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Don't look down: the consequences of job loss in a flexible labour market[*]

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Abstract

We estimate the earnings, hours and income effects of job loss for a representative sample of UK workers from 1991–2007. We follow workers before and after job loss, regardless of their labour market state, and we match displaced workers with similar non-displaced workers. This provides a more comprehensive picture of the effect of job loss in the UK than previously available. Job loss causes a long-run reduction in income which is mainly due to reductions in monthly pay rather than in employment propensity. Income from other labour market states and from welfare payments does little to compensate for income losses. This lack of a "safety net" means that job loss in the UK has a similar impact to job loss in the US.

JEL codes: J65, J63, C21.

Keywords: Job loss, displacement, unemployment, unemployment insurance.

1 Introduction

Every year in the UK, one in every four workers will end their current employment spell, and one in every six of these will do so because they lose their job.[1] Evidence shows that job loss, or displacement, has a large and persistent impact on workers' earnings and well-being in general.[2] Quantifying the size of these losses, and understanding how workers respond to displacement is therefore important from the perspective of policy-makers wishing to ameliorate its impact. However, systematic evidence for the UK is surprisingly limited. We therefore provide new and more comprehensive evidence of the costs of displacement in the UK. We follow workers for up to a decade after displacement across all subsequent labour market states and therefore track all individual sources of income, including welfare payments. Earlier studies have typically considered these aspects in isolation; we provide a unified treatment. We show that the patterns of wage and employment loss are more similar to those in the US compared to those in other European countries. Displaced workers in the UK typically do not remain unemployed for long, but if they re-enter employment it is in lower-paying jobs. Income from alternative sources, such as self-employment and welfare benefits, does little to compensate displaced workers.

The existing literature on the effects of displacement typically finds that there are very large short-term consequences on employment and earnings, and that earnings losses persist for many years. Studies for the US include Ruhm (1991), Jacobson et al. (1993) (henceforth JLS) and more recently Couch and Placzek (2010) and Davis and von Wachter (2011). Since JLS, it has become increasingly common to rely on administrative data, such as social security earnings records, to estimate the cost of displacement. These data offer several advantages. They provide an externally validated measure of displacement based on plant closure or large employment reductions, accurate measures of earnings, and large samples. However, administrative data are often silent on what happens to displaced workers who leave employment and enter other labour market states such as self-employment, training or early retirement. Administrative data also often contain little demographic information which would allow the construction of suitable counterfactuals.

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[1] 1991–2008, BHPS. See Table 1.

[2] The literature has considered the costs of displacement on outcomes including health and mortality, family dissolution, fertility and the educational success of the children of displaced workers.

Finally, administrative data rarely contain information on working time, which means that one cannot determine whether falls in pay are caused by reductions in hours or wages.

It remains the case that very little is known about the effects of job displacement in the UK. Administrative data (such as social security records) are not currently available to researchers in the UK. The only existing estimates of the earnings losses of displacement come from Borland et al. (2002), who use household survey data from the early 1990s and Hijzen et al. (2010), who use employer survey data matched to firm registers for the period 1994–2003.[3] The UK is an interesting test-case for the study of displacement because it has one of the lowest levels of employment protection in the OECD,[4] and also offers very low state benefits for unemployed workers.[5] An important issue then is whether these institutional features lead to similar post-displacement earnings and income patterns to other "flexible" labour markets such as the US.

In this paper we use household survey data, which offers a number of advantages over the available administrative datasets. First, we can follow individuals through all labour market states before and after displacement. This means that we do not need to exclude individuals from the analysis who subsequently have zero earnings from employment.[6] Second, we have information on income from all sources, including welfare payments and earnings from self-employment. This allows us to directly assess the extent to which alternative sources of income and the welfare system compensate for lost earnings. Third, survey data from a long panel allows us to use a much richer set of pre-displacement characteristics with which we can match displaced and non-displaced workers. Finally, we are able to decompose changes in pay into changes in wages and hours of work. In contrast to the earlier work using survey data for the UK (Borland et al., 2002), we are able to follow a larger sample of displaced workers over a much longer period of time. Our methods allow us to measure earnings loss at various points in time after displacement, and also allow us to follow workers regardless of their subsequent labour market state.[7] In

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[3]Doiron and Mendolia (2011) use the same survey data as in our paper to study the effects of displacement on divorce, which we discuss in more detail in Section 2.

[4]See Venn (2009), which ranks the UK 38th out of 40 for the extent of employment protection.

[5]OECD measures from http://www.oecd.org/els/benefitsandwagesstatistics.htm show that the UK has the least generous net replacement rates for the initial phase of unemployment in the OECD.

[6]This is a common restriction used by those with social security earnings data.

[7]Borland et al. (2002) only compare earnings for those workers who return to employment after displacement. By definition, this is a selected sample of displaced workers.

addition, we explicitly construct matched treatment and control groups and use methods which allow us to directly compare our results with those from other countries.

However, the use of survey data also has some limitations. First, our measure of displacement is self-reported rather than inferred from plant closure or employment reductions. It therefore seems possible that some of the displacements we observe are not the result of job destruction which is exogenous to the individual. To mitigate this, we compare displaced workers to non-displaced workers who have observably similar pre-displacement characteristics and labour market outcomes, and we allow for selection on unobserved fixed characteristics. We also compare workers who report "redundancy" as opposed to "dismissal". Second, self-reported displacements may suffer from recall bias (for example, respondents may be more likely to accurately recall more costly events). To mitigate this possibility, we consider only recall information from the previous 12 months. Finally, our survey data has smaller sample sizes which limit the extent to which we can reliably estimate the impact of displacement on narrowly defined sub-groups.

We show that job displacement in the UK causes an immediate loss in income of nearly 40%, and a long-run reduction in income of approximately 10%. These estimates are similar to the only comparable results for the UK (Hijzen et al., 2010). However, the estimated composition of the loss is different. Hijzen et al. found that the majority of earnings loss is accounted for by lower employment rates rather than lower earnings. In contrast, the results in this paper show that the majority of the long-run loss (80%) is accounted for by a reduction in post-displacement earnings rather than lower employment rates. This suggests that the consequences of displacement in the UK are very similar to the US. Couch and Placzek (2010), who use a comparable methodology, find immediate losses of 32% and long-run losses of 12%. Our results are also consistent with the large US literature which uses survey data, and which therefore relies on self-reported displacement.[8]

We do find a small long-run reduction in the probability of employment, because some displaced workers enter a variety of other labour market states, namely long-term unemployment, self-employment, sickness or disability and early retirement. However,

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[8]See Table 1 in Couch and Placzek (2010).

income from these sources does little to compensate the income losses following displacement. Total income from other sources, including self-employment income, unemployment insurance, retirement income and invalidity benefit reduce losses by only 15% in the first 12 months after displacement, and by about 12% after 10 years.

The paper is structured as follows. In Section 2 we explain how our paper relates to the existing literature on job displacement. In Section 3 we describe the data that we use and how we construct a measure of displacement. Section 4 explains our basic method, which is a variant of a standard difference-in-difference model. In Section 5 we illustrate the basic patterns in the data and in Section 6 we report the effects of displacement on earnings and non-labour income as well as hours of work. Section 7 concludes and discusses our findings in the context of the "flexibility" of the UK labour market.

2   Literature review

This paper relates to three main areas of the literature that examine the impact of job displacement. The first is the long run impact on employment and earnings. The seminal article is JLS, who use administrative data for Pennsylvania between 1980 and 1986 to examine the earnings losses of high seniority men who separate from plants which experience large ($> 30\%$) employment falls. They find that even six years after the event, earnings losses remain at 25% compared to pre-displacement levels. These contrast with somewhat smaller estimates using survey data such as the Panel Study of Income Dynamics (Stevens, 1997) and the Displaced Worker Survey (Farber, 1997). Couch and Placzek (2010) argue that these very large estimated losses are primarily due to the fact that JLS examine a period of particularly high displacement among manufacturing workers in a heavily industrialised state. To demonstrate this, Couch and Placzek use similar data, but examine Connecticut from 1993–2004. Although immediate losses remain high at 32–33%, the estimates of long run losses are reduced to 13–15% after six years.[9] These are in the range of earlier estimates. They are also remarkably consistent with the analysis of Morissette

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[9]Couch et al. (2011) retain the assumption found in Jacobson et al. (1993) that individuals must have positive earnings in every year post displacement. When they drop this assumption losses rise by 15-18%: See footnote 14.

et al. (2013) for Canada. For the UK, Hijzen et al. (2010), using employer survey data matched to firm registers, also find large and persistent effects. In the first five years, losses from firm closure are in the range 18–35% and for mass layoffs 14–25%. However, in contrast to the US literature, Hijzen et al. (2010) argue that these are substantially the result of the high and persistent non-employment rates of the displaced rather than lower earnings on return to work. Couch and Placzek (2010) emphasise the potential importance of the use of matching estimators to control for systematic selection in those who are displaced, although they find only weak evidence for an overstatement of the estimated impact of displacement without matching, perhaps because they have a limited number of demographic variables available for matching.

The second related area concerns the impact of welfare payments on the earnings losses of the displaced and whether this can lead to systematic differences between countries.[10] Welfare payments can provide temporary compensation for short-term earnings losses, but may also prolong search. Increased search duration has two countervailing effects on earnings losses because as well as extending periods out of work, it may also lead to higher post-displacement wages.[11]

Schmieder et al. (2010) use administrative data to examine mass layoffs in West Germany in 1982. Those displaced from stable jobs have long term earnings losses of 10–15%. This is mainly due to a decline in post-displacement wages, as in the US. Schmieder et al. note that although they are examining displacement in a recession year, which may lead to larger losses, they argue that earlier studies using survey data which find smaller losses for Germany (e.g. Burda and Mertens, 2001) include workers subject to temporary layoffs whose losses are likely to much lower. They also show that, even in a country such as Germany with a relatively generous welfare system, the payments only compensate for a small fraction of the earnings losses and only in the immediate aftermath of displacement.

Ehlert (2012) examines the role that welfare benefits play in moderating the impact of transitions from work to unemployment using survey data in both the US and Germany.

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[10]See OECD (2013) for a summary table of other studies.

[11]For the US, both Couch and Placzek (2010) and Jacobson et al. (1993) find that earnings losses and wage losses are actually concentrated among those that claim benefits. This is because only those that cannot find immediate employment register to receive benefits.

The methodology makes it difficult to make direct comparisons because the sample includes both voluntary and involuntary job separations. Nevertheless, Ehlert does find similar post-displacement trajectories for displaced workers in both countries.[12] However, men in the US rely relatively more on family resources to buffer their income compared to men in Germany, who rely more heavily on the welfare state. Womens' income losses are mainly compensated by higher partner earnings in both countries. Single individuals suffer in the US in particular, as they lack both state and family support.

Nordic countries are regarded as having the most comprehensive social safety nets. Eliason (2011) examines the long-run effects of plant closures, and the potentially mitigating impact of social insurance using longitudinal data for Sweden. He finds significant and long-lasting impacts of displacement on the earnings of married males, but somewhat smaller than that found for the US.[13] Average annual losses are approximately 6% 4–12 years after plant closure. Eliason finds a big initial uptake in unemployment insurance, but only limited impacts on sickness/disability insurance or other means tested benefits.

Hardoy and Schøne (2014) look at the role that the welfare state plays in mitigating income losses from displacement in 2002 using register data for Norway. The authors also account for family effects and tax payments. They find that annual earnings decline by only approximately 5% after displacement and, although they only have three post-displacement years, argue this effect is persistent.[14] Norway has a particularly generous welfare system in comparison to most countries and 15–20% of short term losses are compensated by unemployment benefits. However, once health-related benefits, public transfers and changes to tax are accounted for, the negative impact on the household is reduced by 65%.

The third area to which our paper relates is whether displacement leads to entry into other labour market states, such as self-employment or early retirement. For example, Farber (1999) uses the Displaced Workers Survey (DWS) for 1994 and 1996 and examines the status of individuals a year after displacement. He finds high entry rates of dis-

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[12]Comparisons with other studies are also hindered because Ehlert considers household income of married couples rather than individual income.
[13]No effect is found for married women.
[14]This result is comparable to that of Huttunen et al. (2011), who estimate an initial loss of 4.8%, which remains at 3% after seven years.

placed workers into alternative work arrangements as a temporary state before re-entering employment. Von Greiff (2009) estimates that displacement doubles the probability of entering self-employment within one year of the displacement. Fairlie and Krashinsky (2012) examine business creation using the PSID and CPS. Although the focus is on liquidity constraints, they analyse data separately for job losers and non-job losers. The find high entry rates into self-employment from wealthy elderly job losers who have both the necessary capital and relatively poor alternative employment prospects. Such an effect is also found by Nykvist (2008) using register-based data for Sweden. Examining the impact of plant closures in 1987 and 1988 she finds that displacement almost doubles the likelihood of entry into self-employment and those in worse labour market positions react more strongly.[15]

Turning to the impact on retirement, Chan and Stevens (1999, 2001) find, for the US, that men who are displaced postpone retirement in an attempt to rebuild savings. Tatsiramos (2010), using household survey data on 45-64 year olds from the European Community Household Panel from Germany, Italy, the UK and Spain, finds that individuals in those countries which offer relatively generous unemployment benefits and early retirement provisions are less likely to return to work before 60 and more likely to retire post 60. For Norway, Huttunen et al. (2011) find that the most important impact of displacement is not in terms of reduced earnings, but in terms of the probability of movement out of the labour force due to the generosity of early retirement schemes and disability pensions.

In this paper we provide the first comprehensive evidence of the costs of job displacement in the UK which takes into account all three of these aspects. We consider short- and long-term losses; we consider the effect of welfare payments; and we consider the effect of transitions into other labour market states. Further, we are able to precisely match displaced workers with a comparable control group thanks to detailed pre-displacement characteristics.

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[15]Other papers examining this topic include Reize (2000), Caliendo and Kritikos (2010) and Niefert (2010).

3 The data

The British Household Panel Survey (BHPS) is an annual survey of about 5,500 households recruited in 1991, containing approximately 10,000 interviewed individuals. The sample is intended to be nationally representative. Adults in the sample are re-interviewed annually; children are interviewed when they reach the age of 16. Individuals in the sample are followed regardless of whether they remain in the same household or join new households. The BHPS continued until 2008, when it was replaced by and incorporated into the UK Household Longitudinal Survey (HLS).[16]

We use data from all BHPS waves 1–18, interviews for which took place from 1991 to 2009. After appending all 18 waves, the data contain 32,379 individuals and 238,992 person-years. We use all members of the original sample who have full interview outcomes, which results in a sample containing 29,264 individuals and 219,592 person-years. The data are an (approximately) annual panel. For each individual we observe a sequence of interviews from waves 1991 to 2009.[17] The median duration between interviews is almost exactly one year, and 90% of interviews take place within 400 days of the previous interview. Each respondent is asked to report their current labour market status at the time of the interview. In addition, they are asked for information on any labour market spells which began after the 1st September in the previous year, including start and end dates and the reasons why jobs ended.

Using the recall information from the following year's interview, we calculate when the spell in progress at the date of interview ended, and the reason why it ended.[18] In our basic specification, displacement is defined as occurring if an employment spell ends due to "redundancy" or "dismissal" (the full list is given in Appendix A.) As noted by Borland et al. (2002), the distinction between redundancy and dismissal in the UK is

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[16]The first interviews for the BHPS sample in the HLS did not take place until 2010 and 2011, meaning that there is a much larger gap (median 645 days) between the final interview in the BHPS and the first interview in the HLS, during which labour market status and earnings are not recorded. We therefore use only BHPS data in this paper. A detailed description of the BHPS data can be found in Taylor et al. (2010). The HLS is described in Institute for Social and Economic Research (2011).

[17]The precise interview date varies over the year, although 85% of interviews take place in September, October or November. A small number of interviews take place in the following year, hence some interviews take place in 2009.

[18]The precise method for creating the link between the recall information from wave $t+1$ and the current information from wave t is described in detail in Appendix A.

Year	Number of obs.	Number of emp. spells	Prop. emp. spells ending in displacement	Prop. emp. spells ending other reasons	Prop. emp. spells continuing
1991	8,309	4,093	0.058	0.168	0.774
1992	7,907	3,852	0.062	0.187	0.751
1993	7,873	3,782	0.050	0.197	0.753
1994	7,970	3,877	0.052	0.208	0.740
1995	8,027	3,994	0.049	0.191	0.760
1996	8,483	4,277	0.042	0.214	0.745
1997	8,164	4,230	0.047	0.224	0.729
1998	8,076	4,212	0.045	0.229	0.726
1999	9,129	4,580	0.045	0.226	0.730
2000	10,667	5,555	0.046	0.230	0.723
2001	16,142	7,867	0.042	0.194	0.764
2002	14,501	7,110	0.039	0.204	0.757
2003	14,647	7,375	0.034	0.204	0.761
2004	13,293	6,644	0.036	0.205	0.759
2005	13,371	6,669	0.036	0.156	0.808
2006	12,836	6,343	0.034	0.176	0.790
2007	12,396	6,160	0.037	0.158	0.805
2008	475	265	0.042	0.170	0.789
All years	182,266	90,885	0.043	0.197	0.761

Table 1: Basic sample characteristics 1991–2008. The sample includes only those individuals who have an interview in the following wave and therefore includes only interviews from wave 1–17. Displacement in this table is defined to include redundancy, dismissal or the end of a fixed-term contract.

somewhat blurred. In particular, those answering that they were "dismissed/sacked" may include both those who were dismissed for individual reasons as well as those whose job was destroyed for external reasons. In contrast, Doiron and Mendolia (2011) argue that the distinction between these two responses is important, and that individuals who are dismissed for individual reasons may report that they were made redundant. We examine the distinction between these two responses empirically, by testing whether post-displacement behaviour is distinct.

The resulting data are described in Table 1. The proportion of employment spells observed in wave t which are still in progress in wave $t + 1$ is between 70% and 80%. Of those which end, approximately 18% are classified by the respondents as "displacement" (redundancy, dismissal or temporary jobs ending). Note that the measurement of displacement used here will tend to be an under-estimate for short spells of employment, because a spell which starts after the interview in wave t and ends before the interview in wave $t + 1$ will not be recorded.

The prevalence of displacement reported in the BHPS is almost identical to the Office
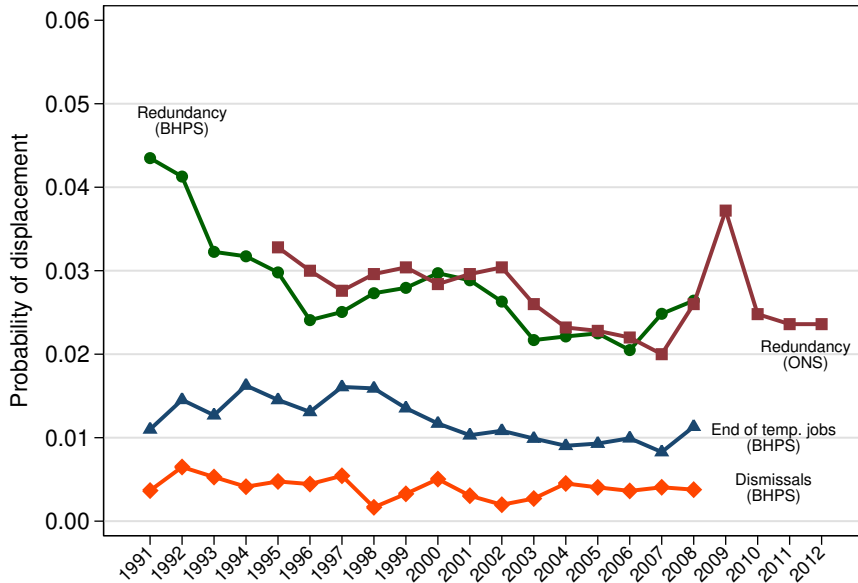
Figure 1: Estimates of probability of displacement from BHPS compared to Office for National Statistics redundancy series (Heap, 2004). The ONS series is calculated from the UK quarterly Labour Force Survey.

for National Statistics redundancy series calculated from the Labour Force Survey, shown in Figure 1.[19] Both series reflect the improving labour market up until the 2008–2009 recession. The incidence of temporary jobs ending is less strongly counter-cyclical, while the dismissal rate is too small to draw conclusions.

4 Methods

We define a series of dummy variable D_i^c, $c = 1991,\dots,2007$ which take the value 1 if individual i experiences displacement between the wave $t = c$ and the wave $t = c+1$ interview, and $D_i^c = 0$ if they do not. Those with $D_i^c = 0$ will therefore include individuals who change job between c and $c+1$ for reasons other than displacement.[20] D_i^c is constant for each individual for a given value of c, but each individual has a separate indicator for

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[19]Series BEIR, calculated as the number of respondents whether working or not working, who reported that they had been made redundant or had taken voluntary redundancy in the previous three months as a fraction of number of employees in the previous quarter. See Heap (2004) for a discussion of ONS measure of redundancy.

[20]Therefore we do not restrict the control group to include only those who continue in employment after wave c. This contrasts with JLS, whose control group consists only of those who *remain in the same firm*. Their definition of earnings losses is therefore "the change in expected earnings if ... the worker would be displaced ... rather than being able to keep his or her job indefinitely." (Jacobson et al., 1993, p.691). Instead, our counterfactual is more general, and is intended to measure the earnings of the displaced workers had they not been displaced. In Appendix D we demonstrate that this restriction on the control group has a significant impact on the estimated losses.

each cohort c. We refer to the sample with $D_i^c = 1$ as the cohort c treatment group and those with $D_i^c = 0$ as the cohort c control group.

To construct the data for a particular cohort, we restrict the sample to all those who are interviewed in wave c and wave $c + 1$, who are in employment in wave c and aged between 20 and 60 in wave c. Note therefore that the control group for a particular cohort may include those who are in the treatment group in other cohorts. Similarly, the treatment group for a cohort may include those who are in the control group in other cohorts.

Define y_{it} to be the outcome of interest for individual i in wave t. These outcomes include employment status (e.g. in employment, in self-employment, hours of work) and various measures of income (e.g. income from employment, self-employment, welfare payments). y_{it} is measured both before $t \leq c$ and after $t > c$ the displacement event. We wish to estimate the impact of D_i^c on y_{it}. The least restrictive method would be to estimate a standard difference-in-difference model separately for each displacement cohort. However, we observe a relatively small number of displacements in each cohort (see Table 1), and so we instead stack together cohorts and impose the restriction that the effect of displacement relative to the displacement date is the same for each cohort.[21] Once stacked, each row in the data is identified by i, c and t because individuals may appear in several cohorts.

For those with $D_i^c = 1$ we record the date on which the displacement occurred. This date is recorded to the nearest day, although since it comes from recall information in the next wave of data, it seems likely to be somewhat approximate. For those with $D_i^c = 0$ we choose a random date in between the interview in waves t and $t + 1$, drawn from a uniform distribution. The difference between the interview date and the displacement (or non-displacement) date, grouped into years, is relative time, denoted r_{ict}. Thus $r_{ict} = 0$ in the year immediately preceding the displacement and $r_{ict} = 1$ in the year immediately after. We restrict attention to $-10 \leq r_{ict} \leq 10$ to ensure sufficient numbers of treated and control observations in each year.

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[21]See Section 6.4 for evidence on how losses vary by cohort.

Our principle estimating equation is then

$$y_{ict} = \alpha_{ic} + \sum_{r=-9}^{10} \gamma^r T_t^r + \sum_{r=-4}^{10} \delta^r (T_t^r D_{ic}) + \epsilon_{it}. \qquad (1)$$

We include a person-cohort fixed effect α_{ic} which captures any pre-existing difference in y_{it} between the treatment and control groups more than five years before displacement. The dummy variables T_t^r indicate time relative to the displacement event which occurs between $r = 0$ and $r = 1$. The coefficient δ^r is a difference-in-difference estimate of the effect of a displacement which occurred r years earlier. δ^r is estimated for five years $(r = -4, -3, \ldots, 0)$ before displacement to allow for the possibility that displacement has effects before the event, and for up to 10 years $(r = 1, 2, \ldots, 10)$ after displacement. We allow the errors ϵ_{it} to be clustered by i across cohorts. The difference-in-difference estimate δ^r controls for any pre-existing difference in y_{it} between the treatment and control groups in the base years, which are at least five years before displacement $(r = -10, -9, \ldots, -5)$.[22]

We can allow for differences in pre-existing earnings trends between the treatment and control groups. JLS note that one can estimate this model by deviating each variable from the person-specific time-trend (as opposed to the person-specific mean in the FE model) and estimating by OLS. Alternatively, one can difference the data and then estimate using FE (Wooldridge, 2010, p.375).

We can also control for differences in observable characteristics between the treatment and control groups during the pre-displacement period. We do this by a combination of one-to-one and propensity score matching, which ensures that we are comparing similar individuals in the treatment and control groups. We match only individuals from the same cohort. This means that individuals cannot be matched with themselves, and that individuals are matched with others who face the same aggregate labour market conditions. Following Rosenbaum and Rubin (1983), define $p(\mathbf{x}_i)$ as the probability of experiencing displacement in the future given a vector of characteristics $\mathbf{x}_i$. $p(\mathbf{x}_i)$ is estimated using a Probit model. The matched sample then consists of displaced and non-displaced individuals who have similar values of $\widehat{p(\mathbf{x}_i)}$. Once a suitably matched sample is obtained,

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[22]Choosing a base year too close to $r = 0$ means that any pre-displacement dip in earnings will tend to increase the estimate of δ^r.

the average effect of displacement on the displaced can be estimated by simply comparing y_{it} between the (matched) treatment and control groups for any value of r. One can also use difference-in-difference models to additionally control for any level differences which remain after matching. Using survey data allows us to estimate a rich model for $p(\mathbf{x}_i)$ which includes detailed pre-displacement characteristics.

5   Descriptive evidence

Before providing formal estimates of the cost of displacement, in this section we the key patterns in the data. We show the extent to which displaced workers are non-randomly selected, and we show the patterns of employment, earnings and income before and after displacement.

The largest sample we use comprises all individuals who are in employment, aged between 20 and 60 and who have information on the outcome of the employment spell in progress at the time of the interview. Our basic definition of displacement includes redundancy and dismissal, but excludes the end of temporary jobs. The resulting treatment group comprises 2,499 individuals (37,631 observations) and the control group comprises 78,823 individuals (1,162,570 observations). The sample is illustrated in Table 2. The number of observations declines as we move further away from the displacement event because of the start and end of the sample period. Nevertheless, we have a reasonable sample size of displaced workers who are observed a long time before and after displacement. The former helps us to match the control and treatment group more precisely, while the latter allows us to measure long-run effects of displacement.

In Table 3 we compare the characteristics of the treatment and control groups 12 months before and five years before displacement. In the first column (< 12 months before displacement) we can see that displaced workers have shorter tenure, work in smaller firms, are more likely to work in manufacturing and in a manual occupation. They are less likely to be union members, which reflects the fact that union membership is concentrated in the public sector, and displaced workers are more likely to be in the private sector. Displaced workers are more likely to be men, less likely to have a degree and less likely to be living

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Relative time		Control group (not displaced at $r = 0$)	Treatment group (displaced at $r = 0$)
$r \leq -10$	>10 years before	61,728	1,574
$-9 \leq r \leq -5$	5–10 years before	153,131	4,401
$r = -4$	4–5 years before	49,081	1,432
$r = -3$	3–4 years before	55,651	1,649
$r = -2$	2–3 years before	62,388	1,857
$r = -1$	1–2 years before	70,152	2,143
$r = 0$	< 12 months before	78,823	2,499
$r = 1$	< 12 months after	78,894	2,477
$r = 2$	1–2 years after	73,533	2,344
$r = 3$	2–3 years after	68,170	2,215
$r = 4$	3–4 years after	62,502	2,069
$r = 5$	4–5 years after	56,825	1,910
$6 \leq r \leq 10$	5–10 years after	195,051	7,010
$r > 10$	>10 years after	96,641	4,051

Table 2: Sample sizes for treatment and control groups by relative time

with a partner. Most of these differences between the control and treatment groups are also visible in the second column (5–6 years before displacement). In addition, workers who are going to be displaced in five years time are much more likely to be unemployed (7% compared to 3%).

The bottom panel of Table 3 also compares the seven outcome variables we analyse in Section 6. 12 months before displacement, the treated have 5% lower wages and 6% higher hours of work, consistent with the fact that the the treated are more likely to be working full-time. Note that self-employment and benefit income are a tiny fraction of total income because the sample is restricted to be those in employment. Differences in pay and hours between the displaced and non-displaced five years before displacement are much smaller and insignificantly different from zero. There are two possible explanations for the fact that the relative earnings of the displaced workers decline prior to displacement. One is that workers who are going to be displaced experience negative shocks to their wages and job quality as they approach the point of displacement. The second is that the sample observed in employment five years before displacement is a non-random selection of those who experience displacement.

To gain an understanding of how employment patterns evolve before and after displacement, Figure 2 illustrates the probability of observing the sample in different labour market states by relative time. Panel (a) shows that one year after displacement more than

	< 12 months before displacement $(r = 0)$			5–6 years before displacement $r = -5$		
	$D_i^c = 1$	$D_i^c = 0$	p-value	$D_i^c = 1$	$D_i^c = 0$	p-value
Employed	1.00	1.00		0.77	0.80	[0.011]
Self-employed	0.00	0.00		0.03	0.03	[0.525]
Unemployed	0.00	0.00		0.07	0.03	[0.000]
Other labour market state (includes missing)	0.00	0.00		0.10	0.12	[0.086]
Number of times interviewed before	4.82	5.27	[0.000]	3.62	3.75	[0.149]
Number of times displaced before	0.20	0.11	[0.000]	0.13	0.09	[0.000]
Displacement cohort (BHPS wave)	9.17	10.01	[0.000]	11.71	12.01	[0.003]
Tenure (years)	4.27	5.10	[0.000]	4.14	4.55	[0.022]
Firm employs < 25 workers	0.38	0.33	[0.000]	0.29	0.32	[0.083]
Works in manufacturing	0.33	0.18	[0.000]	0.35	0.21	[0.000]
Works in manual occupation	0.48	0.40	[0.000]	0.47	0.43	[0.005]
Union member	0.36	0.52	[0.000]	0.39	0.52	[0.000]
Private sector	0.89	0.65	[0.000]	0.86	0.66	[0.000]
Works > 30 hours per week	0.85	0.80	[0.000]	0.86	0.81	[0.000]
White ethnic group	0.94	0.95	[0.055]	0.96	0.96	[0.254]
Born in UK	0.95	0.95	[0.901]	0.95	0.95	[0.983]
Lives in South East	0.23	0.22	[0.193]	0.26	0.25	[0.332]
Female	0.38	0.52	[0.000]	0.41	0.53	[0.000]
Age	38.39	39.13	[0.001]	35.46	35.57	[0.735]
Has degree	0.37	0.47	[0.000]	0.33	0.40	[0.000]
Married or cohabiting	0.70	0.75	[0.000]	0.65	0.70	[0.001]
Real monthly wage last month (£)	1481.23	1564.61	[0.001]	1213.57	1223.43	[0.772]
Real self-employment income last month (£)	0.88	6.08	[0.105]	52.93	34.67	[0.106]
Real monthly labour income last month (£)	1478.40	1553.25	[0.002]	1241.05	1249.51	[0.805]
Real benefit income last month (£)	42.23	50.36	[0.006]	57.77	55.35	[0.558]
Real monthly total income last month (£)	1562.71	1655.43	[0.000]	1342.36	1351.39	[0.792]
Total hours per week	39.94	38.22	[0.000]	32.83	32.32	[0.360]
Normal hours per week (excl. overtime)	36.19	34.25	[0.000]	29.32	28.85	[0.311]
N	2,499	78,823		1,209	42,235	

Table 3: Characteristics of displaced and non-displaced workers before displacement. Both displaced and non-displaced groups are selected from those aged 16–60 at the time of displacement. Tenure, firm size, industry, occupation, union membership and hours per week refer only to those in employment. 'Degree' includes university degree, teaching qualifications and any other technical, professional or higher qualifications.

60% of the displaced are already re-employed, and this increases to over 70% two years after displacement. After three years the probability of employment declines for both the treatment and control groups because both groups move into other labour market states, as shown in the remaining panels (recall that the control group are not restricted to be those who remain with their employer). Thus, employment rates in the control group also decline for $r > 0$. Panel (a) also shows clearly how the pre-displacement employment patterns differ between the two groups, with the displaced experiencing lower pre-displacement employment rates.
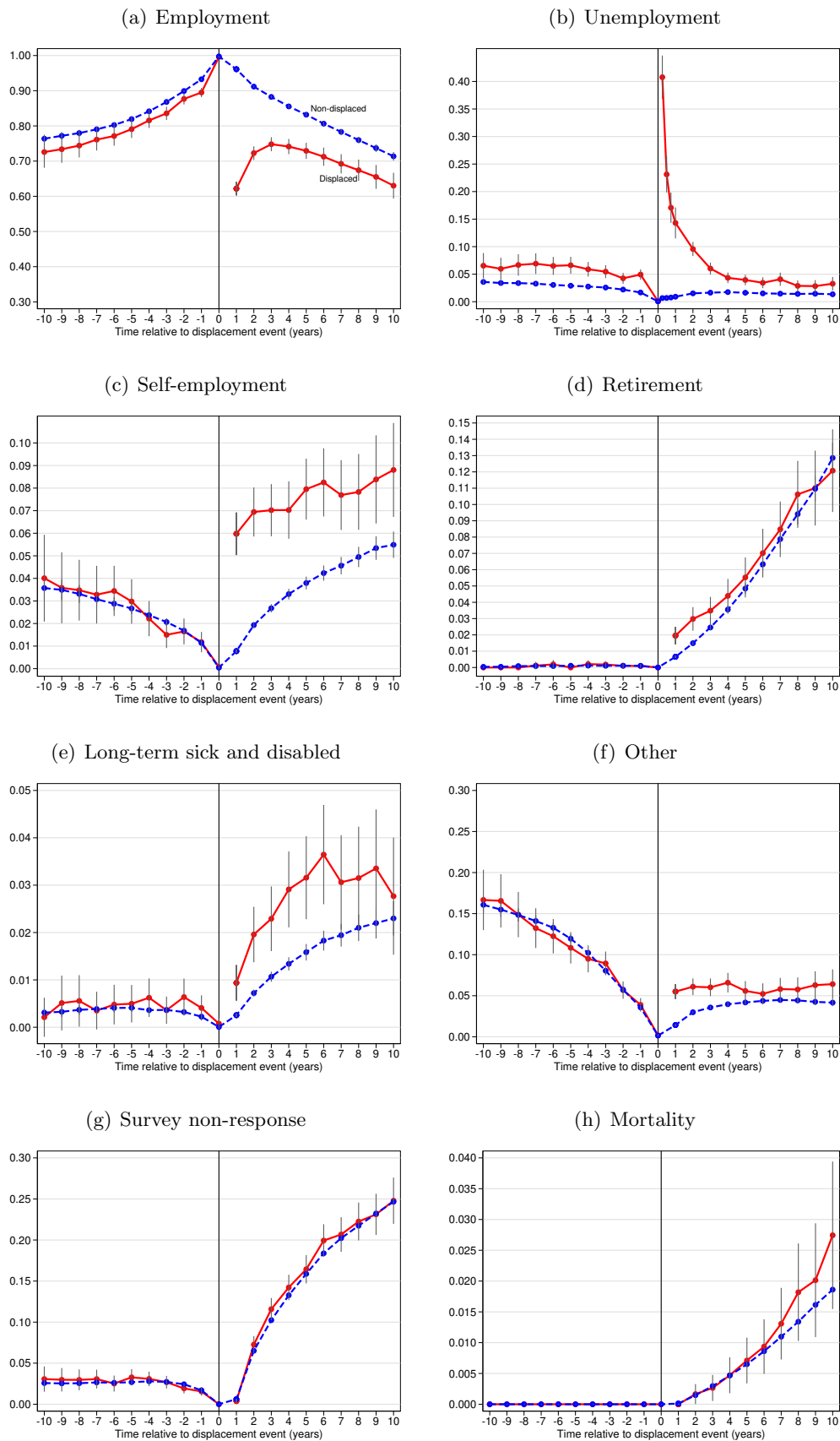
Figure 2: Probability of different self-reported labour market states before and after displacement 1991–2007. 95% confidence intervals around the mean based on clustered standard errors.

17

An advantage of the survey data, in contrast to the administrative data available for the UK, is that we can also calculate what happens to individuals who are not in employment.[23] This is illustrated in the remaining panels of Figure 2. Panel (b) shows that large increases in unemployment are short-lived. In this panel we also show estimates for three, six and nine months after displacement.[24] After three months, more than 40% of displaced workers classify themselves as unemployed, but this falls rapidly to less than 15% after 12 months. After five years, unemployment rates amongst the treatment group are only slightly higher than in the control group. The pre-displacement difference in unemployment rates is also very clear from panel (b).

In panel (c) we show that displacement causes a sudden burst of entry into self-employment. After five years, 8% of displaced workers report themselves to be self-employed, compared to 4% of the control group. Panel (d) shows that displaced workers enter retirement more quickly in the first few years, but this effect is relatively short-lived.[25] Panel (e) shows that displacement is also associated with higher rates of self-reported sickness, although small sample sizes render our estimates rather imprecise after six years. The remaining labour market states (essentially, family care and education) are shown in Panel (f).

The employment patterns shown in Panels (a)–(f) in Figure 2 are conditional on being interviewed: labour market status is missing for those individuals who do not participate in the survey. However, it turns out that attrition from the panel is almost identical in the treatment and control groups, shown in panel (g). The BHPS allows us to identify individuals who left the survey due to death, and this is shown in panel (h). There appears to be a small but increasing difference in mortality rates between displaced and non-displaced workers, which is consistent with the higher self-reported levels of sickness shown in panel (e), but note that the imprecision of the estimates means that we cannot reject the null of no effect on mortality.

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[23]As noted in the literature review, administrative data for some countries (in particular Norway and Sweden) do contain information on labour market states other than employment, and also on receipt of income from welfare payments.

[24]Although the survey is annual, the fact that displacement occurs at different points within the year means that some interviews take place within three, six and nine months of the displacement date.

[25]Most other studies consider a younger age cut-off in order to reduce the post-displacement retirement rate. This is usually because the studies cannot determine labour market state unless it is employment.

To summarise, Figure 2 shows that, 10 years after displacement, there is approximately an 8 percentage point gap in employment between displaced and non-displaced workers. This gap is made up from higher rates of unemployment (2 pp), self-employment (3.3 pp), ill-health and mortality (0.5 pp), and other labour market states such as education and family care (2.2 pp).

We now turn to the pattern of pay and other income following displacement. The patterns of employment in Figure 2 show that alternative sources of income from unemployment benefit, self-employment and retirement may reduce the costs of displacement. In contrast to administrative data, our survey data sheds some light on this issue. First, suppose that we have no information on income outside of the labour market, and we assume that indviduals not in employment have zero income. The resulting pattern of pay is shown in panel (a) of Figure 3. One can see that pay losses mimic very closely the pattern of employment shown in panel (a) of Figure 2. After 10 years, pay per month in the treatment group is £330 or 23% lower than in the control group.

If we examine only those in employment then we can estimate the loss which occurs as a result of lower monthly pay in subsequent jobs, rather than the total loss (which includes periods of zero earnings). The resulting estimates are shown in Panel (b) of Figure 3. In this case after 10 years the earnings gap is £289 per month or 15%. In other words, of the total 23% gap in earnings, about two-thirds is accounted for by a monthly earnings gap for those in employment, and the remaining one-third by an employment gap. A couple of other features are interesting. First, note that there is some indication of different pay growth rates before displacement, although the effects are not large. Second, there is little indication of any narrowing of the pay loss even after 10 years; if anything the pay gap gets bigger.

In Panel (c) we measure total monthly labour income, which includes any earnings from self-employment which occur after displacement. Differences between (c) and (a) are very minor because self-employment income is relatively unimportant. Note that from Figure 2 we know that by year 10 about 7% of the treatment group are in self-employment compared to 4% of the control group, and including self-employment earnings reduces the earnings gap from 23% to 18%. In Panel (d) we include gross monthly income from all

(a) Gross pay last month, zero for non-employment

(b) Gross pay last month, employment spells only
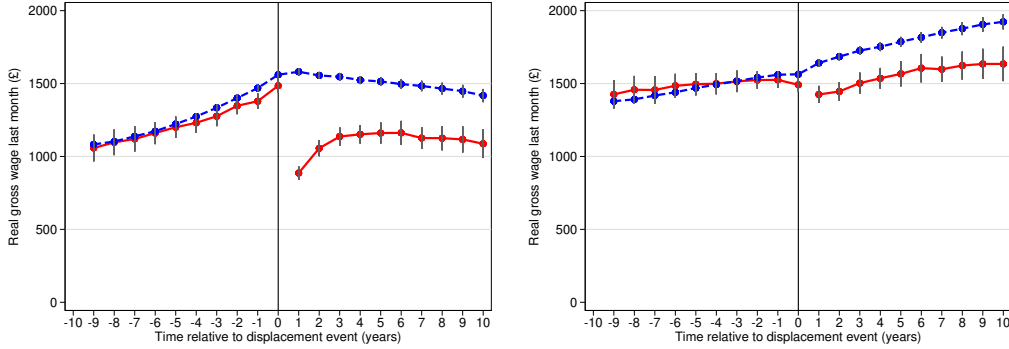
(c) Gross labour income last month
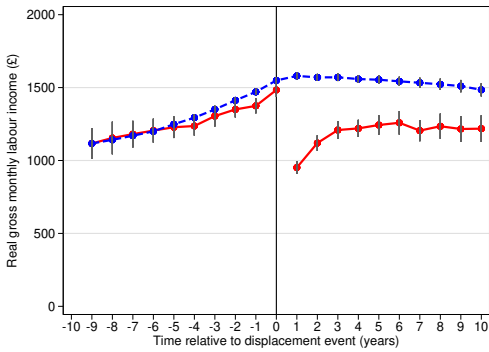
(d) Gross total income last month
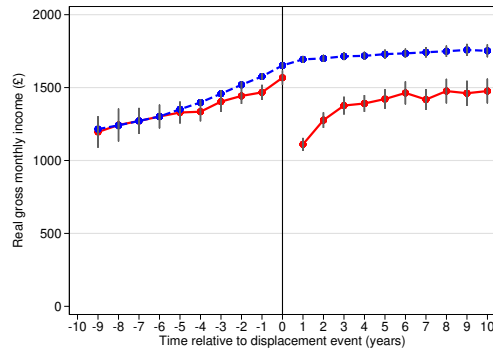
Figure 3: Individual gross income by treatment and control groups. Relative time is grouped into annual intervals. In the first panel, pay is zero for all non-employment spells. The second panel uses only spells of employment. 95% confidence intervals around the mean based on clustered standard errors.

sources, which includes benefit and pension payments. This only has a minor effect on income, reducing the gap after 10 years from 18% to 16%.

6 Results

In this section we control for the pre-existing differences in levels and trends between displaced and non-displaced workers, and we also ensure that displaced and non-displaced workers are comparable by matching on their pre-existing characteristics and labour market histories. In order to control for pre-existing differences in levels and trends in earnings, it is important that we have sufficient pre-displacement information. For this reason we keep only individuals in cohorts 6 (displaced after their 1996 interview) to 17 (displaced after their 2007 interview) to ensure that all observations have at least six years of pre-

	(1) Pay last month	(2) Pay last month, emp. spells only	(3) Self-emp income last month	(4) Total labour income last month	(5) Benefit income last month	(6) Total income last month
3–5 years before	−0.009	−0.013	−0.011	−0.016	−0.002	−0.015
	(0.015)	(0.014)	(0.008)	(0.018)	(0.002)	(0.017)
1–3 years before	−0.025	−0.034**	−0.013	−0.044**	−0.006**	−0.043**
	(0.018)	(0.017)	(0.008)	(0.019)	(0.003)	(0.018)
< 1 year before	−0.037*	−0.056***	−0.008	−0.046**	−0.005	−0.044**
	(0.020)	(0.019)	(0.008)	(0.020)	(0.003)	(0.020)
< 1 year after	−0.382***	−0.151***	0.021**	−0.366***	0.011***	−0.330***
	(0.027)	(0.026)	(0.010)	(0.025)	(0.003)	(0.024)
1–3 years after	−0.259***	−0.180***	0.016	−0.250***	0.005	−0.224***
	(0.025)	(0.023)	(0.011)	(0.026)	(0.004)	(0.024)
3–5 years after	−0.219***	−0.156***	0.008	−0.223***	−0.000	−0.207***
	(0.030)	(0.029)	(0.011)	(0.027)	(0.004)	(0.026)
5–7 years after	−0.224***	−0.176***	0.022	−0.203***	0.007	−0.184***
	(0.031)	(0.029)	(0.015)	(0.031)	(0.005)	(0.027)
7–10 years after	−0.169***	−0.148***	0.030	−0.161***	0.007	−0.157***
	(0.042)	(0.042)	(0.022)	(0.037)	(0.008)	(0.032)
Number of obs.	674,022	587,332	674,022	673,813	674,022	674,022
Number of indiv.	9,648	9,637	9,648	9,648	9,648	9,648

Table 4: FE estimates of the cost of displacement on individual pay and income. Table reports estimates of δ^r from Equation (1) expressed as a proportion of total income last month of the treatment group at $r = 0$. Both displaced and non-displaced groups are selected from those aged 16–60, who are in employment in wave c.

displacement information. From Equation (1), note that we treat the observations $r < −4$ as the base period, but allow for the fact that displacement may have effects on pay in the period $−4 \le r \le 0$.

We begin by reporting estimates based on unmatched displaced and non-displaced workers which rely on fixed-effects to remove unobserved differences between the groups, and we then report estimates based on samples matched using propensity score matching.

6.1 Unmatched samples

Our base model is Equation (1), which controls for individual fixed-effects. Results are reported in Table 4.[26] Because we may observe zero pay, we estimate (1) in unlogged form and express the resulting coefficient estimates as a proportion of the mean pay of displaced workers at $r = 0$.

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[26]In Appendix B we compare estimates from difference-in-differences, fixed-effects, fixed-effects with group trends and fixed-effects with individual trends.

Column (1) of Table 4 shows some evidence of a pre-displacement effect on pay which increases up to the point of displacement. This may either be due to pay falls within firms (for example, employers who are in difficulty paying lower wages) or due to selection of those who are going to be displaced into lower-paying firms. In the short-run, earnings fall by nearly 40% and then recover, but are still 17% lower than the counterfactual after 7–10 years.

If we compare column (1) and (2) we can gauge the extent to which these losses are caused by falls in monthly pay or differences in employment rates, because column (2) only considers those in employment. In the short-run after displacement, 60% of the earnings loss is due to lower employment rates, while 40% is due to lower pay (0.151/0.382). As time passes, a larger fraction of the earnings loss is accounted for by falls in monthly pay because a larger fraction of the displaced sample re-enters employment. In the final row we see that 88% (0.148/0.169) of the loss is accounted for by falls in monthly pay. These essentially replicate the patterns in the raw data shown in Figure 3.

In column (3) we show that self-employment income is unimportant in mitigating either the short- or the long-run loss. There is a small increase in self-employment income of about 2% (albeit imprecisely estimated after one year). In column (4) we show that losses in total labour income (which includes self-employment income) are only slightly smaller than the losses in earnings shown in column (1).

In column (5) we report estimates of the impact of benefit income. Recall from panel (b) of Figure 2 that unemployment is typically a short-run experience, and as a result benefit effects are small and short-lived. Only 21% of the displaced sample report being unemployed one year after the displacement, and only 60% of these report receiving any benefit income. Those who are unemployed and in receipt of benefit receive an average of £313 per month in benefit income, which is consistent with an increase in benefit income as a result of receiving out of work benefit (Job Seekers' Allowance). However, this implies that the average benefit income across the whole displaced sample is a very small fraction of total income. The only significant effect is a 1% increase in total income in the first year after displacement. In column (6) we take all sources of income together, which shows that in the short-run, losses are reduced from 38% to 33%, and in the long-run losses are

22

reduced from 17% to 16%.

As we noted in the introduction, administrative data do not typically allow one to determine whether the fall in monthly pay is the result of falls in wages or falls in hours. In Table 5 we therefore report the corresponding estimates of the effect of displacement on normal and total hours. Normal hours are calculated from the question "Thinking about your (main) job, how many hours, excluding overtime and meal breaks, are you expected to work in a normal week?". Total hours are the sum of normal hours and overtime hours, calculated from the question "How many hours overtime do you usually work in a normal week?". Individuals who are not in employment are assigned zero hours of work.

Column (1) of Table 5 should be compared with the same column (1) in Table 4. In the year immediately following displacement monthly pay is 38% lower, while total hours are 28% lower. This is unsurprising since both include the large fraction of displaced workers who have zero hours of employment after displacement. After 10 years, monthly pay is 17% lower while total hours are 7% lower, suggesting that the majority of the fall in earnings is driven by lower wages rather than lower hours of work. Column (2) of Table 5 shows how hours change conditional on being in employment. Here we see that hours of work in the jobs in which the displaced are re-employed are approximately 5% lower, and this fall is persistent over 10 years. This may partly be because displaced workers are more likely to be re-employed in part-time work (Farber, 1999). It may also be because these jobs offer less opportunity for overtime. In Column (4) we see that the fall in normal hours is about two percentage points smaller than the fall in total hours. Nevertheless, the results in Table 5 show that, in the long-run (after 10 years), the majority of the fall in earnings is due to a fall in wages, not hours.

6.2 Matched samples

As shown in Table 3, the treatment and control groups differ significantly in their observable characteristics before displacement occurs. To deal with this problem we use propensity score matching rather than regression to ensure that all members of the treatment and control group lie in the common region of the displacement propensity distribution. We match within cohort, so that an individual displaced in year c is matched only with

	(1) Total hours per week	(2) Total hours per week, emp. spells only	(3) Normal hours per week	(4) Normal hours per week emp. spells only
3–5 years before	0.006	−0.012	0.010	−0.006
	(0.014)	(0.008)	(0.012)	(0.007)
1–3 years before	0.007	−0.022**	0.018	−0.005
	(0.015)	(0.009)	(0.013)	(0.008)
< 1 year before	0.038**	−0.023**	0.048***	−0.003
	(0.017)	(0.010)	(0.014)	(0.008)
< 1 year after	−0.277***	−0.052***	−0.238***	−0.029***
	(0.021)	(0.012)	(0.018)	(0.010)
1–3 years after	−0.116***	−0.048***	−0.096***	−0.031***
	(0.021)	(0.011)	(0.018)	(0.009)
3–5 years after	−0.083***	−0.038***	−0.068***	−0.024**
	(0.022)	(0.012)	(0.019)	(0.010)
5–7 years after	−0.097***	−0.055***	−0.078***	−0.036***
	(0.023)	(0.014)	(0.021)	(0.012)
7–10 years after	−0.068**	−0.058***	−0.046*	−0.033**
	(0.029)	(0.018)	(0.025)	(0.016)
Number of obs.	666,106	579,726	672,380	585,794
Number of indiv.	9,642	9,621	9,646	9,634

Table 5: FE estimates of the cost of displacement on individual hours. Table reports estimates of δ^r from Equation (1) expressed as a proportion of the dependent variable of the treatment group at $r = 0$. Both displaced and non-displaced groups are selected from those aged 16–60, who are in employment in wave c.

individuals not displaced in year c. In our base specification we impose the restriction that the treated and controls have common support, we allow for up to 10 nearest neighbours and we restrict the difference in the propensity to be no more than 0.005. In other words, the control group must have a propensity of being displaced less than 0.5% different from the treatment group. The propensity score is generated by a Probit model on a vector $\mathbf{x}_i$ which contains measures of pre-displacement labour market history, firm tenure, firm size, sector of employment, occupation, union status, ethnic group, country of birth, region, sex, age, education and marital status. See Table C.1 in Appendix C.

In the first column of Table 6 we compare the observable characteristics of the treatment and control groups in the 12 months before displacement. If one compares these differences with those shown in Table 3 it is clear that matching has eliminated the difference between the treatment and control groups (in Appendix C we report various balancing tests in more detail). There does remain a small difference in monthly wages and there-

	< 12 months before displacement			5–6 years before displacement		
	$D_i^c = 1$	$D_i^c = 0$	p-value	$D_i^c = 1$	$D_i^c = 0$	p-value
Employed	1.00	1.00		0.81	0.81	[0.800]
Self-employed	0.00	0.00		0.03	0.02	[0.365]
Unemployed	0.00	0.00		0.06	0.05	[0.076]
Other labour market state	0.00	0.00		0.10	0.12	[0.109]
Number of times interviewed before	6.06	6.00	[0.677]	3.71	3.66	[0.637]
Number of times displaced before	0.23	0.23	[0.753]	0.13	0.16	[0.082]
Displacement cohort (BHPS wave)	11.79	11.79	[1.000]	11.80	11.83	[0.827]
Tenure (years)	4.57	4.56	[0.988]	4.37	4.35	[0.949]
Firm employs < 25 workers	0.38	0.38	[0.908]	0.29	0.35	[0.001]
Works in manufacturing	0.33	0.33	[0.824]	0.35	0.34	[0.657]
Works in manual occupation	0.45	0.45	[0.977]	0.46	0.48	[0.286]
Union member	0.35	0.35	[0.744]	0.40	0.39	[0.701]
Private sector	0.90	0.90	[0.949]	0.85	0.86	[0.545]
Works > 30 hours per week	0.86	0.87	[0.386]	0.86	0.85	[0.832]
White ethnic group	0.92	0.92	[0.910]	0.96	0.96	[0.788]
Born in UK	0.95	0.95	[0.891]	0.95	0.95	[0.881]
Lives in South East	0.19	0.20	[0.666]	0.26	0.25	[0.690]
Female	0.40	0.40	[0.782]	0.43	0.41	[0.154]
Age (years)	38.95	38.99	[0.910]	35.91	35.40	[0.205]
Has degree	0.41	0.41	[0.818]	0.32	0.32	[0.779]
Married or cohabiting	0.69	0.70	[0.900]	0.66	0.66	[0.981]
Real monthly wage last month (£)	1525.66	1611.44	[0.016]	1225.67	1207.73	[0.663]
Real self-employment income last month (£)	0.00	0.00		50.85	34.11	[0.482]
Real monthly labour income last month (£)	1528.57	1582.94	[0.077]	1247.88	1239.28	[0.845]
Real benefit income last month (£)	51.20	43.94	[0.098]	58.64	52.03	[0.188]
Real monthly total income last month (£)	1626.55	1680.49	[0.092]	1351.91	1333.95	[0.682]
Total hours per week (£)	39.67	40.05	[0.267]	32.83	33.45	[0.365]
Normal hours per week (excl.overtime)	36.11	36.09	[0.942]	29.40	29.87	[0.435]
N	1,413	11,125		894	7,074	

Table 6: Characteristics of displaced and non-displaced workers before displacement, after propensity score matching on characteristics at $r = 0$. "Real monthly pay" refers to pay from employment, while "Real monthly earnings" also includes any self-employment earnings.

fore earnings at $r = 0$, but all other characteristics are insignificantly different between the displaced and non-displaced samples. The second column of Table 6 provides a stronger test of whether matching has successfully removed differences between the displaced and non-displaced groups, because we are comparing 5–6 years before the displacement occurs. Even here, matching has greatly reduced the differences between the displaced and non-displaced groups. There remains a very small difference in unemployment propensity (significant at 10%), but this difference is greatly reduced from the difference in unemployment propensity observed in the unmatched sample. There also remains a difference of 3 percentage points in the number of times displaced before, but in fact in this case it
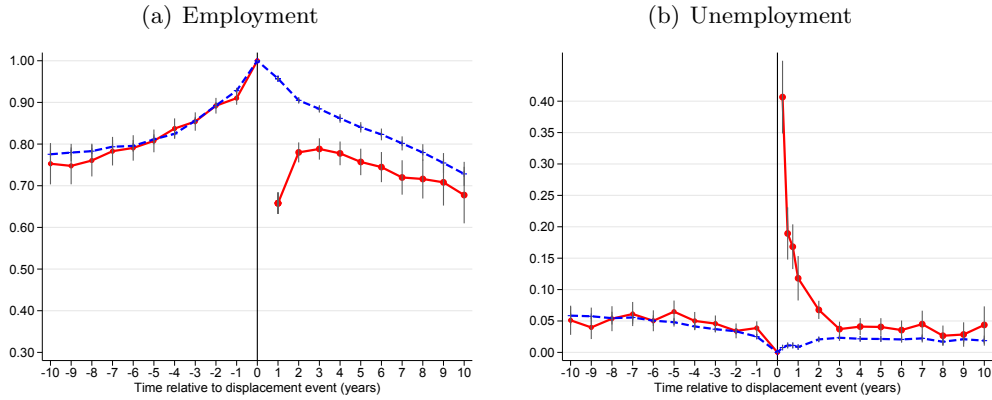
Figure 4: Probability of different self-reported labour market states before and after displacement 1991–2007, matched samples. 95% confidence intervals around the mean based on clustered standard errors.

is the non-displaced group who have a higher displacement rate from earlier periods.

A graphical illustration of the effectiveness of matching is provided by Figure 4, which can be compared with the unmatched comparison in panels (a) and (b) of Figure 2. After matching, the non-displaced comparison group has almost identical pattern of pre-displacement employment and unemployment.

After matching, we estimate a difference-in-difference model, and the results are reported in Table 7. Comparing column (1) between the unmatched (Table 4) and matched (Table 7) results, we see that matching has very little effect on the estimated loss of displaced workers in the first three years after displacement. However, after five years the matching estimates are about 20% smaller (less negative), and after 10 years about 30% smaller than those estimated from the unmatched samples. Since the treatment group in both sets of estimates is almost identical, this tells us that the income growth of the matched control group is worse than that of the unmatched control group. Matching therefore reduces estimated long-run losses. Comparing column (2), the matched samples from Table 7 also produce estimates of pay (conditional on employment) losses after 10 years which are approximately 30% smaller than the unmatched FE estimates from Table 4. Our basic conclusions regarding the importance of other sources of labour income and benefit income remain the same as from the unmatched FE estimates: none of these income sources do much to mitigate losses. Column (6) shows that total income loss 7–10 years after displacement is about 10%, compared to a 12% loss in column (1).

26

In Table 8 we report estimates of the effect of displacement on hours of work, after matching. As with the income results in Table 7, matching reduces the estimated falls in hours. In particular, column (2) of Table 8 shows that those displaced workers who are re-employed have only marginally lower hours than the matched sample of non-displaced workers. In other words, the 10% fall in earnings shown in column (2) of Table 7 is entirely driven by falls in wages.

6.3   Selection into displacement

A potential bias to our estimates arises if there is a particular type of selection into displacement. If, for example, those who are displaced are selected on the basis of a negative *shock* to an unobservable characteristic (e.g. performance) which affect wages, then we will over-estimate the cost of displacement. As noted by Gibbons and Katz (1991), redundancies which are not caused by plant closure may allow for some discretion in terms of who gets displaced. However, we think that this bias is unlikely to be a major component of the observed estimate, for a number of reasons.

First, only a very specific type of selection into displacement threatens our identification strategy. The selection has to be on the basis of a shock to performance which starts before $r = 1$, but it also has to be persistent from $r = 1$ onwards. If it reverts after $r = 1$ then the long-term DiD estimates are unbiased. If selection into displacement is on the basis of permanent differences in performance then our DiD methodology deals with the selection. If the shock occurs before $r = 0$ then we can test for it by examining the patterns of pre-displacement wages. In Table 7 estimates of δ prior to displacement are insignificantly different from zero until $r = -1$. If selection into displacement is on the basis of different pre-displacement trends in performance then our estimates which allow for differing trends deal with selection. In Table B.1 (column 3), we show that the difference in the pre-displacement trends are estimated to be close to zero.

Second, this particular type of selection seems much less likely to occur in a carefully matched sample. We have already seen from the matched samples that the time-series pattern of employment is remarkably similar for the displaced and non-displaced workers (see Figure 4). This means that if there is still an unobserved difference in performance

	(1) Pay last month	(2) Pay last month, emp. spells only	(3) Self-emp income last month	(4) Total labour income last month	(5) Benefit income last month	(6) Total income last month
3–5 years before	0.016	0.009	−0.015*	0.008	0.000	0.009
	(0.019)	(0.019)	(0.008)	(0.021)	(0.003)	(0.021)
1–3 years before	−0.002	0.000	−0.016*	−0.018	−0.001	−0.019
	(0.021)	(0.020)	(0.008)	(0.022)	(0.003)	(0.022)
< 1 year before	−0.040*	−0.046**	−0.010	−0.033	0.003	−0.031
	(0.022)	(0.022)	(0.008)	(0.022)	(0.003)	(0.022)
< 1 year after	−0.367***	−0.106***	0.015	−0.346***	0.017***	−0.310***
	(0.028)	(0.029)	(0.011)	(0.028)	(0.004)	(0.027)
1–3 years after	−0.230***	−0.145***	0.010	−0.217***	0.011***	−0.191***
	(0.027)	(0.027)	(0.011)	(0.028)	(0.004)	(0.027)
3–5 years after	−0.179***	−0.119***	−0.000	−0.181***	0.004	−0.165***
	(0.032)	(0.034)	(0.011)	(0.031)	(0.005)	(0.030)
5–7 years after	−0.172***	−0.112***	0.005	−0.156***	0.010*	−0.132***
	(0.036)	(0.038)	(0.015)	(0.037)	(0.006)	(0.035)
7–10 years after	−0.120**	−0.101*	0.014	−0.116**	0.007	−0.105**
	(0.051)	(0.058)	(0.024)	(0.049)	(0.009)	(0.046)
Number of obs.	155,280	133,138	155,280	155,238	155,280	155,280
Number of indiv.	6,035	6,035	6,035	6,035	6,035	6,035

Table 7: PSM estimates of the cost of displacement on individual earnings and income.

	(1) Total hours per week	(2) Total hours per week, emp. spells only	(3) Normal hours per week	(4) Normal hours per week emp. spells only
3–5 years before	−0.011	−0.013	−0.006	−0.007
	(0.014)	(0.010)	(0.012)	(0.008)
1–3 years before	−0.023**	−0.015*	−0.010	−0.002
	(0.011)	(0.009)	(0.009)	(0.007)
< 1 year before	−0.009	−0.009	0.000	0.001
	(0.008)	(0.008)	(0.007)	(0.007)
< 1 year after	−0.315***	−0.022**	−0.277***	−0.013
	(0.015)	(0.010)	(0.013)	(0.009)
1–3 years after	−0.134***	−0.020**	−0.120***	−0.017**
	(0.014)	(0.010)	(0.012)	(0.008)
3–5 years after	−0.092***	−0.007	−0.089***	−0.014
	(0.017)	(0.012)	(0.015)	(0.010)
5–7 years after	−0.101***	−0.020	−0.096***	−0.024*
	(0.021)	(0.015)	(0.018)	(0.012)
7–10 years after	−0.056*	−0.012	−0.050*	−0.011
	(0.029)	(0.024)	(0.026)	(0.019)
Number of obs.	153,410	131,353	154,912	132,798
Number of indiv.	6,033	6,032	6,035	6,035

Table 8: PSM estimates of the cost of displacement on individual hours.

shocks between $r = 0$ and $r = 1$ it would have to be entirely uncorrelated with earlier shocks.

Third, redundancy criteria in the UK are made on the basis of organisational requirements (such as which jobs must be lost) rather than the performance of individual workers. In the UK, poorly-performing workers can normally be dismissed only after formal dismissal procedures.

Finally, if negative selection were a major problem which affected self-reported measures of displacement, we expect that estimates from survey data will be systematically larger than estimates from administrative data which use externally verified measures of displacement. The evidence from Couch and Placzek's (2010) literature review does not support this assertion.

To further examine the issue of selection in the data, we consider the distinction between those those whose job spell ended due to redundancy and those that ended due to dismissal. We would expect that selection into displacement as a result of a shock to performance will be more likely for those who are dismissed. Of the 2,499 displacements, the vast majority (2,225) are "made redundant" and only 264 are "dismissed/sacked". We perform propensity score matching separately on each sub-group and estimate a difference-in-difference model on the matched samples.

Table 9 reports the results. Columns (1) and (2) repeat the base model for all displacements. Columns (5) and (6) report the estimates for the "dismissal" sample. These estimates are substantially larger, particularly for periods further away from the displacement date. Dismissed workers' wage losses are estimated to be around 30% of the pre-displacement wage after 10 years, compared to only 10% for workers made redundant, shown in columns (3) and (4). These results are consistent with the hypothesis that selection into dismissal is more likely than selection into redundancy. However, the very small number of dismissals means that the results would not change greatly if they are excluded.

	All displacements		Made redundant		Dismissed/sacked	
	(1)	(2)	(3)	(4)	(5)	(6)
	Pay last month	Pay last month, emp. spells only	Pay last month	Pay last month, emp. spells only	Pay last month	Pay last month, emp. spells only
3–5 years before	0.016	0.009	0.034*	0.021	−0.061	−0.042
	(0.019)	(0.019)	(0.019)	(0.020)	(0.078)	(0.079)
1–3 years before	−0.002	0.000	0.024	0.018	−0.094	−0.038
	(0.021)	(0.020)	(0.022)	(0.022)	(0.080)	(0.080)
< 1 year before	−0.040*	−0.046**	−0.009	−0.013	−0.109	−0.091
	(0.022)	(0.022)	(0.023)	(0.024)	(0.081)	(0.088)
< 1 year after	−0.367***	−0.106***	−0.351***	−0.094***	−0.421***	−0.086
	(0.028)	(0.029)	(0.030)	(0.031)	(0.090)	(0.110)
1–3 years after	−0.230***	−0.145***	−0.207***	−0.125***	−0.335***	−0.194**
	(0.027)	(0.027)	(0.029)	(0.029)	(0.093)	(0.095)
3–5 years after	−0.179***	−0.119***	−0.153***	−0.098**	−0.427***	−0.338***
	(0.032)	(0.034)	(0.036)	(0.038)	(0.115)	(0.121)
5–7 years after	−0.172***	−0.112***	−0.158***	−0.118***	−0.404***	−0.358***
	(0.036)	(0.038)	(0.039)	(0.042)	(0.142)	(0.135)
7–10 years after	−0.120**	−0.101*	−0.098*	−0.093	−0.300*	−0.296*
	(0.051)	(0.058)	(0.054)	(0.063)	(0.176)	(0.162)
Number of obs.	155,280	133,138	142,953	122,975	13,476	10,912
Number of indiv.	6,035	6,035	5,651	5,651	1,087	1,087

Table 9: DiD estimates on matched samples, comparison of displacement type.

6.4 Losses by cohort

The pooled models impose the restriction that the effect of displacement relative to the displacement date is the same for all cohorts. For example, the effect of displacement in 2000 on an individual's earnings in 2002 is the same as the effect of displacement in 2002 on an individual's earnings in 2004. The structure of our data implies that estimates of the loss for periods further from the displacement event are identified by earlier cohorts, since later cohorts are followed for a shorter period of time. Davis and von Wachter (2011) show that the cost of displacement varies substantially across cohorts, depending in particular on economic conditions at the time of displacement: workers displaced in a downturn suffer much larger losses. Thus, it is possible that the slow recovery in earnings may be driven by that fact that earlier cohorts were displaced during the weaker labour market in the early 1990s. In Figure 5 we report estimates of (1) for four displacement cohorts. To maintain reasonable sample size, each cohort comprises three years: 1996–1998, 1999–2001, 2002–2005 and 2006–2008.

It is clear that the pattern of earnings recovery is very similar across cohorts, and there is no systematic change in the rate of recovery over the sample period. This may be a
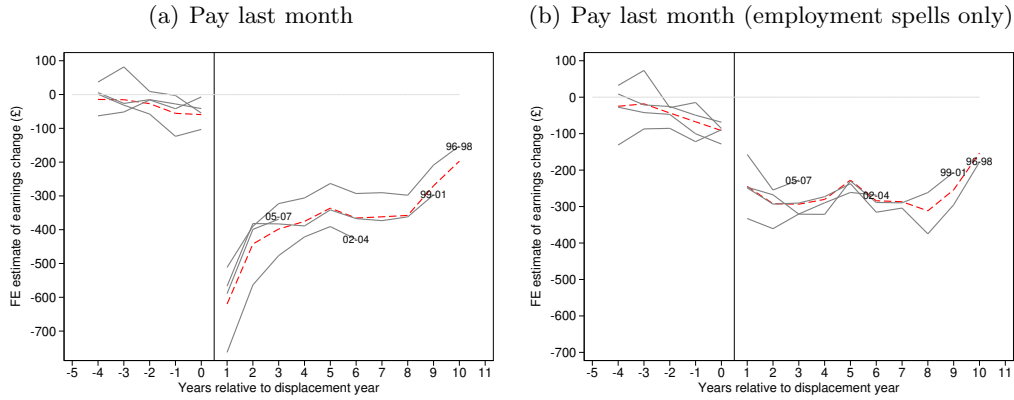
Figure 5: FE estimates by cohort (each cohort contains three years). Dotted line indicates average across all cohorts.

result of the fact that business cycle fluctuations were small over the sample period (which pre-dates the "Great Recession" of 2008–2009), and therefore the time-series pattern of earnings recovery is unlikely to be affected by the business cycle.

6.5 Further robustness checks

Our strategy in this paper is to provide estimates of the cost of displacement based on the largest sample of displaced workers. In part this is driven by the relatively small numbers of displaced workers observed in the survey data, but also by a desire to provide estimates which are representative of all displacements. In contrast, much of the literature, following JLS, restricts the group of displaced workers in various dimensions, such as age, firm tenure and the observation of post-displacement earnings. In Appendix D we investigate the impact of various sampling decisions on the estimated loss. We show that most of these decisions have only a small impact on estimated losses, consistent with findings from the US, as in von Wachter et al. (2009). The most significant sampling decision is that which relates to the control group. If the control group is restricted to include only those who remain with their employer from $r > 0$ onwards, estimated losses are larger and there is no recovery in relative pay because the control group have faster growth in pay.

31

7 Conclusion

In this paper we provide comprehensive evidence of the costs of job displacement in the UK. We consider short- and long-term losses, the effect of welfare payments, the effect of transitions into other labour market states, and the distinction between hours and wages. Further, we are able to precisely match displaced workers with a comparable control group using detailed pre-displacement characteristics. Once matched, the displaced and non-displaced samples exhibit very similar pre-displacement trends in both employment patterns and earnings.

Earnings losses in the short-run are almost 40% of pre-displacement earnings; after 10 years losses are approximately 10% of pre-displacement earnings. Our estimates of the total loss lie in between the estimated losses from firm closure and mass-layoffs reported by Hijzen et al. (2010). This is consistent with the fact that our self-reported measure of displacement in this paper is a mixture of firm-closure and other layoff events.

We find that the majority of the long-run loss (80%) is accounted for by a reduction in post-displacement monthly pay. This is consistent with evidence from the US, and paints rather a different picture of the UK labour market to Hijzen et al. (2010), who use employer survey data linked to firm register data, and who find that the majority of the earnings loss is accounted for by lower employment rates rather than lower pay. It seems plausible that workers who are displaced are less likely to reappear in the employer survey data if low-paying employers are less likely to respond to the survey. This would explain why the earlier work finds lower post-displacement employment rates and higher post-displacement wage rates. In contrast, in the current study the response rates do not differ between those who are displaced and not displaced (see Figure 2, panel g).

In addition, in this paper we are able to match displaced workers to a control group on the basis of a detailed set of pre-displacement characteristics. We are also able to show that the majority of the earnings loss is caused by falls in wages rather than falls in hours of work. Our findings suggest that the consequences of displacement in the UK are closer to those found in the US literature than previously thought.

We find that income from other labour market states and from welfare payments

does little to compensate. Total income from other sources, including self-employment income, unemployment insurance, retirement income and invalidity benefit reduce losses by only 15% in the first 12 months after displacement, and by about 12% after 10 years. The relative unimportance of welfare payments provides an explanation as to why losses in the UK appear so similar to those found for the US, and contrast to losses from Nordic countries which are typically rather smaller.

The use of survey data has some limitations. First, our measure of displacement is self-reported rather than inferred from plant closings or mass-layoffs. It has been argued (e.g. Topel, 1990) that respondents are more likely to report more costly events (such as displacements which result in long spells of unemployment), especially after a long lag. However, we mitigate this by only using recall information from the previous calendar year. Recent evidence from the US from Couch and Placzek (2010, p.587) suggests that estimated earnings losses from survey data are actually quite similar to those from administrative data. Second, self-reported displacement might also mix up genuinely exogenous separations (such as those caused by plant closures or reductions in employment) with dismissals which may be related to individual shocks to performance. However, we have shown that the great majority of layoffs are reported as redundancies, and that excluding dismissals from the sample does not substantially reduce the estimated loss.

Our results show that the UK labour market is "flexible" in the sense that most displaced workers are able to re-enter employment relatively quickly after displacement, and in the long-term, unemployment rates are only slightly higher for displaced workers compared to the control group. This reflects the fact that government policy has made it increasingly unrewarding and difficult to claim unemployment benefits for an extended period (Manning, 2009). This flexibility, however, does not mean that displacement is painless. First, in the short term, welfare payments represent only a small fraction of lost earnings. Second, in the long term, displaced workers face substantial downgrading of their pay. This may be a combination of the fact that they find work in low-paying firms, and the fact that they lose specific human capital. One important future research question is then to investigate the extent to which a lack of regional and occupational mobility prevents the efficient re-allocation of workers from shrinking firms and sectors to

new jobs in growing firms and sectors. A second is to evaluate the effectiveness of specific government interventions intended to support displaced workers and communities which have experienced mass-layoff events.[27]

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[27]For example, the closure of the Redcar steelworks in October 2015 led to a £80m support package, approximately £25,000 per displaced worker. As of October 2016, no information on the effectiveness of this support was available (Tighe, 2016).

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Appendix A Using job history data to create detailed displacement information

The data are drawn from the British Household Panel Survey (BHPS) waves 1-18, 1991–2008. The 18 waves contain 32,379 individuals and 238,992 person-years. We keep members of the original sample and full interview outcomes, resulting in 29,264 individuals and 219,592 records.

The start date of the labour market spell in progress at the time of interview is asked to the day. However, missing values are quite common.[28] If the start day is unknown we assume the first day of the month. If the start month is unknown we assume January.[29] After making these changes there are just 5,134 records with a missing start date. These are records where the start year is unknown. In addition, a very small number of records (less than 0.05%) have a start or end date that is inconsistent with the date of interview; these dates are approximated by the interview date.

Information on the reason for the end of employment spells is available from a respondent's employment history data. If the spell in progress at the time of the interview starts after 1st September in the previous interview year then the employment history data contains recall information on all spells going back until a spell start date occurs before 1st September in the previous year. In total there are 67,125 labour market spells in the employment history data, of which 40,837 refer to spells of employment. Respondents are asked "which of the statements on the card best describes why you stopped doing that job?":

1. Promoted
2. Left for a better job
3. Made redundant
4. Dismissed/sacked
5. Temporary job ended

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[28]51% of records from the current labour market data have a missing start day, 14% have a missing start month and 1% have a missing start year. 56% of records from the employment history data have a missing start day, 11% have a missing start month and 3% have a missing start year.

[29]In some cases the start month is coded as "Spring", "Summer", "Autumn" or "Winter". We recode these as April, July, October and January respectively.

Reason for ending job	All emp. spells	. . . in progress at date of last interview	. . . with consistent status at last interview
Made redundant	4,349	2,717	2,523
Dismissed or sacked	886	419	361
Temporary job ended	3,947	1,291	1,061
Other reason	31,275	19,398	18,118
Unknown	380	157	121
Total	40,837	23,982	22,184

Table A.1: Number of displacements observed 1991–2009. Based on answers to the question 'tell me which of the statements on the card best describes why you stopped doing that job?"

6. Took retirement

7. Health reasons

8. Left to have a baby

9. Look after family

10. Look after another person

11. Other reason

The broadest definition of displacement includes those spells which are reported to end in (3) "made redundant", (4) "dismissed or sacked" or (5) "temporary job ended". Under this broad definition there are over 8,000 displacements (4,349+886+3,947), as shown in the first column of Table A.1. However, not all of these jobs were in progress at the time of the last interview. Restricting to those spells which were in progress at the time of the last interview reduces the number of displacements observed to just over 4,400 (2,717+419+1,291). Note that the majority of temporary jobs which ended were not in progress at the time of the last interview, which makes sense since these will tend to be shorter spells.

However, there are discrepancies between the information in the employment history data and the contemporaneous data. The earliest spell in the employment history data should be the spell which was in progress during the last interview. In the final column of Table A.1 we count only spells which were recorded as "in paid employment" at the time of the last interview. This results in a total of 3,945 (2,523+361+1,061) displacements of jobs which were in progress at the time of the previous interview date.

Year	Made redundant	Dismissed or sacked	Temp. job ended	Other reason	Spell not ended	Outcome not known	Total
1991	178	15	45	688	3,167	880	4,973
1992	159	25	56	720	2,892	623	4,475
1993	122	20	48	744	2,848	525	4,307
1994	123	16	63	806	2,869	561	4,438
1995	119	19	58	763	3,035	421	4,415
1996	103	19	56	914	3,185	418	4,695
1997	106	23	68	948	3,085	501	4,731
1998	115	7	67	965	3,058	519	4,731
1999	128	15	62	1,033	3,342	601	5,181
2000	165	28	65	1,279	4,018	832	6,387
2001	227	24	81	1,523	6,012	1,273	9,140
2002	187	14	77	1,447	5,385	1,109	8,219
2003	160	20	73	1,506	5,616	1,200	8,575
2004	147	30	60	1,365	5,042	888	7,532
2005	150	27	62	1,039	5,391	831	7,500
2006	130	23	63	1,115	5,012	817	7,160
2007	153	25	51	975	4,956	786	6,946
2008	7	1	3	45	209	6,539	6,804
Total	2,479	351	1,058	17,875	69,122	19,530	110,415

Table A.2: Number of displacements from employment spells in progress at interview date. The column labelled "not known" includes all spells for which no reason is given for ending a job. A small number of interviews took place in 2009, which is why a few displacements are observed in 2008.

We then attach the information on displacement to the previous interview, so that for each spell in progress at the time of interview we have information on how that spell ended (if it ended before the next interview). From the original sample of 219,592 interview records, 110,415 interviews occur during a spell of employment. For 19,530 of these spells we do not know whether or how it ended, either because there was no interview in the following year (15,650) or there is incomplete information from the employment history data (3,880). Table A.2 summarises the number of displacements. Stata code which constructs the data as described is available from the authors on request.

Appendix B DiD vs. FE vs. time-trends estimates

In this section we compare the difference between estimating a basic DiD model which controls for differences in group means, our preferred FE specification (1), a FE specification which allows for differences in trends, and our PSM estimate.

Column (1) reports the basic DiD model. The estimate of β is insignificantly different

	(1) DiD	(2) FE	(3) FE + group trends	(4) FE + indiv.trends	(5) PSM + DiD
β	−0.013				−0.013
	(0.023)				(0.023)
ω				−0.002	
				(0.005)	
3–5 years before	−0.009	−0.009	−0.002	−0.013	0.016
	(0.018)	(0.015)	(0.018)	(0.019)	(0.019)
1–3 years before	−0.025	−0.025	−0.015	−0.051*	−0.002
	(0.020)	(0.018)	(0.027)	(0.029)	(0.021)
< 1 year before	−0.038*	−0.037*	−0.024	−0.066*	−0.040*
	(0.020)	(0.020)	(0.034)	(0.039)	(0.022)
< 1 year after	−0.384***	−0.382***	−0.368***	−0.425***	−0.367***
	(0.027)	(0.027)	(0.042)	(0.052)	(0.028)
1–3 years after	−0.249***	−0.259***	−0.242***	−0.329***	−0.230***
	(0.027)	(0.025)	(0.048)	(0.061)	(0.027)
3–5 years after	−0.200***	−0.219***	−0.200***	−0.313***	−0.179***
	(0.032)	(0.030)	(0.059)	(0.075)	(0.032)
5–7 years after	−0.215***	−0.224***	−0.201***	−0.329***	−0.172***
	(0.035)	(0.031)	(0.068)	(0.088)	(0.036)
7–10 years after	−0.197***	−0.169***	−0.143*	−0.279**	−0.120**
	(0.049)	(0.042)	(0.080)	(0.114)	(0.051)
Number of obs.	674,022	674,022	674,022	599,059	155,280
Number of indiv.	9,648	9,648	9,648	9,467	6,035

Table B.1: Comparison of DiD, FE, FE with time-trend and PSM estimates of the cost of displacement on individual monthly earnings. Table reports estimates of δ^r from variants of Equation (1), expressed as a proportion of the dependent variable of the treatment group at $r = 0$. β is the coefficient on a treatment dummy in a model without individual fixed effects. ω is the coefficient on a treatment dummy interacted with a linear time trend. Both displaced and non-displaced groups are selected from those aged 16–60, who are in employment in wave c.

from zero, demonstrating that in the base period (more than five years before displacement) there is no significant difference in monthly earnings. There is a small decrease in earnings in the pre-displacement period of up to 3.8%. Short-term losses are 38% (< 1 year after) and long-term losses (7–10 years after) are 20%.

Column (2) reports our base model (1), which yields very similar results but with slightly smaller long-term losses. The difference between (1) and (2) occurs because our sample is an unbalanced panel: each cohort has a different number of pre- and post-displacement observations.

Column (3) reports a variant of (1) which also allows for a pre-displacement group trend difference (i.e. a time-trend interacted with the displacement dummy). The estimate of ω suggests that the displaced had very slightly lower pre-displacement earnings growth (note that this is earnings growth in the period $r < -4$) but this effect is not significantly different from zero. Because the difference in pre-displacement earnings growth is small,

this model yields similar estimates to the base model. In column (4) we report estimates of a model which allows for an individual-specific time-trend. This model produces significantly larger estimates of earnings losses, because the pre-displacement earnings growth of the displaced group is estimated to be faster, on average, than the control group. However, we treat these estimates with some caution because estimates of ω_{ic} are based on relatively few time periods and lead to a loss of precision of all the estimates – note that the standard errors in column (4) are typically twice as large as the standard errors in column (2).

Finally in column (5) we report our PSM estimate. As noted in the main text, the use of PSM effectively removes a large number of observations from the control group which are not observably similar to those in the control group. Doing this reduces the long-term estimate somewhat, but has little difference on the estimated losses immediately after displacement.

Appendix C   Matching results

	N	Mean	S.D.	Min	Max
No. of waves since first appearance	52,673	7.39	4.35	1	17
No. of time displaced before	52,673	0.14	0.41	0	6
No. of times in self-employment before	52,673	0.13	0.74	0	16
No. of times unemployed before	52,673	0.16	0.59	0	12
No. of times in other labour market state before	52,673	0.64	1.56	0	15
No. of times interviewed before	52,673	6.19	4.35	0	16
Current tenure in firm	52,673	5.29	6.16	0	50
Current firm employs < 25 workers	52,673	0.33	0.47	0	1
Current firm in manufacturing sector	52,673	0.17	0.38	0	1
Current job in manual occupation	52,673	0.38	0.49	0	1
Current workplace has union representation	52,673	0.52	0.50	0	1
Current firm is in private sector	52,673	0.65	0.48	0	1
Current job is full-time $\geq$ 30 hours per week)	52,673	0.80	0.40	0	1
White ethnic group	52,673	0.94	0.24	0	1
Born in UK	52,673	0.95	0.21	0	1
Lives in South East	52,673	0.19	0.39	0	1
Female	52,673	0.52	0.50	0	1
Age	52,673	39.71	10.65	20	60
Age^2	52,673	1690.16	855.54	400	3600
Has higher education	52,673	0.50	0.50	0	1
Married	52,673	0.76	0.43	0	1

Table C.1: Descriptive statistics of variables used in the estimation of the propensity score. All displacement cohorts together, calculated at $r = 0$ i.e. the interview immediately before displacement.

In Table C.1 we report descriptive statistics of the variables used in the propensity score matching. In Table C.2 we report a set of balancing statistics comparing the displaced and non-displaced samples before and after propensity score matching. Each displacement cohort is matched separately, ensuring that a displaced worker is only matched with a non-displaced worker from the same cohort. Columns (3) and (4) report the sample sizes before and after matching, which show that almost all displaced workers appear in the matched samples. Columns (5) and (6) report the number of t-tests of the difference in means between the control and treatment groups which are significantly different from zero at the 95% and 90% significance levels. Column (7) reports the median bias of all covariates before and after matching. Columns (8) and (9) report Rubin's B and Rubin's R statistics.

In Figure C.1 we show how matching successfully removes pre-existing differences in pay and employment status between the displaced and non-displaced samples. Panels (a) and (c) show that the displaced sample is less likely to be in employment and more likely

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Cohort		N (non-displaced)	N (displaced)	Balancing t-tests 5%	Balancing t-tests 10%	Median bias	B	R
6	Unmatched	2,909	89	3/21	6/21	7.982	66.396	0.773
6	Matched	753	89	0/21	0/21	2.005	17.059	1.064
7	Unmatched	2,922	87	8/21	8/21	10.423	79.289	0.992
7	Matched	681	86	0/21	0/21	1.874	16.601	0.831
8	Unmatched	2,851	80	7/21	7/21	12.972	91.205	0.576
8	Matched	609	80	0/21	0/21	2.551	16.340	0.775
9	Unmatched	4,046	139	8/21	9/21	12.509	84.490	0.741
9	Matched	1,053	139	0/21	1/21	2.657	17.833	0.708
10	Unmatched	4,495	130	10/21	12/21	17.491	98.912	0.855
10	Matched	964	127	0/21	0/21	1.739	15.512	0.861
11	Unmatched	5,283	138	8/21	11/21	14.578	86.876	0.643
11	Matched	1,063	137	1/21	1/21	2.550	17.239	1.435
12	Unmatched	5,190	154	10/21	10/21	12.443	103.774	0.508
12	Matched	1,139	152	0/21	0/21	1.663	15.486	1.088
13	Unmatched	4,908	120	9/21	10/21	14.183	91.622	0.579
13	Matched	985	119	0/21	0/21	3.023	16.542	0.757
14	Unmatched	4,883	130	11/21	13/21	17.578	103.771	0.592
14	Matched	986	128	0/21	0/21	1.787	14.249	0.915
15	Unmatched	4,760	120	9/21	11/21	16.075	78.773	1.004
15	Matched	1,004	119	0/21	0/21	1.064	9.650	0.959
16	Unmatched	4,574	119	7/21	8/21	8.017	82.487	0.636
16	Matched	947	119	0/21	0/21	2.499	18.099	0.784
17	Unmatched	4,428	118	10/21	10/21	14.206	74.822	0.837
17	Matched	941	118	0/21	0/21	1.675	12.774	0.956

Table C.2: Samples sizes and outcomes of matching. Table reports number of observations at $r = 0$ (immediately before displacement) before and after matching. All statistics calculated using psmatch2 provided by Leuven and Sianesi (2014).

to be unemployed before displacement, but these differences are removed in the matched samples in panels (b) and (d). Similarly, panels (e) and (g) show that some indication that pay growth is lower in the displaced sample. But panels (f) and (h) show that after matching there is no difference either in the level or trend of pay before displacement.

Appendix D   Alternative control groups

Starting with our baseline sample of 2,499 displaced workers (row 1), we first restrict the sample to those displaced between 1996 and 2007 (row 2) to allow for sufficient pre-displacement information. JLS restrict the sample to those who are aged 21–50 at the time of displacement (row 3). They consider only workers in firms employing at least 50

(a) Employment (unmatched)

(b) Employment (matched)

(c) Unemployment (unmatched)

(d) Unemployment (matched)

(e) Pay last month (unmatched)

(f) Pay last month (matched)

(g) Pay last month, emp. spells only (unmatched)
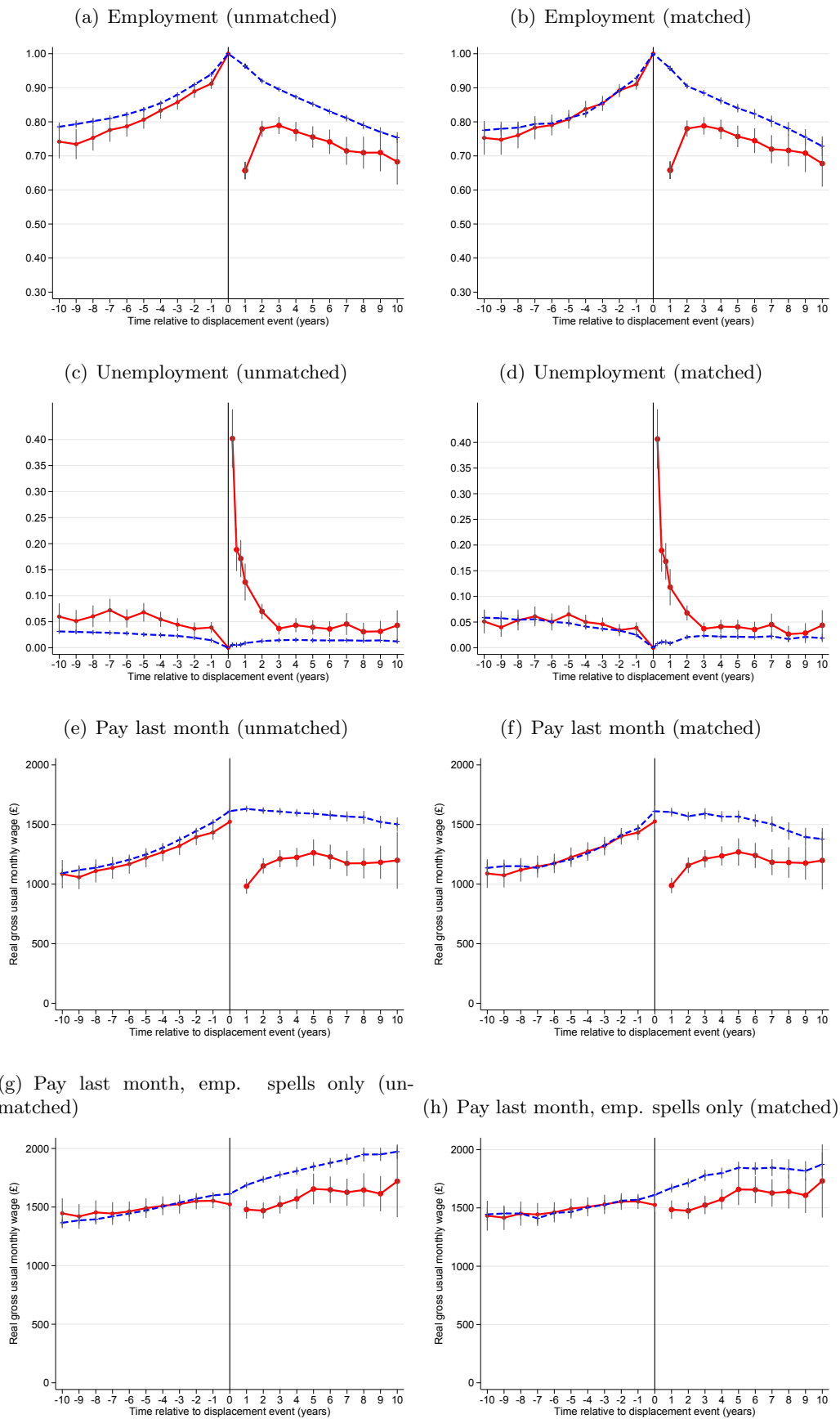
(h) Pay last month, emp. spells only (matched)

Figure C.1: Comparison of employment status and pay before and after matching.

Sample	Treatment	Control	Prop. of sample
(1) In employment at $r = 0$ aged ≥ 20 and ≤ 60	2,499	78,823	
(2) In cohort $1996 \ldots 2007$	1,788	61,783	
(3) Aged ≥ 21 and ≤ 50 at $r = 0$	1,352	48,381	78%
(4) Working in a firm employing at least 50 people at $r = 0$	646	25,896	42%
(5) At least six years of tenure at $r = 0$	126	6,154	10%
(6) Have positive earnings in each calendar year after displacement	88	4,802	7%
(7) Remain with same employer from $r = 0$ onwards	88	3,164	5%

Table D.1: Sampling restrictions made by JLS. See in particular Jacobson et al. (1993, Appendix C, p.708).

people (row 4).[30] They also consider only "high tenure" separators who have at least six years of tenure (row 5), which leaves us with just 10% of the original sample. They also consider only workers who have positive wage or salary earnings in each calendar year (row 6). Finally, the control group used by JLS consists only of those who remain with the same employer (row 7). It is clear from Table D.1 that these sample restrictions remove the great majority of displaced workers from the sample.

	(1) Full sample	(2) Age 21–50 at $r = 0$	(3) Firm employs ≥ 50 at $r = 0$	(4) Tenure ≥ 6 at $r = 0$	(5) +ve earnings in each year $r > 0$	(6) Control group remains with same firm
3–5 years before	0.016	0.039*	−0.003	0.055*	0.015	0.030
	(0.019)	(0.022)	(0.027)	(0.029)	(0.024)	(0.022)
1–3 years before	−0.002	0.034	0.023	0.056*	0.006	0.001
	(0.021)	(0.024)	(0.031)	(0.032)	(0.024)	(0.024)
< 1 year before	−0.040*	−0.006	−0.032	0.018	−0.016	−0.039
	(0.022)	(0.024)	(0.032)	(0.034)	(0.024)	(0.026)
< 1 year after	−0.367***	−0.299***	−0.455***	−0.444***	−0.314***	−0.399***
	(0.028)	(0.031)	(0.045)	(0.062)	(0.034)	(0.032)
1–3 years after	−0.230***	−0.172***	−0.294***	−0.285***	−0.184***	−0.322***
	(0.027)	(0.029)	(0.042)	(0.052)	(0.032)	(0.032)
3–5 years after	−0.179***	−0.116***	−0.227***	−0.191***	−0.132***	−0.337***
	(0.032)	(0.037)	(0.048)	(0.067)	(0.040)	(0.040)
5–7 years after	−0.172***	−0.115***	−0.211***	−0.153*	−0.108**	−0.382***
	(0.036)	(0.042)	(0.056)	(0.079)	(0.044)	(0.047)
7–10 years after	−0.120**	−0.038	−0.128	−0.075	−0.061	−0.374***
	(0.051)	(0.057)	(0.080)	(0.113)	(0.062)	(0.067)
No. of obs.	155,280	114,778	74,433	37,636	102,566	105,704
No. of indiv.	6,035	4,757	3,343	1,571	3,569	3,556

Table D.2: Comparison of different control groups.

Because of the small resulting sample size, it is not practical to estimate the costs of displacement while imposing all these sample restrictions simultaneously. Instead, in Table D.2 we report the effect of each sample restriction in turn. Column (1) repeats our

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[30]They make this restriction to ensure that their definition of "mass-layoff" is meaningful.

baseline estimate. Column (2) shows that losses for the younger sample (which excludes most of those who retire) are smaller, and in particular the final row suggests that this younger sample experience a stronger recovery in earnings. Note that this is not due to a difference in retirement behaviour, since we have established that retirement differences between displaced and non-displaced workers have disappeared after 10 years (see panel (d) of Figure 2).

Column (3) shows that workers displaced from large firms experience larger falls in earnings initially (-45% as opposed to -37% in the baseline sample), but this difference is largely eliminated after 10 years. Column (4) shows a similar picture for high-tenure workers: larger initial falls, but the difference disappears after 10 years. Column (5) shows that the earnings restriction reduces estimated earnings losses, which is unsurprising since some of the earnings loss is a result of periods of non-employment. The most significant sampling decision is shown in Column (6). If the control group is restricted to those who remain in the sample employer, the recovery in earnings is almost completely eliminated, because the control group's earnings remain much higher.