



# **The longer term impacts of job displacement on labour market outcomes**

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### Disclaimer

The results in this working paper are not official statistics, they have been created for research purposes from the Integrated Data Infrastructure (IDI), managed by Statistics New Zealand. The opinions, findings, recommendations, and conclusions expressed in this working paper are those of the authors, not Statistics NZ, Ministry of Business Innovation and Employment, the OECD, or Motu Economic and Public Policy Research Trust. Access to the anonymised data used in this study was provided by Statistics NZ in accordance with 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, household, business, or organisation, and the results in this working paper have been confidentialised to protect these groups from identification. Careful consideration has been given to the privacy, security, and confidentiality issues associated with using administrative and survey data in the IDI. Further detail can be found in the Privacy impact assessment for the Integrated Data Infrastructure available from [www.stats.govt.nz](http://www.stats.govt.nz). The results are based in part on tax data supplied by Inland Revenue to Statistics NZ under the Tax Administration Act 1994. This tax data must be used only for statistical purposes, and no individual information may be published or disclosed in any other form, or provided to Inland Revenue for administrative or regulatory purposes. Any person who has had access to the unit record data has certified that they have been shown, have read, and have understood section 81 of the Tax Administration Act 1994, which relates to secrecy. Any discussion of data limitations or weaknesses is in the context of using the IDI for statistical purposes, and is not related to the data's ability to support Inland Revenue's core operational requirements.

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## **Abstract**

This paper analyses the longer term impacts of involuntary job loss of workers subsequent employment, earnings, and income support in New Zealand. It uses data from the Survey of Family, Income and Employment (SoFIE) to identify job displacements over the period 2001–10, matched to administrative data from Statistics New Zealand’s Integrated Data Infrastructure (IDI) covering the period 1999–2015, to facilitate at least five years of post-displacement observations. Following Dixon and Maré (2013), our analysis focuses on workers who had been employed for at least one year before their job displacement. Using both regression-adjustment and propensity score matching methods, we estimate that experiencing a job displacement substantially affected workers employment, earnings and income over the following five years. Compared to workers who did not lose their jobs, we estimate their employment rate was 20-25% lower in the year following displacement and, although their employment gradually improved, was still 8-12% lower five years later. Similarly, we estimate displaced workers’ conditional earnings and total income were 25-30% lower in the first year and 13-22% lower five years after being displaced. Such adverse effects are partly counterbalanced by higher levels of welfare benefit receipt and income support: benefit receipt was 6-11% and 3-4% higher after one and five years. We also find that the impacts were stronger for workers displaced from jobs during the great recession from 2008, with about 5% larger short and longer-term effects on employment, which were balanced by 3-5% higher rates of benefit receipt.

## **JEL codes**

J63, J64, J65

## **Keywords**

Displaced workers, matching, SoFIE, IDI

## **Summary haiku**

Job loss hurts workers  
low employment and earnings  
even with support.

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# 1 Introduction

A dynamic economy with firm entry and exit, together with a responsive labour market involving job creation and destruction, is considered central to economic growth and prosperity (Davis and Haltiwanger, 1992). The process of job destruction typically involves involuntary job loss, and unemployment and loss of earnings, for affected workers in the short term. However, the longer term impacts of such losses is critical for understanding the extent of adjustments costs borne by affected workers. Furthermore, there is little evidence of the comparative impacts for workers displaced at different stages of the business cycle.

In this paper, we address these two issues and analyse the longer term effects of job-displacement in New Zealand over the period 2001–10. This period covers the economic expansion until the end of 2007, and the domestic recession and global financial crisis from 2008. We are able to examine the relative impacts for workers displaced before and after the start of the recession, and we are able to analyse the impacts for at least five years after a displacement for all workers. Our analysis focuses on workers' employment and earnings, as well as their receipt of government income support. The latter is of interest in terms of its effect on moderating the adverse impacts on displaced workers income, and particularly its adequacy in the aftermath of the great recession.

Our approach follows and extends that of Dixon and Maré (2013), who used longitudinal data from Statistics New Zealand's Survey of Family Income and Employment (SoFIE) to examine the short-run impacts of displacement on workers subsequent outcomes. Dixon and Maré (2013) only had access to data from the first seven waves of SoFIE, spanning the period October 2001–September 2009. In contrast, the data available for the present study is richer in two important dimensions. First, the full eight waves of SoFIE data is available, covering the period through to September 2010: this provides two waves of data covering the period October 2007 through September 2010, which included New Zealand's domestic recession from the end of 2007 and the global financial crisis from the third quarter of 2008. Thus we are able to observe reasonable samples of job displacements that occur during both the economic expansion until the end of 2007 and the subsequent recession from 2008. This enables us to compare and contrast both the characteristics of displaced workers during these two phases and the subsequent impacts on their labour market outcomes.

Second, the SoFIE sample has been integrated into Statistics New Zealand's Integrated Data Infrastructure (IDI). Our IDI sample contains administrative data on employment and earnings over the period from April 1999 until September 2015. For individuals in SoFIE matched to the IDI, this ensures the observation period covers at least two and a half years prior to, and at least five years following, any job-displacement event measured in SoFIE. This

facilitates a longer follow-up period to examine the impact of displacement, and provides richer pre-displacement controls.

The theory behind the effects of involuntary job loss associated with job displacement suggests that displaced workers may have substantially worse employment and wage opportunities following displacement. This may occur due to the loss of firm- or sector-specific human capital, job-match quality between the worker and firm, or union or industry wage premiums. These effects may be particularly strong for workers with high levels of seniority or long tenure in their jobs. Although workers displaced during a recession likely have worse re-employment prospects than those displaced during an expansion, the predictions of the relative effect of displacement is unclear because opportunities for other workers are also likely worse during a recession. However, the impacts of displacement may be difficult to identify if workers who are displaced are non-randomly selected from the wider population of workers. The main econometric issue for estimating the true impacts of displacement will be trying to disentangle the confounding effects of other factors from measured differences in outcomes of displaced and non-displaced workers.

Involuntary job displacements in New Zealand are relatively rare compared to measures from other countries.<sup>1</sup> Also, displaced workers have somewhat different characteristics and pre-displacement employment and earnings experiences compared to non-displaced workers. That said, we present event study analyses of labour market outcomes for displaced and non-displaced workers, using three alternative comparison groups: first, a random sample of non-displaced workers; second, a sample of non-displaced workers, with similar socio-demographic characteristics and employment and earnings histories to the displaced workers; and third, to examine whether the impacts from the previous approaches are driven by the effects associated with workers ending a job, we select a random sample of non-displaced workers who report a job end. These event studies suggest similar substantial and persistent adverse impacts of displacement.

We then provide complementary econometric analyses of the impacts of job displacement on various labour market outcomes, using each of the three comparison samples. The first uses a standard regression approach to estimate the effects of job displacement, controlling for differences in workers' observable socio-economic factors, their employment and earnings histories, and fixed unobserved effects. This analysis uses the data for the random sample of workers employed in months when a displacement occurred. The second uses a matching approach to select a comparison sample of non-displaced workers with similar characteristics and labour market histories to the sample of displaced workers, and then estimate the impacts

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<sup>1</sup> For example, Dixon and Maré (2013) and Hyslop and Townsend (2016b) estimate annual displacement rates of 1.8% and 2.2% using SoFIE data. In contrast, Borland et al. (2002) estimate job displacement rates of 5-6% in Britain and Australia, although this includes a broader category of job-loss including "layoff, plant closure or the end of a contract" (p. 35), and the OECD (2013) report a range of annual displacement rates of 2-7% across countries and data sources.

of displacement by comparing the average outcomes of workers in the displaced sample versus those in the non-displaced control sample. The third estimates the impacts of job displacement among workers who end a job, using a regression approach to control for differences between displaced and non-displaced workers.

The estimates from these approaches are remarkably similar. First, we estimate that displaced workers' employment rate was 20-25% lower in the year following displacement than comparable non-displaced workers and, although their employment gradually improved, was still 8-12% lower five years later. In addition, we estimate their conditional earnings were 25-30% lower in the first year and 13-22% lower after five years being displaced. The adverse employment and earnings effects are partly counterbalanced by higher levels of welfare benefit receipt and income support: benefit receipt was 6-11% and 3-4% higher after one and five years. However, displaced workers' total income, which provides a more comprehensive measure of individuals' income support, was about 30% lower in the first year after displacement and still 20% lower five years after. We also find that the impacts were stronger for workers displaced from jobs during the great recession from 2008, with about 5% larger short and longer-term effects on employment, which were balanced by 3-5% higher rates of benefit receipt.

The paper is organised as follows. In the next section we review the literature on the impact of involuntary job loss. In section 3 we describe the SoFIE and IDI data used, including the definition of job displacement, and the sample characteristics. Section 4 outlines the two approaches we use to estimate the impacts of job displacement on workers subsequent outcomes, and section 5 presents and discusses the results. The paper concludes with a summary of the main results and implications in section 6.

## **2 Literature review**

The OECD (2013) provides a broad recent review of the literature of the impacts of job displacement on earnings and related labour market outcomes. In this section we focus on some of the more salient issues and findings from the international and New Zealand literature.

There are several reasons why workers may have worse subsequent labour market outcomes as a result of a job displacement. First, displaced workers may experience a significant loss of firm or sector-specific human capital. This may be particularly true for workers with strong seniority or long job-tenure, who may have acquired significant firm-specific skills from on-the-job training and investment, which will affect their future job and lower their earnings prospects. Second, there may be the loss of a high quality match between the displaced worker and the firm, particularly for long tenure workers, which may require substantial time and search costs to replace in a new firm. Third, displaced workers may lose union or industry wage

premiums. As a result of each of these factors, displaced workers subsequent employment and wage opportunities are likely to be worse than if they had not experienced a job displacement.

There is substantial variation in reported displacement rates in the literature. This is partly due to disagreement about the formal definition of displacement and practical difficulties in measuring displacement. The OECD (2013) defines a job displacement conceptually as “involuntary job separations due to economic or technological reasons or as a result of structural change” (page 194). In the Displaced Worker Supplement (DWS) to the US Current Population Survey (CPS), the Bureau of Labor Statistics (BLS) defines a job loss as involuntary if was due to either a plant closing, slack work, or the position or shift was abolished (Farber, 2004).<sup>2</sup> However, in practice, job displacements may be either firm-identified, often measured from longitudinally linked employer-employee administrative data, as due to mass layoffs or firm closure; or (individual) self-identified from household surveys, as due to redundancy, layoff, lack of work etc., or to just-cause dismissal.

Partly in an attempt to improve consistency across countries, the OECD (2013) restricts samples to employees aged 20-64, with at least one year of tenure with the same employer.<sup>3</sup> Based on these requirements, they report that job displacement affects 1.5-6.5% of workers annually across various countries and data sources (Figure 4.1). Firm-identified displacement rates appear to be higher than self-defined rates, and displacement rates are generally higher during the great recession of 2009-10 than over the period 2000-08.<sup>4</sup> The link between a country’s rate of job loss and its employment protection laws appears to be relatively weak. For example, while Germany and France have low rates of job loss together with high levels of protection (as measured in the OECD (2013) Figure 2.6), and the US and UK have high rates of job loss and low levels of protection, Japan and New Zealand have low levels of both, and Portugal has high levels of both.

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<sup>2</sup> Farber (2004), also notes that the BLS also restricts job loss to stable longer term job holders, defined as workers who had at least three years of tenure in the job. Also, in a survey of job displacement literature, Fallick (1996) argues that, in addition to job displacement excluding workers dismissed with just cause, there are three characteristics that are usually associated with worker displacement: (1) there is a structural (rather than cyclical) cause for the displacement; (2) displaced workers have a limited ability to return to a comparable job within a reasonable span of time; and (3) displaced workers are strongly attached to the sector in which they were employed (page 5). However, the latter two characteristics are arguably associated more with workers’ opportunities and sectoral attachment respectively, than a job-loss event. Fallick also discusses the official US federal government (Bureau of Labor Statistics, BLS) definition of a displaced worker: being aged at least 20 years, with at least 3 years of tenure, who lost their job due to slack work, the abolition of a position or shift, or plant closing or relocation (page 6). This official definition also seems less strict than argued above.

<sup>3</sup> The tenure restriction is “to avoid picking up job separations that happen soon after hiring (and may be the result of the firm and employee deciding that they were not well-matched, rather than for economic reasons)” (page 196); the age restriction excludes young workers for similar reasons.

<sup>4</sup> Using the DWS for the US, Farber (2010) estimates that the 3-year rate of involuntary job loss ranges from 7–11% in the private sector over the period 1980–2007, which is lower than the firm-identified annual rate of about 5% reported in the OECD’s (2013) Figure 4.1 The DWS is a biennial supplement to the CPS in February (January from 1984 to 1992), and asks about job loss over the preceding 3 calendar years (5-years until 1992). Estimated public sector involuntary job loss is about one-quarter of the private sector rate. Both Farber (2010) and other researchers (e.g. Von Wachter et al. 2009, comparing the DWS with administrative data) argue that the DWS likely understates the true extent of involuntary job loss in the US due to recall bias and perhaps ambiguity among workers who left a job as to whether it was (in)voluntary (e.g. in the case of a redundancy payout).

The international literature finds substantial long term impacts of job displacement on workers outcomes. In the US, adverse employment effects tend to be short-term, while wage and earnings effects are more persistent. For example, using US survey data, Ruhm (1991) found that displaced workers were three-times more likely to be unemployed in the year following displacement than comparable workers who did not experience displacement, but this effect had largely dissipated after 4 years; in contrast, displaced workers' earnings losses were 10-15% lower four years after displacement. Similarly, using DWS, Farber (2004) finds strong short-run declines in employment (between 60% and 75% of displaced workers are employed at the time of the DWS), and earnings (up to 10% lower weekly earnings among those re-employed) of displaced workers.<sup>5</sup>

In a seminal study using administrative employer-employee matched data to identify displaced workers as those who leave their jobs as part of mass-layoffs from firms in the US (Pennsylvania), Jacobson et al. (1993) estimate that displaced workers earnings are substantially lower than comparative workers even after 6 years (on the order of 25% lower their pre-displacement earnings). For a variety of reasons, Jacobson et al.'s (1993) analytical sample is quite selective,<sup>6</sup> and it is not clear the overall effect of this on the estimated impact of displacement. Following Jacobson et al.'s (1993) approach for comparability, Von Wachter et al. (2009) and Davis and von Wachter (2011) examine the long-run impacts of displacement (up to 20 years), and conclude that job displacement has long-lasting effects on workers' earnings. For example, von Wachter et al. (2009) estimate initial earnings loss for male workers of about 30% on average, which declines to about 20% after 10 years, and recovers little after that. They also find similar impacts for workers with pre-displacement tenure of 3 rather than 6 years; and estimate somewhat smaller, although still substantial, short and longer term earnings losses for workers displaced at the peak of the economic recovery than workers displaced during the recession.

Hijzen et al. (2010) analyse the long term effects of job displacement on workers incomes in the UK using matched worker-firm data. They estimate earnings loss on the order of 50% in the first year after displacement, and about 10% and 20% after five years for displacements associated with mass layoffs and firm closures respectively. In contrast to US findings of relatively small employment effects, Hizen et al. finds that earnings losses in the UK are mainly due to employment effects, and the longer term impacts are largely associated with workers

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<sup>5</sup> Both the employment and earnings impacts are, on average, larger for less educated workers. As well as only facilitating short-run impacts (up to three years between the displacement date and the DWS survey), as Farber acknowledges, such earnings impacts likely understate the true impact because of foregone earnings growth, given there is no readily available comparison group of non-displaced to compare such earnings growth.

<sup>6</sup> First, because the reason for a job separation is not observed in such administrative data, they construct a "mass layoff" sample of workers whose firms' employment dropped at least 30% in the year following the worker's separation. Second, as this criteria is less robust for small firms, they also restrict the sample to firms with at least 50 workers. Third, they also focus on workers with stable, long-tenure (at least 6 years) relationships with the firm. Finally, as their data are only for Pennsylvania, to reduce the chance of overstating earnings losses due to workers moving out of state, they require workers to have earnings in each year.

who experience long term unemployment. They also find that the impacts of displacement are larger for males, skilled workers, older workers and those with longer job-tenure: the latter three dimensions consistent with larger impacts on workers with greater firm- or sector-specific human capital.

Upward and Wright (2015) analyse the impacts of self-reported job loss in the UK using British Household Panel Survey (BHPS) data. They estimate short-run earnings losses of nearly 40%, and long-run losses after 10 years of about 10%, and that the majority (about 80%) of the long-run earnings loss is due to lower wages (monthly earnings) conditional on employment,<sup>7</sup> in contrast to Hijzen et al.'s (2010) conclusion of lower employment rates. They also find that income from alternative sources, including self-employment, unemployment insurance, retirement income, contributes little to compensate the loss of earnings following displacement: reducing the loss of earnings by about 15% in the first year and 12% after 10 years.

Borland et al. (2002) analyse the effects of involuntary job loss in the UK and Australia. For the UK, they estimate displacement causes weekly earnings to fall about 15% in the year after displacement on average, although the effect is largely confined to workers who have a gap in employment, and depends on workers' characteristics. For Australia, they find workers experience substantial impacts on hours worked in the two years after displacement, ranging from about -80% in the first quarter to -20% in the eighth quarter, and these effects are largely due to weeks not employed. Borland et al. (2002) also find that re-employment following displacement is lower for older workers, but higher for males than females.

Schmieder et al. (2010), using administrative data to identify mass layoffs in West Germany in 1982, estimate displaced workers experience long-term earnings losses of 10-15%. They note that their estimates are larger than other studies for Germany using survey data, which include temporary layoffs that tend to have lower losses. Despite greater levels of job protection in Germany, these losses are comparable to those for the US and other countries, and that welfare payments provide relatively little compensation for earnings loss.

The international literature also finds evidence that the effects of displacement tend to be felt by workers prior to the displacement event. For example, Ruhm (1991) finds evidence of declining wage and employment two years before the subsequent permanent layoff; Jacobson et al. (1993) find evidence of declining relative earnings as much as four years before displacement, with stronger decline in the year prior; and Hijzen et al. (2010) find wages are lower over the three years before displacement.

There have been two recent studies of the effects of job displacement in New Zealand. Each of these studies find substantial negative impacts of job displacement on workers' subsequent labour market outcomes. First, Dixon and Stillman (2009) used matched employer-

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<sup>7</sup> Upward and Wright (2015) estimate that displaced workers have about 11% (8 percentage point) lower annual employment rates after 10 years.

employee administrative data, that is now part of the IDI, to identify workers who experienced involuntary job loss associated with firm closure or restructuring. Dixon and Stillman (2009) found that workers who lost their jobs due to firm closure experienced substantial employment and earnings loss, relative to observably comparable workers who didn't experience such a job loss. For example, displaced workers' employment rates were 17% lower one year after the firm closure, and remained 12% lower four years after closure; while their monthly earnings were 22% and 16% lower one and four years after firm closure.

Second, Dixon and Maré (2013) used longitudinal survey data from SoFIE. The main advantages of SoFIE relative to the administrative data are that it collected direct information on the reason for a job ending as well as typical socio-demographic information on the survey respondents. In particular, if an individual gave the reasons for a job ending as because "laid off/dismissed/made redundant", Dixon and Maré (2013) classified this event as a job displacement. In an attempt to reduce the confounding effects of just-cause dismissals on involuntary job displacements (i.e. separations associated with lay-offs and redundancies) from this survey response, their analysis focused on the subsample of such workers who had at least one year's tenure prior to the job ending.

Using this criteria, Dixon and Maré (2013) found that the incidence of job displacement in New Zealand over the period 2001–09 was 1.8%, but substantially higher (3.3%) during the recession of 2007–09. They also found substantially adverse impacts on displaced workers subsequent employment and earnings. For example, relative to comparable workers who did not experience a job displacement, the employment rate of those who did was 27% lower in the first year after displacement, 14% in the second year, and 8% in the third year. Furthermore, among the displaced workers who were re-employed, their hourly wages were 12% lower in the first year, 11% lower in the second, and 7% lower in the third year following displacement; while their average weekly or annual earnings were also 15–20% lower over the three years following displacement. This set of results suggest that the effects of displacement are extensive, affecting both workers chances of being employed and their level (i.e. hours) of employment if employed, as well as the hourly wage rate they earn when employed.

### **3 The IDI matched data**

The analysis in this paper uses data from Statistics New Zealand's Integrated Data Infrastructure (IDI). The primary data source used within the IDI is the Survey of Family, Income and Employment (SoFIE), which was an eight annual wave longitudinal household survey covering the period from October 2001 until September 2010. Individuals in the SoFIE sample are matched to other data sources in the IDI using their name, gender and date of birth. The IDI administrative data on employment, earnings and benefit income that we use is linked

employer-employee data (LEED) from Employer Monthly Schedules (EMS) which, since April 1999, each employer must file with Inland Revenue (IRD).

The first annual SoFIE survey was conducted between October 2002 and September 2003: 15,100 randomly selected households were approached, and responses were collected from 11,500 households consisting of 22,200 eligible adults and 7,500 children under age 15. The longitudinal SoFIE sample consisted of all eligible individuals (both adults and children) who responded in wave-1: the original survey members (OSMs). SoFIE attempted to track these OSMs in subsequent waves, and surveyed all members of households that included an OSM in each wave. The first wave of SoFIE collected retrospective information on individuals' employment over the period from the start of the calendar month one year earlier until the interview date.<sup>8</sup> Subsequent annual surveys collected retrospective information over the period since the last interview.

Job displacements in this paper are identified in the same way as in Dixon and Maré (2013). Specifically, individuals' were identified as potentially having experienced a job-displacement event during a SoFIE wave if they reported:

1. they had left a job, and
2. the reason for leaving was "Laid off / Dismissed / Made redundant".

As Dixon and Maré (2013) note (page 7), the latter response means that the potential job-displacement measure confounds lay-offs and redundancies, with job-loss because of worker misconduct. Assuming most dismissals are associated with short duration jobs, in order to reduce the incidence of dismissals based on this classification of displacement, Dixon and Maré (2013) focussed on the subsample of such workers who had held the job for at least one year. This is in line with the criteria in the OECD (2013) studies, and we will also adopt the one-year tenure requirement in our analysis.

The EMS lists each worker an employer paid earnings to, and withheld pay-as-you-earn (PAYE) tax from, in each month. The EMS system provides a full coverage of PAYE tax withholdings, and thus includes other non-earnings payments, such as working-age benefit payments. Thus, for the matched SoFIE sample, we use monthly administrative employment and earnings information over the period from April 1999 until September 2015. This time coverage provides at least 30 months of pre-displacement outcomes to control for differences between displaced and non-displaced workers, and can analyse the displacement impacts on workers' outcomes for at least 5 years after displacement.

Our sample consists of all individuals surveyed in SoFIE, aged 20-64, who can be matched to the IDI spine, and all of our outcome information comes from the EMS tables in the IDI. Because we don't have access to the migration information in the IDI, we are not able to

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<sup>8</sup> For example, individuals in households interviewed in the first month (during October 2002) provided information on the period from October 2001 until the interview date.

distinguish between non-observation in the EMS because of unemployment from sample attrition related to migration, which may differ for displaced and non-displaced workers. Although we don't think this is a major issue, in our analysis below we estimate the EMS observation rates for displaced and non-displaced workers are about 91% and 95% in the first year after displacement, and 83% and 86% in the fifth year. The lower coverage rates for displaced workers may reflect selective migration (or self-employment) following displacement, but they are also consistent with displaced workers being less likely to be employed and/or their partners' being the primary benefit recipient.

Our primary focus will be on the labour market outcomes of working-aged individuals, aged 20-64 years, who experience a job-displacement event from a job they have held for at least 1 year. We will refer to displaced workers as those workers who ever report a potential job displacement in SoFIE, and non-displaced workers as those who never report a potential job-displacement. We use the job-spell data in SoFIE to identify the calendar months of each reported job, and align this with monthly EMS data. Given the labour market outcome data used in our analysis is from the administrative EMS records, we don't restrict our focus to the longitudinal SoFIE sample and consider the full sample of individuals ever interviewed in SoFIE. Our measure of individuals' benefit income represents first-tier benefit income that is measured in the EMS records, and does not include other second-tier income support (such as Accommodation Supplements and Disability Allowances) or Working for Families (WfF) payments and tax credits, which were not available for this analysis. An individual's total income is the sum of their wage and salary earnings and benefit income. "Family" earnings (income) is the sum of the worker's and, when they exist, their spouse's earnings (income). All earnings and incomes used in the analysis have been adjusted using the CPI to be in constant June quarter 2016 \$-values.

For each worker who reported being displaced in SoFIE, we first define their focal displacement date as the month in which they were displaced from a job. For most workers who report a single displacement over the SoFIE waves, this is simply the calendar month and year associated with that displacement. For those who reported multiple displacements in SoFIE, we select the first displacement month from a job with at least 1-year's tenure, or the month of first displacement if the tenures associated with each of their displaced-jobs were all less than 1 year. This focal displacement month provides the date relative to which we will measure pre- and post-displacement outcomes.

Appendix Table A1 summarises the selectivity of the sample of displaced workers used for our analysis. Column 1 summarises the characteristics of all workers who report a job displacement in the SoFIE survey, where the characteristics are measured in the focal displacement month. Column 2 summarises the corresponding characteristics of the subsample who are matched to the IDI data. As over 98% of displaced workers are matched in the IDI, the

average characteristics of the matched sample are almost identical to the full sample. Columns 3 and 4 compare the subsamples of IDI-matched displaced workers with less than one-year versus at least one-year's tenure on the job at displacement. Displaced workers with less than one-year's tenure account for almost 30% of all potentially displaced workers. These workers are relatively more likely to be female, younger, Asian or Maori, and single, than those with longer job tenure. Short-tenured workers are also less likely to be observed as employed in LEED in their displacement month, and have lower earnings reported in either SoFIE or LEED.<sup>9</sup>

In Table 1 we present various summary statistics for our analytical sample of SoFIE workers who can be matched to the IDI, together with the sample of non-displaced workers employed in any focal displacement month.<sup>10</sup> Column 1 describes the sample of displaced workers with at least 1-year job tenure in the focal displacement months, while column 2 describes the corresponding pooled monthly sample of non-displaced employed workers with at least 1-year's tenure.<sup>11</sup> First, displaced workers are more likely to be male (57% compared to 49% of non-displaced workers), are slightly younger on average, slightly more likely to be Maori, and have lower qualifications than never displaced workers. Displaced workers are also less likely to be partnered, and have slightly smaller families.

In terms of labour market outcomes, 96% of displaced workers report a job-end in SoFIE in their focal displacement month, implying a high but imperfect concordance in the reported survey information; in contrast, the rate of reported job-ends among non-displaced workers is 10%. Displaced workers' job-tenure is 1.1 years less than that of non-displaced workers on average (5.7 versus 6.8 years), which is reflected in 63% of displaced workers had job-tenure of 1-5 years, compared to 54% of non-displaced workers. It is worth noting that only 88% of the displaced workers are observed as employed in the LEED data in their SoFIE-identified displacement month, compared to 93% of non-displaced workers in these same months. These are likely to be due to a combination of false-positive matching of individuals in the IDI, and misclassification errors associated with the SoFIE and LEED observed employment.<sup>12</sup> In

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<sup>9</sup> The substantially lower LEED earnings in the month of displacement for short tenured workers is consistent with them being less likely to receive (substantial) redundancy payments and having less accumulated leave payment.

<sup>10</sup> The main focus of our analysis in this paper is on the outcomes of displaced workers themselves, rather than their partners' or families. However, we report some of descriptive partner and family characteristics of these samples in appendix Table A2. The partners' of displaced and non-displaced workers have similar employment and earnings on average, and differences in the joint family earnings and incomes largely reflect differences in those of the workers. One of the issues in trying to analyse partners (or joint family) outcomes over the longer term using the administrative data sources is that the dissolution of couples is unobserved outside the SoFIE sample period.

<sup>11</sup> That is, while the sample characteristics of the displaced workers are measured in the month of their focal displacement, those of non-displaced workers are pooled over all non-displaced worker months in which a displacement occurs.

<sup>12</sup> For example, in other research (Hyslop and Townsend 2017), we have compared SoFIE and LEED measures of employment, and estimated misclassification rates associated with annual employment of 1-3% among those employed and up to 15% among the non-employed. We expect the monthly misclassification rates to be substantially larger.

addition, that the LEED employment rate is lower for displaced than non-displaced workers, likely reflects further inaccuracy associated with the measured timing of the displacement.<sup>13</sup>

Displaced workers' average SoFIE reported monthly earnings in their displacement month are about 30% lower than non-displaced workers average earnings in these same months, which may reflect either lower earning capacity or part-month earnings.<sup>14</sup> In contrast, the displaced workers' average monthly wage and salary earnings from LEED are more than three-times that of non-displaced workers in these months (nearly \$16,000 versus \$4,800).<sup>15</sup> Displaced workers high monthly earnings are consistent with them receiving either redundancy payments or accumulated leave payments in addition to any regular monthly earnings in these months, and suggests these payments amount to 2-3 months of earnings on average.

In Table 1 we also present the employment rates and average (natural logarithm of) earnings of displaced in the period prior to displacement, compared to those of non-displaced workers. Excluding the 6-months immediately before the displacement month to avoid contamination from misreporting, the employment rates and average earnings are similar for displaced and non-displaced workers. The annual employment rates of both groups in the year to 6-months before displacement (labelled "months [-18,-7]") are 94-95% (average monthly employment rates of 90-91%), and in the year prior to that (labelled "months [-30,-19]") are 91% (average monthly employment rates of 83-85%). Displaced workers average earnings are 8% higher, and 6% lower, than non-displaced workers over these periods respectively.

A secondary focus of our analysis is on whether the impacts of job displacement were different for workers who were displaced from a job before versus during the great recession that began in 2008. Because the New Zealand economy went into recession at the start of 2008, before the global financial crisis (GFC) started in the third quarter 2008, we treat the start of 2008 as the break point between the pre- and post-recession periods. Columns 3 and 4 of Table 1 describe the corresponding subsamples of displaced workers, i.e. workers who were displaced before or after the start of 2008. The characteristics are broadly similar across the subsamples. However, post-2008 displaced workers are relatively more likely to be men, older, single, and have no qualifications than those displaced before 2008. They also reported 10-15% lower earnings on average in their month of displacement.

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<sup>13</sup> That is, the non-displaced workers are more likely to be measured in an ongoing job-spell (before and after the observation month), while displaced workers are less likely to be employed after displacement, so mistiming of the displacement will also reduce the observed employment rate.

<sup>14</sup> An individual's SoFIE earnings in the displacement month are calculated by first estimating that month's earnings from each job spell using the spell earnings and spell start and end dates, and then summing these monthly earnings across job spells. That the fraction of displaced workers receiving welfare benefits in the LEED data was about twice that of non-displaced workers (6%, compared to 3%), and the average benefit income was lower, suggests part-month effects play some part at least.

<sup>15</sup> Non-displaced workers average LEED earnings are about 4% higher than their SoFIE reported earnings, which is consistent with differences observed in the broader SoFIE sample (Hyslop and Townsend, 2016a).

### **3.1 Event study descriptions of job displacement effects**

Before formally investigating the impact of job-displacement on workers subsequent labour market outcomes, we begin by providing some graphical descriptions of the patterns of worker outcomes around the point of displacement, and compare these to the outcomes of non-displaced workers. For this analysis, we construct two comparison groups of non-displaced workers. The first comparison group is based on a randomly matched sampling approach, and we will refer to it as the “random comparison” sample. In particular, for each displaced worker, we randomly select 10 non-displaced comparison workers among those report being employed in SoFIE in the focal displacement month.<sup>16</sup> We then align the time profile of the displaced and comparison non-displaced workers outcomes relative to their “displacement month”, which is normalised to month-zero. This facilitates descriptive event study comparisons of the average outcomes of the displaced workers and their non-displaced comparators over the consistent period from 30 months prior to displacement to 60 months following displacement.<sup>17</sup>

As the socio-economic characteristics, and employment and earnings of displaced and non-displaced workers shown in Table 1 differ, these differences may result in non-comparable labour market outcomes of the two groups, even in the absence of displacement. For this reason, we also select a propensity-score “matched comparison” sample of 10 non-displaced workers for each displaced worker that controls for differences in socio-demographic characteristics and employment and earnings histories of displaced and non-displaced workers. The details of this propensity-score matched sample are discussed below.

We begin by examining the employment rate patterns before and after the month of displacement. Figure 1 compares the wage and salary employment rates of the displaced workers and the two non-displaced comparison samples over the period spanning from 30 months before, to 60 months after, displacement. The left hand panels (a) compare the employment rates of the displaced worker and the “random comparison” samples for the full sample period, and for displacements before and after 2008; while the right hand panels (b) show the corresponding figures for the displaced worker and “matched comparison” samples.

Focusing first on the full sample period and the random comparison in panel (a), the employment rates of the two groups before displacement appear remarkably similar. Over the months -15 to -3, the LEED-employment rates of each group are roughly constant and almost exactly equal (0.93, the average LEED employment rate of non-displaced workers seen in Table 1). The employment rates gradually diverge over the range from months -15 to -30: displaced

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<sup>16</sup> That is, for consistency between the displaced and non-displaced worker samples, we use individuals’ SoFIE reported employment status to determine selection eligibility. As seen in Table 1, the LEED measured employment rates is lower for both displaced and non-displaced workers, resulting in employment rates less than 1 near to the displacement date in the figures below. The random selection is done with replacement, meaning that the same non-displaced worker may be matched to more than one displaced worker, either in the same month or (more likely) in different months that they are employed.

<sup>17</sup> However, because we focus on a fixed age sample of those aged 20-64, younger workers tend to “age-in” to the sample analysis over time and, conversely, older workers will “age-out” of the sample.

workers' employment rate is about 2-3% lower than non-displaced workers at month -30. The difference at month -30 suggests workers who become displaced have a slightly lower attachment than other workers. There is also a noticeable drop in the employment rate of displaced workers in the 2-3 months prior to displacement, which we suspect is associated with timing errors in the reported displacement month in SoFIE relative to the LEED data. As expected, the pre-displacement employment rates of the matched comparison sample in panel (b) closely match those of the displaced workers, except in the 1-3 months immediately prior to displacement.<sup>18</sup>

The post displacement patterns of employment of both comparison groups are also remarkably similar and, compared to the patterns for displaced workers, each imply substantial and adverse employment effects of job-displacement over the five year follow-up period. In particular, displaced workers' employment rate falls over 30 percentage points in the 3-6 months around displacement, from about 93% to about 61%, compared to roughly stable employment of non-displaced workers. There is then a gradual increase in employment over the following 12-18 months to about 73% by month 18, during which time the employment rates of the comparison samples decline gradually to about 85%, suggesting the displacement effect is still -12% at this point. The displaced and non-displaced employment rates continue to narrow slowly over the next 6-12 months, but displaced workers remain on the order of 10 percentage points less likely to be employed 3-5 years after being displaced. The longer term differences between the displaced and matched-comparison sample are perhaps 1-2 percentage points lower than the differences with the random-comparison sample, but are still substantial.

The employment rate patterns are broadly similar for job-displacements that occur before and after 2008. However, the adverse employment impacts appear to be about 5 percentage points greater for workers displaced after 2008: this is true both in the short run, where the fraction of displaced workers employed 3 months after displacement falls to 57% compared to 61% for workers displaced before 2008, and over the longer term, where the employment rate recovery is only to about 70% compared to 75% pre-2008. In addition, the employment rates of the comparison groups tend to track a little higher over post-2008 compared to pre-2008 period, which also increases the implied displacement impact over the latter period.

We next consider workers' average log(monthly earnings) patterns around displacement, conditional on being employed in Figure 2, which is organised in the same way as Figure 1.<sup>19</sup> As

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<sup>18</sup> There is evidence in the literature the displacement may adversely affect employment and earnings before displacement occurs – e.g. Jacobson, et al. (1993), Dixon and Maré (2013). Although we see no evidence of this in our data, other than in the drop in the LEED employment rate in the 3 months before displacement, we exclude the 6-months preceding displacement in matching employment and earnings histories.

<sup>19</sup> We prefer to compare conditional earnings differences of displaced and non-displaced workers in order to abstract from the extensive, employment, margin contribution. This is partly because of possible attrition from the IDI sample, so that non-observed earnings may reflect sample attrition rather than zero earnings. However, comparing conditional earnings is subject to possible non-random selectivity effects associated with who are employed among the displaced and non-displaced workers. For example, if displaced workers who find re-employment are not representative of all displaced workers and are of higher ability on average, their average earnings may overstate the

with the employment rate trends, log(earnings) of displaced workers before displacement are remarkably similar to those of both of the comparison groups over the full sample period, although the random comparison group average is again 1-3% higher. Noticeably, there is no apparent pre-displacement drop in earnings that has been found in the literature (e.g. Jacobson et al. (1993), Dixon and Maré (2013)). Consistent with the substantially higher average earnings for displaced workers seen in Table 1 in the displacement month, displaced workers earnings are about 50 log points higher than the comparison groups on average.<sup>20</sup>

Earnings of workers fall substantially in the 2-3 months immediately after displacement, by about 40 log points (50%) on average. Displaced workers earnings then recover over the next 6-9 months, to be about 20 log points (or 20%) lower than non-displaced workers on average 1 year after displacement. Their earnings continue to recover more slowly over the following 4 years, so that the difference is around 15% five years after displacement. These patterns imply that, as well as displaced workers experiencing substantial (total) employment loss over the five years following displacement, they also experience substantial earnings loss associated with either fewer hours of work or lower (hourly) wages conditional on being employed.

The earnings patterns in the pre-2008 period are broadly similar to the full sample patterns. However, the random comparison average earnings are almost the same as the displaced workers prior to displacement, and the post displacement differences appear slightly smaller, suggesting the earnings recovery prospects were better in this period. Conversely, the post-2008 patterns are somewhat different, although also less smoothly estimated. In particular, the lower average earnings of displaced workers prior to displacement suggests they were more negatively selected on earnings during the recession; while comparison with the matched sample suggests the post-displacement impacts on earnings were relatively smaller over this period (about -30 log points short term, and -10 log points long term).

Figure 3 graphs the trends in the benefit receipt rates, average log(monthly benefit income) conditional on benefit receipt, and average log(monthly total individual income) conditional on receiving LEED income, of the displaced workers and non-displaced comparison samples over the full sample period. Individuals' total LEED income, which combines wage and salary earnings and benefit income, is the most inclusive income measure we observe for workers in the LEED data, although it excludes any self-employment earnings and other family income, in particular partners' income.

The first pair of graphs show declining benefit receipt rates prior to displacement, with the receipt rate being higher for the displaced workers than either comparison sample. Displaced workers then experience a large (11 percentage point) increase in benefit receipt

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average potential earnings of all displaced workers: if so, differences between displaced and non-displaced workers earnings will understate the impact of displacement.

<sup>20</sup> Displaced worker's average log(earnings) are also noticeably higher in the months immediately before or after displacement, consistent with some mistiming between SoFIE and LEED.

from 4% before displacement to 15% within 3 months of displacement. The fraction receiving benefits then declines steadily over the following year to around 9%, and then more slowly to about 8.5% over the remaining follow-up period.

The second pair of graphs show relatively noisy and weak patterns of average benefit income among those receiving benefits. The third pairs of graphs in Figure 3 show monthly average  $\log(\text{total income})$  patterns of the three groups that are qualitatively similar to those of average earnings in Figure 2. In particular, displaced workers total incomes drop by about 45 log points soon after displacement relative to non-displaced workers on average, then recover 20-25 log points over the following 12-18 months, but remain 15-20% lower at the end of the five year follow-up period.

We have also plotted patterns of average outcomes for displaced and non-displaced workers' partners, and total family income, before and after displacement in Figure 4 and Figure 5. Although these figures are suggestive of possible effects, they should be interpreted with some caution: both because the samples are conditional on partners existing, and as partners are identified from the SoFIE survey information, these samples will change over time and are only available over the period 2001–2010.<sup>21</sup> Nonetheless, Figure 4 suggests that partners' employment rates appear largely unaffected (except possibly 3-5 years after displacement), while their conditional earnings may also fall in the period following displacement, although the trends are relatively noisy. However, there is a noticeable increase in the benefit receipt rates of displaced workers' partners following displacement, which suggests that the increase in displaced workers' benefit receipt rates captures only part of the increase in income support received by displaced workers and their families.

The conditional benefit income received by partners in Figure 5 are too noisy to draw clear conclusions about the trends, but the trends of  $\log(\text{family income})$ , which combines the worker's and their spouse's income, closely match those of displaced workers'  $\log(\text{earnings})$  and  $\log(\text{total income})$  discussed earlier.

We have also provided, in the appendix, event study graphs for a variety of alternative samples and/or outcomes using random comparison groups. Figure A1 shows the employment rate and  $\log(\text{monthly earnings})$  trends for all displaced workers (i.e. including those with less than one year of job tenure). Figure A2 shows the trends in monthly earnings (in levels not logs) for all displaced workers and those with at least one year of tenure. Figure A3 shows, for workers with at least one year of tenure, workers average benefit income, average total individual income, and average total family income. Figure A4 shows average earnings for the pre-2008 and post-2008 subsamples for workers with at least one year of tenure; and Figure A5 shows the analogous average total worker income for these subsamples. Finally, Figure A6

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<sup>21</sup> Also, because an individual's partner may change over time, the samples do not necessarily consist of the same partners.

shows the average earnings and average income for partners of workers with at least one year of tenure. Each of these figures show broadly similar patterns to those discussed above, using alternative metrics.

As a further robustness check, we consider whether the apparent impact of job displacement is simply associated with ending a job. To do this, we conducted an event study comparing outcomes of displaced workers matched to a random sample of non-displaced workers who reported a job-end in SoFIE in the same months. A selection of the results from this analysis are presented in Figure A7 for employment, log(earnings), benefit receipt, and log(total individual income). One difference of note, in the employment rate trends in panel (a), is that non-displaced workers who report a job-end experience about a 5% drop in employment in the 2-3 months following the job end, which may be due to some workers exiting employment at the end of a job. The employment rate of this group then tracks about 5% lower than that of the broader sample of non-displaced workers shown in Figure 1. Other than this point, the patterns are remarkably similar to the patterns for the broader samples of non-displaced workers discussed above, showing broadly similar pre-displacement outcomes and large adverse employment and earnings impacts after displacement.

In summary, these event study figures suggest job-displacement has large adverse impacts on short term employment and conditional earnings (lower hours and/or wages) and, although somewhat reduced, continued substantial longer term effects. There are also large increases in earnings payments in the months around displacement (on the order of 2-3 months earnings, on average), consistent with either redundancy or accumulated leave payments. There are also increased rates of benefit receipt, by the worker and also their partner, indicates displaced workers receive some degree of government income support. Finally, workers total individual income measured in LEED remains substantially lower than non-displaced workers after five years.

## **4 Estimating the impacts of job displacement**

We now turn to the framework for estimating the effects of displacement on workers outcomes. Building on the event study patterns discussed above, the main outcomes we focus on are workers' post-displacement employment and earnings patterns, and the receipt of government income support from working-age benefit payments. The characteristics of displaced and non-displaced workers in Table 1, and the patterns of outcomes in Figure 1 and Figure 2, suggest that job displacement is not random for workers and so it is necessary to control for such differences on the resulting outcomes.

In this section, we present estimated displacement impacts based on two distinct and complementary approaches. First, we estimate the impacts using a regression-adjusted approach that controls for the effects of observed characteristics that may be correlated with

displacement and subsequent labour market outcomes, as well as the effect of any constant unobserved factors. Second, we using matching methods to select a counterfactual group of non-displaced workers matched to each displaced worker, and then estimate the impacts of job displacement as the difference between the average outcomes of the displaced and non-displaced comparison samples of workers. We discuss each of these methods and estimates in turn.

#### 4.1 Regression-adjusted Job displacement impacts

The event study profiles of average outcomes discussed in the previous section for displaced workers around the month of displacement, compared to non-displaced workers randomly matched to a displacement month, suggest that although the two groups have different characteristics and pre-displacement outcome profiles, such differences may be amenable to a standard regression function control approach. This is the approach we use in this section to derive preliminary estimates of the impacts of job displacement on workers' subsequent labour market outcomes.

In order to estimate the effect of displacement on labour market outcome  $y$  (such as being employed, receipt of benefit,  $\log(\text{earnings})$  or  $\log(\text{income})$ ), we use a regression approach to control for a variety of possibly confounding factors. These include observed socio-demographic and labour market history differences between displaced and non-displaced workers, possible (unobserved) aggregate differences between displaced and non-displaced workers, secular and/or seasonal time effects, and possibly unobserved time-invariant worker-specific effects. In particular, we consider specifications within the following regression model:

$$y_{it} = X'_{it}\beta + \delta_0 \cdot DW_i + \sum_{s=1}^5 \delta_s \cdot DW_{it}^s + \gamma_t + \alpha_i + \varepsilon_{it} \quad (1)$$

where  $y_{it}$  is the outcome of interest for worker- $i$  in month- $t$ ;  $X_{it}$  is a vector of observable socio-demographic, and labour market history, characteristics of the worker;  $DW_i$  is an indicator variable for whether worker is a displaced worker (i.e. ever experienced a job-displacement in the data);  $DW_{it}^s$  ( $s=1, \dots, 5$ ) are dummy variables for whether the worker was displaced from a job between ( $s-1$ ) and  $s$ -years ago;<sup>22</sup>  $\gamma_t$  are calendar time effects to capture either seasonal effects or aggregate trends;  $\alpha_i$  is a worker-specific fixed effect; and  $\varepsilon_{it}$  is a residual term. The coefficient on the displaced worker dummy variable,  $\delta_0$ , allows for displaced workers to have different average outcomes (pre-displacement) than non-displaced workers, irrespective of displacement. The displacement impacts of interest are captured by the coefficients on the post-displacement variables ( $\delta_s, s = 1, \dots, 5$ ), which we will generally allow to vary by year since displacement over the post-displacement period.<sup>23</sup>

<sup>22</sup> As we discuss above, Figure 2 shows no sign of earnings decline prior to displacement. Because of this, we simply control for a displaced worker effect that allows for a fixed pre-displacement difference.

<sup>23</sup> Note that, each of these displacement effects measure the impact over the respective year since displacement, so will average the within-year effects observed in the figures.

This regression-adjusted estimation approach has several advantages. First, we are able to use the full sample of longitudinal data for displaced and non-displaced workers although, for consistent periods of observation, we use the random matched sample used for the event study discussed in section 3.1 over the range from 30 months prior to displacement until 60 months after displacement. Second, the sampling errors of the regression are straightforward to estimate, using standard methods to control for clustering effects, etc. Third, we are able to control for unobserved worker-specific fixed effects that may affect the outcome, in addition to the observable factors included in the regression. However there are potential weaknesses in the method as well: the principal weakness being the assumption that the regression specification adequately controls for outcome differences between displaced and non-displaced workers. If displaced and non-displaced workers have quite different characteristics this implies the functional form may require a degree of extrapolation to control for such differences.

## **4.2 Matching-based analysis of job displacement impacts**

The second method we apply to identify the impacts of displacement on workers, is to select a matched comparison sample of observationally similar workers who did not experience a job displacement at that point in time, and then compare the average outcomes of the displaced and matched non-displaced comparison samples. This matching approach follows that used by Dixon and Maré (2013), and much of the recent literature. In particular, we use a propensity-score matching approach to identify and select observationally similar non-displaced workers to compare outcomes with those of displaced workers.

This method relies on the “unconfoundedness” assumption of the assignment of displacement across workers. That is, conditional on observable characteristics ( $X_i$ ), a worker’s potential labour market outcomes in the event of displacement or not are independent of whether or not they are displaced. This means that workers’ displacement status is essentially random conditional on  $X_i$ , and there are no unobservable characteristics that jointly affect both whether a worker is displaced and their outcomes. This is a strong and untestable assumption. In addition, matching estimators require there is overlap in the range of probability of being displaced between workers who are and are not displaced. This requirement ensures there are comparable, in terms of the propensity to be displaced, non-displaced workers to provide a counterfactual for displaced workers.

Our propensity-score matching approach involves three stages. First, we specify a Logit model for whether a worker is displaced or not in terms of a set of socio-economic characteristics and labour market histories of the individuals.

$$P(DW_i = 1|X_i) = \frac{\exp(X_i'\beta)}{1+\exp(X_i'\beta)}. \quad (2)$$

This model is estimated using the sample of displaced workers observed in their focal displacement month and non-displaced workers in those months with at least one year of job tenure.

Second, we use the estimated model to select non-displaced workers to use as comparison matches for the displaced workers. The matched selection is done with replacement so a non-displaced worker may be matched to multiple displaced workers. To do this, we estimate the probability of being displaced for all workers in the sample with at least one year of tenure: for worker- $j$ ,  $\hat{p}_j = \frac{\exp(X_j'\hat{\beta})}{1+\exp(X_j'\hat{\beta})}$ , is their “propensity score” associated with being displaced. We then select up-to 10 non-displaced workers whose propensity scores are closest to each of the non-displaced worker’s propensity score, subject to two constraints. First, we use only displaced and non-displaced workers whose propensity scores lie within the “common support” range of non-displaced and displaced worker scores respectively: this restriction ensures observations satisfy the overlap restriction. Second, to be matched, non-displaced workers must have propensity scores that lie within +/-1% of a displaced worker’s propensity score: this ensures matched workers have comparable propensity scores to the displaced workers. That is, a non-displaced worker- $j$  is a matched comparison to displaced worker- $i$  if  $\hat{p}_j$  is one of the 10 closest to  $\hat{p}_i$ , and  $abs(\hat{p}_j - \hat{p}_i) < 0.01$ .

Finally, using the matched sample of displaced and non-displaced workers, we then estimate the impact of displacement on outcome  $y$ , as the average difference between the displaced (actual,  $y_i$ ) and matched non-displaced (counterfactual,  $\hat{y}_{i0}$ ) worker outcomes:

$$\hat{t} = \frac{1}{N_1} \sum_{i=1}^{N_1} (y_i - \hat{y}_{i0}) \quad (3)$$

where  $N_1$  is the number of displaced workers with common overlap propensity score, and  $\hat{y}_{i0} = \sum_j \omega_{ij} \cdot y_j$  is displaced worker- $i$ 's estimated matched counterfactual outcome ( $y_j$  is the outcome of non-displaced worker- $j$ , and  $\omega_{ij} = 1/n_i$  if  $j$  is one of the  $n_i$  matched workers, and  $\omega_{ij} = 0$  if not).

We will estimate the displacement impacts first for a variety of outcomes for a matched sample of displaced and non-displaced over the full sample, and then repeat the exercise for a variety of subsamples. The subsamples will be the pre-2008 and post-2008 subperiods; by age of worker (20-29, 30-49, and 50-64); and separately for male and female workers.

## 5 Empirical impacts of job displacement

In this section we discuss the results of each of the regression-adjusted and propensity score matching analyses of the impacts of job-displacement on workers subsequent labour market outcomes and income support.

## 5.1 Regression-adjusted estimates

Our regression analysis uses data from the random matched sample of displaced and non-displaced workers who have at least one year of tenure at the time of focal displacement. In order to have a consistent period relative to displacement, we use observations from 30 months before to 60 months after displacement.

First, Table 2 presents estimated effects from four specifications of regression (1) for the impact of job-displacement on workers monthly wage and salary employment. All of the specifications include a vector of worker level observed characteristics (a female dummy variables, age and age-squared in the current month; and age and age-squared, both interacted with female, education, ethnicity, and 1-digit industry and occupation in the focal month of displacement), calendar year dummy variables for aggregate time effects, and calendar month dummy variables for seasonal patterns. For each of the regressions we present estimated standard errors which allow for clustering at the worker level.

The first regression results that we present in column (1) includes a single post-displacement dummy variable, which restricts the displacement impacts to be constant throughout the post-displacement period. The results imply that displaced workers had nearly 5% higher employment rates before displacement, but the impact of displacement was to lower their employment rate by about 15% on average over the five year post-displacement period. We relax this constant displacement effect constraint in column (2), and allow the impacts to vary discretely over each of the annual (12-month) periods post-displacement. The displacement impact estimates are broadly in line with the event study trends seen in Figure 1: we estimate a large (-21%) impact during the first year, about one-third lower in the second year (-14%), and slightly lower again (between -12 and -13%) three to five years after displacement. Each of these estimates are highly statistically significant.

In column (3) we interact the post-displacement annual dummy variables with a post-2008 dummy variable in order to examine whether the impacts of displacement are larger in the post-2008 period. The results imply that the adverse impact of displacement were somewhat lower during the pre-2008 economic expansion than the post-2008 recession, with the differences being strongly significant 1-2 years after displacement and weakly significant 3-5 years after. The estimated employment impacts were -18.6% for workers displaced pre-2008 and -25.2% for those displaced post-2008 in the first year following displacement; -11.8% and 18.3% respectively in the second year; and about -11% and between -14% and -16% respectively three to five years after displacement.

The final specification we consider in column (4), allows for worker-specific fixed effects. The results show stronger adverse impacts for workers displaced before 2008, and smaller and statistically insignificant (worse) effects for post-2008 displaced workers, than in column (3).

Comparing these two sets of results suggests that displaced workers are more positively selected

Table 3 contains estimates of the impacts of displacement on the other main outcomes of interest – i.e. workers' log(earnings), benefit receipt, log(benefit income) and log(total income). The results in this tables comes from regression specifications analogous to that in column (4) of Table 2;<sup>24</sup> we also present the corresponding set of results from the column (3) specifications in appendix Table A3. The job-displacement impacts on monthly wage and salary earnings, in the first column, are strongly negative among workers displaced before 2008 and slightly worse for workers displaced after 2008 (although the differences are not statistically significantly). The impacts are -31 log points (about -26%) in the first-year after displacement, and about -20% (-19 to -22 log points) over the subsequent four years. We estimate 1-3 percentage point larger impacts (in the final column) for workers log(total income) in the pre-2008 period, and the post-2008 effects are about 7% (significant) larger in the first three years after displacement.

The estimates in the second column of Table 3 show pre-2008 displaced workers receipt of welfare benefits were 6% higher in the first year, 4% in the second year, and gradually fell to about 3% after five years. The impacts on benefit receipt were statistically significantly, and substantially, higher during the recessionary period: 10% higher in the first year, 5-7% in subsequent years. Finally, conditional on receiving a benefit, displaced workers average benefit income was also 13-16% higher, although these estimates tend to be imprecisely estimated, and there is little evidence of differences between workers displaced before or after 2008.

The corresponding estimates presented in Table A3 are broadly similar to the preferred specification, estimates in Table 3. However, the pre-2008 earnings and total income are generally smaller, and the post-2008 differences are larger and more statistically significant. We have also estimated analogous regressions using the sample of displaced workers randomly matched to non-displaced workers who report a job-end in those months. Table A4 summarises the results for the fixed-effects specifications, which are very similar to the corresponding estimates in Table 2 and Table 3.

## **5.2 Propensity score matching estimates**

We now turn to our propensity score matching approach to estimating job-displacement impacts. We first discuss the estimation model and properties of the displaced and non-displaced workers' propensity scores. We estimate the propensity score model, equation (2), using a vector of variables that control for worker's age, gender, education, ethnicity, job tenure, industry and occupation, and the numbers of months employed and average log(monthly earnings) over the two annual periods from months 7-18 and 19-30 prior to the focal month.

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<sup>24</sup> Note that, because of the noticeably high earnings and incomes of displaced workers in the displacement month and months immediately before and after displacement, we also include dummy variables for these months in the log(earnings) and log(total income) regressions.

This model is estimated using all displaced worker observations in their focal month of displacement, together with a 1% random sample of non-displaced workers who are employed in those months with at least one year of tenure.<sup>25</sup>

We present the propensity score model estimation results in Table 4. The first two columns contain the model estimates (and standard errors). These estimates confirm that men, older workers, and non-Asian workers are significantly more likely to be displaced. Also, those with shorter (1-2 years) job-tenure are more likely to be displaced than workers with longer than three years tenure, and there are some significant differences across industries and occupations. The estimates show that the pre-displacement employment and earnings variables have individually statistically insignificant predictive effects on displacement. The pseudo-R<sup>2</sup> of 0.06 indicates the model has some predictive power, but it is not overly strong. The latter two comments are consistent with the comparatively similar event study patterns discussed above for the random comparison versus matched comparison samples, particularly the close fitting patterns in the period prior to pre-displacement. The implication of this is that job-displacement is largely a random event, at least with respect to the set of observable characteristics we are able to control for.

This conclusion is also consistent with broadly similar distributions of displaced (“Treated”) and non-displaced (“Untreated”) workers’ propensity scores, shown in panel (a) of Figure 6. That is, although the displaced workers tend to have somewhat higher propensity to be displaced than non-displaced workers, meaning that displacement is not random conditional on the covariates used in the estimation, the broad overlap and similarity between the two distributions is consistent with the relatively weak explanatory power of the propensity score model estimated. Also, there are only 6 displaced workers in the estimation sample whose propensity score does not lie within the range of non-displaced workers propensity scores: these displaced workers are excluded from the subsequent impact analysis.

Using the estimated propensity score model, we then select a matched sample of displaced and non-displaced comparison workers. To do this, for each of the displaced workers with propensity scores within the common support range, we select up to 10 non-displaced workers from the full sample of observations whose propensity scores lie in the common support range, are closest to (and within 1 percentage point of) that of the displaced worker. The final four columns in Table 4 compare the average values of each of the propensity score model’s covariates of the displaced and matched non-displaced sample. The results confirm that the matched samples are statistically balanced.

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<sup>25</sup> We use a random sample of non-displaced workers because monthly job-displacements are extremely rare events (approximately 0.2% over our sample). A reduced 1% sample of non-displacements results in the average displacement rate being higher (about 20%) in the estimation sample, which facilitates greater discrimination in the matching phase that follows. Also, 12 displaced workers are dropped from the model estimation due to perfect fit of the model associated with the included covariates.

We use this matched sample to estimate the impact of job-displacement on the analogous set of outcomes examined using the regression-adjusted approach above. For each outcome, we compare the average of the monthly outcomes during each of the five years after displacement. The results are presented in Table 5, with each outcome in a column, and the annual impact estimates for each year in the rows.<sup>26</sup> Although the bootstrap method is the standard approach to estimate standard errors associated with matching estimators, Abadie and Imbens (2008) prove that this is not valid for nearest neighbour matching with replacement, and they recommend using analytic standard errors derived in Abadie and Imbens (2006). We follow their advice and present Abadie-Imbens (AI) standard errors for all of the propensity-score matching estimates below.<sup>27</sup>

The estimated impacts on employment are in column (1), show larger first-year impacts (-24 percentage points), and somewhat smaller impacts 3-5 years (-8 to -9 percentage points) after displacement than shown in Table 2. There are similar patterns of higher estimated short-term impacts on average conditional log(earnings), benefit receipt and conditional log(total income), and smaller estimates of the longer-term impacts compared to the regression-adjusted estimates. Nonetheless the longer term (4 or 5 years after displacement) for these outcomes remain statistically significant and substantial, on the order of 14-15% lower conditional earnings, 4-5 percentage point effects on benefit receipt, and 18-19% lower total income.

### **5.3 Subsample analyses**

We next repeat the propensity score matching analysis for a variety of subsamples, using the same approach as described above, and discuss the results in this section. For each subsample of displaced workers, we re-estimated the propensity score model, selected a matched comparison group of non-displaced workers, and then estimated the job-displacement impacts as the average difference between the subsample displaced and matched non-displaced workers' outcomes.

We don't present the full set of preliminary propensity score model estimation, but panels (b) and (c) of Figure 6 compare the propensity score distributions, based on separate estimation of the pre-2008 and post-2008 sub-samples respectively, and the appendix Figure A8 contains the analogous distributions for each of the other subsamples we analyse. The distributions of propensity scores in the pre-2008 period are broadly similar to the full period; while the post-2008 distributions are more widely spread, reflecting the higher displacement rates during the recession; and again displaced workers tend to have higher displacement propensities.

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<sup>26</sup> We have also estimated the impacts as-at the middle month of each post-displacement year. The estimated impacts are similar to those presented here. Note, the sample sizes shown in the table decline over time because of older workers ageing out of the 20-64 working age range.

<sup>27</sup> We have also computed bootstrap standard errors for these estimates. For the full sample estimates, the estimated bootstrap standard errors are almost the same as the AI standard errors presented. However, the bootstrap standard errors estimated in the subsamples analysed are generally substantially smaller than the AI standard errors. For this reason we think presenting the AI standard errors is somewhat more conservative.

We begin by focusing on job-displacement in the pre-2008 expansion versus post-2008 recession. Table 6 contains the respective estimates of job-displacement during the pre-2008 and post-2008 periods. The first column shows that the estimated impact on employment was typically 5-6 percentage points worse for workers displaced after 2008 than before; while the impacts on benefit receipt are similarly 3-4 percentage points worse in the recession. The estimated impacts on conditional earnings and incomes are also somewhat worse in this period, although the differences are less systematic.

Younger workers likely have less firm- and industry-specific human capital associated with the job, so that a job-displacement should have a smaller impact on their subsequent labour market outcomes than for older workers. To examine this hypothesis, our second set of subsamples is by age of worker at job-displacement, where we distinguish young (<30 years), prime-aged (30-49) and older (50-64) workers. The estimated impacts for these subsamples are in Table 7. The results generally confirm the hypothesis, with the estimated impacts of job displacement increasing with the age of displaced workers. Also, although there are sizeable and significant effects for younger workers in the first and second years after displacement, the impacts are generally smaller and insignificant (although still negative) four and five years after. The exception is in benefit receipt, where the impact drops only slightly after the first year, and benefit receipt rates of displaced workers are estimated to be 5-7 percentage points higher than they would have been in the absence of the job-displacement. Conversely, the estimated longer-term impacts are much larger for older workers: 11-12 percentage points lower employment, and about 25-30% lower conditional earnings and incomes, four and five years after being displaced.

The final sub-sample analysis we consider is for male and female workers separately. These results are presented in Table 8. Although the individual differences are not statistically significant, the differences are persistent and indicate that job displacement has larger effects on female employment and benefit receipt. For example, female employment rates are 9-11 percentage points lower 3-5 years after displacement compared to 7-9 percentage points for males, and their benefit receipt rates are 1-3 percentage points higher. However, we estimate smaller effects on female conditional earnings and incomes.

## **6 Concluding discussion**

This paper has analysed the impacts of involuntary job loss on workers employment, earnings and receipt of income support payments over the following five years. It uses the SoFIE survey information to identify a sample of workers who experienced an involuntary job-displacement over the period from 2001–2010, and compares their subsequent outcomes to those of workers in SoFIE who never reported a job-displacement. The SoFIE analytical sample is then matched to administrative data from the IDI to track outcomes over the post-displacement follow-up period.

We present three complementary sets of analyses of the impacts of job-displacement. First, a graphical event study, that shows the monthly trends in workers employment rate, log(earnings) conditional on being employed, benefit receipt rates, and conditional log(benefit income) and log(total individual income), for displaced workers and two alternative samples of non-displaced workers. Second, a regression-adjusted approach to estimate the impacts of displacement on these outcomes, which controls for a variety of observed and unobserved worker characteristics. Third, a propensity score matching approach to estimate the impacts, by comparing the average outcomes of displaced and similarly-matched non-displaced workers.

The patterns of estimated impacts across these three analyses are broadly similar. First, we estimate displaced workers on average have 20-25% lower short-term employment rates over the first year following displacement, and 8-13% lower longer-term employment three-five years later. The estimated impacts are consistently about 5% worse for workers displaced during the recession from 2008 than during the economic expansion before 2008. Given the differences in the sample periods, definition of displacement, and sample selection and use of IDI outcomes, our estimates are comparable to those of Dixon and Stillman (2009) and Dixon and Maré (2013). Our first-year estimates lie between Dixon and Stillman's (2009) 17% estimate based on firm closures, and Dixon and Maré's (2013) 27% which includes workers displaced in 2008 and 2009, while our estimated longer term impacts are similar to the latter's second-year estimate of 14%, and the former's fourth-year estimate of 12%.

Second, we estimate that, conditional on being employed, displaced workers' earnings were about 25% lower in the first year and about 15% lower five years after displacement. This implies that, as well as having lower employment, displaced workers either work fewer hours or earn lower hourly wages in their subsequent jobs. Although we are unable to identify these separate hours and wage effects in the monthly earnings data in the IDI, using Dixon and Maré's (2013) estimates of 7-12% lower wages 1-3 years after displacement, suggests the estimated earnings effects were roughly evenly split between lower hours and lower wages. We also find some evidence that conditional earnings are more adversely affected for workers displaced during the recession, although the differences are smaller and less precisely estimated than for employment.

Third, the receipt of government income support increased, which helped to offset the adverse employment and earnings impacts following job-displacement. We estimate benefit receipt rates were 6-11% higher in the first year, and 3-4% higher in the fifth year, after displacement for workers displaced before 2008. In line with the stronger impacts on employment for works displaced after 2008, the impacts on benefit receipt were 2-4% higher during this period. In addition, the descriptive event study indicates there was an increase in benefit receipt by displaced workers' partners following displacement, and we have only been able to measure main (Tier-1) benefit income support, and thus miss other forms of support

displaced workers are likely to have received. For these reasons, the effective increase in government income support for displaced workers and their families is likely to be greater than that measured simply by the workers' benefit receipt in the IDI.

Finally, we estimate that displaced workers' total individual income was about 30% lower in the first year after displacement, and still about 20% lower in the fifth year after. This measure, which combines wage and salary earnings and benefit income, provides a more complete income measure for the roughly 90% of our analytical samples that have income. Subject to the caveats that it misses benefit income received by their partners as a result of displacement, as well as any increase in self-employment earnings by displaced workers, these estimates show that job-displacement has substantial long-term effects on workers labour market outcomes and income support.

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Table 1: Sample descriptive statistics

Socio-demographic Variables:	Displaced Workers	Non-Displaced Workers	Displaced Workers	
			pre-2008	post-2008
Female	0.43 (.50)	0.51 (.50)	0.44 (.50)	0.41 (.49)
Age	41.8 (12.3)	42.4 (11.6)	41.6 (12.0)	42.2 (12.6)
Ethnicity:				
European	0.78 (.42)	0.78 (.41)	0.78 (.41)	0.77 (.42)
Maori	0.12 (.33)	0.11 (.31)	0.13 (.33)	0.12 (.32)
Pacifika	0.05 (.22)	0.05 (.21)	0.05 (.21)	0.06 (.24)
Asian	0.03 (.17)	0.04 (.20)	0.03 (.16)	0.03 (.18)
Other	0.02 (.13)	0.02 (.12)	0.02 (.13)	0.02 (.14)
Education:				
No qualification	0.20 (.40)	0.16 (.37)	0.18 (.39)	0.23 (.42)
High school	0.30 (.46)	0.26 (.44)	0.32 (.47)	0.27 (.44)
Vocational	0.36 (.48)	0.37 (.48)	0.36 (.48)	0.35 (.48)
Bachelor degree	0.10 (.30)	0.13 (.33)	0.09 (.29)	0.11 (.31)
Higher degree	0.05 (.21)	0.08 (.27)	0.05 (.21)	0.04 (.21)
Family size	2.69 (1.38)	2.76 (1.40)	2.68 (1.37)	2.69 (1.38)
Has partner	0.61 (.49)	0.65 (.48)	0.64 (.48)	0.55 (.50)

Table 1 (continued)

Labour Market Variables:	Displaced Workers	Non-Displaced Workers	Displaced Workers	
			pre-2008	post-2008
Job-end reported	0.96 (.19)	0.10 (.30)	0.97 (.18)	0.96 (.20)
Job tenure (SoFIE)	5.67 (6.94)	6.77 (7.27)	5.71 (7.12)	5.61 (6.66)
Fraction: 1-2 years	0.28 (.45)	0.20 (.40)	0.29 (.45)	0.27 (.45)
2-3 years	0.18 (.38)	0.15 (.35)	0.18 (.39)	0.16 (.37)
3-5 years	0.17 (.38)	0.19 (.40)	0.17 (.38)	0.18 (.38)
5-10 years	0.20 (.40)	0.23 (.42)	0.18 (.38)	0.22 (.42)
10+ years	0.17 (.38)	0.23 (.42)	0.18 (.38)	0.16 (.37)
Employed: SoFIE	1	1	1	1
LEED (W&S emp)	0.88 (.32)	0.93 (.26)	0.89 (.32)	0.87 (.34)
Monthly Earnings: SoFIE	\$3,321 (\$3,788)	\$4,651 (\$3,967)	\$3,406 (\$3,717)	\$3,183 (\$3,902)
LEED (W&S earnings)	\$15,994 (\$28,843)	\$4,829 (\$4,240)	\$16,815 (\$30,254)	\$14,634 (\$26,322)
Benefit receipt: LEED	0.06 (.23)	0.03 (.18)	0.06 (.23)	0.06 (.24)
Monthly benefit income: LEED	\$886 (\$521)	\$1,070 (\$484)	\$962 (\$500)	\$765 (\$539)
Pre-displacement:				
Employed in months [-18,-7]	0.95 (.21)	0.94 (.24)	0.95 (.21)	0.96 (.20)
Average log(earnings) [-18,-7]	7.77 (1.84)	7.69 (2.07)	7.74 (1.86)	7.82 (1.80)
Employed in months [-30,-19]	0.91 (.29)	0.91 (.29)	0.90 (.30)	0.91 (.28)
Average log(earnings) [-30,-19]	7.31 (2.48)	7.37 (2.46)	7.24 (2.53)	7.42 (2.40)
No. monthly observations	1,245	638,784	768	474
No. individuals	1,245	15,195	768	474

Notes: All samples are restricted to 20-64 year olds, with at least 1-year of job tenure. Entries reported are means (and standard deviations in parentheses). For displaced workers, the focal displacement month is the month of their first displacement (or first displacement from a job with at least 1-year's tenure). The non-displaced sample consists of all observations of workers never displaced, working in any month that a displacement occurred. Means are of non-missing observations: in particular, earnings and incomes are conditional on employment or receipt of income, and measured in constant June quarter 2016 \$-values. For displaced workers Tenure is of displaced job.

Table 2: Regression-adjusted estimates of Job-displacement impacts on LEED W&S employment

	(1)	(2)	(3)	(4)
Post-displacement	-0.153 (.010)	---	---	---
1 <sup>st</sup> year post-displacement	---	-0.211 (.011)	-0.186 (.014)	-0.203 (.014)
2 <sup>nd</sup> year post-displacement	---	-0.142 (.012)	-0.118 (.015)	-0.138 (.015)
3 <sup>rd</sup> year post-displacement	---	-0.127 (.012)	-0.109 (.015)	-0.129 (.015)
4 <sup>th</sup> year post-displacement	---	-0.122 (.013)	-0.114 (.016)	-0.136 (.016)
5 <sup>th</sup> year post-displacement	---	-0.124 (.013)	-0.110 (.016)	-0.134 (.016)
Post-2008 interaction:				
1 <sup>st</sup> year post-displacement	---	---	-0.066 (.023)	-0.037 (.023)
2 <sup>nd</sup> year post-displacement	---	---	-0.065 (.025)	-0.035 (.025)
3 <sup>rd</sup> year post-displacement	---	---	-0.052 (.025)	-0.021 (.026)
4 <sup>th</sup> year post-displacement	---	---	-0.028 (.026)	0.009 (.027)
5 <sup>th</sup> year post-displacement	---	---	-0.047 (.028)	-0.005 (.028)
Displaced worker	0.049 (.007)	0.046 (.007)	0.048 (.007)	---
R-squared	0.058	0.059	0.060	0.460
No. Observations:				
Total	1,653,927	1,653,927	1,653,927	1,653,927
Displaced Worker Obs	109,122	109,122	109,122	109,122
Non-displaced Worker Obs	1,544,808	1,544,808	1,544,808	1,544,808
No. displaced workers	1,245	1,245	1,245	1,245
No. non-displaced workers	9,396	9,396	9,396	9,396

Notes: Estimated standard errors in parentheses, clustered at the worker-level. All regressions include the following set of control variables: female, age, and age-squared in the current month; age, age-squared (both interacted with female), education, ethnicity, and 1-digit occupation and industry in the focal month of displacement; and separate year and month effects. Specification in column (4) controls for individual-specific fixed effects. Each of the post-displacement effects measure the impact over the respective year since displacement (or the full displacement period in column (1)), so averages within-year impacts.

Table 3: Regression-adjusted estimates of Job-displacement impacts on other outcomes

	log(W&S earns)	Benefit Receipt	log(Benefit income)	log(Total income)
1 <sup>st</sup> year post-displacement	-0.306 (.022)	0.063 (.008)	0.160 (.065)	-0.330 (.021)
2 <sup>nd</sup> year post-displacement	-0.218 (.021)	0.042 (.008)	0.148 (.072)	-0.233 (.020)
3 <sup>rd</sup> year post-displacement	-0.202 (.021)	0.038 (.009)	0.129 (.081)	-0.215 (.021)
4 <sup>th</sup> year post-displacement	-0.194 (.023)	0.034 (.009)	0.149 (.094)	-0.216 (.022)
5 <sup>th</sup> year post-displacement	-0.212 (.026)	0.032 (.010)	0.139 (.104)	-0.245 (.025)
Post-2008 interaction:				
1 <sup>st</sup> year post-displacement	-0.017 (.033)	0.038 (.015)	-0.001 (.076)	-0.068 (.034)
2 <sup>nd</sup> year post-displacement	-0.042 (.034)	0.026 (.015)	-0.018 (.084)	-0.065 (.034)
3 <sup>rd</sup> year post-displacement	-0.046 (.037)	0.029 (.016)	0.012 (.100)	-0.075 (.037)
4 <sup>th</sup> year post-displacement	-0.016 (.039)	0.029 (.017)	0.041 (.115)	-0.032 (.035)
5 <sup>th</sup> year post-displacement	0.007 (.042)	0.021 (.017)	0.052 (.128)	0.005 (.040)
R-squared	0.645	0.517	0.428	0.637
No. Observations:				
Total	1,365,705	1,653,927	90,666	1,425,084
Displaced Worker Obs	83,901	109,122	9,579	90,741
Non-displaced Worker Obs	1,281,801	1,544,808	81,084	1,334,343
No. displaced workers	1,215	1,245	435	1,218
No. non-displaced workers	9,063	9,396	2,139	9,098

Notes: Estimated standard errors in parentheses, clustered at the worker-level. All regressions include the following set of control variables: individual-specific fixed effects, age, and age-squared in the current month; age, age-squared (both interacted with female); and separate year and month effects. The conditional log(earnings), log(benefit income) and log(individual income) regressions also include separate dummy variables for the month of displacement and months before and after displacement for the displaced workers. Each of the post-displacement effects measure the impact over the respective year since displacement, so averages within-year impacts.

Table 4: Propensity-score model estimates

Displacement	PSM estimates		Covariate balancing tests				
	Coef.	Std. Err.	Mean			t-test	
			DW	Non	%bias	t-stat	p> t
female	-0.152	(.078)	0.431	0.439	-1.5	-0.37	0.711
age	0.009	(.003)	41.88	41.81	0.6	0.15	0.882
Education							
High school	0.046	(.100)	0.300	0.308	-1.7	-0.41	0.684
Vocational	-0.043	(.097)	0.359	0.353	1.1	0.28	0.778
Bachelor degree	0.043	(.144)	0.099	0.097	0.6	0.16	0.87
Higher degree	-0.139	(.182)	0.044	0.044	-0.1	-0.03	0.976
Ethnicity							
European	0.657	(.190)	0.780	0.773	1.5	0.38	0.707
Maori	0.659	(.210)	0.123	0.129	-1.9	-0.46	0.645
Other	0.765	(.304)	0.018	0.017	0.8	0.2	0.841
Pacific	0.567	(.236)	0.051	0.051	-0.1	-0.02	0.987
Job tenure							
2-3 years	-0.052	(.102)	0.177	0.176	0.2	0.06	0.955
3-5 years	-0.347	(.105)	0.173	0.176	-0.8	-0.19	0.848
5-10 years	-0.400	(.103)	0.199	0.195	0.9	0.24	0.812
10+ years	-0.682	(.111)	0.173	0.168	1.1	0.28	0.78
Pre-displacement:							
6-18 months prior							
No. months emp	-0.023	(.024)	10.857	10.833	0.8	0.19	0.845
1(No. months>0)	0.204	(.732)	0.954	0.952	1.2	0.31	0.753
Avg log(earnings)	0.061	(.094)	7.775	7.748	1.3	0.36	0.722
19-30 months prior							
No. months emp	0.004	(.017)	9.954	9.902	1.3	0.33	0.744
1(No. months>0)	0.772	(.668)	0.904	0.901	0.8	0.21	0.834
Avg log(earnings)	-0.101	(.089)	7.300	7.278	0.9	0.22	0.824

Table 4 (continued)

Displacement	PSM estimates		Covariate balancing tests				
	Coef.	Std. Err.	Mean			t-test	
			DW	Non	%bias	t-stat	p> t
1-digit Occupation:							
Professionals	-0.191	(.137)	0.096	0.099	-0.7	-0.21	0.83
Technicians & Trades	0.062	(.124)	0.127	0.127	0	0.01	0.99
Community & Personal Service	0.253	(.123)	0.155	0.157	-0.6	-0.15	0.88
Clerical & Administrative	-0.074	(.139)	0.107	0.101	2	0.53	0.59
Sales Workers	-0.185	(.234)	0.038	0.040	-0.7	-0.17	0.87
Machinery Ops & Drivers	0.023	(.144)	0.105	0.102	1	0.23	0.82
Labourers	0.087	(.137)	0.137	0.134	0.9	0.19	0.85
Residual Categories	0.173	(.155)	0.081	0.087	-2.4	-0.55	0.58
Missing	1.489	(.594)	0.001	0.003	-3.9	-1.18	0.24
1-digit Industry:							
Mining	-0.091	(.511)	0.005	0.005	0.3	0.06	0.95
Manufacturing	0.045	(.202)	0.227	0.223	1	0.24	0.81
Electricity, Gas, Water	-0.542	(.466)	0.005	0.005	0.4	0.12	0.91
Construction	-0.025	(.218)	0.103	0.099	1.3	0.29	0.77
Wholesale Trade	-0.214	(.222)	0.078	0.081	-1.2	-0.28	0.78
Retail Trade	-0.483	(.216)	0.103	0.105	-0.6	-0.14	0.89
Accommodation & Food	-0.345	(.265)	0.033	0.030	1.8	0.44	0.66
Transport, Postal & Warehousing	-0.267	(.236)	0.054	0.050	1.7	0.40	0.69
Info Media & Telecoms	-0.474	(.301)	0.019	0.017	1.1	0.27	0.79
Finance & Insurance	-0.316	(.252)	0.040	0.039	0.5	0.11	0.91
Real Estate Services	-0.429	(.213)	0.112	0.115	-0.9	-0.22	0.83
Professional, Scientific & Technical	-1.371	(.266)	0.026	0.027	-0.5	-0.16	0.87
Admin & Support Services	-1.467	(.250)	0.038	0.037	0.5	0.18	0.86
Public Admin & Safety	-1.384	(.241)	0.046	0.049	-1	-0.32	0.75
Education & Training	-0.391	(.271)	0.026	0.028	-1.1	-0.25	0.80
Health Care & Social Assistance	-0.862	(.260)	0.031	0.032	-0.3	-0.09	0.93
Intercept	-2.242	(.357)					
LogLikelihood	-3176.29						
Pseudo R-squared	0.0586						
LR (Chisq, 45 df)	395.33						
No. DWs	1,233						
No. non-DWs	6,390						
No. unmatched DWs	6						

Notes: Estimated standard errors are in parentheses. The matching model estimated is a Logit model for whether a worker was displaced. The estimation sample includes all displaced workers in their "focal displacement" months and a 1% random sample of non-displaced workers from those months. The omitted occupation category is Managers, and the omitted industry category is Agriculture, Forestry and Fishing.

Table 5: Propensity-score matched estimates of Job-displacement impacts – full sample

Post-displacement	Averaged across months of the post-displacement year				
	Employed	log(monthly earnings)	Benefit receipt	log(benefit income)	log(total income)
Year-1	-0.242 (.012)	-0.314 (.030)	0.096 (.009)	-0.028 (.059)	-0.370 (.027)
Year-2	-0.134 (.013)	-0.253 (.029)	0.066 (.008)	0.059 (.074)	-0.288 (.026)
Year-3	-0.092 (.013)	-0.217 (.030)	0.062 (.009)	-0.028 (.080)	-0.222 (.026)
Year-4	-0.080 (.014)	-0.155 (.028)	0.048 (.009)	-0.025 (.072)	-0.185 (.026)
Year-5	-0.078 (.014)	-0.143 (.029)	0.043 (.008)	0.020 (.095)	-0.187 (.028)
Sample sizes:					
1.: DW	1,227	1,065	1,227	285	1,119
non-DW	6,393	6,051	6,393	318	6,081
2.: DW	1,206	984	1,206	201	1,053
non-DW	6,300	5,760	6,300	345	5,838
3.: DW	1,188	957	1,188	177	1,026
non-DW	6,216	5,460	6,216	336	5,559
4.: DW	1,161	912	1,161	156	978
non-DW	6,105	5,226	6,105	345	5,358
5.: DW	1,143	885	1,143	147	951
non-DW	6,000	5,010	6,000	315	5,157

Notes: Entries are estimated displacement effects for each year following displacement, with estimated standard errors in parentheses. Standard errors are analytical standard errors estimated using the Abadie and Imbens (2006) method – see text for discussion of bootstrap standard error comparison. The sample sizes give the numbers of displaced (DW) and non-displaced (non-DW) workers used in each estimation. Each of the post-displacement effects measure the impact over the respective year since displacement, so averages within-year impacts.

Table 6: Propensity-score matched estimates of Job-displacement impacts – pre- and post-2008

	Employed	log(monthly earnings)	Benefit receipt	log(benefit income)	log(total income)
Pre-2008:					
Year-1	-0.218 (.015)	-0.299 (.043)	0.079 (.011)	-0.072 (.172)	-0.351 (.039)
Year-2	-0.109 (.016)	-0.240 (.039)	0.051 (.010)	0.104 (.189)	-0.267 (.037)
Year-3	-0.077 (.017)	-0.188 (.045)	0.047 (.010)	-0.095 (.172)	-0.200 (.037)
Year-4	-0.078 (.017)	-0.161 (.041)	0.034 (.011)	-0.057 (.151)	-0.182 (.038)
Year-5	-0.064 (.018)	-0.158 (.042)	0.028 (.010)	-0.047 (.138)	-0.201 (.040)
Post-2008:					
Year-1	-0.269 (.020)	-0.309 (.059)	0.110 (.018)	0.303 (.382)	-0.378 (.072)
Year-2	-0.168 (.022)	-0.301 (.076)	0.078 (.018)	-0.142 (.339)	-0.343 (.080)
Year-3	-0.121 (.024)	-0.281 (.086)	0.082 (.016)	0.268 (.288)	-0.290 (.092)
Year-4	-0.097 (.026)	-0.163 (.100)	0.080 (.015)	0.194 (.393)	-0.213 (.090)
Year-5	-0.117 (.027)	-0.165 (.106)	0.076 (.015)	0.261 (.565)	-0.225 (.095)

Notes: Entries are estimated displacement effects for each year following displacement, with estimated standard errors in parentheses. Standard errors are analytical standard errors estimated using the Abadie and Imbens (2006) method – see text for discussion of bootstrap standard error comparison. Each of the post-displacement effects measure the impact over the respective year since displacement, so averages within-year impacts.

Table 7: Propensity-score matched estimates of Job-displacement impacts – by age of worker

	Employed	log(monthly earnings)	Benefit receipt	log(benefit income)	log(total income)
<b>Younger (&lt;30)</b>					
Year-1	-0.179 (.028)	-0.229 (.090)	0.086 (.019)	-0.291 (.807)	-0.255 (.106)
Year-2	-0.070 (.030)	-0.144 (.078)	0.050 (.019)	-0.160 (.407)	-0.148 (.113)
Year-3	-0.052 (.032)	-0.187 (.112)	0.076 (.020)	-0.527 (.678)	-0.167 (.099)
Year-4	-0.024 (.036)	-0.036 (.146)	0.053 (.020)	-0.020 (.686)	-0.080 (.137)
Year-5	-0.031 (.038)	-0.034 (.154)	0.067 (.018)	-0.118 (.788)	-0.133 (.137)
<b>Prime (30-49)</b>					
Year-1	-0.230 (.017)	-0.275 (.054)	0.102 (.012)	-0.071 (.306)	-0.331 (.050)
Year-2	-0.126 (.018)	-0.230 (.046)	0.070 (.012)	0.074 (.114)	-0.272 (.050)
Year-3	-0.078 (.018)	-0.197 (.048)	0.057 (.012)	0.005 (.248)	-0.203 (.050)
Year-4	-0.074 (.019)	-0.182 (.040)	0.054 (.012)	-0.025 (.234)	-0.191 (.046)
Year-5	-0.071 (.019)	-0.156 (.041)	0.046 (.011)	-0.097 (.219)	-0.163 (.040)
<b>Older (&gt;=50)</b>					
Year-1	-0.315 (.022)	-0.454 (.083)	0.096 (.017)	0.237 (.213)	-0.552 (.076)
Year-2	-0.196 (.025)	-0.401 (.091)	0.069 (.017)	0.151 (.233)	-0.432 (.083)
Year-3	-0.150 (.028)	-0.294 (.077)	0.056 (.018)	0.306 (.324)	-0.333 (.070)
Year-4	-0.125 (.030)	-0.294 (.080)	0.037 (.019)	0.106 (.490)	-0.345 (.076)
Year-5	-0.111 (.032)	-0.265 (.084)	0.035 (.018)	0.188 (.859)	-0.343 (.081)

Notes: Entries are estimated displacement effects for each year following displacement, with estimated standard errors in parentheses. Standard errors are analytical standard errors estimated using the Abadie and Imbens (2006) method – see text for discussion of bootstrap standard error comparison. Each of the post-displacement effects measure the impact over the respective year since displacement, so averages within-year impacts.

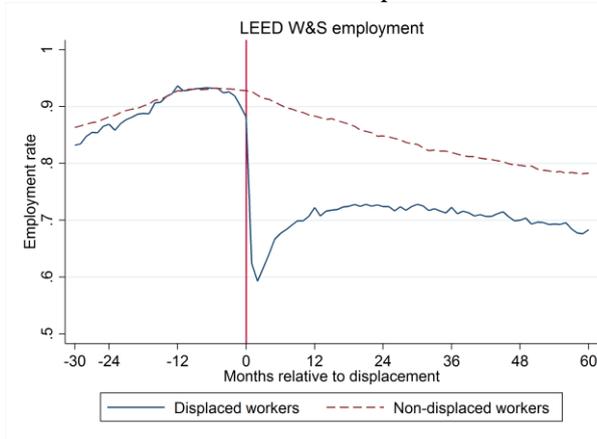
Table 8: Propensity-score matched estimates of Job-displacement impacts – by sex of worker

	Employed	log(monthly earnings)	Benefit receipt	log(benefit income)	log(total income)
<b>Males</b>					
Year-1	-0.234 (.016)	-0.259 (.040)	0.090 (.011)	-0.124 (.140)	-0.349 (.038)
Year-2	-0.127 (.018)	-0.220 (.039)	0.049 (.010)	0.025 (.313)	-0.270 (.038)
Year-3	-0.089 (.018)	-0.216 (.040)	0.049 (.010)	-0.088 (.341)	-0.245 (.038)
Year-4	-0.078 (.019)	-0.200 (.041)	0.044 (.010)	-0.046 (.216)	-0.224 (.038)
Year-5	-0.074 (.019)	-0.183 (.037)	0.044 (.010)	-0.095 (.196)	-0.233 (.035)
<b>Females</b>					
Year-1	-0.251 (.018)	-0.345 (.053)	0.099 (.015)	0.136 (.089)	-0.383 (.053)
Year-2	-0.142 (.020)	-0.292 (.059)	0.082 (.015)	0.244 (.128)	-0.295 (.053)
Year-3	-0.107 (.020)	-0.223 (.065)	0.080 (.015)	0.049 (.161)	-0.197 (.056)
Year-4	-0.090 (.022)	-0.111 (.066)	0.058 (.015)	0.078 (.145)	-0.142 (.058)
Year-5	-0.101 (.022)	-0.126 (.074)	0.050 (.014)	0.060 (.247)	-0.144 (.066)

Notes: Entries are estimated displacement effects for each year following displacement, with estimated standard errors in parentheses. Standard errors are analytical standard errors estimated using the Abadie and Imbens (2006) method – see text for discussion of bootstrap standard error comparison. Each of the post-displacement effects measure the impact over the respective year since displacement, so averages within-year impacts.

Figure 1: Monthly employment rate – Workers with 1+ year tenure

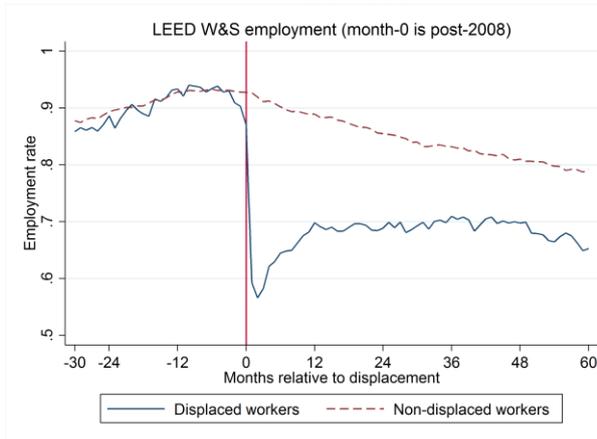
(a) Random comparison:  
Full sample



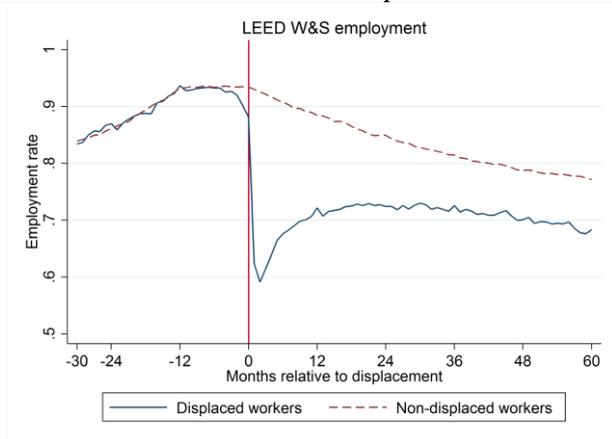
Pre-2008



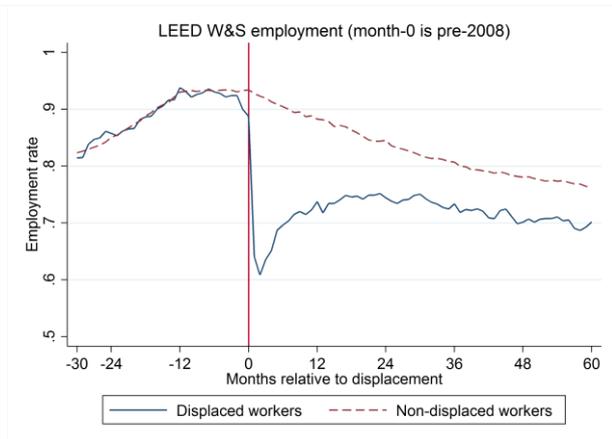
Post-2008



(b) Matched comparison  
Full sample



Pre-2008



Post-2008

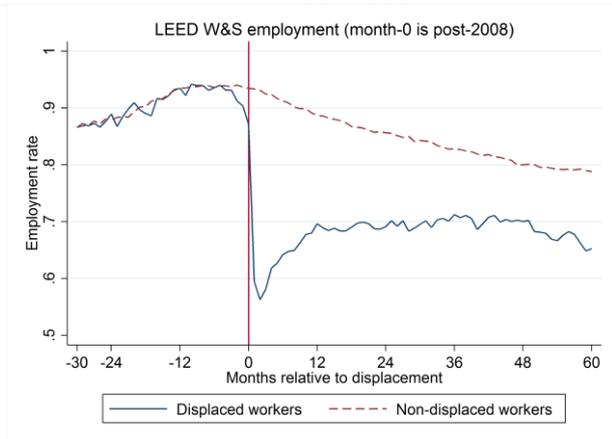
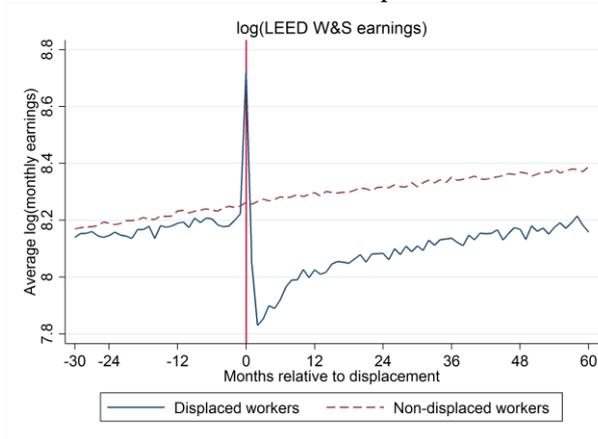


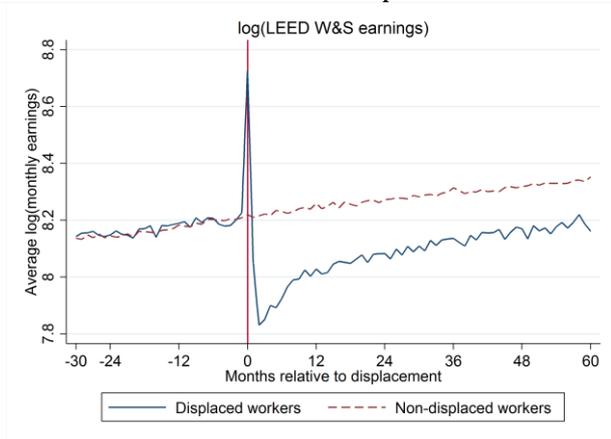
Figure 2: Monthly log(W&S earnings) – Workers with 1+ year tenure

(a) Random comparison:  
Full sample

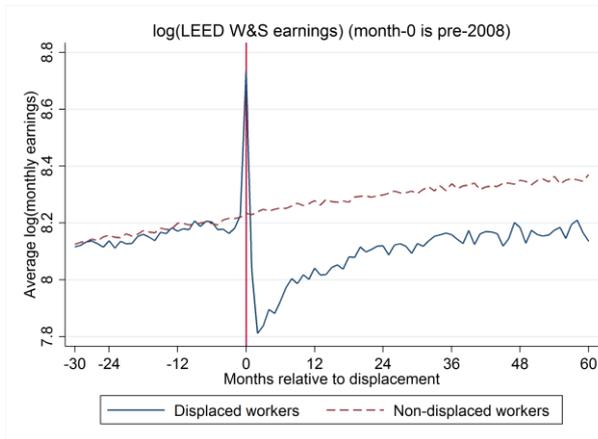


Pre-2008

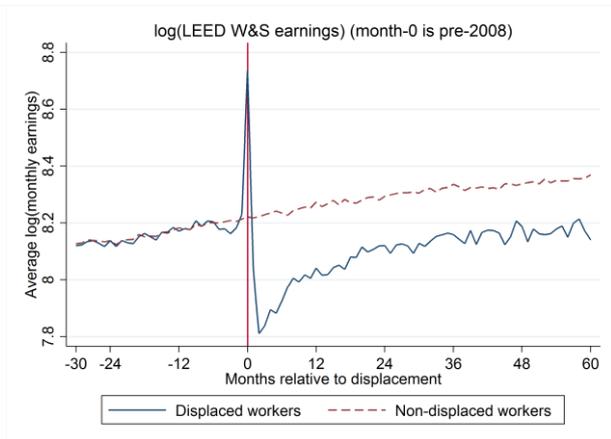
(b) Matched comparison  
Full sample



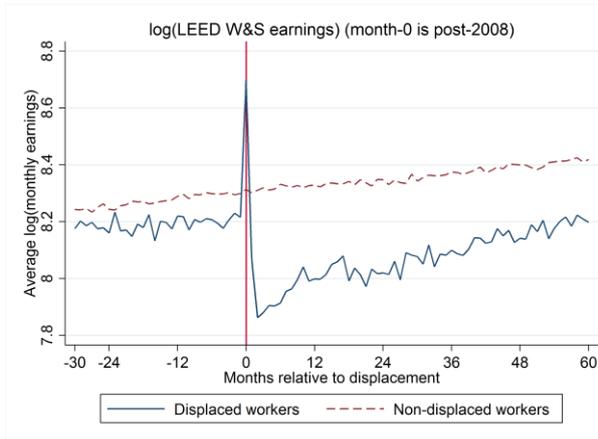
Pre-2008



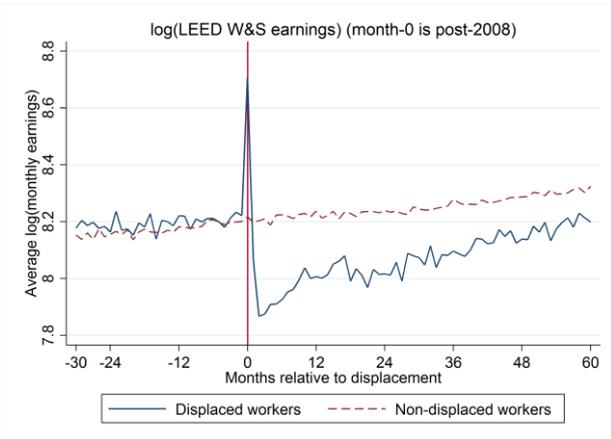
Post-2008



Post-2008



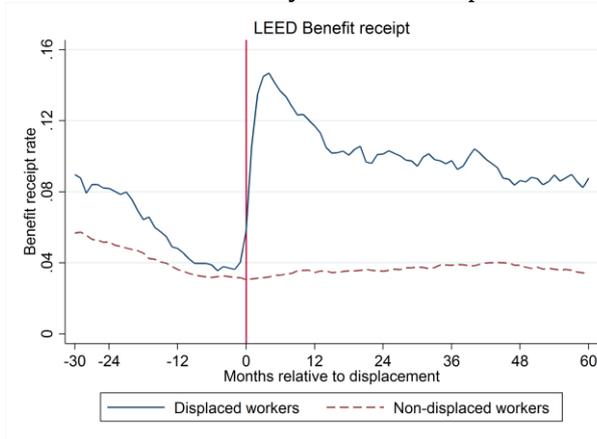
Post-2008



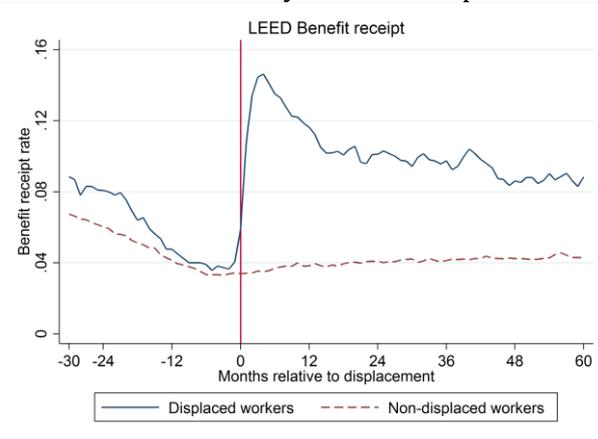
Post-2008

Figure 3: Benefit receipt and log(income) – Workers with 1+ year tenure

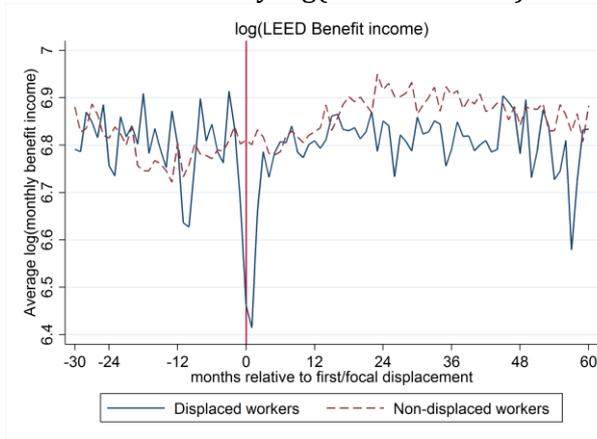
(a) Random comparison:  
Monthly benefit receipt



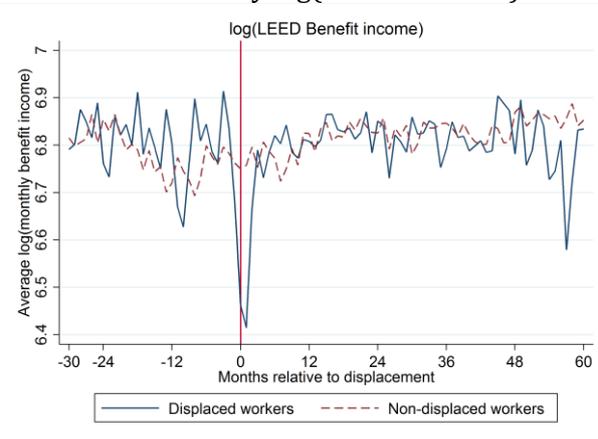
(b) Matched comparison  
Monthly benefit receipt



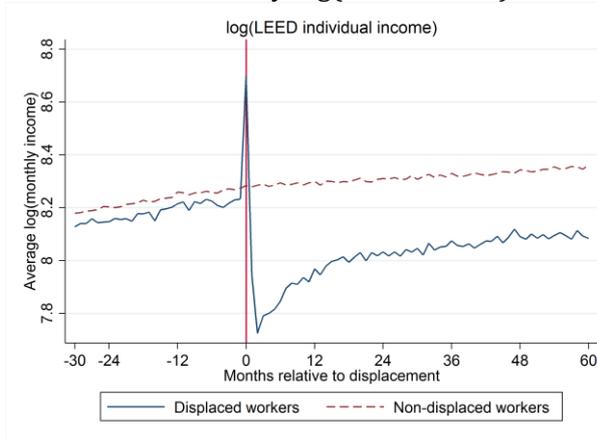
Monthly log(benefit income)



Monthly log(benefit income)



Monthly log(total income)



Monthly log(total income)

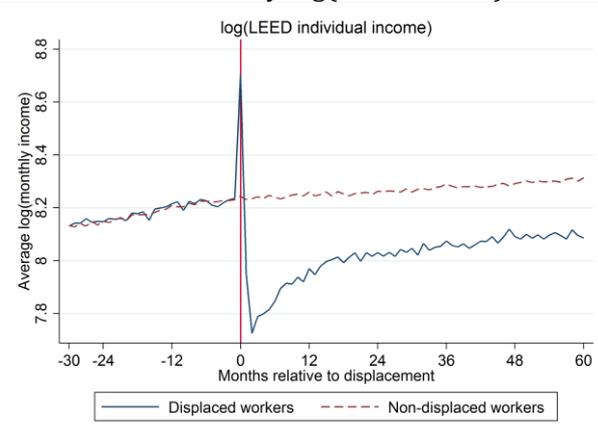


Figure 4: Partner's Employment, Benefit receipt, and log(earnings) – Workers with 1+ year tenure

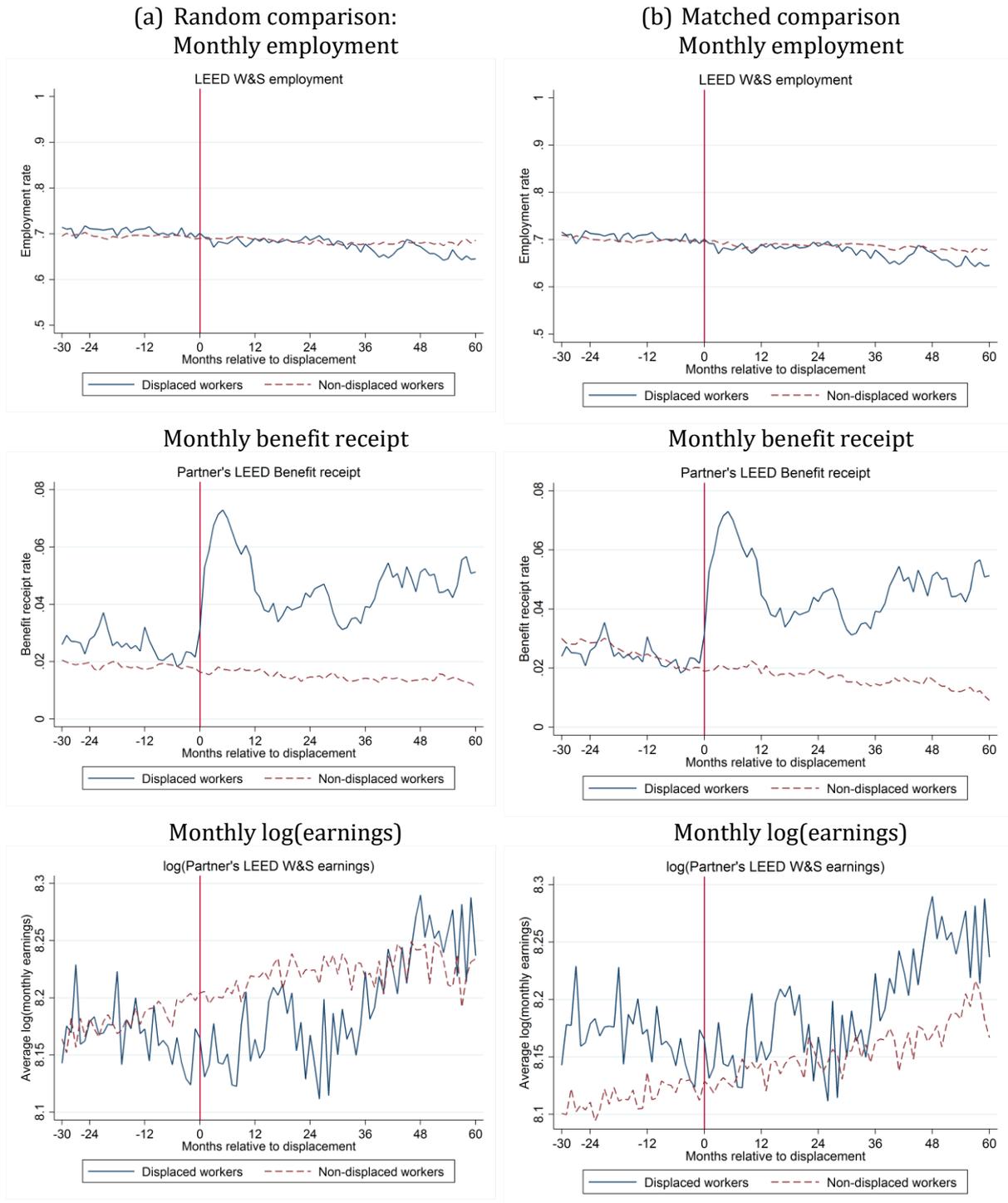


Figure 5: Partner's log(total income) and Family log(income) – Workers with 1+ year tenure

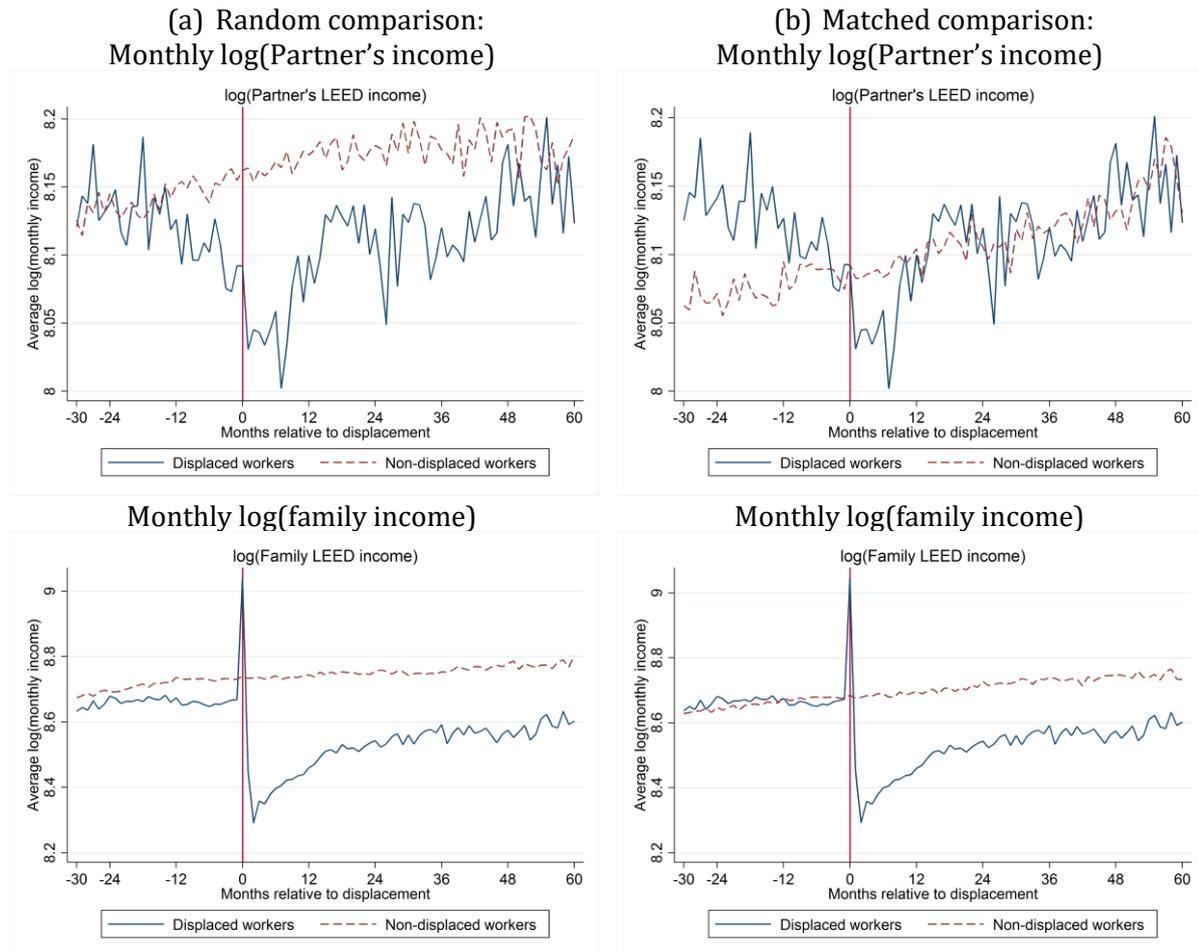
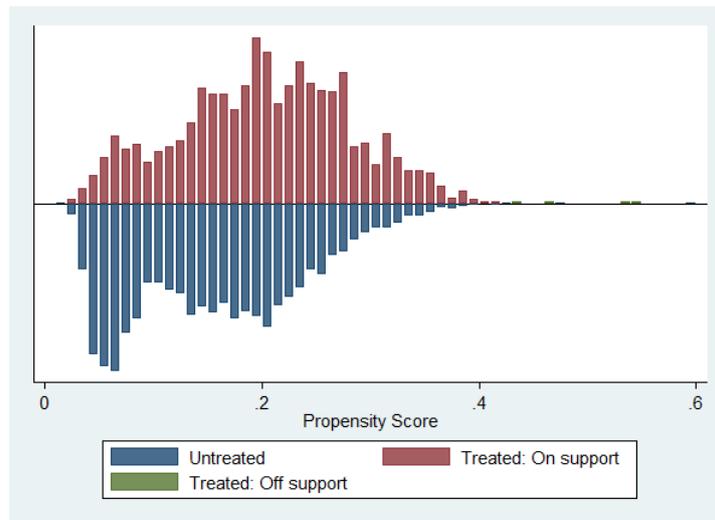
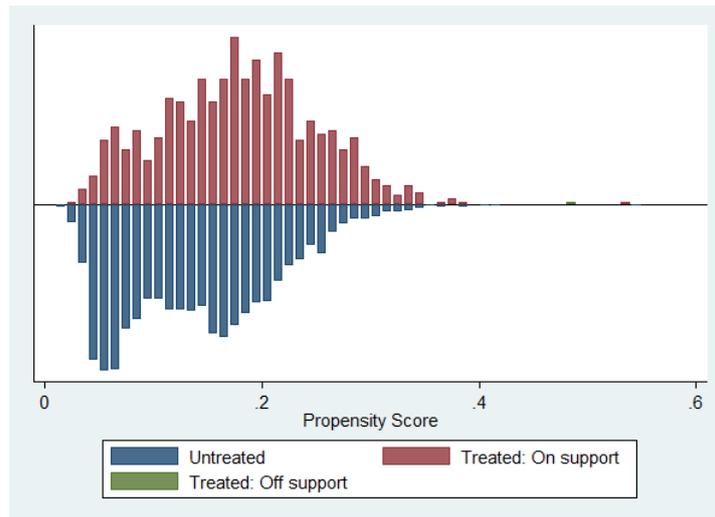


Figure 6: Propensity score distributions – Displaced and non-displaced workers

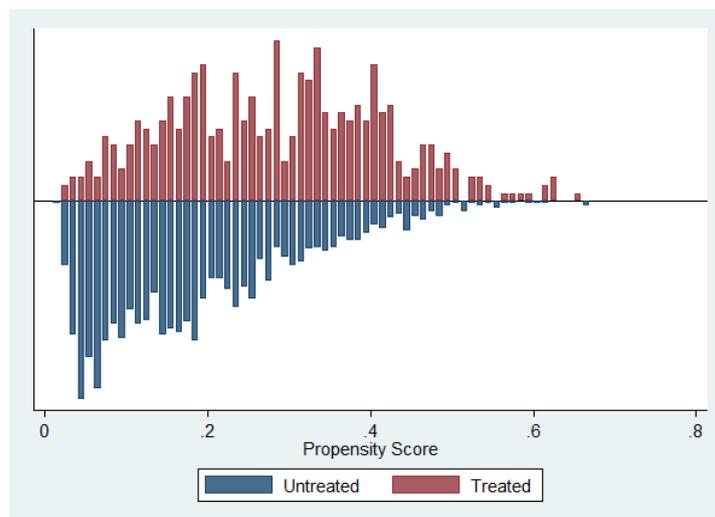
(a) Full sample



(b) Pre-2008



(c) Post-2008



## Appendix

Table A1: Characteristics of displaced workers

Socio-demographic Variables:	All	IDI Matched	By tenure	
			<1year	>=1year
Female	0.44 (.50)	0.44 (.50)	0.46 (.50)	0.43 (.50)
Age	39.4 (12.8)	39.3 (12.8)	33.4 (12.0)	41.8 (12.3)
Ethnicity:				
European	0.76 (.43)	0.76 (.43)	0.72 (.45)	0.78 (.42)
Maori	0.14 (.35)	0.14 (.35)	0.18 (.38)	0.12 (.33)
Pacifika	0.05 (.22)	0.05 (.22)	0.04 (.20)	0.05 (.22)
Asian	0.03 (.18)	0.03 (.18)	0.05 (.22)	0.03 (.17)
Other	0.02 (.13)	0.02 (.13)	0.01 (.12)	0.02 (.13)
Education:				
No qualification	0.20 (.40)	0.20 (.40)	0.19 (.39)	0.20 (.40)
High school	0.30 (.46)	0.30 (.46)	0.30 (.46)	0.30 (.46)
Vocational	0.37 (.48)	0.37 (.48)	0.4 (.49)	0.36 (.48)
Bachelor degree	0.09 (.29)	0.09 (.29)	0.08 (.26)	0.1 (.30)
Higher degree	0.04 (.20)	0.04 (.20)	0.03 (.18)	0.05 (.21)
Family size	2.67 (1.41)	2.66 (1.40)	2.62 (1.46)	2.69 (1.38)
Has partner	0.55 (.50)	0.56 (.50)	0.44 (.50)	0.61 (.49)

Table A1 (continued)

Labour market Variables:	All	IDI Matched	By tenure	
			<1year	>=1year
Job-end reported (SoFIE)	0.96 (.19)	0.96 (.19)	0.97 (.18)	0.96 (.19)
Job tenure (SoFIE)	4.02 (6.43)	4.01 (6.38)	0	5.67 (6.94)
Fraction with tenure:				
<1 year	0.29 (.46)	0.29 (.46)	1	0
1-2 years	0.20 (.40)	0.20 (.40)	0	0.28 (.45)
2-3 years	0.12 (.33)	0.12 (.33)	0	0.18 (.38)
3-5 years	0.12 (.33)	0.12 (.33)	0	0.17 (.38)
5-10 years	0.14 (.35)	0.14 (.35)	0	0.20 (.40)
10+ years	0.12 (.33)	0.12 (.33)	0	0.17 (.38)
Employed: SoFIE	1	1	1	1
LEED (W&S Emp)	0.84 (.37)	0.86 (.35)	0.80 (.40)	0.88 (.32)
Monthly earnings: SoFIE	\$2,985 (\$3,337)	\$3,004 (\$3,357)	\$2,238 (\$1,740)	\$3,321 (\$3,788)
LEED (W&S earnings)	\$12,691 (\$25,511)	\$12,691 (\$25,511)	\$3,916 (\$8,022)	\$15,994 (\$28,843)
Benefit receipt: LEED	0.08 (.28)	0.09 (.28)	0.15 (.36)	0.06 (.23)
Monthly benefit income: LEED	\$902 (\$513)	\$902 (\$513)	\$917 (\$508)	\$886 (\$521)
Pre-displacement outcomes				
Employed in months [-18,-7]	0.90 (.31)	0.91 (.28)	0.81 (.39)	0.95 (.21)
Average log(earnings) [-18,-7]	7.15 (2.56)	7.29 (2.39)	6.12 (3.06)	7.77 (1.84)
Employed in months [-30,-19]	0.82 (.38)	0.84 (.37)	0.67 (.47)	0.91 (.29)
Average log(earnings) [-30,-19]	6.53 (3.14)	6.66 (3.04)	5.09 (3.63)	7.31 (2.48)
No. monthly observations	1,794	1,761	516	1,245
No. individuals	1,794	1,761	516	1,245

Notes: All samples are restricted to 20-64 year olds. Entries reported are means (and standard deviations in parentheses). The focal displacement month is the month of their first displacement (or first displacement from a job with at least 1-year's tenure). Means are of non-missing observations: in particular, earnings and incomes are conditional on employment and income receipt, and are measured in constant June quarter 2016 \$-values. For displaced workers Tenure is of displaced job.

Table A2: Displaced workers' partner and family employment and earnings

	s workers	Non-Displaced workers	Displaced workers	
			pre-2008	post-2008
<b>Partner's:</b>				
SoFIE employment	0.74 (.44)	0.72 (.45)	0.75 (.44)	0.74 (.44)
LEED W&S Employment	0.70 (.46)	0.69 (.46)	0.70 (.46)	0.70 (.46)
SoFIE earnings	\$4,188 (\$2,800)	\$4,614 (\$4,372)	\$4,170 (\$2,865)	\$4,224 (\$2,677)
LEED earnings	\$4,992 (\$6,623)	\$4,788 (\$4,664)	\$5,131 (\$7,545)	\$4,723 (\$4,333)
<b>Family (worker + partner):</b>				
SoFIE family monthly earnings	\$5,869 (\$5,159)	\$7,411 (\$5,762)	\$5,972 (\$5,218)	\$5,702 (\$5,061)
LEED family W&S earnings	\$18,207 (\$29,381)	\$7,564 (\$6,132)	\$19,217 (\$31,210)	\$16,570 (\$26,097)
LEED family income	\$17,889 (\$29,010)	\$7,665 (\$6,088)	\$18,790 (\$30,802)	\$16,419 (\$25,786)
No. monthly Obs	1,245	638,784	768	474
No. Individuals	1,245	15,195	768	474

Notes: All samples are restricted to 20-64 year olds, with at least 1-years job tenure. Entries reported are means (and standard deviations in parentheses). For displaced workers, the focal displacement month is the month of their first displacement (or first displacement from a job with at least 1-year's tenure). The non-displaced sample consists of all observations of workers never displaced, working in any month that a displacement occurred. Means are of non-missing observations: in particular, earnings and incomes are conditional on employment or income receipt, and are measured in constant June quarter 2016 \$-values. For displaced workers Tenure is of displaced job.

Table A3: Regression-adjusted estimates of Job-displacement impacts on other outcomes

	log(W&S earnings)	Benefit Receipt	log(Benefit income)	log(Total income)
Post-displacement impacts				
1 <sup>st</sup> year	-0.228 (.023)	0.060 (.008)	0.017 (.054)	-0.284 (.021)
2 <sup>nd</sup> year	-0.134 (.022)	0.038 (.008)	0.068 (.053)	-0.175 (.021)
3 <sup>rd</sup> year	-0.114 (.023)	0.034 (.009)	0.042 (.057)	-0.153 (.022)
4 <sup>th</sup> year	-0.110 (.025)	0.029 (.009)	0.045 (.065)	-0.158 (.024)
5 <sup>th</sup> year	-0.125 (.027)	0.025 (.009)	-0.033 (.069)	-0.179 (.026)
Post-2008 interaction:				
1 <sup>st</sup> year	-0.040 (.037)	0.040 (.016)	-0.025 (.056)	-0.093 (.038)
2 <sup>nd</sup> year	-0.081 (.038)	0.029 (.016)	-0.040 (.057)	-0.097 (.037)
3 <sup>rd</sup> year	-0.094 (.039)	0.031 (.017)	0.003 (.064)	-0.131 (.040)
4 <sup>th</sup> year	-0.078 (.040)	0.035 (.017)	0.043 (.071)	-0.091 (.039)
5 <sup>th</sup> year	-0.051 (.045)	0.034 (.017)	0.156 (.078)	-0.052 (.044)
Displaced worker	-0.045 (.017)	0.010 (.006)	-0.024 (.048)	-0.028 (.015)
R-squared	0.280	0.060	0.090	0.280
No. Observations:				
Total	1,365,705	1,653,927	90,666	1,425,084
Displaced Worker Obs	83,901	109,122	9,579	90,741
Non-displaced Worker Obs	1,281,801	1,544,808	81,084	1,334,343
No. displaced workers	978	1,245	114	1,062
No. non-displaced workers	7,566	9,396	558	7,947

Notes: Estimated standard errors in parentheses, clustered at the worker-level. All regressions include the following set of control variables: female, age, and age-squared in the current month; age, age-squared (both interacted with female), education, ethnicity, and 1-digit occupation and industry in the focal month of displacement; and separate year and month effects. The conditional log(earnings), log(benefit income) and log(individual income) regressions also include separate dummy variables for the month of displacement and months before and after displacement for the displaced workers. Each of the post-displacement effects measure the impact over the respective year since displacement, so averages within-year impacts.

Table A4: Regression-adjusted estimates of Job-displacement impacts using Job-end sample

	Employed	log(W&S earns)	Benefit Receipt	log(Benefit income)	log(Total income)
Post-displacement impacts:					
1 <sup>st</sup> year	-0.202 (.014)	-0.323 (.021)	0.064 (.008)	0.130 (.062)	-0.346 (.020)
2 <sup>nd</sup> year	-0.136 (.015)	-0.251 (.021)	0.044 (.008)	0.144 (.068)	-0.259 (.020)
3 <sup>rd</sup> year	-0.127 (.016)	-0.240 (.022)	0.040 (.009)	0.125 (.077)	-0.245 (.021)
4 <sup>th</sup> year	-0.132 (.016)	-0.238 (.023)	0.037 (.009)	0.143 (.088)	-0.249 (.022)
5 <sup>th</sup> year	-0.128 (.016)	-0.259 (.027)	0.034 (.010)	0.130 (.099)	-0.281 (.025)
Post-2008 interactions:					
1 <sup>st</sup> year	-0.031 (.023)	-0.018 (.033)	0.035 (.015)	0.024 (.078)	-0.068 (.034)
2 <sup>nd</sup> year	-0.025 (.025)	-0.043 (.035)	0.020 (.015)	0.0002 (.085)	-0.065 (.034)
3 <sup>rd</sup> year	-0.013 (.026)	-0.044 (.037)	0.024 (.016)	0.031 (.100)	-0.075 (.037)
4 <sup>th</sup> year	0.018 (.027)	-0.009 (.039)	0.026 (.017)	0.069 (.113)	-0.028 (.035)
5 <sup>th</sup> year	-0.005 (.028)	0.013 (.043)	0.025 (.017)	0.081 (.126)	0.003 (.040)
R-squared	0.446	0.620	0.513	0.420	0.616
No. Observations					
Total	1,645,983	1,307,538	1,645,983	106,728	1,384,803
Displaced Worker Obs	108,849	83,757	108,849	9,573	90,588
Non-displaced worker Obs	1,537,134	1,223,781	1,537,134	97,158	1,294,215
No. displaced workers	1,242	1,212	1,242	432	1,218
No. non-displaced workers	9,444	9,156	9,444	2,418	9,198

Notes: All regressions control for individual-specific fixed effects, and include the following set of control variables: age, and age-squared in the current month; age, age-squared (both interacted with female), and separate year and month effects. The conditional log(earnings), log(benefit income) and log(individual income) regressions also include separate dummy variables for the month of displacement and months before and after displacement for the displaced workers. Each of the post-displacement effects measure the impact over the respective year since displacement, so averages within-year impacts.

Figure A1: Monthly employment rate and log(earnings) – All workers

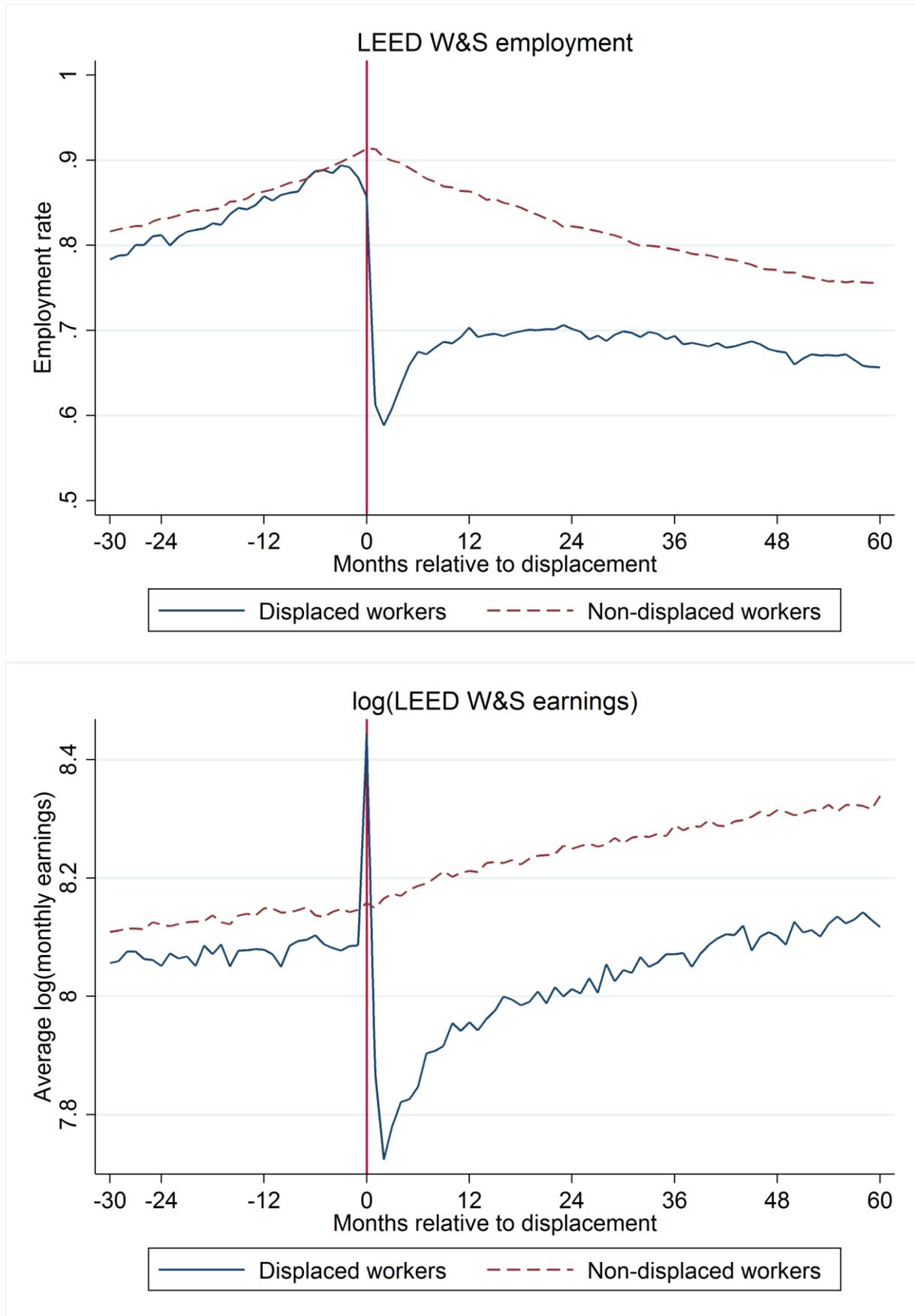
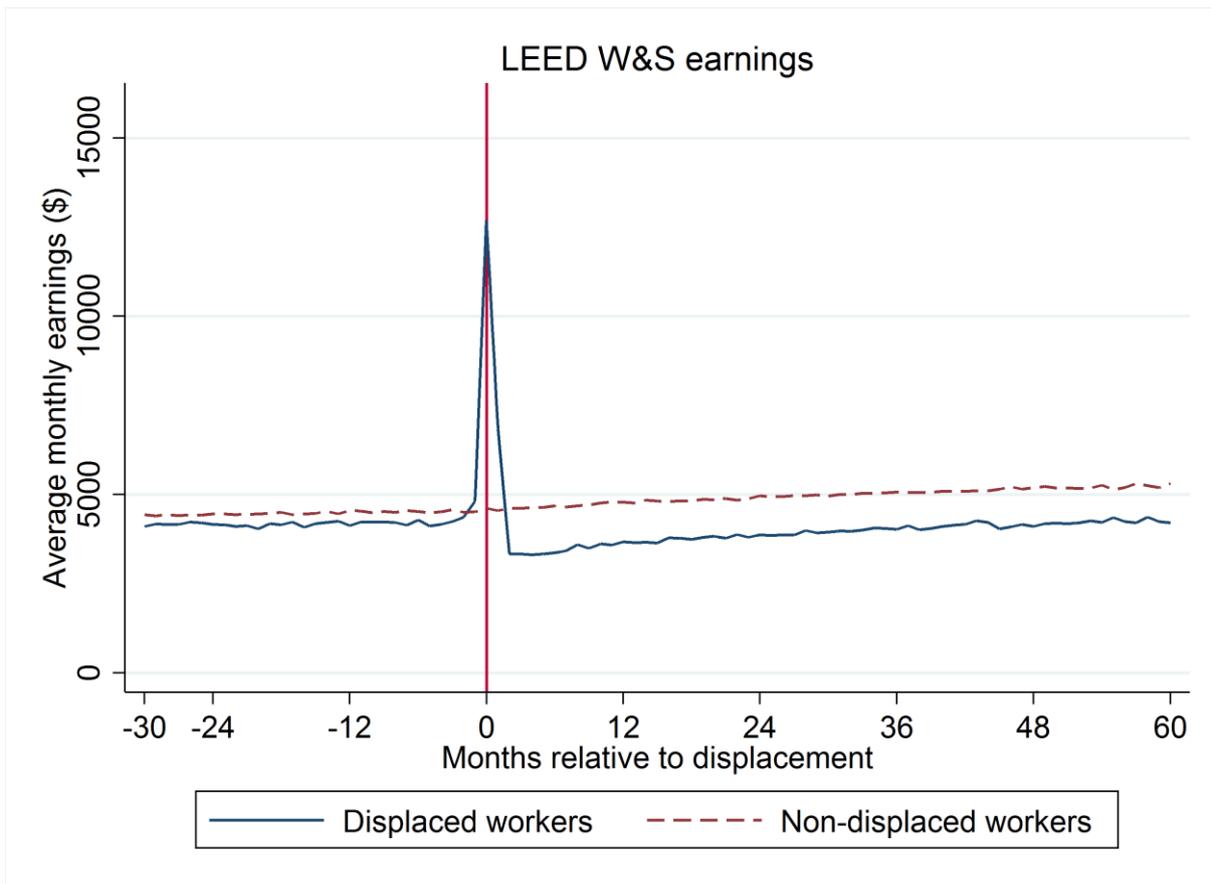


Figure A2: Monthly earnings

(a) All workers



(b) Workers with 1+ year tenure

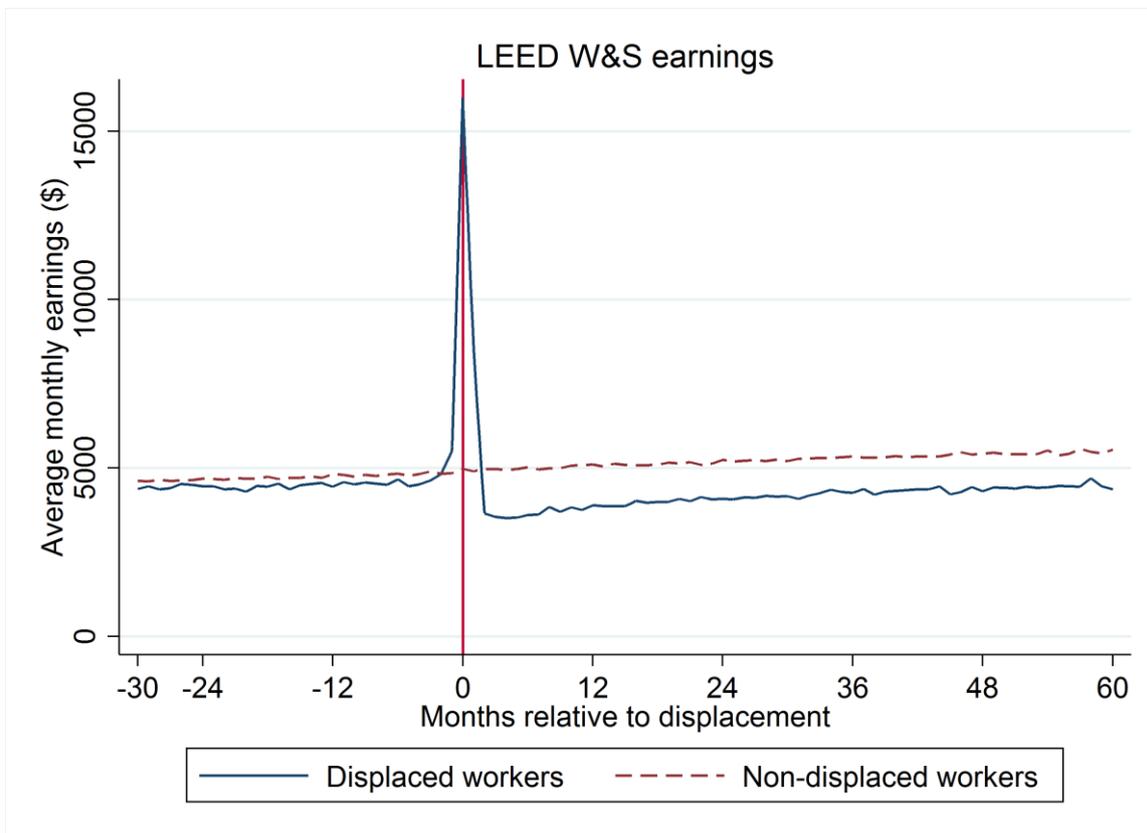


Figure A3: Monthly benefit, individual and family income – Workers with 1+ year tenure

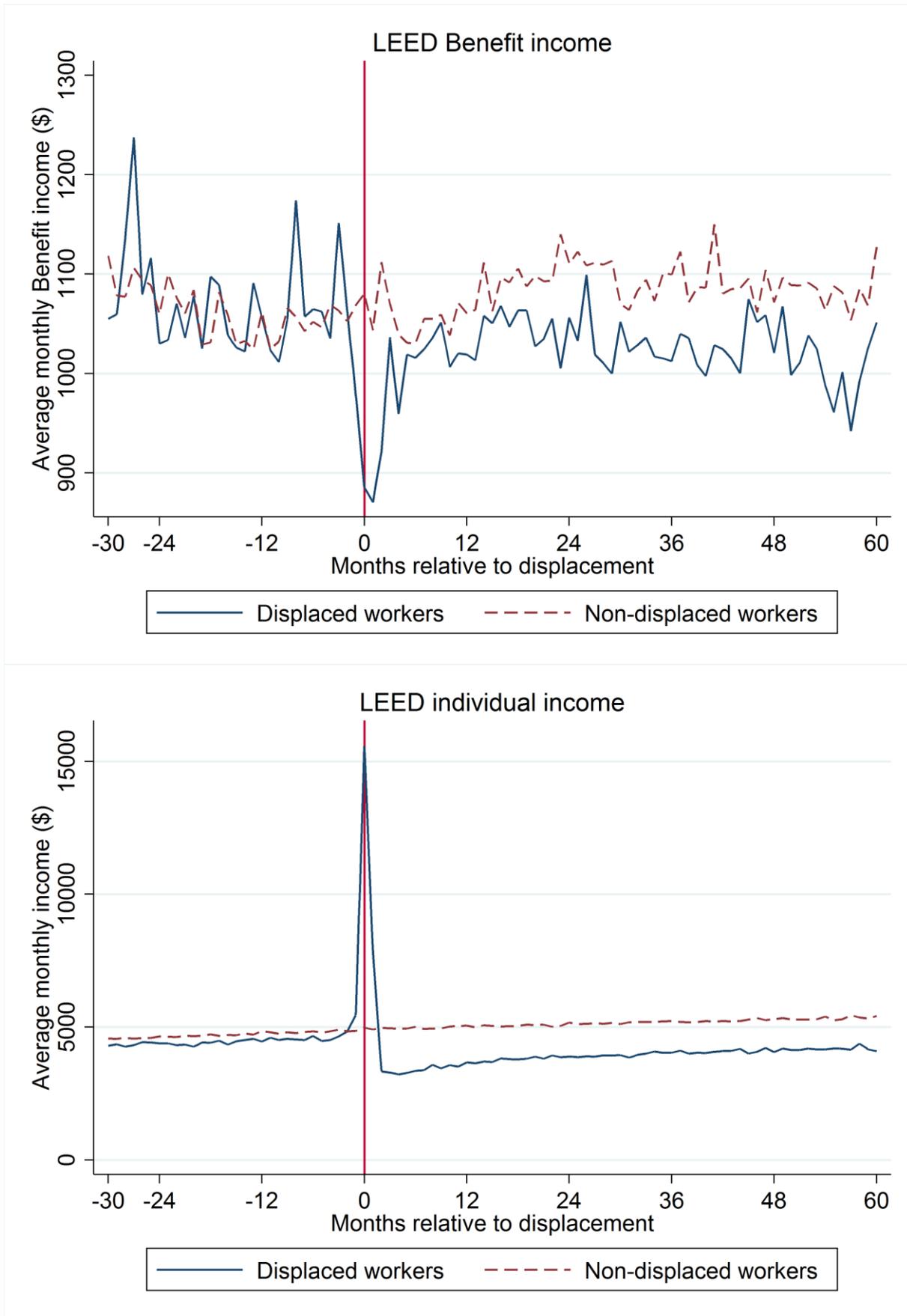


Figure A3 (continued)

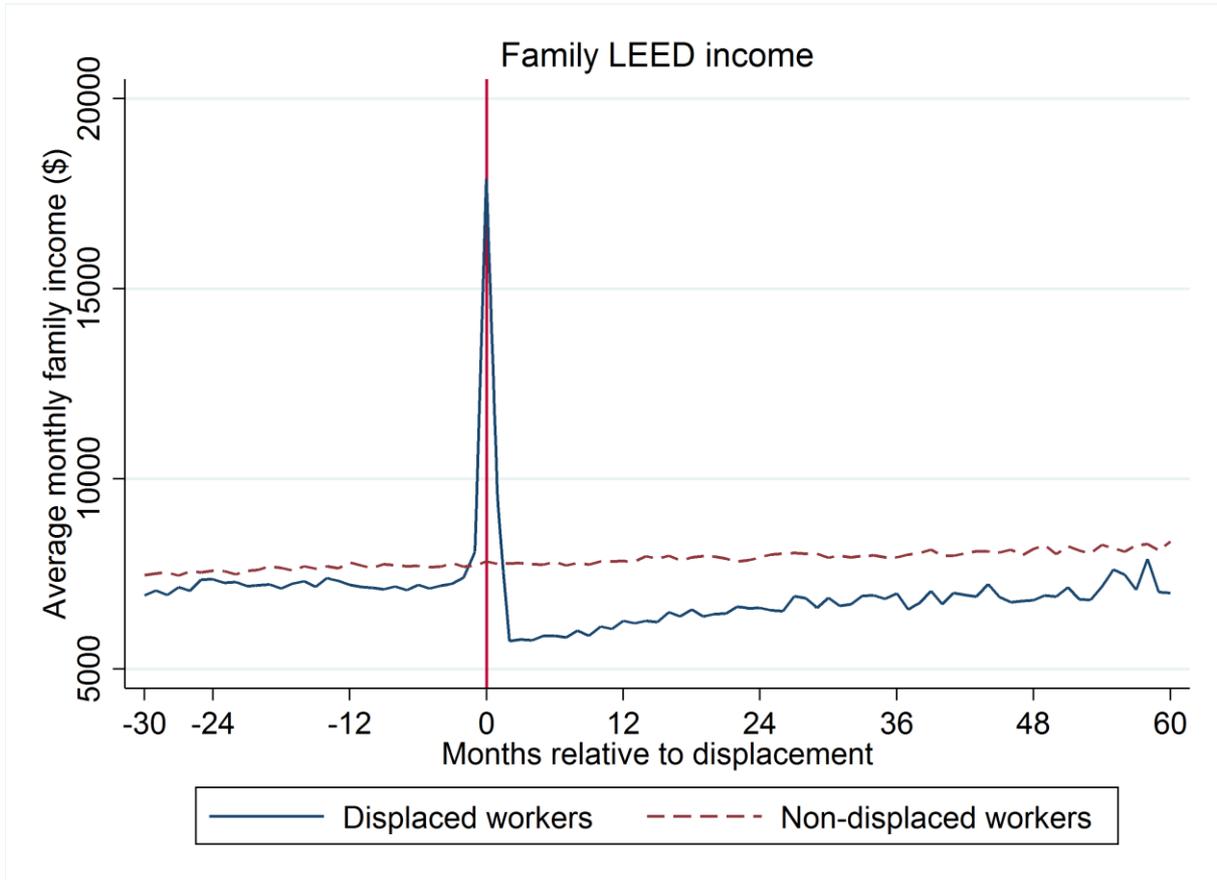


Figure A4: Monthly Earnings – Workers with 1+ year tenure, before and after 2008

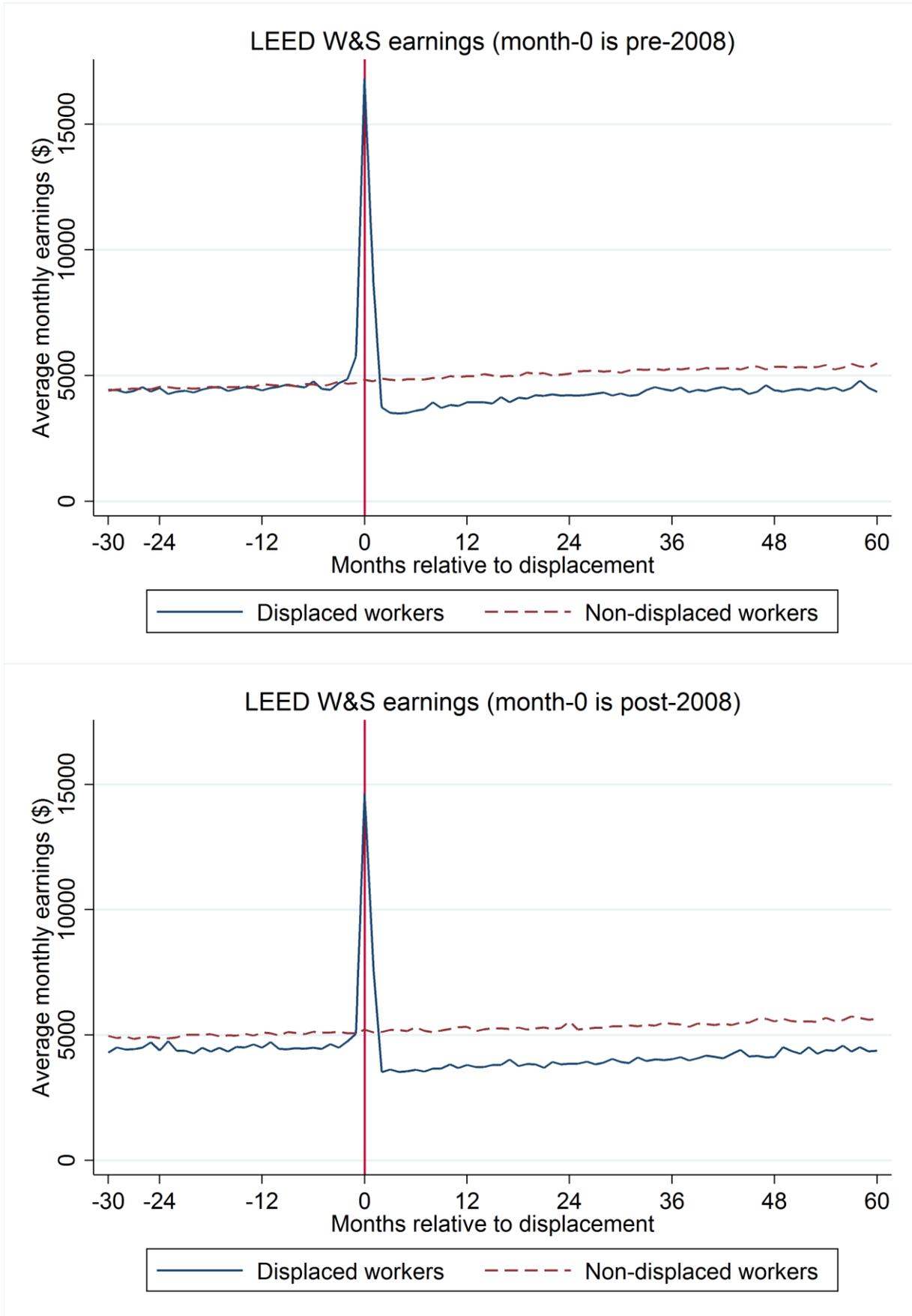


Figure A5: Monthly Individual income – Workers with 1+ year tenure, before and after 2008

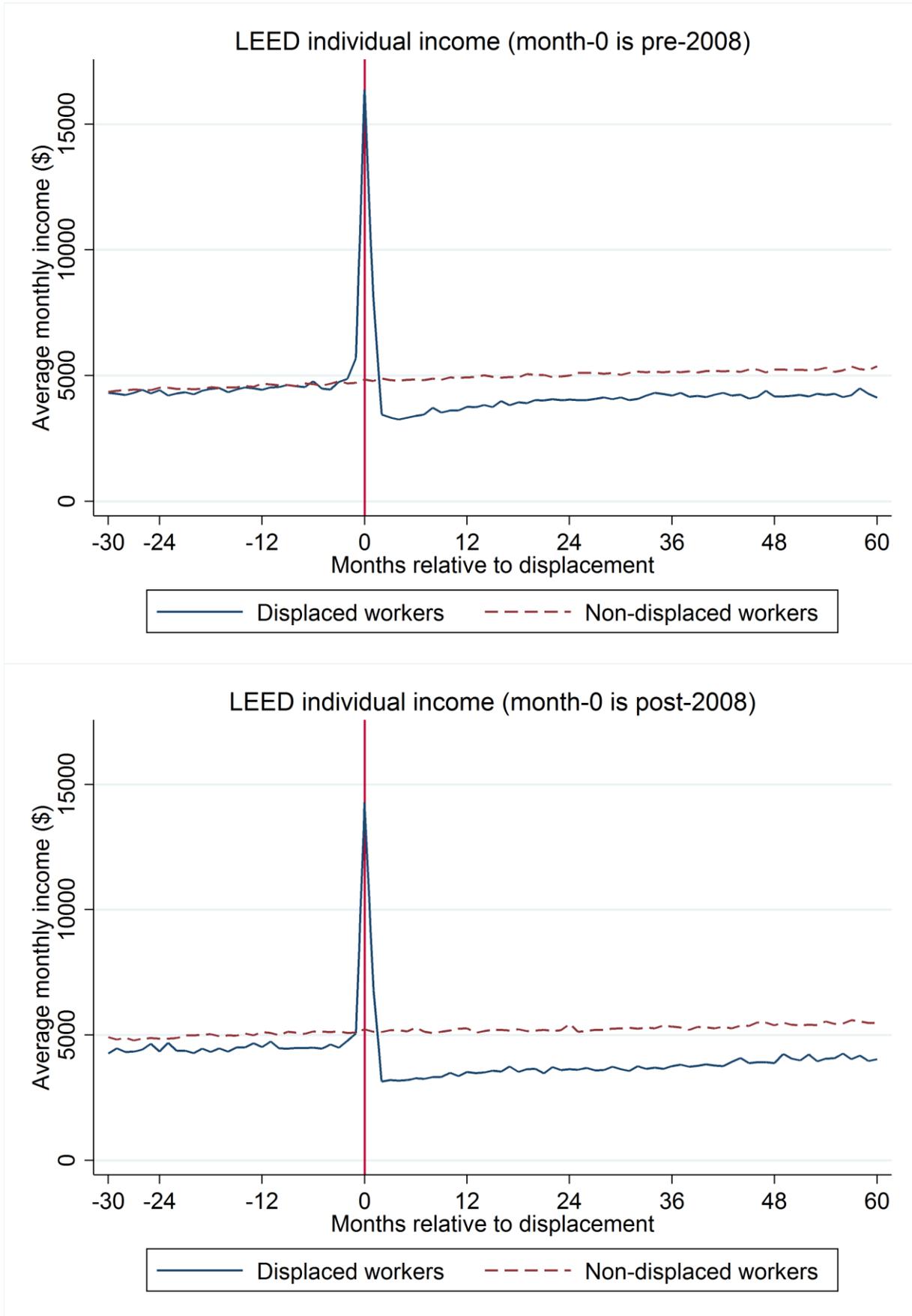


Figure A6: Partner's earnings and income – Workers with 1+ year tenure

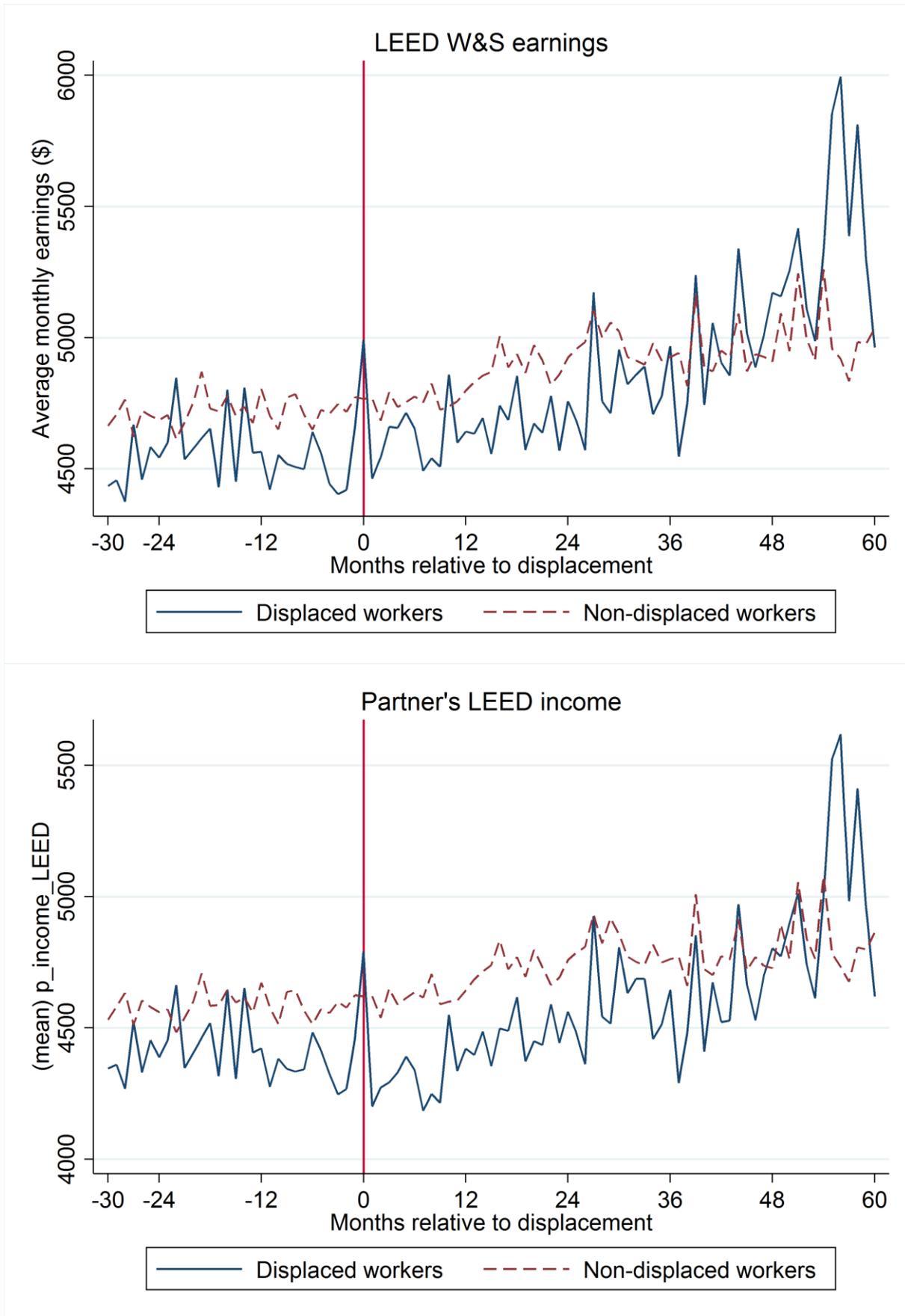
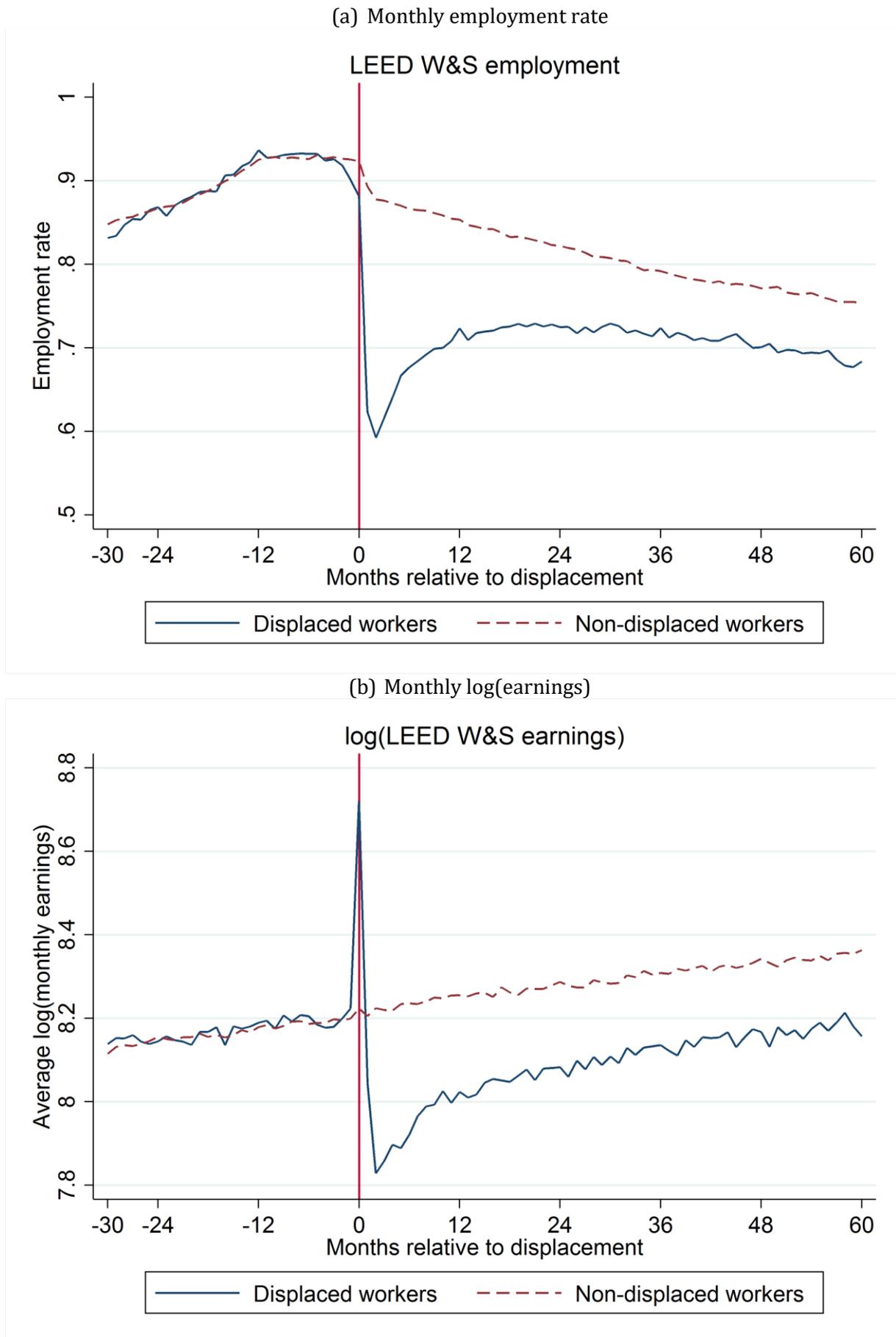
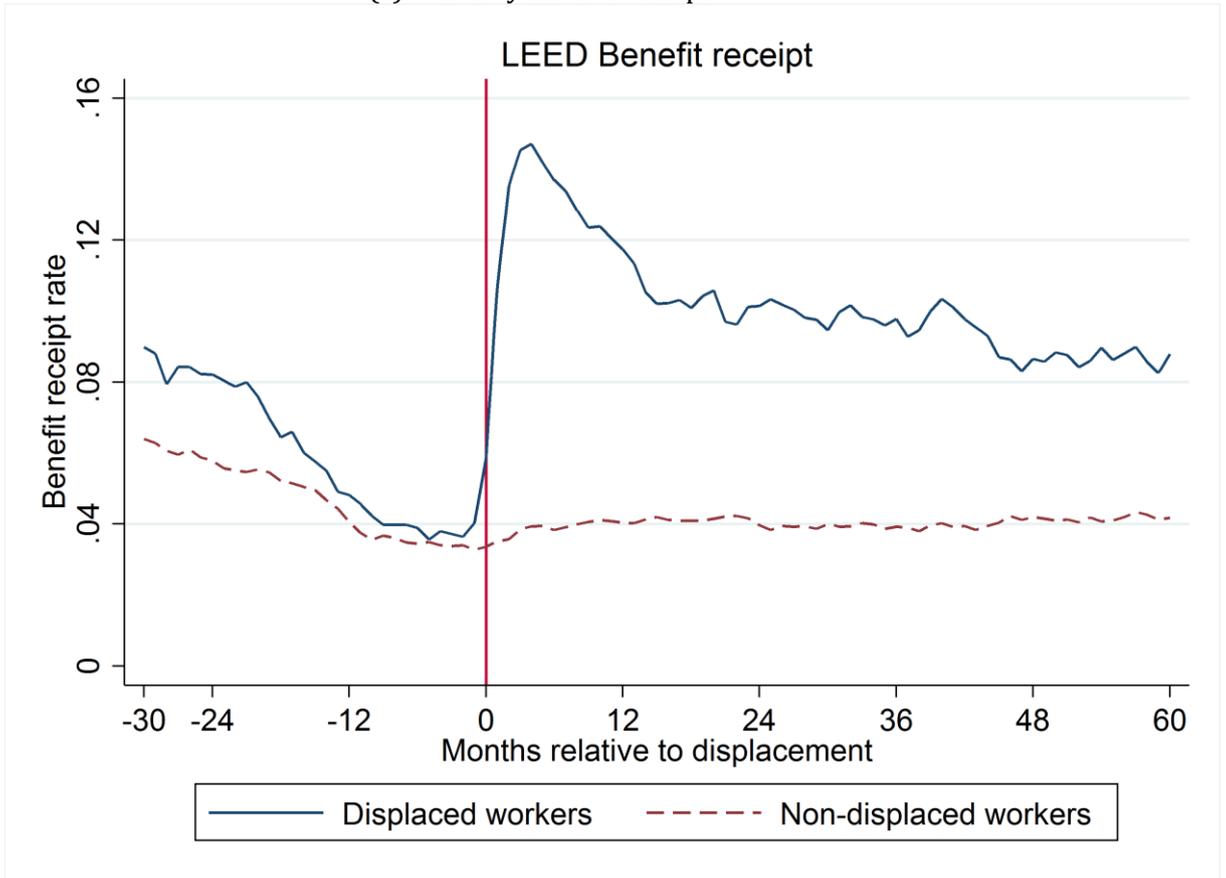


Figure A7: Event study outcomes among Job-end workers – Workers with 1+ year tenure



(c) Monthly benefit receipt



(d) Monthly log(Total individual income)

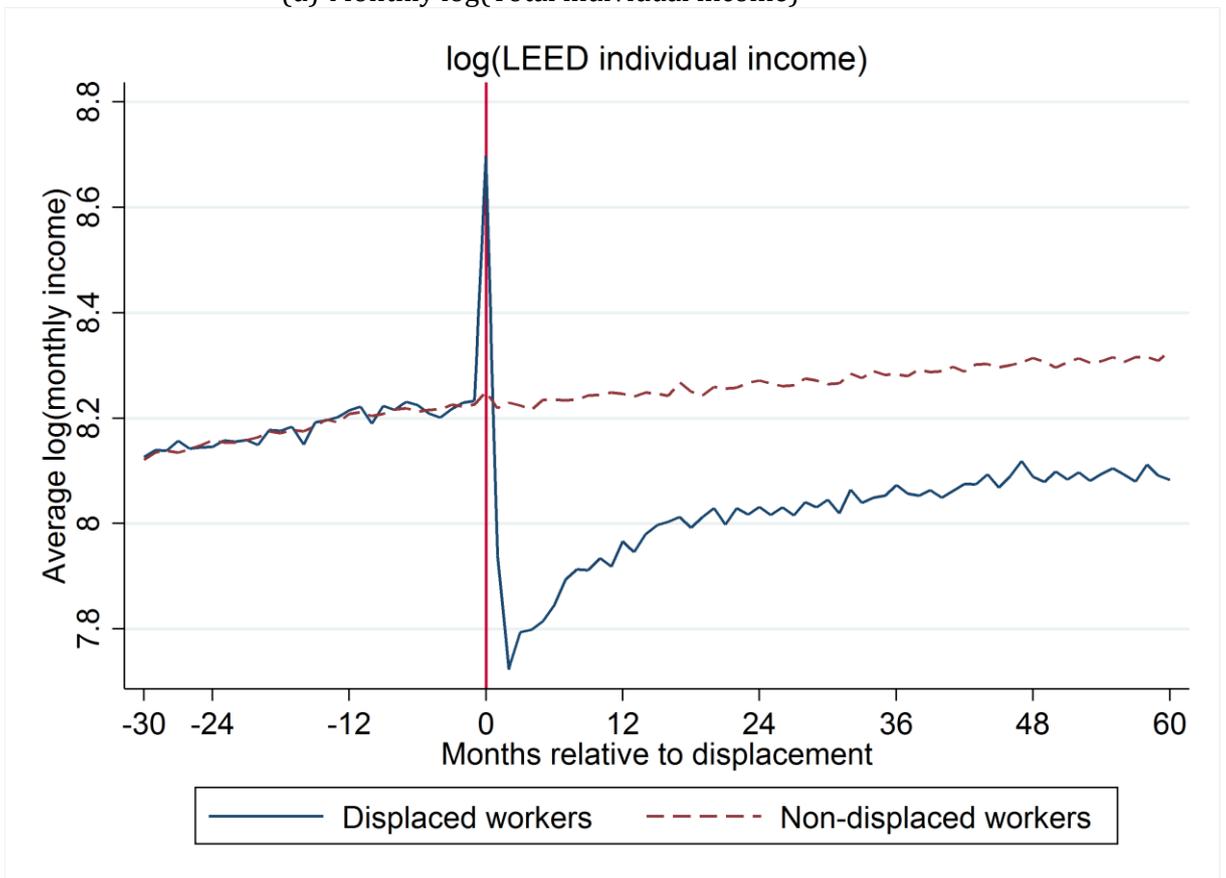
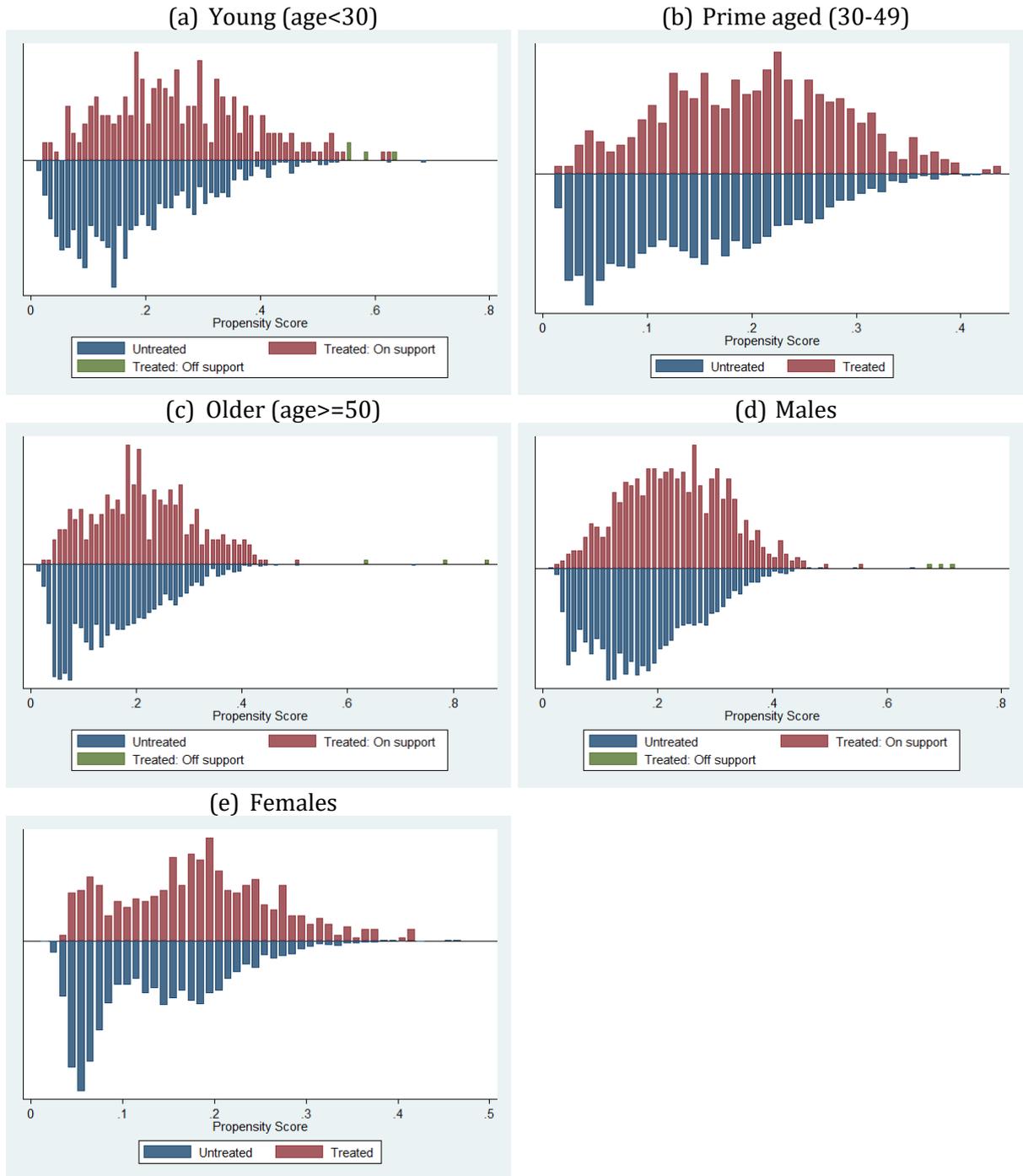


Figure A8: Propensity score distributions – Displaced and non-displaced workers



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