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Don't Look Down: New Evidence on Job Loss in a Flexible Labour Market

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Abstract

We estimate the earnings, hours and income effects of job loss (displacement) for a representative sample of UK workers from 1991--2007. We are able to follow workers before and after displacement regardless of their labour market state, and we are able to precisely match displaced workers with observably similar non-displaced workers. We show that job loss is associated with a long-run reduction in income which is mainly due to reductions in monthly pay conditional on employment rather than in employment propensity. Entry into unemployment is very short-lived, and while there is some entry into other labour market states, this does little to compensate for income losses. Total income from welfare payments, including unemployment insurance, retirement income and invalidity benefit reduces losses by only 15% in the first 12 months after job loss, and become even less important as time passes. The lack of a "safety net" means that job loss in the UK has a similar effect to job loss in the US.

Outline

1 Introduction

A large and growing literature examines the impact of job loss on workers. This literature typically finds that job loss has very large short-term consequences on employment and earnings, and that earnings losses persist for many years after job loss. 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). The literature has also expanded to consider the costs of job loss on other outcomes including health and mortality, family dissolution, fertility and the educational success of the children of displaced workers.

Since JLS, it has become increasingly common to rely on administrative data, such as social security earnings records, to estimate the cost of job loss. These data offer several advantages. They provide an externally validated measure of job loss based on firm closure or mass-layoff, accurate measures of earnings and typically very 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 typically contain little demographic information which would allow the construction of suitable counterfactuals. 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 also remains the case that very little is known about the effects of job loss 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 job loss 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.[1] 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,[2] and also offers very low state benefits for unemployed workers.[3] 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 provide the first comprehensive evidence of the costs of job loss in the UK. 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 that they may enter after displacement. This means that we do not need to exclude individuals

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

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

[3] 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.

from the analysis who subsequently have zero earnings from employment.[4] 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.[5]

We show that job loss in the UK causes a long-run reduction in income of approximately 10%, and that the majority of this loss (80%) is accounted for by a reduction in post-displacement earnings. Further, we show that the fall in earnings is predominantly a fall in wages rather than hours of work. These results contradict previous findings for the UK which relied on employer survey data linked to social security records, and suggests that the consequences of job loss in the UK is very similar to that in the US. There is a long-run reduction in the probability of employment, because displaced workers enter a variety of other labour market states, namely self-employment, sickness or disability and early retirement. However, income from these sources does little to compensate for the income losses following job loss. 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 job loss, 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 loss. 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 job loss on earnings and non-labour income as well as hours of work. Section 7 concludes and discusses our findings in the context of evidence for the US and other European countries.

2   Literature review

This paper relates to three main areas of the literature that examine the impact of job loss. The first of these is the long run impact on employment and earnings. The seminal article in this literature is that of JLS, who use administrative data for Pennsylvania between 1980 and 1986 to examine the earnings losses of high seniority men displaced due to mass layoffs. 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

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

[5]Our use of publicly available survey data also offers advantages in terms of transparency and replication. The data are available for download and our results can be readily replicated using standard software.

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 job loss among manufacturing workers in a heavily industrialised state. To demonstrate this, Couch and Placzek (2010) 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.[6] These are in the range of earlier estimates. They are also remarkably consistent with the analysis of Morissette et al. (2013) for Canada.[7] 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 layoff 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.[8]

The extant literature finds that losses vary by age, tenure and sector. Couch and Placzek (2010) emphasise the potential importance of the use of matching estimators to control for systematic selection in those that are displaced, although they find only weak evidence for an overstatement of the estimated impact of job loss without matching, perhaps because they have a limited number of demographic variables available for matching.

The second related area in the literature concerns the impact of welfare payments on the earnings losses of the displaced and whether this can lead to systematic differences between countries.[9] 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.[10]

Schmieder et al. (2010) use administrative data to examine mass layoffs in West Germany in 1982. Their estimates of earnings losses are remarkably similar to those cited above for the US. 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

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[6]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.

[7]Other authors have also emphasised the role of the business cycle in determining the size of the loss e.g. Davis and von Wachter (2011).

[8]Borland et al. (2002) is the only other estimate of the cost of job loss for workers in the UK. It is of interest here because it uses the same survey data as in this paper although over a shorter period(1991–96). However, it is difficult to compare Borland et al.'s results with those of the displacement literature reviewed as they focus only on individuals who return to employment before the end of the sample period, and do not adopt a difference-in-differences methodology. Hence, earnings losses are noticeably smaller.

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

[10]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.

although they are examining job loss 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. Turning to the impact of the social security system, they show that, 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. The methodology makes it difficult to make direct comparisons with the papers previously discussed because the sample includes both voluntary and involuntary job separations. Nevertheless, Ehlert does find similar post-displacement trajectories for displaced workers in both countries.[11] 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 establishment closures, and the potentially mitigating impact of the social insurance using longitudinal data for Sweden. Eliason finds significant and long-lasting impacts of displacement on the earnings of married males,[12] but somewhat smaller than that found for the US. Average annual losses are approximately 6% 4–12 years after establishment 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.[13] Norway has a particularly generous welfare system in comparison to most countries and 15–20% of short term losses are compensated for 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 job loss leads to entry into other labour market states, such as self-employment or early retirement. For example, Farber

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[11]Comparisons with other studies are also hindered because Ehlert considers household income of married couples rather than individual income.

[12]No effect is found for married women.

[13]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.

(1999) uses the Displaced Workers Survey (DWS) for 1994 and 1996 and examines the status of individuals a year later. He finds high entry rates of displaced 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 job loss. 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 capita 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.[14]

Turning to the impact on retirement, Chan and Stevens (1999, 2001) find for the US that men are displaced this leads them to 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. This concurs with the earlier findings of Rege et al. (2009) that if firms lost more than 60% of their employment then the displaced workers were 24% more likely to claim disability allowances than otherwise.

In this paper we provide the first comprehensive evidence of the costs of job loss 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 job-losers with a comparable control group thanks to detailed pre-displacement characteristics.

3 The data

The British Household Panel Survey (BHPS) is an annual survey of about 5,500 households recruited in 1991, containing a total of 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

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

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 (Institute for Social and Economic Research, 2011).[15] A detailed description of the BHPS data can be found in Taylor et al. (2010).

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 then keep all members of the original sample and full interview outcomes, which results in a sample containing 29,264 individuals and 219,592 person-years.

The basic structure of the data is an approximately annual panel. For each individual i we observe a sequence of interviews from waves $t = 1991, \ldots, 2009$.[16] 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 reason why they stopped doing that job.

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.[17] In our basic specification, job loss is defined as occurring if an employment spell ends due to "redundancy" or "dismissal" (the full list is given on Page 31 in Appendix A.) As noted by Borland et al. (2002), the distinction between redundancy and dismissal in the UK is 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 one-fifth are classified by the respondents as "job loss" (redundancy, dismissal or temporary jobs ending). Note that the measurement of job loss used here will tend to understate job loss for short spells of employment: a spell which starts after the interview in wave t and ends before the interview in wave $t + 1$ will not be recorded.

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[15]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.

[16]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 ($t + 1$).

[17]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 job loss	Prop. emp. spells ending for 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. Job loss in this table is defined to include redundancy, dismissal or the end of a fixed-term contract.

The prevalence of job loss reported in the BHPS is almost identical to the Office for National Statistics redundancy series calculated from the Labour Force Survey, shown in Figure 1.[18] 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, $c = 1996,\ldots,2007$ which take the value 1 if individual i experiences job loss between the wave $t = c$ and the wave $t = c+1$ interview, and $D_i^c = 0$ if they do not experience job loss in that time period. Those with $D_i^c = 0$ will therefore include individuals who change job between c and $c+1$ for reasons other than displacement.[19] D_i^c is constant for each individual for a given value of c, but each individual has

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[18]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.

[19]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*.
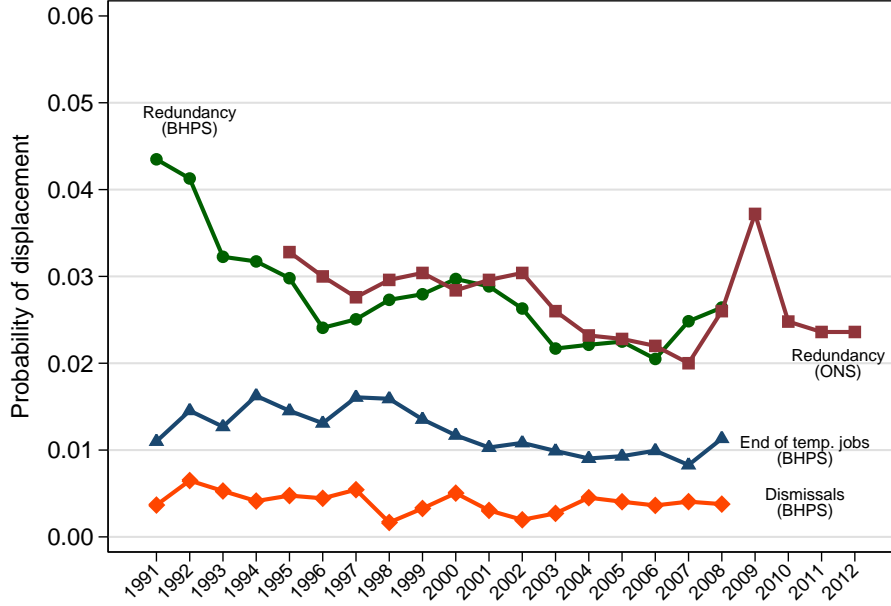
Figure 1: Estimates of probability of job loss from BHPS compared to Office for National Statistics redundancy series (Heap, 2004). The ONS series is calculated from the UK quarterly Labour Force Survey.

a separate indicator for 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 will almost certainly include those who will be 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) and various measures of income (e.g. income from employment, self-employment, welfare payments). y_{it} is measured in each wave both before $t <= c$ 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, separately for each displacement cohort, a standard difference-in-difference model:

$$y_{it} = \alpha^c + \beta^c D_i^c + \sum_{s=1992}^{2008} \gamma^{c,s} T_t^s + \sum_{s=c-4}^{2008} \delta^{c,s}(T_t^s D_i^c) + \varepsilon_{it}^c, \ \ c = 1996,\dots,2007. \quad (1)$$

The dummy variable T_t^s takes the value 1 if $s = t$ and zero otherwise. The coefficient $\delta^{c,s}$ is

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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.

a difference-in-difference estimate of the effect of a displacement which occurred between c and $c+1$ on the outcome y_{it} in year s. For example, $\delta^{1996,2000}$ is an estimate of the effect of a displacement which occurred between the 1996 and 1997 interviews on outcomes in 2000. $\delta^{c,s}$ is estimated for five years before displacement to allow for the possibility that displacement has effects before the job-loss actually occurs. The coefficient β^c captures the pre-existing difference in y_{it} between the treatment and control groups more than five years before displacement.

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.[20] To do this we stack all the waves together and estimate the outcome for time relative to the displacement date. 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 for example $r_{ict} = 0$ in the year immediately preceding the displacement and $r_{ict} = 1$ in the year immediately after. We restrict attention to $-10 \le r_{ict} \le 10$ to ensure sufficient numbers of treated and control observations in each year.

Our pooled difference-in-difference model is then

$$y_{ict} = \alpha + \beta D_{ic} + \sum_{r=-9}^{10} \gamma^r T_t^r + \sum_{r=-4}^{10} \delta^r (T_t^r D_{ic}) + \eta_c + \varepsilon_{it}. \tag{2}$$

We now include cohort fixed-effects η_c and allow the errors ε_{it} to be clustered by i across cohorts. The difference-in-difference estimate δ^r controls for the average difference in y_{it} between the treatment and control groups in the base years, which are all years more than five years before displacement.[21]

A number of refinements of (2) are possible. First, we can replace the α with person-cohort fixed effects α_{ic} and estimate using differences or mean deviations. This removes the treatment indicator D_{ic} and its parameter β. Note that if the panel is balanced then β in (2) completely captures the pre-displacement difference in α_{ic} between the treatment and control

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

groups, and the FE and DiD estimators are identical.

$$y_{ict} = \alpha_{ic} + \sum_{r=-9}^{10} \gamma^r T_t^r + \sum_{r=-4}^{10} \delta^r (T_t^r D_{ic}) + \varepsilon_{it}. \tag{3}$$

Second, we can allow for differences in pre-existing earnings trends between the treatment and control groups. This leads to the estimating equation

$$y_{ict} = \alpha_{ic} + \omega_{ic} t + \sum_{r=-9}^{10} \gamma^r T_t^r + \sum_{r=-4}^{10} \delta^r (T_t^r D_{ic}) + \varepsilon_{it}. \tag{4}$$

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).

Third, one can additionally control for differences in observable characteristics between the treatment and control groups during the pre-displacement period. In this paper 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 $\mathbf{x}_i$. $p(\mathbf{x}_i)$ can be estimated using a Logit model. A matched sample consists of displaced and non-displaced individuals who have similar values of $\widehat{p(\mathbf{x}_i)}$. Once a suitably matched sample is obtained, 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 or fixed-effect models such as (2) or (3) 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 job loss, in this section we illustrate 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).[22] 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 job loss.

Relative time	Control group	Treatment group
>10 years before	61,728	1,574
5-10 years before	153,131	4,401
4-5 years before	49,081	1,432
3-4 years before	55,651	1,649
2-3 years before	62,388	1,857
1-2 years before	70,152	2,143
< 12 months before	78,823	2,499
< 12 months after	78,894	2,477
1-2 years after	73,533	2,344
2-3 years after	68,170	2,215
3-4 years after	62,502	2,069
4-5 years after	56,825	1,910
5-10 years after	195,051	7,010
>10 years after	96,641	4,051

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

In Table 3 we compare the characteristics of the treatment and control groups in the 12 months immediately before the displacement, and five years before. Because the sample is restricted to include only those in employment in the interview preceding displacement there is no difference in the pattern of labour market status in the first column. Displaced workers have previously been displaced more frequently, and earn about 6% less than non-displaced workers. Displaced workers also 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 with a partner.

Most of these differences between the control and treatment groups are also visible 5–6 years before displacement occurs (right hand column of Table 3). In addition, we can see that

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[22]Recall that individuals may appear multiple times in different cohorts.

	< 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		0.77	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	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]
Real monthly pay, last payment	1481.00	1568.96	[0.001]	1462.59	1424.04	[0.305]
Real monthly earnings	1478.40	1553.25	[0.002]	1241.05	1249.51	[0.805]
Real monthly total income	1562.71	1655.43	[0.000]	1342.36	1351.39	[0.792]
Tenure (years)	4.26	5.10	[0.000]	4.14	4.55	[0.023]
Firm employs < 25 workers	0.38	0.33	[0.000]	0.29	0.32	[0.078]
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	[0.006]
Union member	0.35	0.51	[0.000]	0.39	0.52	[0.000]
Private sector	0.88	0.65	[0.000]	0.86	0.65	[0.000]
Works > 30 hours per week	0.85	0.80	[0.000]	0.86	0.80	[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]
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. "Real monthly pay" refers to pay from employment, while "Real monthly earnings" also includes any self-employment earnings. 'Degree' includes university degree, teaching qualifications and any other technical, professional or higher qualifications.

displaced workers are much more likely to be unemployed (7% compared to 3%). However, differences in pay, earnings (which include self-employment earnings) and total income 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 job loss. 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 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 $r > 0$. Our counterfactual is intended to capture what would have happened to displaced workers had they not been displaced, rather than what would have happened to them had they remained in employment with the same employer. 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.

The great advantage of the survey data is that we can also calculate what happens to individuals who are not in employment. 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.[23] 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 in self-employment, 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.[24] Panel (e) shows that displacement is also associated with higher rates of self-reported sickness, although small

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[23]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.

[24]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.
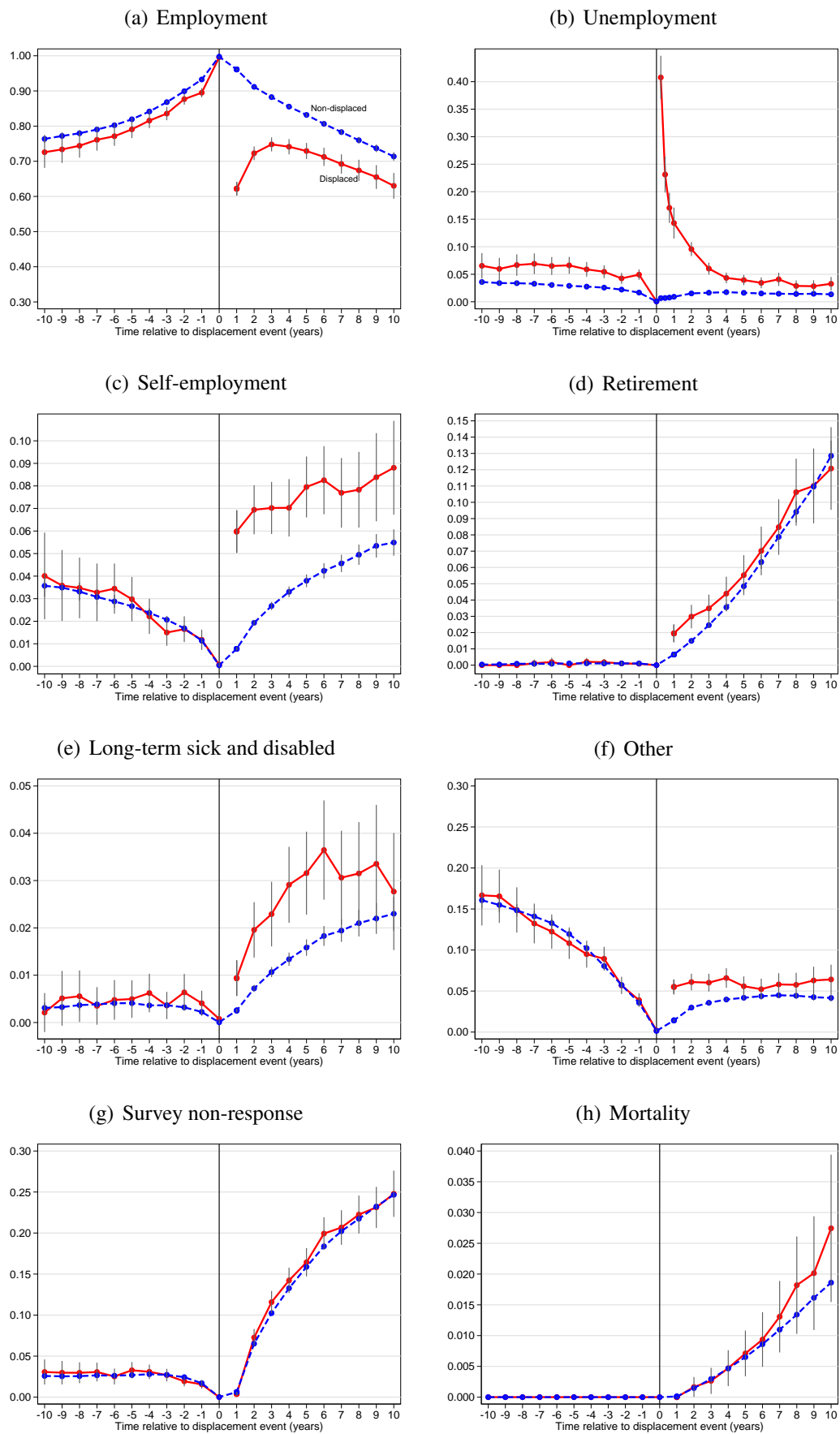
Figure 2: Probability of different self-reported labour market states before and after displacement 1991–2007. 95% confidence intervals around the mean based on clustered standard errors.

14

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.

To summarise, Figure 2 shows that, 10 years after job loss, 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 job loss. The patterns of employment shown in Figure 2 show that alternative sources of income from unemployment benefit, self-employment and retirement may reduce the costs of job loss. 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,[25] 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

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[25] As in Hijzen et al. (2010).

(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
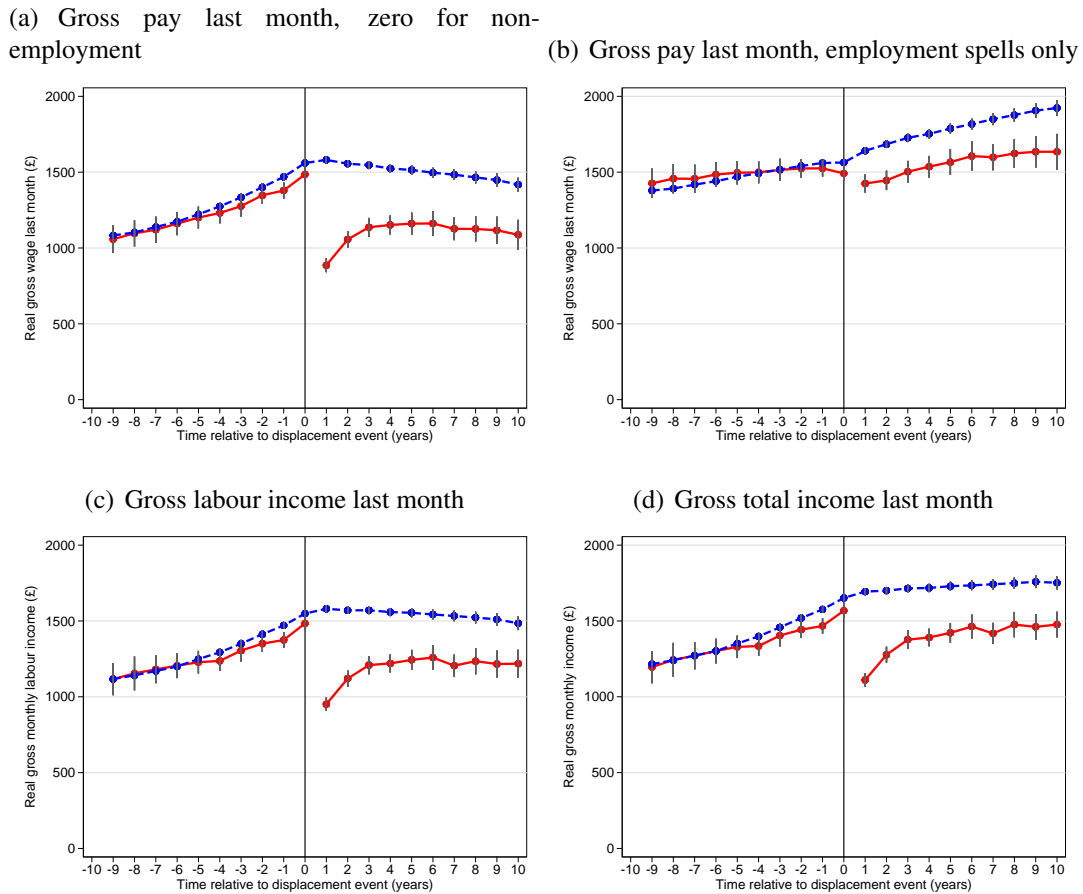
Figure 3: Various measures of individual gross income by treatment and control groups. Relative time is grouped into annual intervals. In the first panel, pay is zero for all non-employment spells. The second panel uses only spells of employment. 95% confidence intervals around the mean based on clustered standard errors.

self-employment which occur after displacement.[26] 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 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

These raw patterns are only suggestive. 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

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[26]This also includes any earnings from additional jobs.

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-displacement information. From (2), 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 $-4 \leq r \leq 0$.

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 (3), which controls for individual fixed-effects. In Appendix B we compare estimates from difference-in-differences, fixed-effects, fixed-effects with group trends and fixed-effects with individual trends. The base model results are reported in Table 4. Because we may observe zero pay, we estimate (3) in unlogged form and express the resulting coefficient estimates as a proportion of the mean pay of displaced workers at $r = 0$.

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, 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 replicated 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

	(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 job loss on individual pay and income. Table reports estimates of δ^r from Equation (3) 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.

(b) of Figure 2 that unemployment is typically a short-run experience, and as a result benefit effects are small and short-lived. The only significant effect is a 1% increase in the first year after displacement. Finally 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 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 total hours and "normal" hours (i.e. without overtime). 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 job-losers who have zero hours of employment. 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 job-losers are more likely to be re-employed in part-time work (Farber, 1999). It may also be because these jobs offer

	(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 job loss on individual hours. Table reports estimates of δ^r from Equation (3) 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.

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 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.

	< 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]
Real monthly wage, last payment	1525.66	1611.44	[0.016]	1225.67	1207.73	[0.663]
Real monthly earnings	1528.57	1582.94	[0.077]	1247.88	1239.28	[0.845]
Real monthly total income	1626.55	1680.49	[0.092]	1351.91	1333.95	[0.682]
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]
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.

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 therefore 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%), and in the number of times displaced before, but in fact in this case it 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.[27]
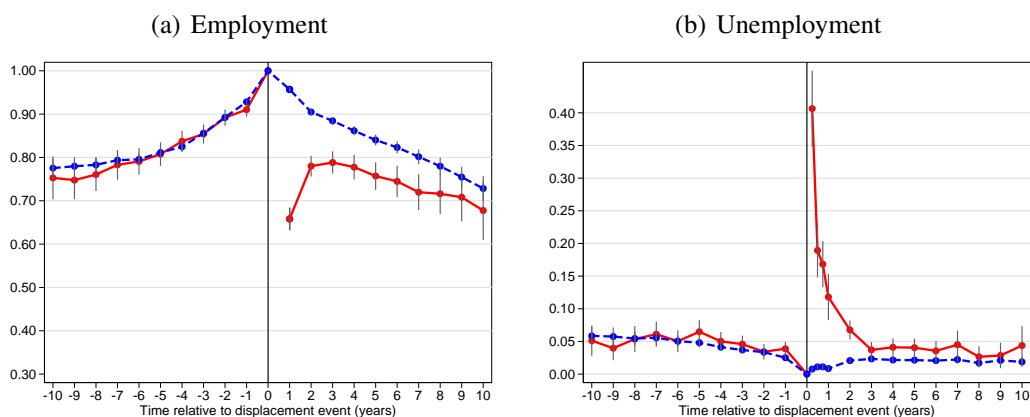
Figure 4: Probability of different self-reported labour market states before and after displacement 1991–2007, matched samples. 95% confidence intervals around the mean based on clustered standard errors.

After matching, we estimate (2), and the results are reported in Table 7. Comparing the unmatched and matched results from column (1), 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 2 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

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[27]A more detailed comparison of employment status and pay before and after matching is shown in Appendix C.

years after job loss is about 10%, compared to a 12% loss in column (1).

	(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 job loss on individual earnings and income. Table reports estimates of δ^r from (2).

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 Layoffs vs. dismissals

The definition of job loss we have used up to this point includes all those whose job spell ended due to "redundancy" or "dismissal". We argued in in Section 3 that the boundary between these two categories may be indistinct. However, Doiron and Mendolia (2011) argue that dismissals are more likely to provide information about worker quality, and that a redundancy signifies that the job loss was not as a consequence of individual workers' behaviour.[28] We therefore also provide a comparison between redundancies and dismissals. Of the 2,499 displacements, the vast majority (2,225) are "made redundant" and only 264 are

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[28]However, as noted by Gibbons and Katz (1991), redundancies which are not caused by plant closure allow for some discretion in terms of who gets laid off, leading to the distinction between "layoffs" and "lemons".

	(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 job loss on individual hours. Table reports estimates of δ^r from (2).

"dismissed/sacked". We perform propensity score matching separately on each sub-group and estimate (2) 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). However, although the estimates are very different, the very small number of dismissals means that the results would not change greatly if they are excluded.

6.4 Losses by cohort

The pooled models (2), (3) and (4) 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,

	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.

since later cohorts are followed for a shorter period of time. Davis and von Wachter (2011) show that the cost of job loss 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 (3) for four displacement cohorts. To maintain reasonable sample size, each cohort comprises three years: 1996–1998, 1999–2001, 2002–2005 and 2006–2008.
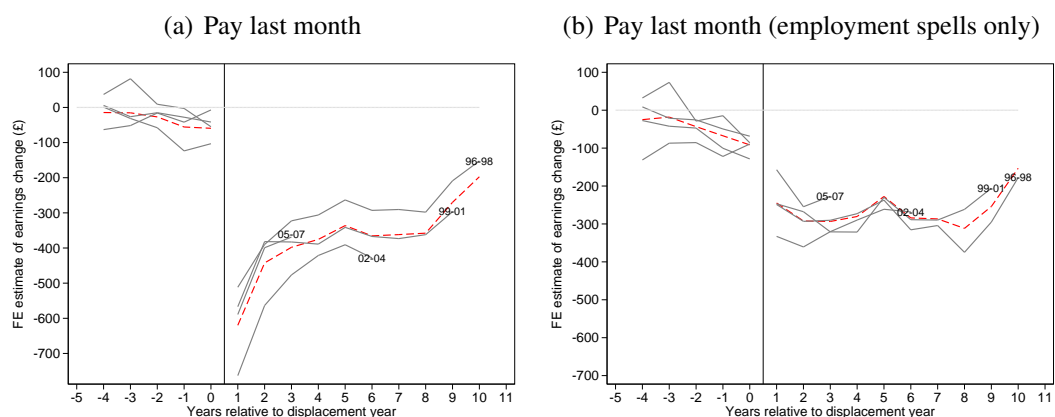

(a) Pay last month   (b) Pay last month (employment spells only)

Figure 5: FE estimates by cohort (each cohort contains three years). Dotted line indicates average across all cohorts.

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 result of the fact that business cycle fluctuations were small over the sample period (which predates the "Great Recession" of 2009–2010), 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 job-loss 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 job loss. 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. Essentially, choosing the control group in this way changes the counterfactual in an uneccessarily restrictive way.

7   Conclusion

In this paper we provide the first comprehensive evidence of the costs of job loss 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 job-losers 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.

Losses in earnings in the short-run are slightly less than 40%, after 10 years losses are approximately 10%. The majority of the long-run loss (80%) is accounted for by a reduction in post-displacement monthly pay. This paints rather a different picture of the UK labour market to Hijzen et al. (2010), who use administrative data, and who find that the majority of the earnings loss is accounted for by lower employment rates rather than lower pay. We suspect that this is due to the fact that the administrative data does not follow individuals through different labour market states, and does not reliably track individuals who regain

work in low-paying firms. In addition, the lack of individual characteristics limits the extent to which the displaced workers can be precisely matched to a control group. Our findings suggest that the consequences of job loss in the UK are closer to those found in the US literature than previously thought. 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.

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 job loss, 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.[29]

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 job losses 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 the actions of the individual. However, we have shown that the great majority of layoffs are reported as redundancies, and that the wage loss from redundancies is identifiably distinct from dismissals.

The availability of data which tracks workers across all labour market states and which identifies firms would allow us to investigate the the source of the wage loss. Are displaced workers re-hired into lower-paying positions, as would be suggested by a job ladder model (Burdett and Mortensen, 1998), or are displaced workers sorted into lower paying firms, as suggested by the empirical literature on the importance of firm-effects in wage determination (Abowd et al., 1999)? However, such data are not currently available to researchers in the UK.

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[29]Although, recent evidence from Germany (Schmieder et al., 2010) suggests patterns of losses which are very similar to those reported here.

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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 then keep members of the original sample and full interview outcomes, resulting in 29,264 individuals and 219,592 records.

Information on the reason for the end of employment spells is available from the 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 theory, this first spell in the job history data should correspond to the spell in progress at the time of the previous interview.

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.[30] If the start day is unknown we assume the first day of the month. If the start month is unknown we assume January.[31] 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.

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

6. Took retirement

7. Health reasons

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[30]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.

[31]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?"

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. As noted, 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.

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.

31

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.

Appendix B   DiD vs. FE vs time-trends estimates

In this section we compare the difference between estimating (2), (3) and (4). Column (1) reports the basic DiD model (2). The estimate of β is insignificantly different 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 (3), which yields very similar results but with slightly smaller long-term losses. Column (3) reports a variant of (3) 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. Finally in column (4) we report estimates of (4), 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,

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

Table B.1: Comparison of DiD, FE and FE with time-trend estimates of the cost of job loss on individual monthly earnings. Table reports estimates of δ^r from Equations (2), (3) and (4) 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.

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).

Appendix C   Matching results

(1) Cohort	(2)	(3) N (non-displaced)	(4) N (displaced)	(5) Balancing t-tests 5%	(6) Balancing t-tests 10%	(7) Median bias	(8) B	(9) 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.1: 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).

In this section 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 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 panes (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

Sample	Treatment	Control	Prop. of sample
(1) In employment at $r = 0$ aged ≥ 20 and ≤ 60	2,499	78,823	
(2) In cohort 1996 . . . 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).

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 people (row 4).[32] 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.

Because of the small resulting sample size, it is not practical to estimate the costs of job loss 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 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

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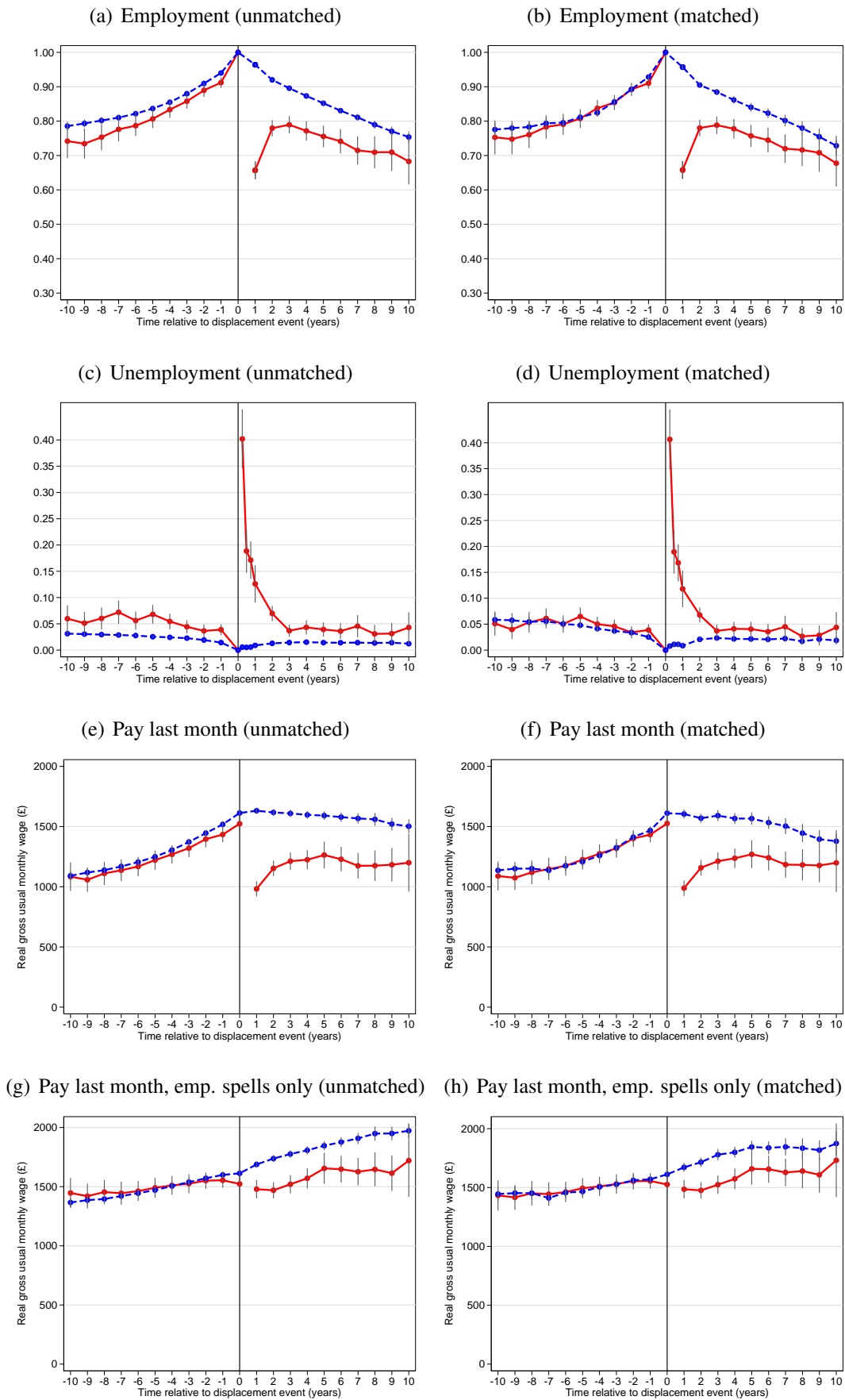[32]They make this restriction to ensure that their definition of "mass-layoff" is meaningful.

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

	(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)
Number of obs.	155,280	114,778	74,433	37,636	102,566	105,704
Number of indiv.	6,035	4,757	3,343	1,571	3,569	3,556

Table D.2: Comparison of different control groups.

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.