 percent reduction in their monthly earnings, on average. The estimated earnings reductions after six months, 12 months, 24 months and 48 months, for those in employment, were 11.4 percent, 9.6 percent, 4.9 percent, and 0.4 percent, respectively. Workers in the closures with some transfers group experienced earnings reductions that were only slightly smaller than those of the main closure group. Consistent with the total earnings impacts, the conditional earnings impacts for workers in branch closures and other restructurings were small and not always negative.

These estimates are consistent with a direct impact of displacement on wages for displaced workers that declines in magnitude over time at a faster rate than the direct impact on employment, or with displacement having little direct impact on wages at any point in time, but there being negative selection into employment in the initial period after displacement (ie, the workers who would have experienced smaller earnings loses after displacement are the ones that leave employment in the short run). However, if the selection bias is more complex (eg changes over time) then other interpretations would be consistent with these results.

The impact of displacement on benefit receipt is examined in table 9. The first column of the table gives the average benefit receipt rate of non-displaced workers at specific points in time after the reference month. Estimates of the impact of displacement show significant short run increases in the benefit take-up rates of workers in the 'complete closure' and 'closure – some transfers' groups. However, by two years after displacement, benefit take-up rates are the same or less among these subgroups of

displaced workers than in the control group. We do not find any significant impacts of displacement on benefit receipt for the branch closure and probable restructuring groups.

The short-run impacts for the 'complete closure' and 'closure-some transfers' groups are small in absolute terms and relative to the employment rate decline for these displacement workers. For example, the benefit receipt rate of workers in complete closures is estimated to be 4.1 percentage points higher 6 months after the displacement, while employment rates are 20.1 percentage points lower. Similarly, at 12 months post-displacement, employment rates are estimated to be 15.7 percentage points lower, while benefit receipts have only increased by 2.2 percentage points. However, because benefit take-up rates of the control group are only between 4.5 percent and 5 percent during this period, in relative terms, displacement leads to large short-run increases in benefit receipt. For example, six months after displacement, benefit rates have increased by 84 percent relative to receipt by the control group, while at 12 months post-displacement there is still a 45 percent relative increase in benefit receipt by displacement workers in the complete closure subsample.

Overall, the variations in outcomes across the four subsamples are consistent with our ex-ante classification of the firm exits in our study population, from probable complete closures to probable firm restructurings. Job displacement had large initial impacts on the employment, earnings and benefit receipt of workers who were displaced in firm closures in which both the establishment and the enterprise ceased and no group transfers were recorded, but much smaller impacts on workers at firms that transferred a large proportion of their employees to other firms before closing (and therefore provided employment opportunities for most affected workers). Displacement also appears to have had a negligible impact on the labour market outcomes of workers in branch closures. This is probably because the majority of these workers either did not lose their jobs or were given the opportunity to continue working at another branch of the enterprise.

We find evidence that displacement has persistent negative impacts on workers at firms that were likely to have experienced full closures in which direct re-employment options were not available. For example, employment rates remained 12 percent lower and total earnings 16 percent lower four years after displacement, compared with workers in the control group. As noted in the beginning of this section, estimates of medium-run impacts are somewhat model dependent, with smaller impacts found when measured in proportional rather than absolute terms. Without a longer-time series of data, it is not possible to conclude whether these impacts will persist in the long-run.

Variations in the impact of displacement

The effects of involuntary job loss are likely to be heterogeneous. Previous research has identified larger impacts on employees who were older or had longer job tenure (for example Chan and Stevens, 2002). In this section, we examine whether the impacts of displacement differ for workers with different characteristics. To keep the results tractable, we restrict our analysis to workers in the 'closure – no transfers' subsample.[13] We again focus on workers who remained at the closing firm until its final month of

[13] The analysis was repeated for workers in the 'closure – some transfers' sub-sample. Although the effects of displacement were generally smaller for this sub-sample, the relative impact patterns found across demographic and job-type groups were broadly similar to those reported here.

operation, and estimate the impacts of displacement relative to the outcomes of the control group. We also restrict the analysis to employment and earnings as these are our main outcomes of interest.

Tables 10 (employment rates), 11 (total monthly earnings), and 12 (monthly earnings conditional upon employment) present results obtained by estimating separate regressions stratified by gender, age group, tenure, earnings quartile, firm size, whether the firm was part of a multi-establishment enterprise, the industry of the firm, and the geographical location of the firm. In each regression, both the displaced worker sample and the control group sample are restricted to the particular sub-group being examined, for example women. Tables A2, A3 and A4 in appendix 2 show the same regression estimates, expressing each coefficient as a percentage of the mean employment rate or earnings of the control group in each period.

Displacement is found to have similar impacts on the employment rates of males and females, with the impacts slightly lower for females at 12, 24 and 36 months but slightly higher at six months and 48 months. Estimates of the impact of displacement on unconditional monthly earnings (table 11) show larger relative losses were experienced by men than by women, but estimates of the impact of displacement on the earnings of the individuals who were re-employed (table 12) point to larger losses for women than for men.

More substantial differences are found in the impact of displacement on differently aged individuals. The employment impacts estimated for 25–34-year-olds are substantially larger than those estimated for the 35–54 and 55–64-year age groups, with employment rates 29 percent lower one year after displacement for 25–34-year-olds, 16 percent lower for 35–54-year-olds, and 16 percent lower for 55–64-year-olds. These age differentials persist for the rest of the follow-up period. Displacement also has a larger impact on the relative earnings of the youngest age group than on the other two age groups, especially in the short run. Unlike previous researchers, we do not find that the impacts of displacement on 55–64-year-olds are substantially worse than those on prime-age adults. The impacts estimated for 55–64-year-olds are generally slightly smaller than, or similar to, those estimated for 35–54-year-olds.

As predicted by human capital theory, the impact of displacement is larger for individuals with longer firm tenure and therefore greater firm-specific human capital. This is true in both the short run and medium run. In particular, employment rates are 26 percent lower and total earnings 27 percent lower one year after displacement for individuals with more than two years of firm tenure, versus 12–13 percent and 17–18 percent lower, respectively, for workers with less tenure. Similarly, three years after displacement, employment rates are 20 percent lower and total earnings are 20 percent lower for workers with more than two years of tenure, versus 8–10 percent and 10–13 percent lower for workers with shorter tenure. Because we only have a minimum of two years of data prior to displacement for some workers in the sample (and at most four years of prior data), we are not able to further distinguish workers with long-term tenure (eg five years plus).

Impacts also vary substantially by the worker's level of earnings in the month immediately before their last month of employment at the firm. Employees in the lowest quartile of monthly earnings (with monthly earnings below $1,900 – a group that is likely to include a large proportion of part-time employees), and employees in the highest quartile, had greater adverse employment impacts than employees in the middle quartiles of the earnings distribution. For example, the employment rate of the lowest quartile of earners was 22 percent lower one year after displacement, versus 18, 17 and

23 percent lower for the second, third and fourth quartile groups. The differentials in total earnings losses were larger: 15, 22, 22 and 43 percent losses for the first to fourth quartile groups respectively, one year after displacement. Workers in the highest quartile of earnings experienced the largest earnings losses, and this is true of both the total and the conditional earnings measures.

We next examine whether impacts vary by firm size, measured in terms of the average number of employees at the closing firm in the period 7–24 months prior to closure. The results indicate that employees in larger establishments (those with 50 or more employees) experienced much smaller adverse employment impacts than those at small and medium-sized establishments, and had the fastest rate of recovery. While the pattern of total earnings losses shows no clear variation by size of firm, the conditional earnings estimates for re-employed workers also suggest that workers in establishments with 50 or more employees were less severely affected than those in small and medium-sized business units. The outcomes of workers at single-establishment enterprises are also compared with those of workers at multiple establishment enterprises (multis). We find that the employment rate impacts for workers at single-establishment firms were substantially greater than those for workers employed at multis.

The reasons for the pronounced difference in outcomes for larger firms and multis are unclear. Larger firms and firms that are part of multis may employ a different mix of workers than firms that are singles, or they may provide different types and levels of re-employment assistance to the employees they make redundant. These firms are also concentrated in different industries, which may influence displacement outcomes for workers. These results provide further evidence that stratifying by firm type is essential for understanding the impact that firm closures have on workers.

Allowing the impact of displacement to differ for individuals who were employed in different industries at the time of the closure reveals some fairly large differences. We present results for the eight 1-digit industries that employed the largest numbers of people in our complete closure sample. Although the patterns are complex, it is clear that the estimated employment and earnings impacts are larger for workers who were employed in agriculture, manufacturing, retail trade, hospitality, or business services, than for workers in education and in health and community services. Of the industries considered here, workers in the education industry had the smallest employment losses and the most complete recovery during the four years that followed. These industry variations are also likely to reflect differences in the nature of typical closure or restructuring events.

Workers living in different geographical areas face different sets of labour market opportunities. Disaggregating the samples along a rural/urban dimension, we find that the employment impacts of displacement are somewhat larger for workers who were living in the five largest cities at the time of displacement, and for workers in rural locations, than for those living in minor cities and small towns. For instance, displaced workers who were in the five main urban centres at the time of displacement are estimated to have an employment rate that is 19 percent lower than that of similar control group workers after one year. The comparable impacts for displaced workers in other major cities, minor cities or provincial towns, and rural locations are 13, 11, and 15 percent respectively. There is a general pattern of larger impacts for both residents of the largest cities and people living in rural areas, than for the intervening urban size groups. This pattern also appears in the estimates of impacts on total earnings. However, there is no clear differential between main centre workers and other workers in the impact of displacement on the earnings of workers who return to employment.

If we instead disaggregate the sample by regional council area, we find that larger employment and earnings impacts were experienced by workers living in the Auckland region. Regional differences are fairly large when examining the impact of employment. For example, two years after displacement, employment rates were 17 percent lower in Auckland and 14 percent lower in Canterbury versus 9 percent to 10 percent lower elsewhere in New Zealand. In general, impacts are smallest in the less populous regions (which were pooled to form one group in this analysis), although this is not always the case.

A shortage of alternative employment opportunities for workers in rural areas could be a factor contributing to their relatively worse outcomes. In the five main urban centres and the Auckland region, a number of factors may be affecting the relatively poorer employment outcomes of displaced workers. Displacement impacts may be related to the composition of firms that exit in different areas. For example, if firms in the education, and health and community services industries were less likely to close in the main urban areas than elsewhere, this could lead to generally worse post-displacement outcomes (given the industry patterns we observe). The composition of workers at dying firms may also differ across regions in ways that influence the impact of displacement. For example, larger displacement impacts may be found in the main urban areas if closing firms located there tend to employ younger workers, or workers with lower earnings, and/or longer tenured workers. Distinguishing between these competing explanations would require the estimation of a more complex model in which all worker characteristics are interacted with the indicator variables for the post-displacement period. We have left this task for potential future research.

Impacts on workers who left firms prior to a closure or restructuring

In this section, we turn to the question of whether firm closures and restructurings adversely affect workers who leave a closing firm in the year prior to the event. The outcomes of these workers are compared with those of workers who left their jobs at firms that continued to operate in the same reference months, using the same regression methodology as in the previous analyses.

In our discussion of the characteristics of early leavers (table 5), we noted that early leavers in all the firm event groups tended to have higher monthly earnings, a similar or higher level of recent work experience, and similar or longer tenure in their jobs, than the general job leavers who make up our control group.[14]

Figure 6 graphs the unadjusted employment rates of the early leavers before and after their job separation, along with those of the comparison group early leavers. In the months leading to the month of job exit, early leavers from the complete closure and 'closures with some transfers' subgroups had similar employment rates to those of the comparison group job leavers. After the job exit, workers from firms classified as complete closures had somewhat lower employment rates than the comparison group, but this was not true of workers in the 'closure with some transfers' group. These patterns suggest that the experience of working at a firm that is soon to close might have some negative impacts on workers who leave firms prior to closure.

[14] We believe that the job leaver sample is the appropriate comparison group for early leavers prior to displacement, since some of these workers will have left their employer voluntarily. However, it would also be legitimate to compare outcomes for these workers to the main control group of continuous employees.

Tables 13 (employment), 14 (earnings) and 15 (conditional earnings) present the $\delta(L)$ coefficients from the regression models used to estimate the impact of leaving a firm that is soon to close or restructure. Future firm closures are found to have negative impacts on the employment rates of workers in both the complete closure and branch closure samples, but not on the employment rates of workers in the 'closure with some transfers' and other restructuring subsamples. The size of these adverse impacts is also far smaller than the impacts we estimated for workers who stayed at their firm until closure. For example, the employment rates of workers who left a firm that was later classified as a closure are estimated to be around 4 percentage points lower for the first three years after displacement. The employment rate losses of workers who left their job in advance of a branch closure are of similar magnitude.

Workers who left firms classified later as complete closures, closures with some transfers, and branch closures, all experienced small to moderate earnings losses. For example, the earnings of workers who left branch closures and were re-employed were 4–8 percent lower after six months than those of the control group.

These regression estimates suggest that the post-separation employment rates and earnings of some of the workers who left closing firms were lower than would be expected, taking into account their attributes and prior employment histories. One possible explanation is that being part of a failing firm 'scars' workers in some way that affects their future labour market outcomes (although those effects do not appear to be very serious or prolonged on average). Another possible explanation is that a minority of the individuals in the 'early leaver' sample were made redundant as part of the firm's closure or restructuring. Anticipation effects leading to voluntary early departures could also be playing a role, if workers had advance notice of the likely closure of their firm. If employees are aware that their firm is struggling, the more motivated or skilled individuals – those with the best re-employment opportunities – might leave before the firm closes. If positive selection operates, this will tend to lower estimates of the costs of firm closure for early leavers, and raise estimates of the negative impacts for workers who stay till the end.

A comparison of the measured characteristics of early leavers (table 5) with those of employees who stayed with their firm until the closure or restructuring event (table 4) provides mixed evidence of the selection effects that could be at work. Early leavers in both the complete closure and 'closure with transfers' groups were younger and had less recent work experience than workers who stayed until the closure. However, early leavers from closures had significantly higher earnings than those who stayed till closure, suggesting they may have been positively selected. Note, however, that selection effects are likely to operate via unmeasured as well as measured characteristics.

In unreported results, we also examined the impact of displacement on the outcomes of workers who left closing and restructuring firms 13–24 months prior to the firm's exit from LEED. We found no evidence that the future closure or restructuring of the firm had negative impacts on the labour market outcomes of these workers. Therefore, anticipatory effects and any scarring effects from working at a declining firm appear to matter only for workers that leave closing firms in the year prior to closure.

Sources of bias in the displacement impact estimates

Two possible responses to involuntary job loss that we have not yet examined are migration from New Zealand and movement into self-employment. If workers in the

displaced worker sample had different rates of external migration or transitions into self-employment during the post-displacement period than the control group workers, our estimates of the employment and earnings impacts of displacement may be overstated. In this section, we consider the impact of these two potential sources of bias on our results.

LEED contains information on each person's annual self-employment income in each tax year ending 31 March. It is not possible to tell whether the self-employment activity that generated the taxable income was spread evenly across the entire year or concentrated in particular months, and therefore, a reliable measure of monthly self-employment activity can't be derived. To explore the implications of taking self-employment activity into account, we construct an estimated total employment rate measure in which each individual is classified as employed if they received any earnings from wage employment during the month *or* any income from self-employment activity during the year. This total employment rate measure by definition overstates the true total employment rate in any given month.[15]

If we examine the impact of displacement on this total employment measure instead of focusing on wage employment, the magnitude of the impacts for workers in the complete closure and 'closure-some transfers' groups are around one-quarter to one-third smaller than those estimated in the main wage employment regressions. This can be seen by comparing the total employment estimates in table A5 in the appendix with the results already presented in table 6.

Two factors lie behind the reduction in estimated impacts. First, although approximately 12 percent to 15 percent of employees in both the displaced and control group samples received some level of self-employment income in the years prior to the closure, workers in the displaced worker samples were more likely to have received self-employment income in periods when they were not in waged employment. Consequently, there was a smaller pre-displacement gap between the two groups in total employment rates than in wage employment rates. Second, the self-employment rates of employees in the complete closure and 'closure – some transfers' groups appear to be a few percentage points higher in the years after the displacement event than in the years prior to it.[16] The latter suggests a possible shift in employment activity towards self-employment, in response to displacement.

This analysis indicates that the impact of displacement on total employment activity is likely to be smaller than the impact on wage employment activity, which is the focus of this paper. Due to data limitations, however, it isn't possible to accurately determine the extent of the difference. The results presented in appendix table A5 are likely to provide a lower-bound estimate of the impact of displacement on total employment activity. We still find evidence that displacement has a large impact on employment rates for individuals in the complete closure and 'closure – some transfers' groups. For example, the estimates show that total employment activity for workers in the complete closure group is at least 15 percent lower six months after displacement, 12 percent lower one year after displacement, 9 percent lower two years after displacement, and 7 percent

[15] It is overstated for two reasons. Part-year self-employment jobs are treated as full-year self-employment jobs, and people who received income or made losses from a business they owned (in part or in full) are counted as self-employed even if they worked nil hours.

[16] The estimated monthly self-employment rate of workers in the 'closure – no transfers' group was 4.4 percent higher in the period 13–36 months after displacement, than in the period 13–36 months prior to displacement. There was no increase in the self-employment rate of the control group workers between these two periods.

lower four years after displacement. The equivalent figures for workers in the 'closure – some transfers' group are 6, 5, 2, and 0 percent, respectively.

These results also suggest that excluding self-employment income from the measure of 'employment income' we examine in our main analyses leads to an overstatement of the impact of displacement on labour market outcomes. However, we do not try to measure the size of this potential overstatement empirically because it would require us to estimate the proportion of taxable income from self-employed that is related to employment, as opposed to returns to capital, and there are many practical difficulties in doing this. This is another task we leave to future research.

Displaced workers may also consider migrating from New Zealand in response to job loss. Residency status is not tracked in LEED, and therefore, we cannot tell if an individual who does not have any payments recorded in LEED in a particular month is in New Zealand and economically inactive, or is living overseas (or has died). To explore the potential impact of external migration, we calculated the percentage of people in each subsample who exited from the LEED database during the three months immediately after the displacement event (or the reference month in the case of the control group) and did not reappear in the rest of the period for which data are available (a minimum of 36 months).

We find that in the three months after displacement, approximately 3.8 percent of workers in the 'complete closure' group left LEED and did not return within the observation window, a much higher exit rate than calculated for the control group of workers at continuing firms (0.4 percent) or for any of the other displaced worker groups. However, it is slightly lower than the exit rate calculated for the control group of early leavers from non-closing firms in the three months after they left their jobs (4.9 percent), and is similar to or lower than the exit rates calculated for early leavers from firms that later closed or restructured. Therefore, the pattern in these results is consistent with involuntary job loss being associated with external migration *and* with job separations (both voluntary and involuntary) being associated with a higher rate of exits into non-employment. Unfortunately, the data limitations do not allow for a deeper examination of this question. Overall, the evidence suggests that overseas migration explains at most a small percentage of the overall post-displacement decline in employment found for workers in the 'complete closure' group.

5. Summary and conclusions

This study examines the impact of involuntary job loss due to firm closures and restructuring events on workers. It uses data from LEED, an administrative dataset in which the exit of a firm identifier does not always represent a genuine and complete firm closure. All establishments aged two years or older with at least five employees that exited from LEED between April 2001 and March 2004 were selected, and information on the flows of clusters of workers from these firms to other firms was used to classify them according to the likelihood that a complete closure took place. Other firm exits were classified as partial closures or restructurings.

These closures and restructurings were classified into four subsamples, using information on the structure of the firm before its closure and the transfer of groups of employees to other establishments or enterprises: (1) firm closures with no employee group transfers; (2) firm closures in which up to a third of employees were transferred to a new firm identity; (3) closures of branches within multi-establishment enterprises; and (4) the rest, labelled 'probable restructurings'.

These four subgroups are likely to differ in the nature of the 'displacement' faced by the workers. Workers in the first closure group are most likely to have lost their jobs without the option of immediate re-employment elsewhere: that is, no other firm offered employment to a group of them. In the second group, a closure or restructuring is likely to have occurred in which some employees had opportunities to remain employed or be immediately re-employed by a related employer. In the branch closure group, the employees may have been offered opportunities for work at other branches of the enterprise. The final group is likely to include workers who faced a range of different employment situations: redundancy, technical redundancy involving a change of employer but retention in an unchanged position, redeployment to a new position in a restructured organisation, or no change in their employment relationship and position.

Our main analysis examines the impact of displacement on the labour market outcomes of workers who were employed at these firms in the month of the firm's closure or restructuring, during the following four years. This impact is estimated by comparing the changes in outcomes for workers affected by these events with those of a 'control' group of workers, who were employed in the same month at firms that did not close or restructure.

Overall, we find that displacement has small impacts on the employment rates, benefit receipt rates and earnings of workers at all closures and restructurings. The small size of these impacts is likely to reflect the fact that the total sample includes a substantial proportion of people who were not made redundant, experienced a technical redundancy only, or were immediately reassigned to new jobs, rather than having to search for a new one. Measurement error could also be reducing the size of the impact estimates for the total worker sample, because the sample of closures and restructurings is likely to include some events that were purely administrative ID changes, and therefore should not have any real world consequences.

There is considerable diversity in the estimated impacts across the four subgroups of firms that we define. Workers in the 'closure – no transfers' subsample experienced reasonably large and persistent reductions in their average employment rates and earnings after the closure of their firm. This group made up 9 percent of employees in the initial sample of firms that closed or restructured (counting only those who were employed at the month of the closure). The employment rate of these workers was 17 percent lower one year after the firm closed than those for comparable workers at control group firms, and remained 12 percent lower four years after the closure event. The average monthly earnings of the entire group (including those with zero earnings) were 22 percent lower one year after the closure and 16 percent lower four years after the closure. Their benefit receipt rate was also 2.2 percentage points (or 45 percent) higher one year after the closure than that of comparable workers at control group firms.

Workers employed at firms that closed but had some employee group transfers to other firms before the closure, experienced smaller employment and earnings losses on average. Displacement is estimated to have reduced the employment rates of this group by 7 percent at 12 months after closure. After four years, the estimated impact on employment was close to zero. Workers who were employed at firms classified as having branch closures or probable restructurings experienced very small negative employment and earnings impacts, on average.

To summarise, the firm closures and restructuring events that were examined in this paper did not impose significant labour market costs on all affected workers, but did have significant impacts on the subsequent labour market outcomes of particular groups. The variation in impacts is consistent with the view that job displacements have

negative impacts on the labour market outcomes of workers who experience genuine and complete closures, while having limited impacts on individuals who were separated from their jobs with the opportunity for reassignment or re-employment, or who merely experienced a business restructuring.

The study also offers insights into the manner in which displacement impacts can vary across workers with different characteristics. Disaggregating the 'closure – no transfers' subsample, displacement was found to have a larger impact on the employment rates and earnings of 25–34-year-olds than those of prime-age and older individuals. Unlike some previous researchers, we did not find a greater impact of displacement on older workers than on the prime-aged workers. Although we did find differences in the magnitude of impacts between men and women, they were not large or consistent in pattern. Workers who were living in rural areas, and those living in the five main urban centres, had larger employment and earnings losses as a result of displacement than those living in minor cities and towns.

The impacts of displacement were larger for individuals with greater firm tenure (two years or more) and therefore greater firm-specific human capital. This was true in both the short run and medium run, and the differences are relatively large in magnitude. Impacts on both employment and earnings also varied substantially by the worker's level of earnings at the closing firm. Employees with monthly earnings in the lowest quartile of earnings (a group that includes a large number of part-time employees), or with earnings in the highest quartile, had larger employment losses in the first two years after the displacement than employees at the centre of the earnings distribution. The impacts on relative earnings were largest for the highest earnings group.

The effects of job displacement also differed for workers who were separated from different types of firms. Workers who were employed at small and medium-sized establishments (those with less than 50 employees in the period before the closure) had much larger employment losses than workers at establishments with 50 or more employees. The estimated employment impacts were also larger at single-unit firms than at establishments belonging to multiple-unit enterprises (multis). The reasons for these differences are unclear. Larger establishments and establishments that are part of multis may employ a different mix of workers, or they may provide different types and levels of re-employment assistance to the employees they make redundant.

The last part of the analysis considered whether there were impacts on people who worked at closing and restructuring firms in the year (1–12 months) prior to the event. This group includes workers who left their jobs voluntarily before the closure or restructuring was announced, workers who left their jobs voluntarily after it was announced, and workers who were made redundant before the final month of the firm's existence. We found small negative impacts on the employment rates and earnings of workers in the 'closure – no transfers' and branch closure subsamples, and no evidence of employment or earnings losses for the other two subsamples. The small size of the estimated impacts is consistent with firm closures having limited impacts on voluntary job leavers, workers with better alternative opportunities leaving closing firms prior to the last month, and 'early' involuntary job leavers experiencing smaller direct impacts. Unfortunately, without additional information on the nature of the separation, it is not possible to untangle these explanations.

The impact estimates reported for employees in the 'closure – no transfers' subsample provide the cleanest estimates of the impact of involuntary job loss, because these workers were most likely to have experienced job loss without associated re-employment opportunities, and least likely to have been selected in or out of the 'job

loss' outcome by their firm, by the provisions of collective agreements, or by their own decisions. However, these estimates are based on a fairly narrow definition of 'closure', which resulted in a sample in which small and medium-sized firms predominated. This means they may not be fully generalisable to all closures, if 'closure' is more broadly defined.

It is worth noting that the results in this paper only capture the impacts of displacement on workers who leave an establishment that ceases to exist in LEED. We did not examine the impact of downsizings and other restructurings that do not lead to establishment closures, but do lead to job loss. Therefore, our estimates are likely to be uninformative as to how firm downsizings in general affect workers.[17] Other possible limitations on the generalisability of the study results stem from the initial study population restrictions: the exclusion of very small and short-lived firms from the outset (whose employees made up 25 percent of all employees in March 2004), and the exclusion of employees who were aged under 25 years or over 64 at the time of the displacement event.[18]

Another caveat to consider when interpreting the findings of this paper is that the period studied was one of strong employment growth and low unemployment. Previous research in other countries has found significant business cycle variations in the employment and earnings losses experienced by displaced workers (for example, Eliason, Marcus and Donald Storrie 2006; Morisette 2007). In general, the impact of displacement tends to be larger and more sustained when poorer labour market conditions exist.

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[17] This is one particular disadvantage of using administrative data to study job displacements, since these data are not well suited for identifying displaced workers who leave non-closing firms.

[18] Firm closures are not well defined for either very small firms or for those that are only open for short periods of time, and these firms are excluded from most studies of the impact of job displacement. Workers who are aged under 25 years or above 64 years are often excluded from this type of impact assessment because people in these age groups often have options to return to full-time education or to retire. In an administrative dataset like LEED, these educational and retirement outcomes cannot be distinguished from non-employment.

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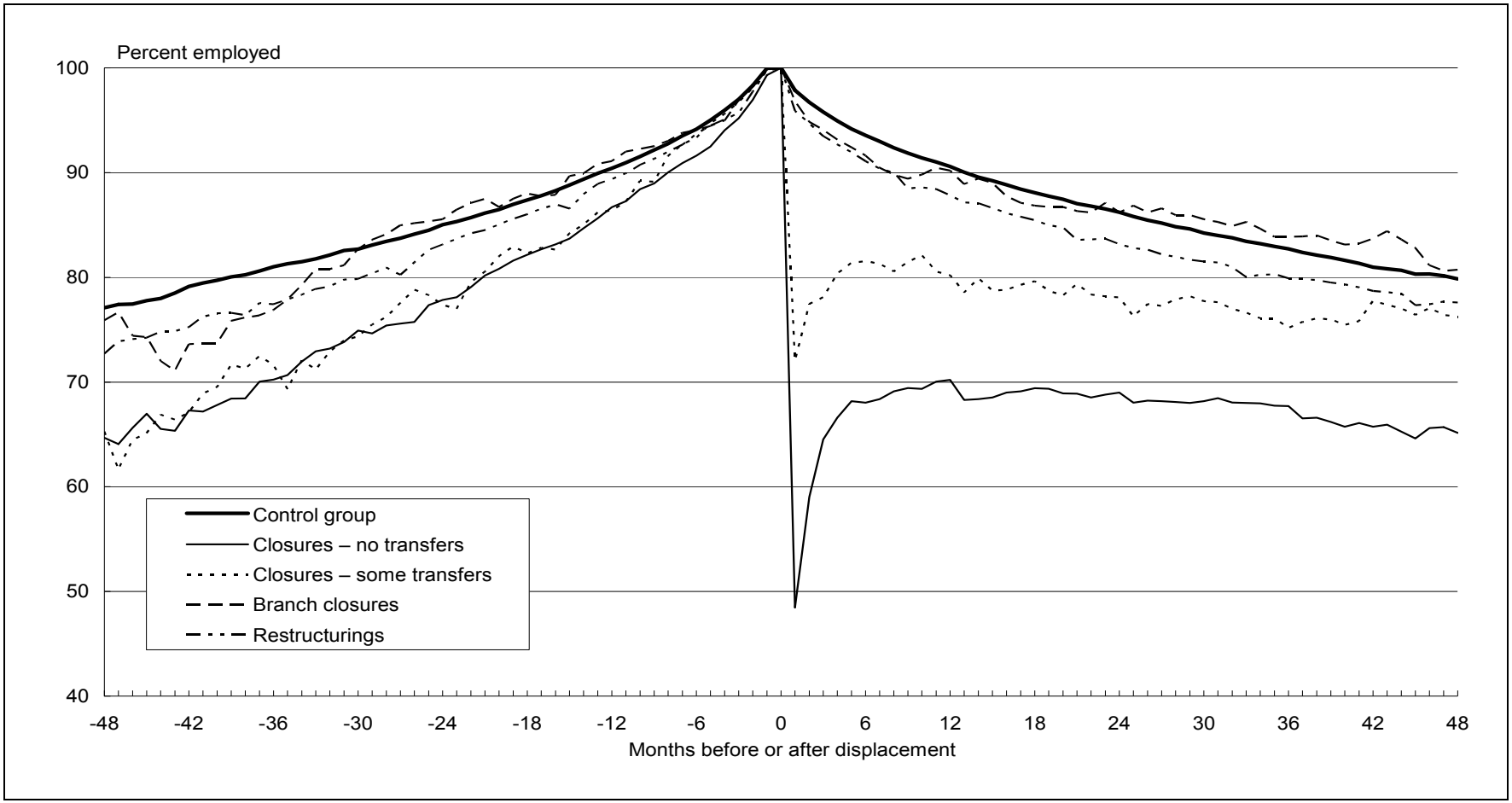
Figure 1

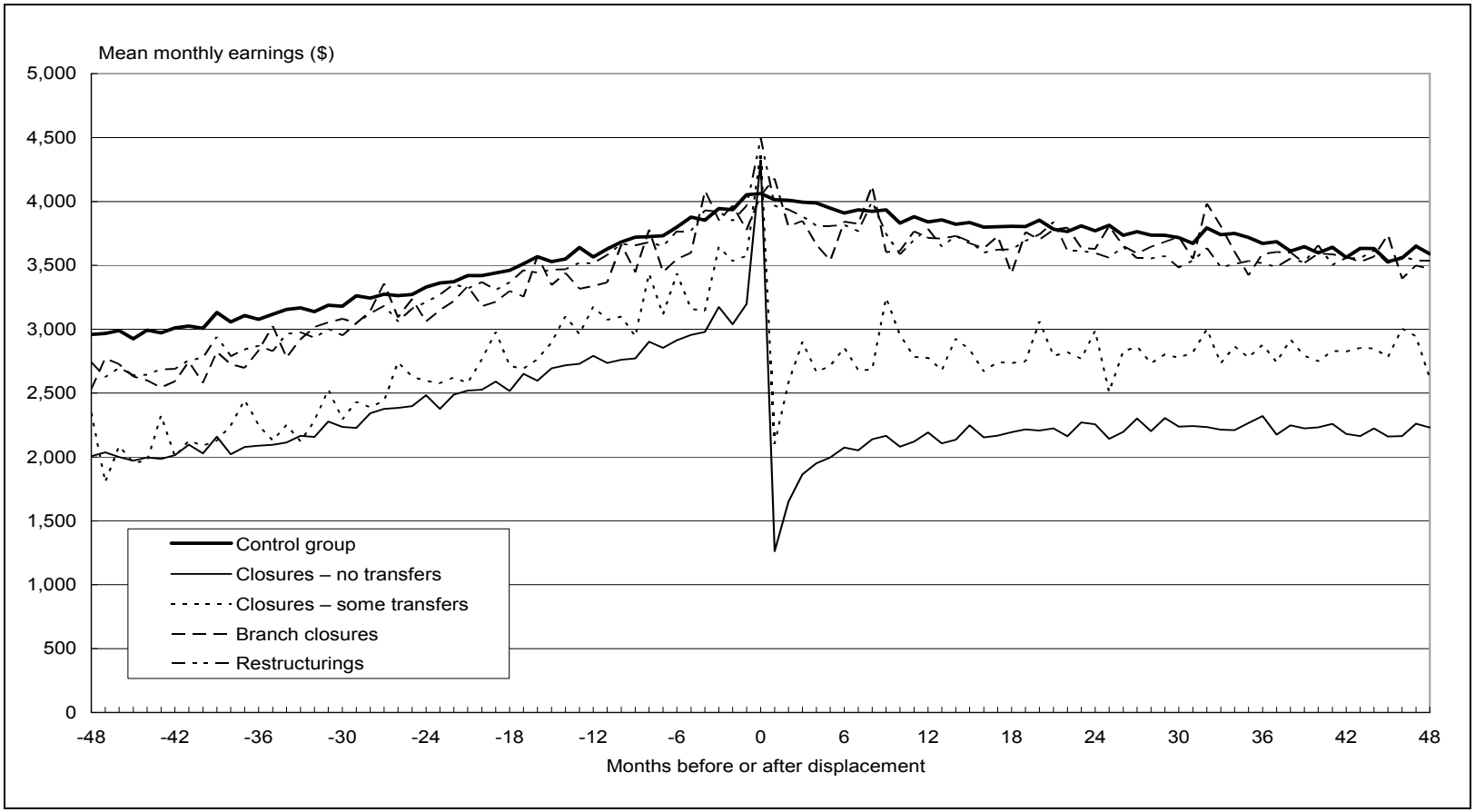
Employment rates before and after displacement, for workers who were employed until the firm closed or restructured



Notes: The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation. The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months. 'Branch closures' are exits of establishments belonging to continuing enterprises, in which there were no employee group transfers. The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Figure 2

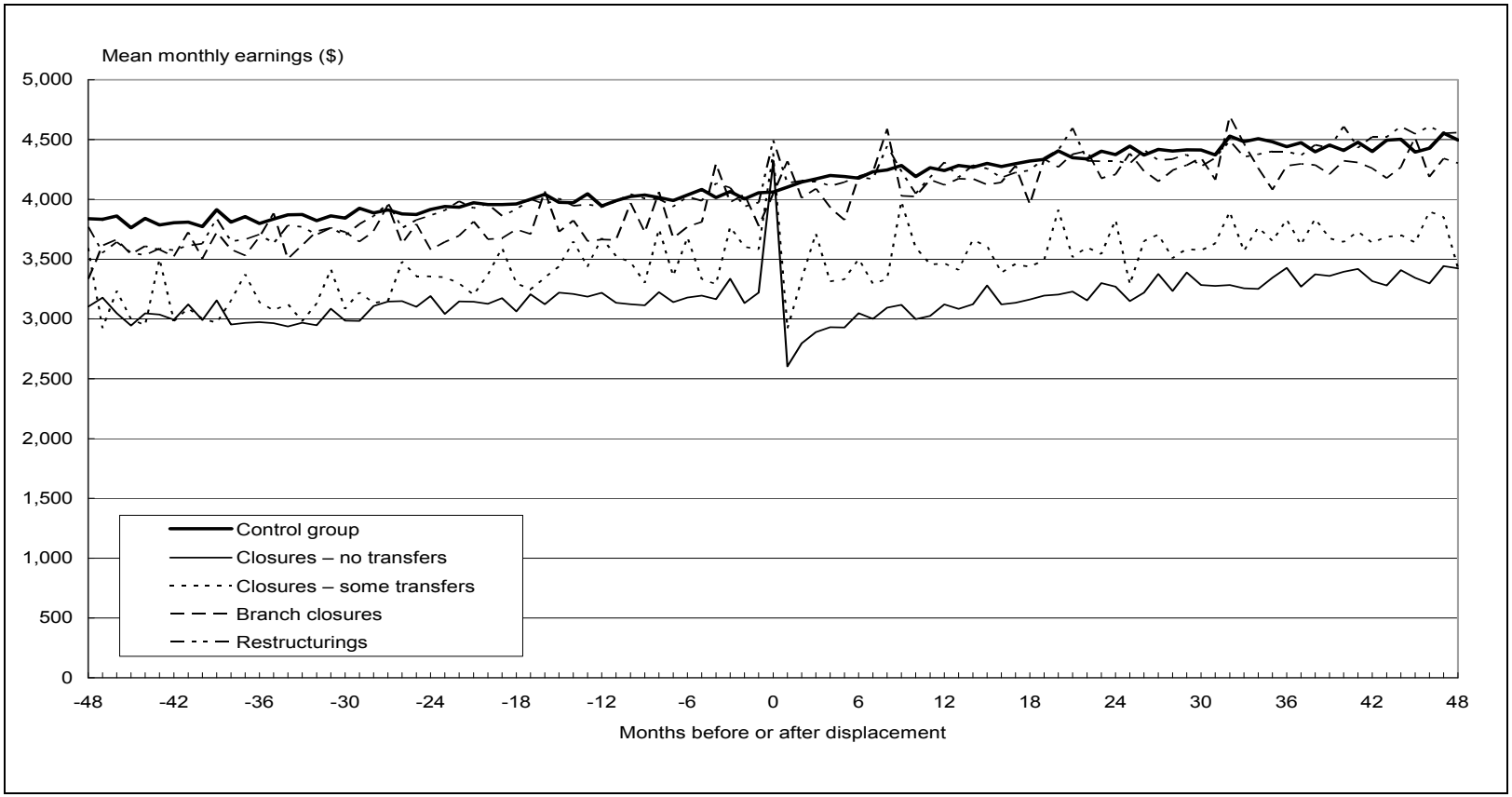
Total monthly earnings before and after displacement, for workers who were employed until the death of the firm



Notes: The monthly earnings averages for each group include data for employees whose earnings were zero in that month. The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation. The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months. 'Branch closures' are exits of establishments belonging to continuing enterprises, in which there were no employee group transfers. The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Figure 3

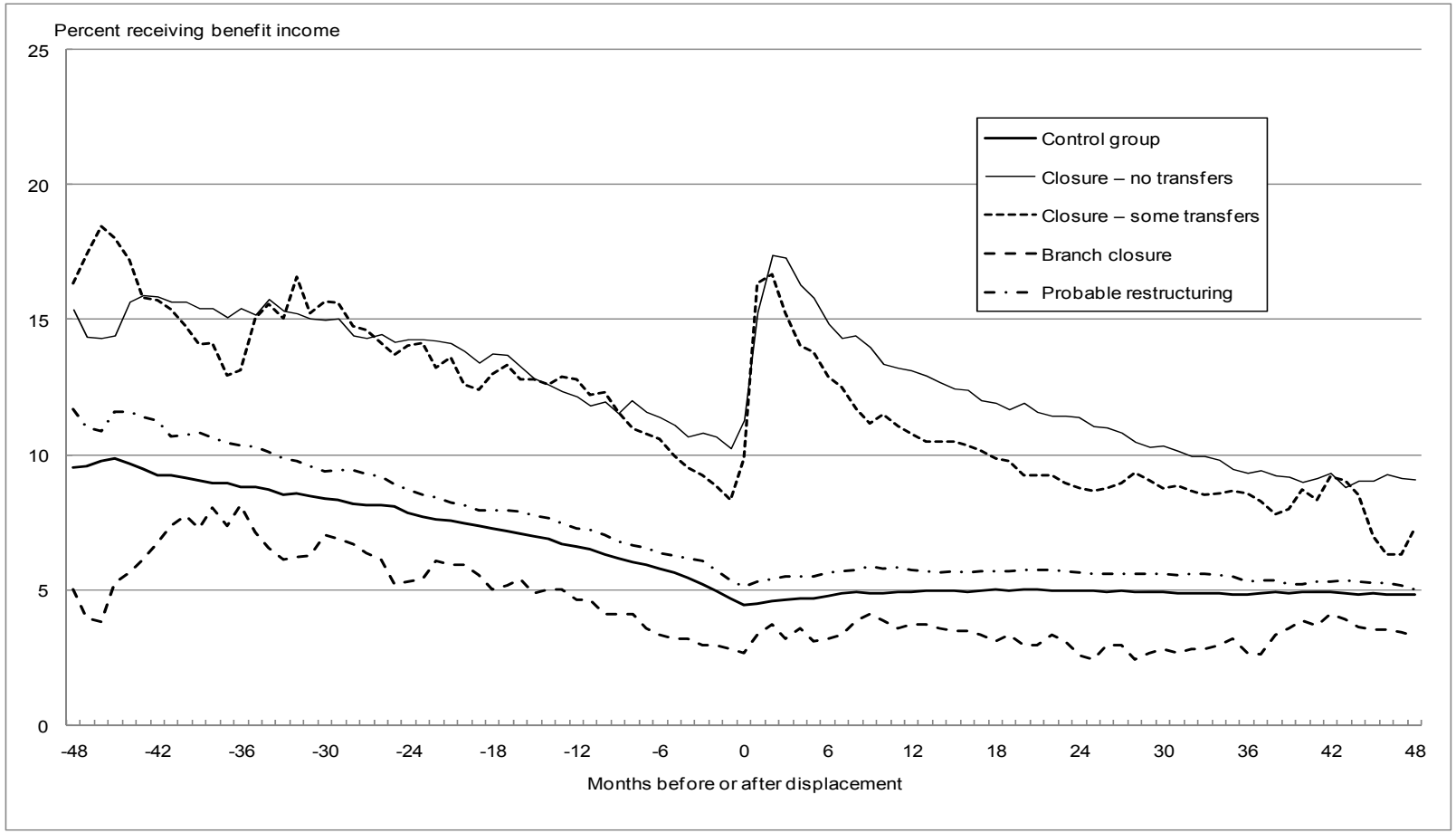
Total monthly earnings before and after displacement, for workers who were employed in each month



Notes: Only employees with positive earnings are included in the calculation of the monthly earnings averages for each group. The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation. The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months. 'Branch closures' are exits of establishments belonging to continuing enterprises, in which there were no employee group transfers. The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Figure 4

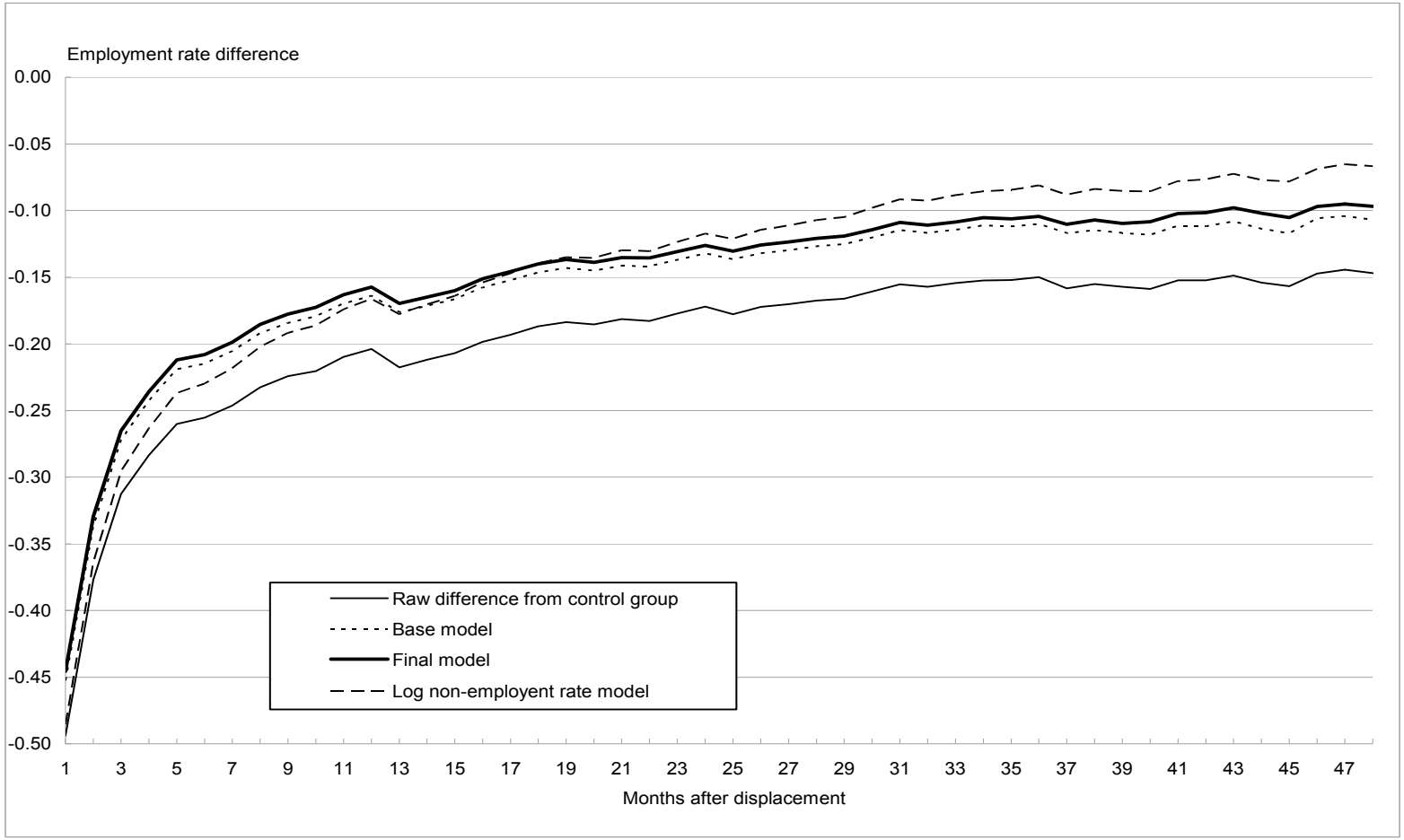
Receipt of benefit income before and after displacement, for workers who were employed until the death of the firm



Notes: 'Receipt of benefit income' means income was received from one of the main working age benefits during the reference month. The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation. The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months. 'Branch closures' are exits of establishments belonging to continuing enterprises, in which there were no employee group transfers. The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Figure 5

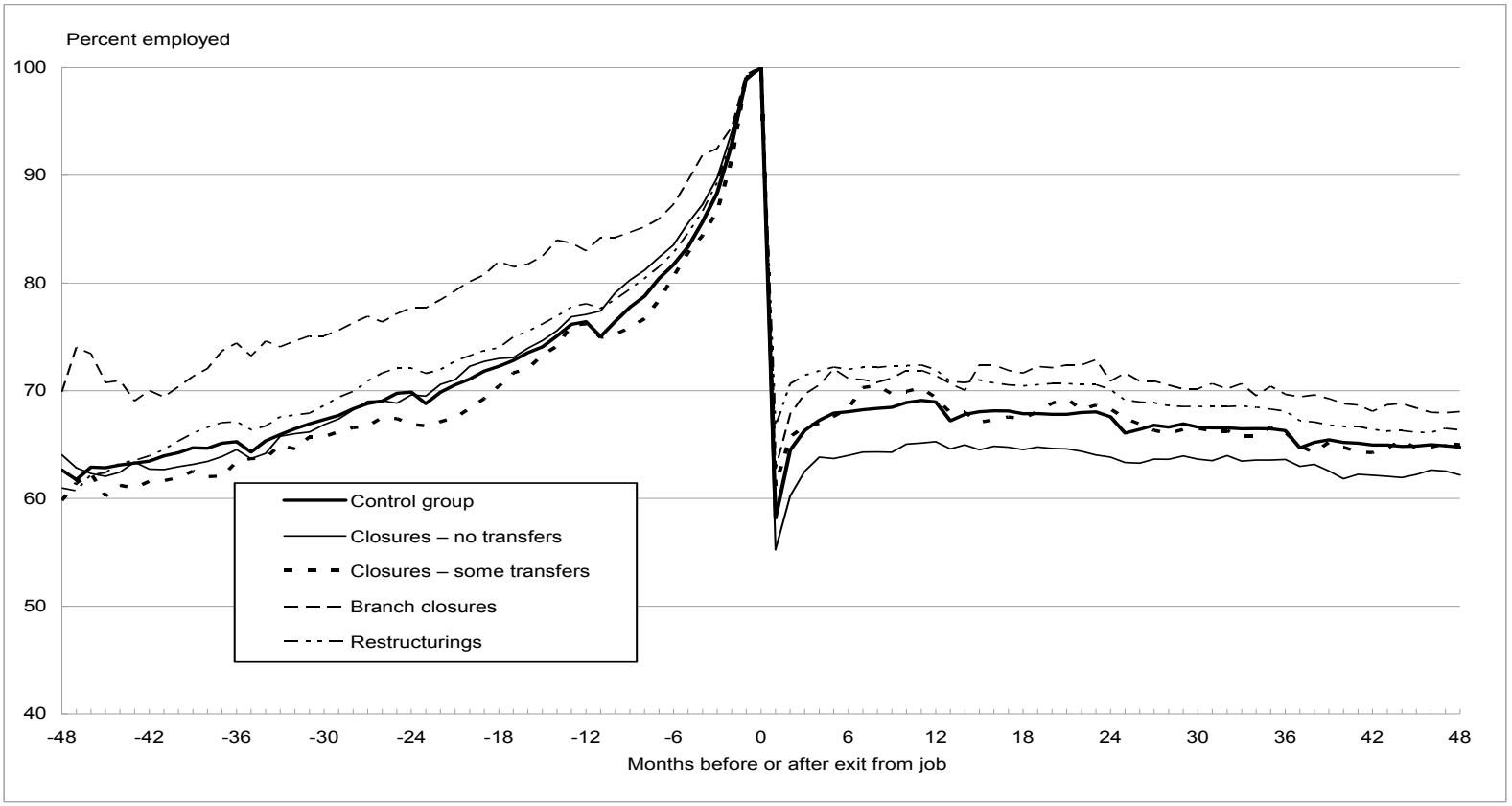
Alternative model specification: Impact of displacement on the employment rates of workers in the 'closure – no transfers' sub-sample



Notes: The base model contains dummy variables for month, year and whether displaced. The final model is estimated with person-specific fixed effects and corresponds to model (4) in table A1. The log non-employment model plot line is calculated from the estimation of an alternative model in which the dependent variable is the log of the non-employment rate.

Figure 6

Employment rates of early leavers before and after their exit from a firm that later closed or restructured



Notes: Early leavers are employees who left a closing or restructuring firm 1–12 months prior to the event. The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation. The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months. 'Branch closures' are exits of establishments belonging to continuing enterprises, in which there were no employee group transfers. The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Table 1

Firms that exited from LEED between April 2001 and March 2004, by type of event

Type of event	2002	2003	2004	2002-04
		Percent of firms		
Group 1: Closure – no transfers[1]	24.0	22.2	26.9	24.4
Group 2: Closure – some transfers[2]	4.2	5.1	5.4	4.8
Group 3: Branch closure[3]	5.2	5.6	5.4	5.5
Group 4: Probable restructuring[4]	66.7	67.2	62.3	65.3
		Number of firms		
Group 1: Closure – no transfers[1]	291	264	312	870
Group 2: Closure – some transfers[2]	51	60	63	171
Group 3: Branch closure[3]	63	66	63	195
Group 4: Probable restructuring[4]	810	798	723	2,328
Total	1,216	1,188	1,161	3,564

(1) The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation.
(2) The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months.
(3) 'Branch closures' are exits of establishments belonging to continuing enterprises, with no employee group transfers.
(4) The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Table 2

Characteristics of firms that exited from LEED and control group firms

Percentages unless noted	All firms that exited from LEED	Firms in the control group	Firms that exited from LEED			
			Closure – no transfers[1]	Closure – some transfers[2]	Branch closure[3]	Probable restruct-uring[4]
Establishment size						
Mean number of employees	20.3	28.3	11.2	23.8	8.1	24.4
Geometric mean number of employees	12.6	15.7	9.2	16.5	7.5	14.6
Less than 10 employees	43.6	34.6	61.9	20.7	82.8	35.3
10 to less than 20 employees	33.5	33.2	29.9	41.4	15.6	35.7
20 to less than 50 employees	17.0	20.9	6.9	27.6	1.6	21.3
50 to less than 100 employees	3.6	6.9	0.7	6.9	0.0	4.6
100 or more employees	2.3	4.5	0.7	3.5	0.0	3.1
Enterprise type (at month of death)						
Multiple-establishment enterprise	44.3	37.3	4.5	17.2	100.0	56.5
Enterprise size						
Mean number of employees	383.7	482.9	13.0	44.1	890.2	505.3
Geometric mean number of employees	35.9	42.1	9.5	24.7	219.1	52.0
Industry						
Agriculture/Forestry/Fishing	6.5	3.9	7.6	10.3	1.6	6.0
Mining	0.3	0.2	0.0	0.0	0.0	0.3
Manufacturing	10.7	12.9	10.1	12.1	3.3	11.4
Electricity/Gas/Water	0.6	0.3	0.4	0.0	0.0	0.8
Construction	6.4	6.0	6.6	5.2	1.6	6.8
Wholesale Trade	9.8	9.2	7.6	8.6	11.5	10.5
Retail Trade	15.5	16.7	15.3	19.0	41.0	13.5
Hospitality	6.7	6.7	12.5	8.6	1.6	5.0
Transport/Storage	5.1	4.4	3.8	8.6	6.6	5.0
Communication	1.4	0.8	1.0	0.0	1.6	1.5
Financial services	4.6	3.2	1.0	1.7	13.1	5.3
Business services	16.1	11.4	15.6	12.1	6.6	17.4
Government	1.2	2.7	0.0	0.0	1.6	1.7
Education	4.0	6.9	6.3	8.6	3.3	3.1
Health/Community services	6.1	7.8	5.6	3.5	4.9	6.6
Cultural/Recreational services	1.9	2.6	3.1	1.7	0.0	1.7
Personal/Household services	3.2	4.2	3.5	0.0	1.6	3.5
Year of death						
2002	34.2	31.9	33.8	29.8	32.3	34.8
2003	33.3	33.2	30.3	33.3	33.9	34.2
2004	32.6	35.0	35.9	36.8	33.9	31.0
Number of firms	3,564	8,013	870	171	195	2,328

(1) The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation.
(2) The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months.
(3) 'Branch closures' are exits of establishments belonging to continuing enterprises, with no employee group transfers.
(4) The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.
Notes: Firm characteristics are measured at six months prior to the death, or the matching month for control group firms, unless otherwise noted.

Table 3

Workers in firms that exited from LEED, by type of event

Type of event	Left at closure or restructuring	Left 1–12 months prior
	Number of workers	
Group 1: Closure – no transfers[1]	3,486	5,337
Group 2: Closure – some transfers[2]	1,095	2,739
Group 3: Branch closure[3]	774	810
Group 4: Probable restructuring[4]	34,290	25,365
All firms that exited from LEED	39,645	34,251
Control group firms	136,986	63,069
	Percent	
Group 1: Closure – no transfers[1]	8.8	15.6
Group 2: Closure – some transfers[2]	2.8	8.0
Group 3: Branch closure[3]	2.0	2.4
Group 4: Probable restructuring[4]	86.5	74.1
All workers at firms that exited from LEED	100.0	100.0

(1) The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation.
(2) The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months.
(3) 'Branch closures' are exits of establishments belonging to continuing enterprises, with no employee group transfers.
(4) The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Table 4

Characteristics of workers who were displaced in the final month of the firm's operation

Sample characteristics	All displaced workers	Control group workers	Displaced workers			
			Closure – no transfers[1]	Closure – some transfers[2]	Branch closure[3]	Probable restruct-uring[4]
Female (%)	50.8	48.1	49.8	49.0	54.7	50.9
Mean age at displacement (years)	40.9	41.6	40.8	40.9	40.9	40.9
Employment during two years prior to closure or matching month						
Months employed (%)	89.4	90.8	86.7	87.4	91.2	89.6
Mean monthly earnings ($)	3,470	3,630	2,750	3,020	3,470	3,560
Mean monthly earnings if employed ($)	3,890	3,990	3,170	3,460	3,800	3,970
Benefit receipt during two years prior to closure or matching month						
Received income from one of the main working-age benefits (mean monthly rate)	7.8	6.5	12.4	11.9	4.5	7.3
Received benefit income and was not employed during the month (mean monthly rate)	2.5	2.2	3.8	3.7	1.2	2.3
Job characteristics in month prior to closure or matching month						
Mean job tenure (months)	20.8	25.7	22.6	22.8	22.4	20.5
Mean monthly earnings ($)	3,890	4,050	3,200	3,570	3,780	3,970
Industry						
Agriculture/Forestry/Fishing	5.9	2.0	6.4	15.9	1.9	5.6
Mining	0.2	0.2	0.3	0.0	0.8	0.3
Manufacturing	12.8	16.9	10.7	7.4	2.3	13.4
Electricity/Gas/Water	1.1	0.8	0.3	0.0	1.6	1.2
Construction	4.5	4.7	6.0	3.0	1.9	4.5
Wholesale Trade	7.4	7.2	5.8	5.8	13.2	7.5
Retail Trade	6.9	7.6	10.2	5.2	28.7	6.1
Hospitality	3.1	3.3	11.2	5.2	0.4	2.2
Transport/Storage	3.9	4.7	4.0	6.9	5.8	3.8
Communication	1.5	1.4	0.7	0.6	1.9	1.7
Financial services	5.4	3.0	1.2	0.8	19.0	5.6
Business services	22.2	15.1	16.1	8.5	7.0	23.6
Government	2.5	6.2	0.0	0.0	3.9	2.8
Education	8.7	10.6	12.2	31.0	3.9	7.7
Health/Community services	10.7	9.9	7.1	9.9	5.0	11.2
Cultural/Recreational services	1.1	2.7	3.5	0.0	1.2	0.9
Personal/Household services	2.2	3.7	4.6	0.0	1.6	2.1

Table 4 *continued*

Characteristics of workers who were displaced in the final month of the firm's operation

Sample characteristics	All displaced workers	Control group workers	Displaced workers			
			Closure – no transfers[1]	Closure – some transfers[2]	Branch closure[3]	Probable restruct-uring[4]
Establishment size (6 months prior to closure)						
Less than 10 employees	15.6	8.9	44.0	9.1	74.1	11.6
10 to less than 20 employees	23.7	15.2	32.5	24.5	23.6	22.8
20 to less than 50 employees	24.9	21.7	15.3	40.2	2.3	25.9
50 to less than 100 employees	11.8	16.5	5.3	7.4	0.0	12.8
100 or more employees	24.1	37.8	2.8	18.7	0.0	26.9
Indicators of job separation						
Ratio earnings in month of closure to mean monthly earnings in previous 12 months	1.14	1.01	1.36	1.24	1.05	1.12
Changed enterprise within 2 months of firm closure (%)	53.6	8.4	100.0	100.0	16.0	48.3
Number of workers	39,645	136,986	3,486	1,095	774	34,290

(1) The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation. (2) The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months. (3) 'Branch closures' are exits of establishments belonging to continuing enterprises, with no employee group transfers. (4) The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Notes: All characteristics are measured in the month that the firm died or the reference month for the control group. Earnings are measured in March quarter 2007 dollar values. The sample is restricted to individuals aged 25-64 at the time of the closure or restructuring event.

Table 5

Characteristics of workers who left exiting firms 1-12 months prior to a closure or restructuring

Sample characteristics	All displaced workers	Control group workers	Displaced workers			
			Closure – no transfers[1]	Closure – some transfers[2]	Branch closure[3]	Probable restruct-uring[4]
Female (%)	49.2	53.2	45.6	46.7	57.4	50.0
Mean age at displacement (years)	39.0	39.0	38.8	38.8	39.5	39.0
Employment during two years prior to leaving job						
Months employed (%)	79.0	77.6	78.7	76.1	84.7	79.3
Mean monthly earnings ($)	2,800	2,430	2,790	2,660	2,930	2,810
Mean monthly earnings if employed ($)	3,540	3,140	3,550	3,490	3,460	3,540
Benefit receipt during two years prior to leaving job						
Received income from one of the main working-age benefits (mean monthly rate)	14.0	15.3	16.6	18.1	10.3	13.1
Received benefit income and was not employed during the month (mean monthly rate)	5.8	6.2	7.1	8.1	3.5	5.4
Job characteristics in month prior to leaving job						
Mean job tenure (months)	15.1	13.9	16.0	14.0	17.7	14.9
Mean monthly earnings ($)	3,470	2,940	3,590	3,570	3,910	3,420
Industry						
Agriculture/Forestry/Fishing	9.5	5.3	9.6	16.6	1.9	8.9
Mining	0.2	0.2	0.1	0.0	0.4	0.3
Manufacturing	13.6	13.2	9.9	14.8	8.2	14.4
Electricity/Gas/Water	1.2	0.5	0.6	0.0	0.4	1.4
Construction	5.8	4.1	8.8	3.1	1.1	5.6
Wholesale Trade	6.3	5.8	5.6	5.7	10.8	6.4
Retail Trade	7.8	8.3	8.4	9.5	32.1	6.7
Hospitality	4.8	6.6	10.6	6.0	1.5	3.6
Transport/Storage	5.1	4.3	4.3	11.3	6.7	4.5
Communication	0.8	1.0	0.5	0.6	1.1	0.9
Financial services	3.4	2.2	0.8	1.4	10.1	3.9
Business services	25.6	18.4	19.3	18.0	10.1	28.2
Government	1.7	5.1	0.0	0.0	1.9	2.3
Education	5.4	11.8	8.5	10.4	3.7	4.2
Health/Community services	6.5	8.4	7.6	2.4	7.8	6.7
Cultural/Recreational services	1.1	2.3	3.3	0.1	1.9	0.7
Personal/Household services	1.3	2.7	2.4	0.1	0.4	1.2

Table 5 *continued*

Characteristics of workers who left exiting firms 1-12 months prior to a closure or restructuring

Sample characteristics	All displaced workers	Control group workers	Displaced workers			
			Closure – no transfers[1]	Closure – some transfers[2]	Branch closure[3]	Probable restruct-uring[4]
Establishment size (6 months prior to closure)						
Less than 10 employees	14.4	8.5	35.4	7.2	67.2	9.1
10 to less than 20 employees	21.4	15.2	32.8	21.0	20.3	19.1
20 to less than 50 employees	23.4	22.7	18.4	34.1	12.6	23.6
50 to less than 100 employees	13.2	17.9	6.6	19.6	0.0	14.3
100 or more employees	27.6	35.7	6.8	18.1	0.0	33.9
Indicators of job separation						
Ratio earnings in final month of employment to mean monthly earnings in previous 12 months	1.52	1.22	1.46	1.84	1.68	1.50
Changed enterprise within 2 months of leaving the job (%)	92.3	92.8	99.7	99.1	84.5	90.3
Number of workers	34,251	63,069	5,337	2,739	810	25,365

(1) The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation. (2) The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months. (3) 'Branch closures' are exits of establishments belonging to continuing enterprises, with no employee group transfers. (4) The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Notes: All characteristics are measured in the month the worker left the firm. Earnings are measured in March quarter 2007 dollar values. The sample is restricted to individuals aged 25-64 at the time of the closure or restructuring event.

Table 6

Regression estimates of the effect of being displaced on future employment

Impact of displacement after:	Mean employment rate for control group	All displaced workers	Displaced workers			
			Closure – no transfers[1]	Closure – some transfers[2]	Branch closure[3]	Probable restruct-uring[4]
		Estimated employment rate impact, in percentage points				
1 month	97.9	-0.052	-0.446	-0.213	-0.018	-0.007
		(0.002)	(0.005)	(0.008)	(0.009)	(0.002)
2 months	96.7	-0.040	-0.329	-0.149	-0.026	-0.007
		(0.002)	(0.005)	(0.008)	(0.009)	(0.002)
3 months	95.8	-0.037	-0.265	-0.133	-0.023	-0.010
		(0.002)	(0.005)	(0.008)	(0.009)	(0.002)
4 months	94.9	-0.032	-0.236	-0.103	-0.023	-0.009
		(0.002)	(0.005)	(0.008)	(0.009)	(0.002)
5 months	94.2	-0.030	-0.212	-0.086	-0.024	-0.010
		(0.002)	(0.005)	(0.008)	(0.009)	(0.002)
6 months	93.6	-0.032	-0.208	-0.078	-0.027	-0.013
		(0.002)	(0.005)	(0.008)	(0.009)	(0.002)
12 months	90.6	-0.029	-0.157	-0.062	-0.010	-0.015
		(0.002)	(0.005)	(0.008)	(0.009)	(0.002)
24 months	86.2	-0.028	-0.126	-0.039	-0.007	-0.018
		(0.002)	(0.005)	(0.008)	(0.009)	(0.002)
36 months	82.7	-0.024	-0.104	-0.033	0.004	-0.016
		(0.002)	(0.005)	(0.008)	(0.009)	(0.002)
48 months	79.8	-0.024	-0.097	-0.009	0.000	-0.017
		(0.002)	(0.006)	(0.011)	(0.011)	(0.002)
		Percentage difference from control group's mean employment rate				
1 month		-5.3	-45.5	-21.8	-1.9	-0.7
2 months		-4.1	-34.1	-15.4	-2.7	-0.8
3 months		-3.8	-27.7	-13.9	-2.5	-1.1
4 months		-3.4	-24.8	-10.9	-2.4	-1.0
5 months		-3.2	-22.5	-9.2	-2.6	-1.0
6 months		-3.4	-22.2	-8.3	-2.8	-1.4
12 months		-3.2	-17.4	-6.8	-1.1	-1.7
24 months		-3.3	-14.6	-4.5	-0.8	-2.1
36 months		-2.9	-12.6	-4.0	0.5	-2.0
48 months		-3.0	-12.1	-1.1	0.0	-2.1
N displaced persons		39,645	3,486	1,095	774	34,290
N control group		136,986	136,986	136,986	136,986	136,986

(1) The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation.
(2) The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months.
(3) 'Branch closures' are exits of establishments belonging to continuing enterprises, with no employee group transfers.
(4) The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Note: Standard errors are in parentheses.

Table 7

Regression estimates of the effect of being displaced on future monthly earnings (full sample)

Impact of displacement after:	Mean earnings of control group ($)	All displaced workers	Displaced workers			
			Closure – no transfers[1]	Closure – some transfers[2]	Branch closure[3]	Probable restruct-uring[4]
			Estimated displacement effect in $			
1 month	4,020	-148.7	-1838.9	-1238.1	334.1	47.6
		(17.0)	(49.8)	(88.4)	(105.1)	(18.0)
2 months	4,020	-165.2	-1484.1	-837.7	-62.0	-12.0
		(17.0)	(49.8)	(88.4)	(105.1)	(18.0)
3 months	4,000	-167.8	-1264.0	-428.0	15.1	-52.1
		(17.0)	(49.8)	(88.4)	(105.1)	(18.0)
4 months	4,000	-211.3	-1155.8	-658.8	-102.4	-103.4
		(17.0)	(49.8)	(88.4)	(105.1)	(18.0)
5 months	3,960	-166.9	-1065.4	-648.2	-229.6	-58.6
		(17.0)	(49.8)	(88.4)	(105.1)	(18.0)
6 months	3,920	-122.0	-946.0	-439.0	90.8	-33.0
		(17.0)	(49.8)	(88.4)	(105.1)	(18.0)
12 months	3,850	-72.2	-827.4	-504.7	71.5	15.1
		(17.0)	(49.8)	(88.4)	(105.1)	(18.0)
24 months	3,780	-160.6	-698.7	-229.1	47.4	-108.6
		(17.0)	(49.8)	(88.4)	(105.1)	(18.0)
36 months	3,680	-132.1	-541.0	-242.2	103.5	-92.3
		(17.0)	(49.8)	(88.4)	(105.1)	(18.0)
48 months	3,600	-183.7	-559.2	-457.8	-57.8	-140.7
		(20.1)	(61.3)	(123.3)	(121.3)	(21.1)
		Percentage difference from control group's mean earnings				
1 month		-3.7	-45.7	-30.8	8.3	1.2
2 months		-4.1	-37.0	-20.9	-1.5	-0.3
3 months		-4.2	-31.6	-10.7	0.4	-1.3
4 months		-5.3	-28.9	-16.5	-2.6	-2.6
5 months		-4.2	-26.9	-16.4	-5.8	-1.5
6 months		-3.1	-24.1	-11.2	2.3	-0.8
12 months		-1.9	-21.5	-13.1	1.9	0.4
24 months		-4.2	-18.5	-6.1	1.3	-2.9
36 months		-3.6	-14.7	-6.6	2.8	-2.5
48 months		-5.1	-15.5	-12.7	-1.6	-3.9
N displaced persons		39,645	3,486	1,095	774	34,290
N control group		136,986	136,986	136,986	136,986	136,986

(1) The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation.
(2) The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months.
(3) 'Branch closures' are exits of establishments belonging to continuing enterprises, with no employee group transfers.
(4) The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Note: Standard errors are in parentheses.

Table 8

Regression estimates of the effect of being displaced on future monthly earnings, for workers who were employed afterwards

Impact of displacement after:	Mean earnings of control group, if working ($)	All displaced workers	Displaced workers			
			Closure – no transfers[1]	Closure – some transfers[2]	Branch closure[3]	Probable restruct-uring[4]
		Estimated displacement effect in log points				
1 month	4,110	-0.028	-0.360	-0.277	0.037	-0.005
		(0.003)	(0.010)	(0.015)	(0.016)	(0.003)
2 months	4,150	-0.021	-0.259	-0.185	-0.020	-0.001
		(0.003)	(0.009)	(0.015)	(0.016)	(0.003)
3 months	4,180	-0.027	-0.207	-0.058	-0.041	-0.012
		(0.003)	(0.009)	(0.015)	(0.016)	(0.003)
4 months	4,210	-0.036	-0.206	-0.130	0.000	-0.022
		(0.003)	(0.009)	(0.014)	(0.016)	(0.003)
5 months	4,200	-0.021	-0.168	-0.093	-0.023	-0.007
		(0.003)	(0.009)	(0.014)	(0.016)	(0.003)
6 months	4,190	-0.001	-0.121	-0.108	-0.008	0.012
		(0.003)	(0.009)	(0.014)	(0.016)	(0.003)
12 months	4,250	0.005	-0.101	-0.122	-0.044	0.019
		(0.003)	(0.009)	(0.014)	(0.016)	(0.003)
24 months	4,380	-0.007	-0.050	-0.024	-0.001	-0.003
		(0.003)	(0.009)	(0.015)	(0.016)	(0.003)
36 months	4,450	0.006	-0.010	-0.013	0.030	0.007
		(0.003)	(0.009)	(0.015)	(0.017)	(0.003)
48 months	4,510	-0.002	-0.004	-0.093	-0.002	0.001
		(0.003)	(0.011)	(0.021)	(0.020)	(0.003)
		Percentage difference from control group's mean earnings				
1 month		-2.8	-30.2	-24.2	3.8	-0.5
2 months		-2.1	-22.8	-16.9	-2.0	-0.1
3 months		-2.6	-18.7	-5.7	-4.0	-1.2
4 months		-3.5	-18.6	-12.2	0.0	-2.1
5 months		-2.0	-15.5	-8.8	-2.3	-0.7
6 months		-0.1	-11.4	-10.2	-0.8	1.2
12 months		0.5	-9.6	-11.5	-4.3	1.9
24 months		-0.7	-4.9	-2.4	-0.1	-0.3
36 months		0.6	-1.0	-1.3	3.0	0.7
48 months		-0.2	-0.4	-8.8	-0.2	0.1
Sample sizes at 1 month after displacement						
N displaced persons		36,111	1,689	789	750	32,880
N control group		134,049	134,049	134,049	134,049	134,049
Sample sizes at 24 months after displacement						
N displaced persons		32,442	2,406	855	669	28,512
N control group		118,095	118,095	118,095	118,095	118,095

(1) The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation. (2) The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months. (3) 'Branch closures' are exits of establishments belonging to continuing enterprises, with no employee group transfers. (4) The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Note: Standard errors are in parentheses.

Table 9

Regression estimates of the effect of being displaced on future benefit receipt

Impact of displacement after:	Mean benefit receipt rate for control group	All displaced workers	Displaced workers			
			Closure – no transfers[1]	Closure – some transfers[2]	Branch closure[3]	Probable restruct-uring[4]
1 month	4.5	0.007	0.048	0.061	0.009	0.001
		(0.001)	(0.003)	(0.005)	(0.006)	(0.001)
2 months	4.6	0.009	0.068	0.064	0.011	0.001
		(0.001)	(0.003)	(0.005)	(0.006)	(0.001)
3 months	4.7	0.009	0.067	0.049	0.005	0.002
		(0.001)	(0.003)	(0.005)	(0.006)	(0.001)
4 months	4.7	0.007	0.056	0.037	0.009	0.001
		(0.001)	(0.003)	(0.005)	(0.006)	(0.001)
5 months	4.7	0.006	0.051	0.035	0.004	0.001
		(0.001)	(0.003)	(0.005)	(0.006)	(0.001)
6 months	4.8	0.006	0.041	0.024	0.004	0.002
		(0.001)	(0.003)	(0.005)	(0.006)	(0.001)
12 months	4.9	0.003	0.022	0.002	0.008	0.001
		(0.001)	(0.003)	(0.005)	(0.006)	(0.001)
24 months	5.0	0.000	0.004	-0.019	-0.004	0.000
		(0.001)	(0.003)	(0.005)	(0.006)	(0.001)
36 months	4.8	-0.003	-0.015	-0.019	-0.001	-0.002
		(0.001)	(0.003)	(0.005)	(0.006)	(0.001)
48 months	4.8	-0.004	-0.020	-0.035	0.008	-0.002
		(0.001)	(0.003)	(0.007)	(0.006)	(0.001)
		Percentage difference from control group's benefit receipt rate				
1 month		16.3	107.8	136.6	19.1	3.0
2 months		20.0	148.7	139.0	24.9	2.9
3 months		19.2	143.5	105.1	11.7	3.9
4 months		15.6	119.9	79.4	18.9	2.8
5 months		13.7	108.3	73.5	7.6	2.3
6 months		12.3	84.4	50.6	8.6	3.7
12 months		6.3	44.8	3.4	15.7	2.2
24 months		-0.1	9.0	-37.3	-8.0	0.3
36 months		-7.0	-30.5	-39.7	-2.4	-3.8
48 months		-7.3	-42.0	-72.0	16.7	-3.4
N displaced persons		39,645	3,486	1,095	774	34,290
N control group		136,986	136,986	136,986	136,986	136,986

(1) The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation. (2) The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months. (3) 'Branch closures' are exits of establishments belonging to continuing enterprises, with no employee group transfers. (4) The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Notes: Standard errors are in parentheses. Benefit receipt refers to receipt of one of the main working-age benefits, including the unemployment, domestic purposes, invalids' and sickness benefits.

Table 10

Regression estimates of the effects of being displaced on future employment, for workers in the 'closure – no transfers' sub-sample

Impact of Displacement:	6 months after	12 months after	24 months after	36 months after	48 months after	N workers at exiting firms	N control group workers
Gender							
Male	-0.175	-0.142	-0.118	-0.090	-0.074	2,304	71,085
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)		
Female	-0.181	-0.128	-0.095	-0.086	-0.095	2,274	65,904
	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)		
Age group							
25–34	-0.270	-0.228	-0.168	-0.144	-0.129	564	18,012
	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)		
35–54	-0.173	-0.126	-0.090	-0.078	-0.071	2,475	78,570
	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)		
55–64	-0.152	-0.112	-0.102	-0.075	-0.076	1,545	40,404
	(0.007)	(0.007)	(0.007)	(0.007)	(0.009)		
Job tenure prior to displacement							
< 1 Year	-0.138	-0.079	-0.057	-0.048	-0.030	1,476	37,758
	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)		
1–<2 Years	-0.133	-0.100	-0.074	-0.064	-0.083	954	23,100
	(0.009)	(0.009)	(0.009)	(0.009)	(0.012)		
2 or more Years	-0.255	-0.217	-0.179	-0.150	-0.139	2,154	76,125
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)		
Monthly earnings prior to displacement (quartile)							
< $1900	-0.242	-0.176	-0.144	-0.132	-0.139	1,878	28,437
	(0.007)	(0.007)	(0.007)	(0.007)	(0.009)		
$1900 – $3200	-0.175	-0.136	-0.125	-0.080	-0.072	1,131	34,287
	(0.007)	(0.007)	(0.007)	(0.007)	(0.009)		
$3200 – $4600	-0.149	-0.122	-0.079	-0.072	-0.077	819	36,051
	(0.007)	(0.007)	(0.007)	(0.007)	(0.009)		
>= $4600	-0.169	-0.152	-0.113	-0.111	-0.098	756	38,208
	(0.007)	(0.007)	(0.007)	(0.007)	(0.010)		
Firm size							
Firm Size 5–9 Employees	-0.227	-0.184	-0.139	-0.104	-0.090	1,422	13,407
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)		
Firm Size 10-19 Employees	-0.164	-0.115	-0.095	-0.078	-0.066	1,593	21,210
	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)		
Firm Size 20-49 Employees	-0.199	-0.156	-0.137	-0.128	-0.136	969	29,778
	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)		
Firm Size 50+ Employees	-0.074	-0.042	-0.012	-0.007	0.015	594	72,594
	(0.009)	(0.009)	(0.009)	(0.009)	(0.014)		
Firm structure							
Single-unit enterprise	-0.197	-0.152	-0.123	-0.103	-0.097	4,164	56,187
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)		
Multi-unit enterprise	-0.084	-0.052	-0.024	-0.029	-0.049	417	80,799
	(0.011)	(0.011)	(0.011)	(0.011)	(0.018)		

Note: Standard errors are in parentheses.

Table 10 *continued*

Regression estimates of the effects of being displaced on future employment, for workers in the 'closure – no transfers' sub-sample

Impact of Displacement:	6 months after	12 months after	24 months after	36 months after	48 months after	N workers at dying firms	N control group workers
Industry							
Agriculture, Forestry, Fishing	-0.172	-0.164	-0.153	-0.144	-0.084	396	2,700
	(0.015)	(0.015)	(0.015)	(0.015)	(0.020)		
Manufacturing	-0.213	-0.156	-0.095	-0.097	-0.088	453	23,193
	(0.010)	(0.010)	(0.010)	(0.010)	(0.012)		
Wholesale trade	-0.207	-0.140	-0.063	-0.065	0.000	264	9,813
	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)		
Retail trade	-0.225	-0.174	-0.139	-0.122	-0.111	414	10,422
	(0.012)	(0.012)	(0.012)	(0.012)	(0.016)		
Accommodation, Restaurants, Cafes	-0.237	-0.177	-0.138	-0.116	-0.110	447	4,581
	(0.014)	(0.014)	(0.014)	(0.014)	(0.018)		
Business services	-0.250	-0.195	-0.150	-0.137	-0.128	648	20,622
	(0.010)	(0.010)	(0.010)	(0.010)	(0.012)		
Education	-0.082	-0.051	-0.031	-0.039	-0.006	765	14,538
	(0.009)	(0.009)	(0.009)	(0.009)	(0.015)		
Health, Community Services	-0.131	-0.084	-0.064	-0.034	-0.102	351	13,593
	(0.013)	(0.013)	(0.013)	(0.013)	(0.017)		
Urban / rural location							
Five main urban centres	-0.224	-0.171	-0.133	-0.115	-0.106	2,136	89,337
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)		
Other major cities	-0.169	-0.119	-0.104	-0.076	-0.104	600	23,670
	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)		
Secondary and minor cities	-0.144	-0.098	-0.062	-0.037	-0.053	360	17,505
	(0.012)	(0.012)	(0.012)	(0.012)	(0.015)		
Rural locations	-0.174	-0.132	-0.129	-0.112	-0.062	318	6,282
	(0.014)	(0.014)	(0.014)	(0.014)	(0.018)		
Regional Council area							
Auckland	-0.274	-0.211	-0.166	-0.142	-0.140	1,332	47,160
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)		
Waikato	-0.200	-0.146	-0.104	-0.065	-0.039	246	9,222
	(0.015)	(0.015)	(0.015)	(0.015)	(0.018)		
Wellington	-0.203	-0.153	-0.103	-0.107	-0.061	444	21,921
	(0.011)	(0.011)	(0.011)	(0.011)	(0.014)		
Canterbury	-0.196	-0.158	-0.135	-0.131	-0.158	396	20,016
	(0.011)	(0.011)	(0.011)	(0.011)	(0.014)		
All other regions	-0.139	-0.100	-0.094	-0.062	-0.072	1,071	38,667
	(0.007)	(0.007)	(0.007)	(0.007)	(0.009)		

Note: Standard errors are in parentheses.

Table 11

Regression estimates of the effects of being displaced on future earnings, for workers in the 'closure – no transfers' sub-sample

Impact of Displacement:	6 months after	12 months after	24 months after	36 months after	48 months after	N workers at dying firms	N control group workers
Gender							
Male	-1017.9	-1012.4	-844.7	-694.3	-768.7	2,304	71,085
	(66.6)	(66.6)	(66.6)	(66.6)	(84.2)		
Female	-609.3	-462.1	-323.9	-235.3	-308.0	2,274	65,904
	(35.4)	(35.4)	(35.4)	(35.4)	(44.9)		
Age group							
25–34	-1047.6	-860.1	-493.8	-358.4	-336.4	564	18,012
	(114.7)	(114.7)	(114.7)	(114.7)	(145.3)		
35–54	-848.6	-807.7	-621.7	-503.5	-640.4	2,475	78,570
	(56.9)	(56.9)	(56.9)	(56.9)	(72.2)		
55–64	-670.3	-588.6	-533.5	-399.7	-386.1	1,545	40,404
	(49.3)	(49.3)	(49.3)	(49.3)	(62.2)		
Job tenure prior to displacement							
< 1 Year	-569.9	-518.8	-369.3	-238.6	-268.8	1,476	37,758
	(67.9)	(67.9)	(67.9)	(67.9)	(87.2)		
1–<2 Years	-675.8	-598.1	-360.5	-356.6	-401.7	954	23,100
	(86.3)	(86.3)	(86.3)	(86.3)	(113.1)		
2 or more Years	-1150.7	-1051.7	-916.6	-757.5	-811.7	2,154	76,125
	(54.6)	(54.6)	(54.6)	(54.6)	(67.6)		
Monthly earnings prior to displacement (quartile)							
< $1900	-376.0	-274.8	-231.5	-226.4	-226.6	1,878	28,437
	(31.4)	(31.4)	(31.4)	(31.4)	(38.7)		
$1900 – $3200	-669.7	-531.6	-504.1	-255.2	-322.3	1,131	34,287
	(41.5)	(41.5)	(41.5)	(41.5)	(50.9)		
$3200 – $4600	-809.0	-738.9	-457.7	-402.4	-646.2	819	36,051
	(61.0)	(61.0)	(61.0)	(61.0)	(77.4)		
>= $4600	-1924.3	-2104.5	-1831.3	-1708.4	-2542.2	756	38,208
	(153.9)	(154.0)	(154.0)	(154.1)	(223.0)		
Firm size							
Firm Size 5–9 Employees	-816.1	-752.8	-615.9	-483.6	-489.5	1,422	13,407
	(64.9)	(64.9)	(64.9)	(64.9)	(77.5)		
Firm Size 10-19 Employees	-731.8	-647.9	-511.0	-396.5	-429.4	1,593	21,210
	(62.7)	(62.7)	(62.7)	(62.7)	(83.2)		
Firm Size 20-49 Employees	-867.7	-797.3	-547.5	-493.5	-408.1	969	29,778
	(83.6)	(83.6)	(83.6)	(83.7)	(101.5)		
Firm Size 50+ Employees	-640.7	-652.8	-679.8	-530.7	-960.8	594	72,594
	(107.5)	(107.5)	(107.5)	(107.5)	(155.8)		
Firm structure							
Single-unit enterprise	-790.8	-715.7	-550.5	-457.6	-486.9	4,164	56,187
	(37.2)	(37.2)	(37.2)	(37.2)	(46.2)		
Multi-unit enterprise	-868.8	-689.9	-705.9	-535.8	-1114.0	417	80,799
	(132.4)	(132.4)	(132.4)	(132.4)	(214.4)		

Note: Standard errors are in parentheses.

Table 11 *continued*

Regression estimates of the effects of being displaced on future earnings, for workers in the 'closure – no transfers' sub-sample

Impact of Displacement:	6 months after	12 months after	24 months after	36 months after	48 months after	N workers at dying firms	N control group workers
Industry							
Agriculture, Forestry, Fishing	-703.5	-596.1	-614.6	-497.1	-203.1	396	2,700
	(108.4)	(108.3)	(108.5)	(108.8)	(144.4)		
Manufacturing	-1224.8	-947.2	-583.1	-413.3	-902.3	453	23,193
	(122.2)	(122.2)	(122.2)	(122.2)	(144.7)		
Wholesale trade	-1078.3	-937.9	-709.9	-756.6	-513.0	264	9,813
	(212.6)	(212.6)	(212.8)	(212.8)	(241.5)		
Retail trade	-661.2	-595.1	-370.5	-382.9	-199.5	414	10,422
	(83.7)	(83.7)	(83.7)	(83.7)	(107.1)		
Accommodation, Restaurants, Cafes	-623.9	-355.7	-294.7	-196.0	-130.7	447	4,581
	(76.5)	(76.5)	(76.5)	(76.5)	(95.3)		
Business services	-1167.4	-1252.7	-1050.4	-1084.2	-1060.0	648	20,622
	(126.9)	(126.9)	(126.9)	(126.9)	(152.0)		
Education	-533.5	-476.9	-382.1	-155.4	-311.4	765	14,538
	(67.3)	(67.3)	(67.6)	(67.7)	(112.3)		
Health, Community Services	-306.7	-208.9	-160.7	-34.1	-235.4	351	13,593
	(89.7)	(89.7)	(89.8)	(89.8)	(116.2)		
Urban / rural location							
Five main urban centres	-1167.5	-973.9	-844.4	-674.8	-642.0	2,136	89,337
	(61.0)	(61.0)	(61.0)	(61.0)	(75.1)		
Other major cities	-424.0	-643.9	-498.0	-339.1	-469.3	600	23,670
	(84.9)	(84.9)	(84.9)	(84.9)	(106.2)		
Secondary and minor cities	-668.1	-350.4	-165.2	-201.8	-408.3	360	17,505
	(101.1)	(101.1)	(101.1)	(101.1)	(126.1)		
Rural locations	-657.0	-708.2	-739.5	-366.2	-508.4	318	6,282
	(112.4)	(112.5)	(112.5)	(112.5)	(143.1)		
Regional Council area							
Auckland	-1357.4	-1077.0	-918.4	-800.2	-814.1	1,332	47,160
	(82.6)	(82.6)	(82.6)	(82.6)	(100.7)		
Waikato	-584.0	-659.2	-432.2	-189.3	-111.6	246	9,222
	(134.0)	(134.0)	(134.0)	(134.0)	(159.6)		
Wellington	-1075.8	-983.5	-930.1	-709.7	-511.6	444	21,921
	(142.9)	(142.9)	(142.9)	(143.0)	(184.2)		
Canterbury	-732.3	-507.4	-558.7	-466.5	-455.8	396	20,016
	(102.4)	(102.4)	(102.4)	(102.4)	(126.2)		
All other regions	-503.8	-602.6	-441.6	-250.6	-423.7	1,071	38,667
	(61.4)	(61.4)	(61.4)	(61.4)	(76.1)		

Note: Standard errors are in parentheses.

Table 12

Regression estimates of the effects of being displaced on future earnings, for workers in the 'closure – no transfers' sub-sample who were employed afterwards

Impact of Displacement:	6 months after	12 months after	24 months after	36 months after	48 months after
Gender					
Male	-0.081	-0.075	-0.046	-0.007	-0.007
	(0.009)	(0.009)	(0.009)	(0.009)	(0.012)
Female	-0.118	-0.102	-0.009	0.019	-0.014
	(0.011)	(0.011)	(0.011)	(0.011)	(0.015)
Age group					
25–34	-0.214	-0.162	-0.085	-0.074	-0.087
	(0.020)	(0.020)	(0.020)	(0.020)	(0.027)
35–54	-0.102	-0.091	-0.041	0.007	-0.016
	(0.009)	(0.009)	(0.009)	(0.009)	(0.012)
55–64	-0.058	-0.061	0.020	0.034	0.026
	(0.014)	(0.014)	(0.014)	(0.014)	(0.018)
Job tenure prior to displacement					
< 1 Year	-0.042	-0.054	-0.003	0.042	0.037
	(0.018)	(0.018)	(0.018)	(0.019)	(0.024)
1–<2 Years	-0.102	-0.090	-0.003	0.017	-0.030
	(0.017)	(0.017)	(0.017)	(0.018)	(0.024)
2 or more Years	-0.156	-0.134	-0.079	-0.047	-0.051
	(0.008)	(0.007)	(0.007)	(0.007)	(0.009)
Monthly earnings prior to displacement (quartile)					
< $1900	-0.086	-0.096	-0.055	-0.041	-0.020
	(0.017)	(0.016)	(0.017)	(0.017)	(0.021)
$1900 – $3200	-0.135	-0.127	-0.064	-0.036	-0.028
	(0.014)	(0.014)	(0.014)	(0.014)	(0.017)
$3200 – $4600	-0.121	-0.094	-0.053	-0.019	-0.106
	(0.013)	(0.013)	(0.013)	(0.014)	(0.018)
>= $4600	-0.112	-0.138	-0.078	-0.053	-0.248
	(0.013)	(0.013)	(0.013)	(0.014)	(0.020)
Firm size					
Firm Size 5–9 Employees	-0.090	-0.074	-0.046	-0.001	0.004
	(0.013)	(0.013)	(0.013)	(0.014)	(0.016)
Firm Size 10-19 Employees	-0.105	-0.081	-0.025	-0.018	-0.034
	(0.012)	(0.012)	(0.012)	(0.013)	(0.017)
Firm Size 20-49 Employees	-0.097	-0.118	-0.019	0.019	0.028
	(0.016)	(0.015)	(0.016)	(0.016)	(0.020)
Firm Size 50+ Employees	-0.087	-0.060	0.017	0.043	-0.036
	(0.019)	(0.019)	(0.019)	(0.019)	(0.028)
Firm structure					
Single-unit enterprise	-0.103	-0.100	-0.030	-0.008	-0.023
	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)
Multi-unit enterprise	-0.106	-0.021	-0.023	0.067	-0.001
	(0.022)	(0.022)	(0.022)	(0.023)	(0.037)

Note: Standard errors are in parentheses.

Table 12 *continued*

Regression estimates of the effects of being displaced on future earnings, for workers in the 'closure – no transfers' sub-sample who were employed afterwards

Impact of Displacement:	6 months after	12 months after	24 months after	36 months after	48 months after
Industry					
Agriculture, Forestry, Fishing	-0.147	-0.177	-0.162	-0.163	-0.100
	(0.030)	(0.030)	(0.031)	(0.031)	(0.041)
Manufacturing	-0.219	-0.155	-0.084	-0.013	-0.060
	(0.021)	(0.021)	(0.020)	(0.021)	(0.025)
Wholesale trade	-0.086	-0.126	-0.127	-0.078	-0.035
	(0.027)	(0.026)	(0.026)	(0.026)	(0.029)
Retail trade	-0.116	-0.054	0.013	0.025	0.061
	(0.024)	(0.024)	(0.024)	(0.025)	(0.031)
Accommodation, Restaurants, Cafes	-0.114	-0.081	0.027	0.036	0.060
	(0.029)	(0.029)	(0.030)	(0.030)	(0.038)
Business services	-0.126	-0.159	-0.027	-0.035	-0.063
	(0.021)	(0.020)	(0.021)	(0.021)	(0.025)
Education	-0.055	-0.003	0.019	0.080	-0.014
	(0.018)	(0.018)	(0.018)	(0.019)	(0.031)
Health, Community Services	-0.077	-0.101	-0.027	0.062	0.010
	(0.027)	(0.027)	(0.027)	(0.027)	(0.037)
Urban / rural location					
Five main urban centres	-0.106	-0.081	-0.022	0.015	0.017
	(0.011)	(0.010)	(0.010)	(0.011)	(0.013)
Other major cities	-0.051	-0.077	-0.027	0.028	0.022
	(0.019)	(0.019)	(0.019)	(0.019)	(0.025)
Secondary and minor cities	-0.221	-0.091	-0.071	-0.074	-0.022
	(0.025)	(0.025)	(0.025)	(0.025)	(0.032)
Rural locations	-0.062	-0.129	-0.098	-0.011	-0.064
	(0.030)	(0.030)	(0.030)	(0.031)	(0.039)
Regional Council area					
Auckland	-0.125	-0.070	-0.008	0.000	-0.002
	(0.014)	(0.014)	(0.014)	(0.014)	(0.018)
Waikato	-0.095	-0.135	-0.020	0.020	0.005
	(0.031)	(0.031)	(0.031)	(0.031)	(0.037)
Wellington	-0.108	-0.149	-0.125	-0.018	-0.027
	(0.022)	(0.022)	(0.022)	(0.022)	(0.028)
Canterbury	-0.142	-0.040	-0.068	-0.059	0.010
	(0.024)	(0.024)	(0.024)	(0.025)	(0.031)
All other regions	-0.067	-0.076	-0.010	0.045	0.036
	(0.015)	(0.014)	(0.015)	(0.015)	(0.019)

Table 13

Regression estimates of the impact of displacement on future employment, for employees who left the firm 1–12 months before the event

Impact of displacement after:	Mean employment rate for control group	All displaced workers	Displaced workers			
			Closure – no transfers[1]	Closure – some transfers[2]	Branch closure[3]	Probable restructuring[4]
		Estimated employment rate impact, in percentage points				
1 month	58.2	0.045	-0.041	0.045	-0.035	0.065
		(0.002)	(0.005)	(0.007)	(0.013)	(0.003)
2 months	64.5	0.024	-0.054	0.028	-0.047	0.041
		(0.002)	(0.005)	(0.007)	(0.013)	(0.003)
3 months	66.3	0.016	-0.049	0.021	-0.048	0.031
		(0.002)	(0.005)	(0.007)	(0.013)	(0.003)
4 months	67.3	0.013	-0.044	0.015	-0.048	0.027
		(0.002)	(0.005)	(0.007)	(0.013)	(0.003)
5 months	67.9	0.009	-0.052	0.014	-0.039	0.023
		(0.002)	(0.005)	(0.007)	(0.013)	(0.003)
6 months	68.1	0.007	-0.052	0.021	-0.049	0.020
		(0.002)	(0.005)	(0.007)	(0.013)	(0.003)
12 months	69.0	0.000	-0.048	0.019	-0.056	0.010
		(0.002)	(0.005)	(0.007)	(0.013)	(0.003)
24 months	67.6	-0.002	-0.049	0.022	-0.046	0.006
		(0.002)	(0.005)	(0.007)	(0.013)	(0.003)
36 months	66.3	-0.007	-0.039	0.013	-0.046	-0.001
		(0.002)	(0.005)	(0.007)	(0.013)	(0.003)
48 months	64.7	-0.012	-0.033	0.019	-0.046	-0.010
		(0.003)	(0.006)	(0.008)	(0.014)	(0.003)
		Percentage difference from control group's mean employment rate				
1 month		7.7	-7.0	7.8	-6.0	11.2
2 months		3.6	-8.4	4.4	-7.3	6.4
3 months		2.4	-7.4	3.2	-7.2	4.7
4 months		1.9	-6.6	2.2	-7.1	3.9
5 months		1.3	-7.7	2.1	-5.8	3.4
6 months		1.0	-7.6	3.0	-7.3	2.9
12 months		0.1	-7.0	2.7	-8.1	1.5
24 months		-0.4	-7.2	3.3	-6.8	0.9
36 months		-1.0	-5.8	2.0	-6.9	-0.2
48 months		-1.9	-5.1	2.9	-7.1	-1.5
N displaced persons		34,251	5,337	2,739	810	25,365
N control group		63,069	63,069	63,069	63,069	63,069

(1) The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation.
(2) The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months.
(3) 'Branch closures' are exits of establishments belonging to continuing enterprises, with no employee group transfers.
(4) The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Note: Standard errors are in parentheses.

Table 14

Regression estimates of the impact of displacement on future monthly earnings, for employees who left the firm 1–12 months before the event (full sample)

Impact of displacement after:	Mean earnings of control group ($)	All displaced workers	Displaced workers			
			Closure – no transfers[1]	Closure – some transfers[2]	Branch closure[3]	Probable restruct-uring[4]
			Estimated displacement effect in $			
1 month	1,750	-34.5	-570.1	-151.6	-77.2	92.6
		(17.9)	(35.7)	(48.7)	(87.0)	(19.5)
2 months	1,920	16.6	-460.9	-85.1	-223.2	136.2
		(17.9)	(35.7)	(48.7)	(87.0)	(19.5)
3 months	2,010	-9.2	-456.6	-120.8	-292.3	106.6
		(17.9)	(35.7)	(48.7)	(87.0)	(19.5)
4 months	2,030	-34.4	-430.3	-103.5	-250.6	63.7
		(17.9)	(35.7)	(48.7)	(87.0)	(19.5)
5 months	2,060	-22.0	-425.0	-135.0	-203.3	81.0
		(17.9)	(35.7)	(48.7)	(87.0)	(19.5)
6 months	2,080	-42.1	-388.4	-98.6	-266.3	44.3
		(17.9)	(35.7)	(48.7)	(87.0)	(19.5)
12 months	2,220	-100.8	-461.6	-82.8	-236.7	-22.8
		(17.9)	(35.7)	(48.7)	(87.0)	(19.5)
24 months	2,280	-110.5	-390.4	-33.0	-238.8	-56.3
		(17.9)	(35.7)	(48.7)	(87.0)	(19.5)
36 months	2,330	-154.5	-402.5	-119.5	-204.6	-104.8
		(17.9)	(35.7)	(48.7)	(87.0)	(19.5)
48 months	2,320	-147.0	-376.1	-8.1	-190.3	-113.4
		(20.4)	(40.5)	(54.9)	(97.2)	(22.2)
		Percentage difference from control group's mean earnings				
1 month		-2.0	-32.7	-8.7	-4.4	5.3
2 months		0.9	-24.0	-4.4	-11.6	7.1
3 months		-0.5	-22.7	-6.0	-14.6	5.3
4 months		-1.7	-21.2	-5.1	-12.4	3.1
5 months		-1.1	-20.6	-6.5	-9.9	3.9
6 months		-2.0	-18.6	-4.7	-12.8	2.1
12 months		-4.5	-20.8	-3.7	-10.7	-1.0
24 months		-4.8	-17.1	-1.4	-10.5	-2.5
36 months		-6.6	-17.3	-5.1	-8.8	-4.5
48 months		-6.3	-16.2	-0.3	-8.2	-4.9
N displaced persons		34,251	5,337	2,739	810	25,365
N control group		63,069	63,069	63,069	63,069	63,069

(1) The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation.
(2) The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months.
(3) 'Branch closures' are exits of establishments belonging to continuing enterprises, with no employee group transfers.
(4) The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Note: Standard errors are in parentheses.

Table 15

Regression estimates of the impact of displacement on future monthly earnings, for employees who left the firm 1–12 months before the event and were employed afterwards

Impact of displacement after:	Mean earnings of control group, if employed ($)	All displaced workers	Displaced workers			
			Closure – no transfers[1]	Closure – some transfers[2]	Branch closure[3]	Probable restruct-uring[4]
		Estimated displacement effect in log points				
1 month	3,000	-0.011	-0.118	-0.087	-0.038	0.016
		(0.005)	(0.012)	(0.016)	(0.028)	(0.006)
2 months	2,980	0.021	-0.007	0.001	-0.034	0.030
		(0.005)	(0.011)	(0.015)	(0.027)	(0.006)
3 months	3,030	0.001	-0.031	-0.018	-0.089	0.011
		(0.005)	(0.011)	(0.015)	(0.026)	(0.005)
4 months	3,010	-0.005	-0.038	-0.020	-0.087	0.006
		(0.005)	(0.011)	(0.015)	(0.026)	(0.005)
5 months	3,040	-0.004	-0.025	-0.049	-0.073	0.006
		(0.005)	(0.011)	(0.015)	(0.026)	(0.005)
6 months	3,060	-0.014	-0.034	-0.016	-0.069	-0.008
		(0.005)	(0.011)	(0.015)	(0.026)	(0.005)
12 months	3,220	-0.006	-0.015	-0.003	-0.054	-0.003
		(0.005)	(0.011)	(0.015)	(0.026)	(0.005)
24 months	3,380	-0.026	-0.027	-0.016	-0.029	-0.026
		(0.005)	(0.011)	(0.015)	(0.026)	(0.006)
36 months	3,520	-0.045	-0.041	-0.046	-0.040	-0.045
		(0.005)	(0.011)	(0.015)	(0.026)	(0.006)
48 months	3,590	-0.031	-0.005	-0.020	-0.040	-0.038
		(0.006)	(0.013)	(0.017)	(0.030)	(0.006)
		Percentage difference from control group's mean earnings				
1 month		-1.1	-11.1	-8.3	-3.7	1.6
2 months		2.2	-0.7	0.1	-3.4	3.1
3 months		0.1	-3.1	-1.8	-8.5	1.2
4 months		-0.5	-3.7	-2.0	-8.3	0.6
5 months		-0.4	-2.5	-4.8	-7.0	0.6
6 months		-1.4	-3.3	-1.6	-6.6	-0.8
12 months		-0.6	-1.5	-0.3	-5.2	-0.3
24 months		-2.5	-2.6	-1.6	-2.9	-2.6
36 months		-4.4	-4.0	-4.5	-3.9	-4.4
48 months		-3.1	-0.5	-1.9	-3.9	-3.7
Sample sizes at 6 months after displacement						
N displaced persons		22,062	2,949	1,677	507	16,932
N control group		36,705	36,705	36,705	36,705	36,705
Sample sizes at 24 months after displacement						
N displaced persons		23,631	3,405	1,869	576	17,781
N control group		42,618	42,618	42,618	42,618	42,618

(1) The 'closure – no transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and there were no transfers of groups of employees to other establishments or enterprises during the final six months of operation.
(2) The 'closure – some transfers' group represents establishment exits in which the corresponding enterprise number also ceased in the same month, and up to one-third of employees were transferred in groups to other establishments or enterprises during the final six months.
(3) 'Branch closures' are exits of establishments belonging to continuing enterprises, with no employee group transfers.
(4) The 'restructuring' group is a residual category that includes all other establishment exits in LEED between April 2001 and March 2004.

Note: Standard errors are in parentheses.

Appendix 1

How firms are identified and tracked in the Business Frame, the Longitudinal Business Frame, and LEED

This appendix outlines the methods used by Statistics NZ to uniquely identify establishments and enterprises on the Business Frame (BF) and Longitudinal Business Frame (LBF), and to track them over time, during the period when the data used in this study were compiled. It draws extensively on Fabling (2006). Changes were made to BF processes in late 2007, and those changes are not discussed here.[19]

LEED is constructed from the integration of tax data with LBF data. The BF is an actively-maintained register of enterprises and establishments (also known as 'geos') in the New Zealand economy. The LBF is a version of the BF in which checking and repair processes have been carried out to identify and maintain the longitudinal continuity of establishments.

The Business Frame (BF)

The key identifier in the BF is the enterprise number. An enterprise number (ENT) is designed to track the continuation of a legal business unit. A new enterprise number is created on the BF whenever a new legal unit is created. When a continuing enterprise changes its legal form in some way, or a business is sold to another business, or merges with another business, the enterprise number will also change. Establishments belonging to enterprises are separately identified using a geo number. Some degree of continuity in geo numbers is maintained over time on the BF.

Specifically, the tax system is used to identify the creation of new enterprises. Whenever a new business IRD number is created within Inland Revenue, a new enterprise is birthed onto the BF with characteristics initially taken from the tax data. If the ENT is assessed as being in tier 1 or 2, a monthly frame update survey is sent out to confirm enterprise characteristics and identify whether the geos (establishments) associated with the enterprise were previously owned by another enterprise. In a very small number of cases, ENTs assessed as being in tier 3 will be sent an update survey. This only occurs when their tax data cannot be used to fill all necessary BF fields and is predominantly used to identify the core BF classifications of industry, meshblock (location), and institutional sector. If geo continuity is identified through the update survey process, then relevant live geos are transferred from the old ENT to the new (birthed) ENT number. The old ENT is ceased if all live geos have been transferred.[20]

An annual frame update survey is also sent to tier 1 firms annually, and tier 2 firms at least tri-annually. This survey may also identify repairs to continuity/changes in geo structure.

The BF process for identifying geo continuity relies on the effectiveness of the update survey process and can be considered more art than science. To aid the process of establishing geo ownership change, more attention is given to industries where some geographically-fixed capital investment is likely to have been made (eg farms, forests,

[19] Since late 2007 the LBF geo-linking processes have also been applied in the BF, after a lag.

[20] In a reasonable number of BF transfers, the 'donating' enterprise continues with live geos.

factories, etc). As such, geo transfers are less likely for units in many service industries.[21]

To summarise, BF processing preserves some geo continuity through its survey processes, but ENT number continuity accurately only indicates the survival of a legal unit.

The Longitudinal Business Frame (LBF)

In 2003 the methods used to maintain establishment continuity in the BF were extended by adding some new administrative processes, to create the Longitudinal Business Frame (LBF).

The purpose of the LBF is to create longitudinal employer identities at the establishment (geo) level. This is done by repairing links between the geo numbers that were assigned on the BF, if there is sufficient evidence that two geos are 'the same'. A Permanent Business Number (PBN) is then assigned to each unique establishment after the repairs have been completed.

There are two repair processes. Geo repairs look at 'new' geos in a month and compare their name and address to geos in the prior month. The geos are deemed to be the same if both geo and enterprise names and addresses are sufficiently similar.

Some important exclusions apply to this matching process. The process is not applied if: (1) geo industry differs at the division level (1-digit ANZSIC); (2) the recipient geo (ie, in the latter time period) is part of a multi-geo enterprise;[22] (3) geos are in ENTs that are part of a BF group structure or have a name including the word 'trust'; (4) geo employment is significantly different (eg, it moves between zero and non-zero, or differs by more than 20 employees); (5) geos are in ANZSIC division 'L' (property and business services) or 'O' (health and community services); or (6) a BF repair has been applied in the 12 months prior to or after this potential repair.

Employer repairs use the Employer Monthly Schedule (EMS) data that is supplied by employers to Inland Revenue and track the movement of employees away from ceased IRD numbers. If another IRD number in the following month is found to have 70 percent of the employees of the previous employer, then continuity between the IRD numbers is asserted. This threshold is relaxed to 60 percent if at least three employees move to the new IRD number. When a match is found, all geos associated with the prior employer IRD number are linked to the new enterprise number.

This matching process has the following important exclusions: (1) geos where the employee count is less than three; (2) group EMS filers (a separate process allocates employees to the geos that belong to a group filer); (3) the donor and recipient enterprise numbers are the same (which occurs when a legal entity gains a new IRD number); (4) the donor ENT has multiple IRD numbers; or (5) the donor ENT has no permanent business number associated with it.[23]

[21] Particularly transport and storage; communication services; finance and insurance; property and business services; health and community services; cultural and recreational services; and personal and other services.

[22] This exclusion exists because geos within an ENT tend to share enough common name/address properties that many (false positives) matches occur.

[23] This could occur because a geo or BF repair has already been conducted. Geos in industry A012 are also excluded if the employee count is less than 20 (this is to remove false positives created by shearing gangs moving between farms).

Another limitation of the employer repair matching process is that the employment matching only considers adjacent months, which means that processes involving the gradual winding down and transfer of EMS filing may be missed (Kelly and Seyb 2005).

Some important points to understand regarding the employer and geo repairs are that the repair processes are not applied in a comprehensive way – instead there are a significant number of exemptions. Multiple real-world events can lead to the same observed links in the LBF, and the rules used only sometimes attempt to address ambiguous firm continuity situations (eg business splits can only yield a continuation of the PBN if one geo retains 70 percent of the employees).

Implications for the identification of firm closures

To summarise, the enterprise numbers used in the BF, LBF and LEED are designed to identify businesses as legal entities and are not designed to track the continuation of enterprises as sets of factors of production.

The PBNs that are used in the LBF and LEED are designed to link establishments longitudinally. The repair processes that are implemented do eliminate a considerable number of 'false' establishment deaths. However, the linking rules and methods are not applied in a fully comprehensive way.

The repair rules were intended to link establishments when there is a reasonably high level of certainty that the establishments being linked are the same. Many business restructuring events bring about changes that are large enough to cast doubt on whether the new operation should really be viewed as a continuation of the old one. If the criteria are not met, a PBN will be ceased. This means there are numerous circumstances in which a PBN number ceases to be used in LEED that are not simple establishment closures, and do not result in job loss for the employees who are affected.

Appendix 2
Additional tables

Table A1

Alternative regression estimates of the effect of displacement on the future employment of workers affected by closures

		Regression specification				
Impact of displacement after:	**Employment rate difference between groups**	**Base model (1)**	**Base model + controls (2)**	**Base model with fixed effects (3)**	**Final model (4)**	**Log non-employment model (5)**
1 month	-0.494	-0.453	-0.453	-0.453	-0.446	-0.485
		(0.006)	(0.005)	(0.004)	(0.005)	
2 months	-0.377	-0.336	-0.337	-0.337	-0.329	-0.364
		(0.006)	(0.005)	(0.004)	(0.005)	
3 months	-0.313	-0.272	-0.273	-0.273	-0.265	-0.296
		(0.006)	(0.005)	(0.004)	(0.005)	
4 months	-0.283	-0.243	-0.243	-0.243	-0.236	-0.263
		(0.006)	(0.005)	(0.004)	(0.005)	
5 months	-0.260	-0.219	-0.220	-0.220	-0.212	-0.237
		(0.006)	(0.005)	(0.004)	(0.005)	
6 months	-0.255	-0.215	-0.215	-0.216	-0.208	-0.230
		(0.006)	(0.005)	(0.004)	(0.005)	
12 months	-0.204	-0.164	-0.165	-0.165	-0.157	-0.166
		(0.006)	(0.005)	(0.004)	(0.005)	
24 months	-0.172	-0.132	-0.133	-0.133	-0.126	-0.117
		(0.006)	(0.005)	(0.004)	(0.005)	
36 months	-0.150	-0.110	-0.112	-0.112	-0.104	-0.081
		(0.006)	(0.005)	(0.004)	(0.005)	
48 months	-0.147	-0.107	-0.109	-0.104	-0.097	-0.067
		(0.007)	(0.007)	(0.006)	(0.006)	

Notes: Standard errors are in parentheses. The regression in column (1) is the base model which contains dummy variables for month, year and whether displaced. The model in column (2) also includes controls for personal and job characteristics. The model in column (3) is estimated with person-specific fixed effects. The model in column (4) is the same as model 3 but also includes dummies for the six months immediately prior to the displacement event, so that these months do not influence the estimation of the displacement coefficient (which captures the average difference between displaced workers and control group workers during the pre-displacement period). This is the preferred specification. The estimates in column (5) are calculated from an alternative model in which the dependent variable is the log of the non-employment rate and the explanatory variables are the same as in model (4).

Table A2

Employment rate impact estimates for workers in the 'closure – no transfers' sub-sample, expressed as a percentage of the mean employment rate of the control group

	Impact of displacement after:				
	6 months	**12 months**	**24 months**	**36 months**	**48 months**
Male	-21.3	-18.4	-16.3	-12.9	-11.3
Female	-23.3	-16.4	-13.1	-12.7	-14.9
Age 25–34	-33.8	-29.3	-23.2	-20.8	-22.8
Age 35–54	-21.6	-15.7	-12.1	-10.7	-10.4
Age 55–64	-19.2	-15.6	-15.3	-12.1	-12.4
Tenure < 1 year	-18.2	-11.5	-9.6	-8.1	-6.7
Tenure 1–<2 years	-17.0	-12.7	-9.0	-9.5	-12.8
Tenure 2 or more years	-30.2	-25.9	-22.8	-19.5	-19.2
Earnings < $1,900	-30.3	-22.4	-19.4	-18.2	-21.5
Earnings $1,900 – $3,200	-20.8	-17.5	-17.2	-12.6	-11.8
Earnings $3,200 – $4,600	-20.1	-16.5	-12.7	-12.1	-11.8
Earnings >= $4,600	-24.6	-22.9	-17.4	-17.3	-14.1
Firm size 5–9 employees	-24.7	-20.9	-16.9	-13.3	-12.4
Firm size 10-19 employees	-19.6	-13.9	-12.1	-10.1	-9.7
Firm size 20-49 employees	-27.4	-23.5	-22.3	-21.3	-22.5
Firm size 50+ employees	-13.1	-1.5	3.2	2.5	7.1
Single-unit enterprise	-24.2	-19.0	-16.4	-14.2	-14.8
Multi-unit enterprise	-13.4	-12.2	-8.5	-9.2	-5.5
Agriculture, forestry & fishing	-24.3	-24.1	-25.7	-23.0	-15.4
Manufacturing	-26.5	-22.1	-14.9	-15.4	-14.7
Wholesale trade	-23.6	-15.7	-5.9	-8.3	0.3
Retail trade	-25.4	-20.0	-16.1	-14.8	-14.9
Accommodation, restaurants, & cafes	-27.5	-21.1	-17.1	-17.0	-19.2
Business services	-28.9	-23.1	-19.2	-18.7	-17.1
Education	-11.4	-6.7	-5.8	-6.0	-2.9
Health & community services	-14.2	-6.6	-5.1	0.1	-11.7
Five main urban centres	-24.0	-18.9	-15.4	-14.0	-13.4
Other major cities	-17.9	-13.0	-11.8	-9.0	-12.8
Secondary and minor cities	-15.4	-10.8	-7.2	-4.5	-6.6
Rural locations	-19.2	-15.1	-15.3	-13.9	-7.9
Auckland	-29.5	-23.5	-19.6	-17.6	-18.0
Waikato	-21.3	-16.0	-12.0	-7.8	-4.7
Wellington	-21.5	-16.8	-11.9	-12.9	-7.5
Canterbury	-20.9	-17.4	-15.5	-15.5	-19.4
All other regions	-14.8	-10.9	-10.8	-7.4	-8.9

Note: See table 10 for the regression coefficients used in the calculation of these percentages.

Table A3

Earnings impact estimates for workers in the 'closure – no transfers' sub-sample, expressed as a percentage of the mean earnings of the control group

	Impact of displacement after:				
	6 months	12 months	24 months	36 months	48 months
Male	-25.7	-25.0	-21.9	-18.0	-19.1
Female	-21.9	-15.8	-13.4	-9.6	-9.9
Age 25–34	-30.9	-25.8	-17.0	-13.1	-14.9
Age 35–54	-24.6	-22.5	-18.2	-14.6	-16.7
Age 55–64	-20.5	-17.8	-18.8	-14.0	-12.1
Tenure < 1 year	-19.7	-17.9	-15.9	-10.4	-10.6
Tenure 1–<2 years	-19.1	-16.9	-10.6	-12.6	-12.9
Tenure 2 or more years	-30.8	-27.3	-24.9	-19.9	-20.6
Earnings < $1,900	-23.2	-14.7	-13.0	-11.9	-13.5
Earnings $1,900 – $3,200	-27.0	-22.2	-23.1	-13.5	-16.0
Earnings $3,200 – $4,600	-24.0	-22.4	-17.4	-16.4	-18.7
Earnings >= $4,600	-41.6	-43.1	-39.1	-35.6	-44.4
Firm size 5–9 employees	-24.7	-23.5	-19.8	-16.4	-16.5
Firm size 10-19 employees	-22.6	-19.4	-17.0	-12.1	-12.1
Firm size 20-49 employees	-27.8	-25.8	-23.8	-19.8	-14.5
Firm size 50+ employees	-30.1	-23.6	-21.9	-21.3	-36.4
Single-unit enterprise	-25.9	-22.8	-19.2	-15.5	-15.6
Multi-unit enterprise	-27.0	-22.5	-26.3	-23.7	-32.5
Agriculture, forestry & fishing	-28.5	-27.8	-32.3	-23.1	-15.6
Manufacturing	-32.7	-29.2	-18.1	-15.1	-26.2
Wholesale trade	-26.1	-19.8	-17.1	-16.6	-11.9
Retail trade	-24.1	-21.4	-16.6	-15.9	-7.4
Accommodation, restaurants, & cafes	-25.8	-12.5	-11.4	-8.7	-8.7
Business services	-32.7	-32.4	-27.1	-27.0	-23.9
Education	-12.2	-8.8	-12.1	-7.2	-6.0
Health & community services	-9.4	-5.5	-4.5	0.6	-6.8
Five main urban centres	-27.9	-23.8	-20.9	-17.2	-17.0
Other major cities	-12.3	-18.2	-14.7	-10.2	-14.2
Secondary and minor cities	-20.5	-11.0	-5.4	-6.6	-13.0
Rural locations	-19.6	-21.5	-22.9	-11.9	-16.2
Auckland	-32.0	-26.2	-22.7	-20.2	-20.8
Waikato	-16.6	-18.3	-12.2	-5.5	-3.3
Wellington	-24.1	-22.8	-21.6	-17.1	-13.7
Canterbury	-19.8	-14.1	-15.5	-13.5	-13.1
All other regions	-14.8	-17.6	-13.5	-7.8	-13.2

Note: See table 11 for the regression coefficients used in the calculation of these percentages.

Table A4

Earnings impact estimates for workers in the 'closure – no transfers' sub-sample (who were employed afterwards), expressed as a percentage of the mean earnings of the control group

	Impact of displacement after:				
	6 months	**12 months**	**24 months**	**36 months**	**48 months**
Male	-7.7	-6.5	-5.3	-1.2	-0.4
Female	-12.0	-9.2	-1.0	2.9	3.0
Age 25–34	-22.1	-16.2	-8.9	-8.0	-7.1
Age 35–54	-11.1	-9.2	-4.5	0.7	0.2
Age 55–64	-3.7	-3.0	1.8	4.4	5.8
Tenure < 1 year	-5.2	-5.5	-1.9	2.0	4.6
Tenure 1–<2 years	-5.7	-4.3	2.1	5.4	0.9
Tenure 2 or more years	-16.2	-12.9	-8.3	-4.0	-2.7
Earnings < $1,900	-7.6	-6.7	-3.0	-2.7	1.4
Earnings $1,900 – $3,200	-15.7	-13.5	-9.3	-5.3	-4.2
Earnings $3,200 – $4,600	-8.3	-10.3	-7.7	-3.9	-7.2
Earnings >= $4,600	-14.5	-14.5	-11.7	-6.2	-20.6
Firm size 5–9 employees	-9.4	-7.7	-4.6	-0.5	0.2
Firm size 10-19 employees	-10.0	-7.9	-2.5	-1.2	-1.5
Firm size 20-49 employees	-10.9	-9.1	-3.5	2.1	6.9
Firm size 50+ employees	-3.7	2.0	10.1	12.4	15.9
Single-unit enterprise	-10.5	-8.5	-3.1	-0.1	0.2
Multi-unit enterprise	-6.3	-4.5	-7.0	2.0	-1.8
Agriculture, forestry & fishing	-17.6	-10.9	-16.9	-16.0	-12.5
Manufacturing	-21.1	-17.5	-8.7	-5.1	-9.2
Wholesale trade	-8.2	-11.1	-10.8	-2.6	-0.5
Retail trade	-12.5	-6.4	-1.9	-0.6	3.1
Accommodation, restaurants, & cafes	-11.5	-4.8	4.0	5.8	7.5
Business services	-12.3	-14.0	-1.8	-1.1	-2.0
Education	0.7	4.5	2.6	7.7	11.4
Health & community services	-6.0	-12.0	-3.9	6.7	7.8
Five main urban centres	-10.0	-7.7	-2.2	1.6	1.7
Other major cities	-5.0	-7.4	-2.7	2.8	2.2
Secondary and minor cities	-19.8	-8.7	-6.8	-7.1	-2.2
Rural locations	-6.0	-12.1	-9.3	-1.1	-6.2
Auckland	-11.8	-6.8	-0.8	0.0	-0.2
Waikato	-9.1	-12.7	-1.9	2.0	0.5
Wellington	-10.3	-13.8	-11.7	-1.8	-2.7
Canterbury	-13.2	-4.0	-6.6	-5.8	1.0
All other regions	-6.5	-7.3	-1.0	4.6	3.7

Note: See table 12 for the regression coefficients used in the calculation of these percentages

Table A5

Regression estimates of the effect of being displaced on total employment

Impact of displacement after:	Mean employment rate for control group	All displaced workers	Displaced workers			
			Closure – no transfers[1]	Closure – some transfers[2]	Branch closure[3]	Probable restruct-uring[4]
		Estimated employment rate impact, in percentage points				
1 month	98.3	-0.036	-0.332	-0.169	-0.014	-0.003
		(0.001)	(0.004)	(0.007)	(0.009)	(0.001)
2 months	97.3	-0.026	-0.237	-0.114	-0.017	-0.002
		(0.001)	(0.004)	(0.007)	(0.009)	(0.001)
3 months	96.6	-0.024	-0.185	-0.102	-0.020	-0.005
		(0.001)	(0.004)	(0.007)	(0.009)	(0.001)
4 months	95.9	-0.021	-0.164	-0.077	-0.014	-0.004
		(0.001)	(0.004)	(0.007)	(0.009)	(0.001)
5 months	95.3	-0.019	-0.147	-0.059	-0.017	-0.004
		(0.001)	(0.004)	(0.007)	(0.009)	(0.001)
6 months	94.8	-0.021	-0.145	-0.052	-0.019	-0.007
		(0.001)	(0.004)	(0.007)	(0.009)	(0.001)
12 months	92.6	-0.018	-0.108	-0.045	-0.009	-0.008
		(0.001)	(0.004)	(0.007)	(0.009)	(0.001)
24 months	89.3	-0.017	-0.081	-0.021	-0.005	-0.011
		(0.001)	(0.004)	(0.007)	(0.009)	(0.001)
36 months	86.8	-0.014	-0.066	-0.015	-0.005	-0.009
		(0.001)	(0.004)	(0.007)	(0.009)	(0.001)
48 months	84.6	-0.013	-0.056	0.007	-0.006	-0.009
		(0.002)	(0.005)	(0.010)	(0.010)	(0.002)
		Percentage difference from control group's mean employment				
1 month		-3.7	-33.8	-17.2	-1.4	-0.3
2 months		-2.7	-24.4	-11.7	-1.8	-0.2
3 months		-2.4	-19.2	-10.6	-2.1	-0.5
4 months		-2.2	-17.1	-8.1	-1.4	-0.5
5 months		-2.0	-15.4	-6.2	-1.8	-0.5
6 months		-2.2	-15.3	-5.5	-2.1	-0.8
12 months		-2.0	-11.7	-4.9	-0.9	-0.9
24 months		-1.9	-9.0	-2.4	-0.5	-1.2
36 months		-1.7	-7.6	-1.8	-0.6	-1.1
48 months		-1.5	-6.6	0.8	-0.7	-1.0
N displaced persons		39,645	3,486	1,095	774	34,290
N control group		136,986	136,986	136,986	136,986	136,986

Note: Standard errors are in parentheses. The dependent variable is total employment, which is set to 1 in a given month if earnings from waged employment were received in that month, or income was received from self-employment at any time during the financial year.