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The Earnings Cost of Business Closure in the UK

by

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Abstract

In this paper we estimate how much it costs workers when their employer goes out of business. We use a large random 1% sample of all employees in the UK over the period 1994–2003, linked to a large panel of UK enterprises. We compare the wages and earnings of workers whose employer disappears with comparable workers whose employer remains in the sample. We use both conventional regression techniques and propensity score matching to control for observable differences between displaced and non-displaced workers. We find that earnings losses are initially large but generally last less than four or five years. Earnings losses are mainly driven by periods of non-employment rather than wage losses for those who are successful in finding work again.

JEL classification: J63, J65, C23

Keywords: Worker displacement, linked employer-employee data, panel data, matching

Outline

1. *Introduction*
2. *Previous estimates*
3. *Data*
4. *Methods*
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Non-Technical Summary

How much money do workers lose when their employer goes out of business? Do they suffer large wage losses? How long does it take them to find another job, and is that job of similar quality? Surprisingly, although there are many papers which try to answer these questions, very few of them relate to the UK. In this paper we remedy this by comparing the earnings of workers whose employer disappears with the earnings of workers whose employer does not.

Why does this matter? Partly because business closure is a very common occurrence: 10% of the businesses in our sample are not in the sample in the following year. Economists believe that the exit of unsuccessful firms, and the entry of new firms, is an important part of the way in which economies adjust to external forces such as international competition and new technology. Business closure is also a politically important event. Governments in many countries have often intervened to prevent it, partly in the belief that the costs are large and long-lasting. So we would like to know whether such intervention is justified.

Worker displacement is also interesting from an academic standpoint, because it provides a way of testing various theories about the labour market. For example, a common explanation for the fact that senior workers get paid more than junior workers is that the former have acquired knowledge and skills which are valued by their current employer. This is called "firm specific human capital". If this skill is valuable, senior workers should suffer large wage falls when they lose their jobs. On the other hand, skills might be more generally useful to a large number of employers, in which case wage losses would be smaller.

A key difficulty in answering the question originally posed is that we don't know what would have happened if these workers had in fact *not* lost their jobs. Perhaps they would have earned low wages anyway because bad firms which go out of business pay low wages. Or perhaps workers who experience displacement are less productive and earn lower wages. To deal with this problem we compare their earnings with a group of workers who are observably very similar, but whose employer does not go out of business.

Our initial results suggest that the main difference between the two groups of workers is in terms of employment, not wages. Unsurprisingly, workers whose employer goes out of business are much less likely to have a job in the three or four years following the event. Less expected is the fact that once these workers do find a job, they earn no less (and sometimes even a bit more) than before the event. These results are at odds with the consensus from the US literature, but are consistent with the idea that wages are less flexible in the UK than the US.

1 Introduction

"...whilst we all feel immense empathy for those who lost their jobs there are a range of new job opportunities coming to the West Midlands." Margaret Hodge, Work and Pensions Minister.[1]

"The jobs we had were highly skilled. Working at Tesco's would obviously be nothing like the same kind of work and the pay would be nowhere near what we used to earn." Former MG Rover worker.[2]

What happens to workers' earnings when their employer goes out of business? Accurate estimates of the earnings losses of firm closure are clearly of direct policy interest. Recent research on job creation and destruction has shown that the entry and exit of firms is an important part of the way in which economies adjust to changing patterns of demand (Davis & Haltiwanger 1992). The costs of firm exit are therefore likely to be a significant part of the overall "adjustment cost" of changing patterns of production.

There is a large literature which estimates the effects of displacement on workers' earnings. This literature is dominated by estimates from the US. Kuhn (2002) suggests that this has partly been because of data availability, and partly because jobs were traditionally perceived to be less secure in the US than in other OECD economies. Surprisingly little is known about these costs for workers in the UK: Borland, Gregg, Knight & Wadsworth (2002) is the only study we are aware of.

One way of summarising these studies is to consider the estimation methodology used, which in turn depends on the type of data available. A number of studies use the Displaced Workers Survey,[3] and adopt a "before and after" comparison of earnings for a group of workers who have experienced displacement. This methodology is used partly because the DWS contains data only on displaced workers, and so an explicit control group is not available.

Following Ruhm (1991) and Jacobson, LaLonde & Sullivan (1993), an alternative strategy is to combine the before and after comparison with a similar comparison for a control group of workers who have not experienced displacement. This is a form of the "difference-in-difference" estimation method, which in this case is implemented by using a fixed-effects

[1] BBC News, 17/6/2005.

[2] The Daily Telegraph 17/6/2005.

[3] See, *inter alia* Podgursky & Swaim (1987), Kruse (1988), Kletzer (1989), Addison & Portugal (1989), Topel (1990), Gibbons & Katz (1991), Carrington (1993), Neal (1995), Kletzer (1996) and Farber (2003).

estimator. These papers use data either from representative household surveys such as the PSID[4] or more detailed administrative data.[5]

The influential paper of Jacobson *et al.* (1993) using administrative data for Pennsylvania suggests that there are large and long-lasting effects of displacement on workers' earnings. Even six years after separation, Jacobson *et al.* estimate that earnings are some 25% lower than their pre-displacement earnings. Further, this loss in earnings is *not* due to higher rates of non-employment.[6]

More recently, efforts have been made to provide estimates for workers in other parts of the world, several of which have appeared in Kuhn (2002). These studies have tended to adopt a similar methodology to that used by Jacobson *et al.* (1993).[7] A number of recent UK studies have provided estimates of the effect of spells of unemployment on subsequent earnings. See, for example, Arulampalam (2001), Gregory & Jukes (2001) and Nickell, Jones & Quintini (2002). However, these papers do not provide a comparable estimate of the effect of displacement *pe se* for two reasons. First, some proportion of displaced workers will not experience unemployment because they find work immediately. Second, displacement is not the only cause of entry into unemployment.

Borland *et al.* (2002) is the only previous UK study which looks at the effects of displacement directly. Borland *et al.* use a sample of workers from the British Household Panel Survey (BHPS) over the period 1991–1996. Displacement is self-reported: individuals are asked the reason why they left their last job. It seems likely that this will overstate the number of genuine displacements. Borland *et al.* find much smaller earnings losses for the UK than Jacobson *et al.* do in Pennsylvania. The raw pay penalty is estimated to be between 2% and 14%, and wage falls are mainly limited to those who experience some time out of employment after the displacement event. These much smaller estimates may reflect the self-reported definition of displacement, or may be a genuine difference between the UK and US labour markets.

We provide the first analysis that explicitly estimates the earnings losses due to enterprise closure in the UK. We further make the following contributions. First, we use a new, much larger, dataset to provide estimates of the earnings loss resulting from firm closure. Our data

[4]Examples include Ruhm (1991) and Stevens (1997).

[5]Examples include Jacobson *et al.*, Stevens, Crosslin & Lane (1994) and Schoeni & Dardia (1996).

[6]"Thus, the substantial earnings losses observed ... are largely due to lower earnings for those who work, rather than an increase in the number of workers without ... earnings." (p.697)

[7]Bender, Dustmann, Margolis & Meghir (1999), Burda & Mertens (2001) and Margolis (1999) analyse data from France and Germany. Huttunen, Møen & Salvanes (2003) and Eliason & Storrie (2004) use large administrative datasets for Finland and Sweden.

come from linking a 1% sample of workers to a large panel (effectively a census from 1997 onwards) of enterprises in the UK from 1994–2003. Second, our definition of displacement is based on the disappearance of enterprises, rather than self-reported job loss. Because we observe firm exit over a long period we are able to track workers' earnings for several years after the displacement event. Third, we implement propensity score matching methods to explicitly compare the earnings of displaced workers with the unobserved counterfactual of displaced workers had they not been displaced. The availability of rich information on pre-displacement characteristics is crucial for the construction of the unobserved counterfactual.

Our main findings suggest the following. First, earnings losses are primarily associated with periods of non-employment (as defined by absence from the NES) rather than with falls in wages for those who are re-employed. This is in sharp contrast to findings from the US, but consistent with the only other UK study on worker displacement (Borland *et al.* 2002). Second, earnings losses do *not* appear to be particularly long-lived. After controlling for observable characteristics displaced workers earnings are not lower than non-displaced workers five years after displacement.

In Section 2 we provide a detailed description of the data construction process. The methodological issues are explained and discussed in Section 3. Section 4 presents the results. Finally, Section 5 concludes.

2 Data

In order to evaluate the impact of business closure on workers we need longitudinal information on workers linked to the businesses they work for, and we need to know when those businesses cease to exist. Survey data on individuals or households (such as the BHPS in the UK or the PSID in the US) typically do not record the identity of workers' employers, nor are they able to identify business closure. We therefore use various datasets made available at the Business Data Lab of the ONS.

The *New Earnings Survey* (NES) is a random sample of 1% of employees who are part of the PAYE tax scheme. The last two digits of an individual's National Insurance number are used to select the sample, and so it can straightforwardly be linked across time to form the New Earnings Survey Panel Dataset (NESPD). Businesses can be identified by a PAYE reference number, although note that in some years this information is not available for all workers. PAYE reference numbers are available in 1994–1996 and every year from 1998 onwards. It is important to appreciate that the NES is a sample only of *employees,* and in addition probably undersamples low-paid employees and those who have recently changed employers (Elias & Gregory 1994).

Individuals in the NES may hold more than one job, and to simplify the subsequent analysis we keep only the highest-paid job for each individual in each year. We also remove the (very small) number of individuals with inconsistent measures of age and sex. The resulting sample has slightly over 150,000 observations per year.

The *Inter-Departmental Business Register* (IDBR) is a list of UK businesses maintained by the ONS. It is used for selecting the sample for various surveys of firms and employees conducted by ONS. A comprehensive description of the IDBR can be found in the Review of the Inter-Departmental Business Register (Office for National Statistics 2001). The IDBR is actually a "live" register which changes frequently. The Business Data Lab does not (yet) have systematic snapshots of the IDBR going back through time.

The *IDBR linking file* is a subset of the IDBR which contains the link between an enterprise reference number and the PAYE reference number used in the NES. As far as we are aware, this file is only available for the years 1997 and 2004. Table 1 shows the number of enterprises and PAYE reference numbers covered by the linking files. Enterprises may have more than one PAYE reference number.

	1997	2004
Number of unique PAYE references	2,543,158	1,742,894
Number of unique enterprise references	2,069,297	1,149,834

Table 1: IDBR linking file

The *Annual Business Inquiry* (ABI) is an annual survey of businesses which, since 1994, has been sampled from the IDBR. The "selected sample" of the ABI is a census of all large businesses employing 250 or more and a sample of smaller businesses. The "non-selected sample" are those businesses in the sampling frame which were not selected for the survey. See Jones (2000) for a more detailed description. The *Annual Respondents' Database* (ARD) contains the information from the ABI for each year. The ARD comprises three aggregation categories. The lowest level of aggregation is the *local unit*: a single plant at a single address. An 'enterprise' may contain one or more local units, and is essentially a firm or business with a relative degree of autonomy. Finally, an *enterprise group* is the group of all enterprises under common control. In addition, an enterprise may record information via several *reporting units*. The vast majority of enterprises have a single reporting unit. However, those enterprises with multiple reporting units are on average very large, and will therefore be important in worker-level data.

It is most straightforward to link the data at the level of the enterprise, because both PAYE reference numbers and enterprise reference numbers are available in the linking file. The closure of an enterprise is also possibly a more easily identifiable economic event as far as workers are concerned. In contrast, the closure of a local unit may in fact be a case of business restructuring, and may lead to worker relocation within enterprises.[8]

2.1 Measures of enterprise closure

Our measure of enterprise closure is based on the enterprise reference number in the ARD, and therefore relies on this reference number being recorded consistently over time. Our basic sample of enterprises is listed in Table 2, together with the number that exit. Obviously we cannot identify exiting enterprises in the final year of the data.

Year	Continue	Exit	% exiting	Total
1994	301,993	40,026	11.70%	342,019
1995	310,342	37,050	10.67%	347,392
1996	301,708	33,016	9.86%	334,724
1997	1,320,365	161,424	10.89%	1,481,789
1998	1,386,354	167,525	10.78%	1,553,879
1999	1,459,824	179,902	10.97%	1,639,726
2000	1,483,215	184,363	11.06%	1,667,578
2001	1,491,961	189,041	11.25%	1,681,003
2002	1,490,486	217,405	12.84%	1,692,949
2003				1,743,642
Total	9,546,248	1,209,752	11.26%	10,741,059

Table 2: ARD sample 1994–2003

Comparing Table 2 with Table 1, we can see that in 1997 the ARD sample comprised 1,481,789 enterprises, while the linking file contains 2,069,297 unique enterprise references. In 2004 however, there appear to be far fewer unique enterprise reference numbers in the linking file. This fall in the number of enterprises seems unlikely to be genuine, though we cannot identify the cause. However, the number of successful links does not seem to be affected by this fall in the number of enterprises in the linking file.

[8]This is in itself an interesting issue, but not the focus of this paper.

2.2 The linking procedure

We first link each year of the NES to the IDBR linking file. This is relatively straightforward because the link is at the level of the enterprise. Figure 1 illustrates the connection between the relevant files for one particular year.

New Earnings Survey 2000

Year	N.I. no.	PAYE reference no.
2000	1	A
2000	2	D
2000	3	E
2000	4	B
2000	5	C

1997 linking file

Year	PAYE ref. no.	Enterprise ref. no.
1997	A	a
1997	B	b
1997	C	c
1997		
1997	X	x
1997	Y	y
1997	Z	z

2004 linking file

2004	B	b
2004	C	f
2004	E	e
2004		
2004	X	x
2004	Y	y
2004	Z	z

ARD 2000

Year	Enterprise ref. no.	Reporting unit ref. no.	Local unit ref. no.
2000	a	a1	a11
2000	b	b1	b11
2000	b	b1	b12
2000	c	c1	c11
2000	c	c2	c21
2000	c	c2	c22
2000	d	d1	d11
2000	d	d2	d21
2000	d	d3	d31
2000			
2000	x	x1	x11
2000	y	y1	y11
2000	z	z1	z11

Figure 1: The linking files: illustration for year 2000

The left-hand panel shows the NES for the year 2000. Each of these individuals has a PAYE reference number, which can in theory be linked to an enterprise reference number using the linking files shown in the middle panel. These enterprise reference numbers can then be used to link to the ARD shown in the right hand panel. Note that some enterprises have multiple reporting units or multiple local units. Without additional information on, for example, location or industry, we cannot associate individuals with individual reporting units or local units.[9]

Because the linking file contains a correspondence between PAYE reference numbers and enterprise reference numbers only for 1997 and 2004, there will be individuals in the NES for whom we cannot find an enterprise in the linking file, and individuals for whom we can only find a match in one particular year.

[9] In related work, Haskel & Pereira (2002) link two years of the NES to the ARD at the level of the reporting unit by using additional local unit information on postcode and industrial classification. This approach is problematic because industrial classification and postcode is not consistently recorded in the NES at the same level of aggregation, and because many postcodes in the NES appear to be miscoded.

An enterprise which existed in the year of the linking file may not exist in the year of the NES. For example, an enterprise which existed in 1997 may not exist in 2000 (exit). Or an enterprise which did not exist in 2000 may exist in 2001 (entry). In Figure 2.2, enterprise a exits at some point between 2000 and 2004, and so does not appear in the 2004 linking file. We must therefore rely on the 1997 link in this case. Similarly, enterprise e enters at some point between 1997 and 2000, and therefore does not appear in the 1997 linking file.

The enterprise reference number may change over time. In Figure 2.2, PAYE reference number C is associated with two enterprise numbers: c in 1997 and f in 2004. This leads to individual number 5 being linked to possibly two apparently different enterprises. This problem may also be caused by PAYE reference numbers changing over time.

Table 3 shows the results of the link between the NES and the IDBR linking file.

Year	No link to either linking file	Link to 1997 linking file only	Link to 2004 only	Link to both linking files, same ent. ref. number	Link to both, different ent. ref. number
1994	82,982	15,858	0	59,884	3,912
1995	43,500	16,943	0	92,712	6,801
1996	24,880	16,199	0	111,645	8,185
1997	151,885	0	0	0	0
1998	20,687	11,999	0	117,961	8,169
1999	21,819	9,902	0	119,154	8,163
2000	49,682	3,623	0	96,518	5,348
2001	140	5,907	N/A	140,688	8,686
2002	406	3,395	6,251	138,576	8,220
2003	878	1,534	30,052	116,377	5,345
Total	396,859	85,360	36,303	993,515	62,829

Table 3: Linking NES to IDBR

Note that in 1997 there are no PAYE reference numbers available in the NES and so we cannot link any individuals to the linking files. Before 1997 the number of links is rather low. It seems unlikely that this is due to enterprise entry and exit; it seems more likely to be due to changing enterprise reference numbers or changing PAYE reference numbers. The quality of the link appears to increase after 2000.

We can now link those individuals whose PAYE reference number matches an enterprise reference number to the ARD. Before we do this, however, we can increase the number of cases where an enterprise reference is available by utilising the longitudinal nature of the NES. Individuals who work for enterprise A at $t-1$ and at $t+1$, but who have no enterprise reference number at t are assumed to have worked in enterprise A at t. Individuals whose local unit postcode and whose five-digit SIC code remain the same at $t+1$ are assumed to be working for the same enterprise as at t. Following these rules allows us to link more individuals, particularly in 1997. Table 4 shows the number of links made between the NES and the ARD.

Year	No link to either linking file	Link to 1997 linking file only	Link to 2004 only	Link to both linking files, same ent. ref. number	Link to both, different ent. ref. number
1994	132,246	6,698	391	22,688	613
1995	115,363	8,134	573	34,955	931
1996	111,013	7,814	611	40,527	944
1997	87,819	6,232	445	55,419	1,970
1998	45,502	11,561	491	97,016	4,246
1999	40,180	9,468	620	104,960	3,810
2000	52,673	5,470	518	93,428	3,082
2001	28,454	5,427	881	116,385	4,276
2002	29,847	3,774	3,996	115,926	3,305
2003	30,839	474	19,516	102,358	999
Total	673,936	65,052	28,042	783,662	24,176

Table 4: Linking NES to ARD

Note that the number of individuals with no link is much greater than in Table 3. This is largely due to the incomplete coverage of the ARD. Before 1997 the ARD only covered manufacturing sectors, for example. The final number of individuals with a linked enterprise reference number is shown in Table 5. The proportion of workers in the NES who can be associated with an enterprise ranges from less than 20% in 1994 (largely due to non-coverage of services in the ARD) to around 80% in more recent years.

Year	Unlinked	Linked	% Linked	Total
1994	132,859	29,777	18.31%	162,636
1995	116,294	43,662	27.30%	159,956
1996	111,957	48,952	30.42%	160,909
1997	89,789	62,096	40.88%	151,885
1998	49,748	109,068	68.68%	158,816
1999	43,990	115,048	72.34%	159,038
2000	55,755	99,416	64.07%	155,171
2001	32,730	122,693	78.94%	155,423
2002	33,152	123,696	78.86%	156,848
2003	31,838	122,348	79.35%	154,186
Total	698,112	876,756	55.67%	1,574,868

Table 5: Number of workers with linked enterprise reference numbers

2.3 Enterprise closure in the linked data

Table 6 reports the proportion of workers experiencing enterprise closure in a given year, which is far lower than the proportion of enterprises which exit (Table 2). This is because the linked worker sample is effectively weighted by firm size, and large firms are less likely to exit.

We are able to use the longitudinal nature of the NES data to check the accuracy of the measure of enterprise closure. As noted earlier, if enterprise reference numbers are not coded consistently across time, this might cause inaccurate measures of business closure. We compare those cases where enterprise reference numbers disappear with the data with changes in the individual's PAYE reference number. Table 6 shows that in about 20% of cases a enterprise reference number disappearance is not associated with a change in the PAYE reference number, which suggests that these enterprises did not in fact exit. We therefore code these as non-exits. This leaves 11,663 enterprise exits observed at the individual level.

	Linked	Enterprise exit at t+1	% exiting	Enterprise exit at t+1 and PAYE ref change	% exiting
1994	29,777	435	1.46%	310	1.04%
1995	43,662	909	2.08%	654	1.50%
1996	48,952	1755	3.59%	1754	3.58%
1997	62,096	767	1.24%	767	1.24%
1998	109,068	2138	1.96%	1461	1.34%
1999	115,048	1565	1.36%	1376	1.20%
2000	99,416	1008	1.01%	661	0.66%
2001	122,693	3749	3.06%	2403	1.96%
2002	123,696	3859	3.12%	2277	1.84%
2003	122,348				
Total	876,756	16,185	1.85%	11,663	1.33%

Table 6: Number of workers in enterprises which exit

2.4 Structure of the resulting linked data

In each year $t = 1994, \dots, 2003$ we observe N_t workers drawn from the New Earnings Survey, indexed $i = 1 \dots, N$. This information refers to April of each year. Each worker has a set of observable characteristics $\mathbf{x}_{it}$, including variables such as the individual's age, sex, industry and occupation. For each worker we also observe y_{it}, a measure of their pay. The pay measure we use is gross weekly pay, including overtime payments.

In each period workers may be linked to the selected and non-selected data from the ARD. As noted, the number of linked workers varies from about 20% in 1994 to over 80% in 2003.

The most significant decision we make regards the treatment of individuals who are not observed in the NES in certain years. We cannot ignore them because to do so would remove any unemployment effects from the resulting estimates. Following Jacobson *et al.* (1993), we assume that years in which an individual is not observed in the NES are years in which the individual is not employed. Jacobson *et al.* assume earnings of zero for these periods. Rather than do this, we allocate these individuals standard rates of the job-seekers allowance.[10] This decision will undoubtedly give us an underestimate of the earnings of individuals who are not in the sample because some of those missed by the NES will not in fact be unemployed.

We should note that there are different methods that can be used to generate periods of unemployment. The first method assumes that any missing row between existing rows is a period of unemployment, but ignores missing rows at the beginning or the end of the sample. This ignores workers who leave the sample permanently. The second method adds in any missing rows from the sample period, giving a balanced panel. When using the second method we only consider workers aged 25–55 so that entry to and exit from the labour force is not confused with periods of unemployment. In Section 4 we look at the impact of these different assumptions.

Define $J(i,t)$ to be the function that maps worker i at time t to enterprise j (see Abowd, Kramarz & Margolis (1999)). For those workers who are linked to the ARD we observe a limited set of information on the enterprise, denoted $\mathbf{z}_{J(i,t),t}$. This could be more simply written as $\mathbf{z}_{jt}$.

A worker is defined as experiencing a business exit if the enterprise they were in at t no longer exists at $t+1$. Define a dummy

$$d_{it} = \begin{cases} 1 \text{ if firm } J(i,t) \text{ does not exist at } t+1 \\ 0 \text{ otherwise} \end{cases} \tag{1}$$

[10] Taken from www.statistics.gov.uk/STATBASE/Expodata/Spreadsheets/D3989.xls.

3 Methods

In common with the recent literature on policy evaluation,[11] we treat a worker displacement (or an enterprise closure) as if it were some kind of "treatment" which may impact upon a worker's future labour market outcomes, in the same way as a training or welfare programme. The key problem is that we cannot observe outcomes for an individual who both experiences and does not experience displacement. Thus, the most important issue is how to construct the counterfactual: what would have happened to a displaced worker had they not been displaced.

A second key issue is the idea that the impact of displacement may vary across individuals. In particular, the effect of displacement on the displaced may not be the same as the effect of displacement on those who have not been displaced. This leads to the important distinction between the "treatment effect on the treated" (TTE or LATE) and the treatment effect on the untreated" (TU) or the "average treatment effect" (ATE). More generally, treatment effects may vary across individuals even within the treatment and control groups.

To simplify this discussion assume we only want to measure the displacement effect on those who are displaced. Let t^* be time relative to the year in which $d_{it} = 1$, so $t^* = 0$ in the year immediately before firm closure. Define w^1_{it} to be the sequence of earnings for a worker which experiences displacement at t^*. Define w^0_{it} to be the (hypothetical) sequence of earnings for the same worker in the absence of displacement. The total cost of displacement for worker i is

$$c_i = \sum_{t^*=t_1}^{t_2} w^1_{it^*} - w^0_{it^*} \quad t_1 \leq 0, t_2 > 0$$

Note that this cost includes any difference in the sequence of earnings before as well as after the event. Practically, this involves creating a vector of dummies $\mathbf{d}$ which indicate forthcoming exits or exits which occurred in the past.

$$\mathbf{d}_{it} = [d^{-k}_{it}, d^{-(k-1)}_{it}, \ldots, d^{-1}_{it}, d_{it}, d^1_{it}, \ldots, d^{k-1}_{it}, d^k_{it}]$$

This very general formulation allows for heterogeneity of effects across different individuals. The problem of constructing a counterfactual amounts to the construction of a series for w^0_{it}.

In this paper we use two methods to estimate c_i. The first is a standard regression method which is largely comparable to that used by Jacobson *et al.*. The basic estimating equation

[11] See Blundell & Costa Dias (2002) for a recent summary.

for earnings is:

$$y_{it} = \alpha_i + \gamma_{t^*} + d_i\delta_0 + \mathbf{d}_{it}\boldsymbol{\delta} + \mathbf{x}_{it}\boldsymbol{\beta} + \epsilon_{it} \tag{2}$$

Equation (2) includes a dummy indicating whether or not the individual is in the treatment group (d_i), a set of parameters for relative time γ_{t^*}, plus the relative time dummies interacted with d_i. Equation (2) also includes an individual-specific fixed effect α_i which is likely to be correlated with $\mathbf{d}_{it}$, and therefore it is important to allow for this in the regressions. Finally, the vector $\mathbf{x}_{it}$ includes a set of covariates which vary across individual i and time t up to the point of displacement.

This method thus estimates c_i from the difference in mean earnings between a group of workers who are displaced at $t^* = 0$ (the treatment group) and a group who are not (the control group). Because the control group may have different observable characteristics to the treatment group, the difference in mean earnings is estimated conditional on a set of characteristics $\mathbf{x}_{it}$. Differences between the treatment and control group which are not observed but which are fixed through time can be eliminated by comparing the within-individual change in earnings over time between the two groups, thus implementing a difference-in-difference estimator.

The second method is to select individuals from the control group who explicitly "match" those in the treatment group on the basis of their pre-displacement characteristics. The counterfactual in this method is more explicitly defined to be a group of individuals who are observably similar to those who are affected by the displacement. This method has two significant advantages over the regression method. First, it compares mean earnings between two groups whose probability of displacement is similar: that is, it compares individuals who have the same *common support*. Second, it does not impose the same effect on the whole population. As well as matching on observed characteristics, we also compare the within-individual change in earnings (as in the regression method), thus combining matching with a difference-in-differences estimator.

We use propensity-score techniques to match individuals in the treatment group with individuals in the control group. The propensity score is estimated using two different Probit regressions.

$$\Pr(d_{i,0} = 1) = \Phi(\beta_1 w_{i,-4} + \beta_2 w_{i,-3} + \beta_3 w_{i,-2} + \beta_4 w_{i,-1} + \beta_5 w_{i,0}) \tag{3}$$

In Equation 3 the probability of experiencing displacement is estimated purely as a function of the sequence of wages over the period $-4 \leq t^* \leq 0$.

$$\Pr(d_{i,0} = 1) = \Phi(\mathbf{x}'_{it}\boldsymbol{\beta}) \tag{4}$$

In Equation 4 the probability of experiencing displacement is estimated as a function of a vector of characteristics $\mathbf{x}_{it}$, which includes age, sex, occupation, sector, firm size (lagged four periods) and the wage (also lagged four periods). In both cases we use one-to-one nearest neighbour matching, meaning that a single individual from the control group is matched with a single individual in the treatment group.

We use these two different propensities because matching on earnings effectively imposes the restriction that pre-displacement earnings are unaffected by displacement. That is

$$w^1_{it^*} = w^0_{it^*} \quad t^* \leq 0$$

This restriction might be unsatisfactory because there might be what is known as an "Ashenfelter dip" in earnings before displacement. For example, firms who are in difficulty might reduce their wages or hours.

Finally, we use the propensity score to match the control and treatment groups by selecting a "nearest neighbour" for each treated: an individual in the control group whose propensity of firm closure (displacement) is the closest to an individual in the treatment group, subject to some distance criteria.

4 Results

4.1 Unmatched average earnings comparisons

The simplest aggregate comparison uses average earnings for the treatment and control group for each year before and after displacement. The treatment group are defined as those displaced in year $t^* = 0$, while the control group are those not displaced in year $t^* = 0$. A separate treatment and control group is therefore defined for each possible year of displacement (1994–2002). We then stack each of the treatment and control groups together to estimate an average effect for all years combined. Individuals may therefore appear in the control group several times, since an individual who is not displaced in year t may also not be displaced in year $t + 1$ and so on. The only restriction we place on the sample is that individuals must be employed (i.e. in the NES sample) in all five years before displacement $-4 \leq t^* \leq 0$. This restricts the sample to displacement events in the period 1998–2002, which in turn means that at most we have five years of post-displacement earnings information.

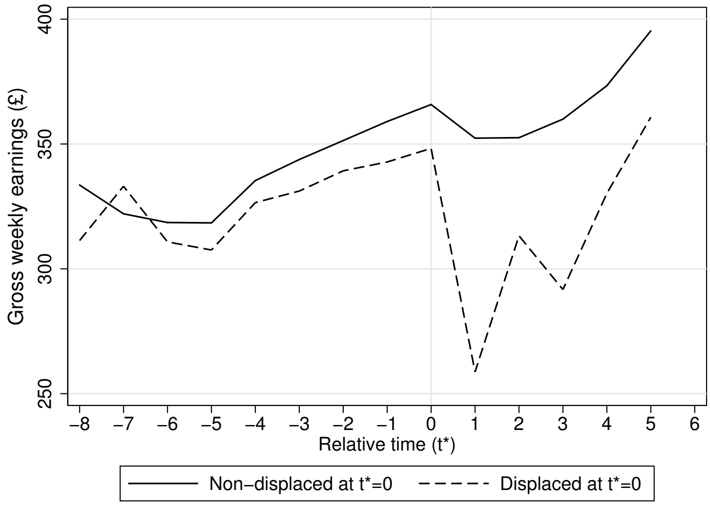

Figure 2: Average earnings by displacement status

Figure 2 shows that workers whose enterprise exits suffer falls in earnings of about 30% in the first year after the displacement, and that earnings take between four and five years to return to the pre-displacement level. If we take the non-displaced as a counterfactual, we can see that the earnings of those who are displaced are also lower in most years before the displacement, and that the gap in earnings between the groups is greater at $t^* = 5$ than it was at $t^* = 0$.

One striking difference between this pattern of earnings and those presented by Jacobson *et al.* (Figure 1) is the earnings of the control group. In our sample the control group experience a small earnings loss at $t^* = 1$. This is due to the fact that we do not restrict the control group to include only those in employment in all years. Therefore although at $t^* = 0$ the whole sample is employed, a proportion of that sample (including some in the control group) will be unemployed at $t^* = 1$. Jacobson *et al.* restrict the control group to include workers who are *never* unemployed.

The average earnings shown in Figure 2 are strongly affected by the proportion of the sample observed in the NES in each year, because those not observed are assumed to be unemployed and receiving job-seekers allowance. Figure 3 plots the proportion of the sample who are in employment (i.e. observed in the NES) in each year relative to $t^* = 0$. By definition the whole sample is employed from $-4 \leq t^* \leq 0$. More than 30% of the displaced sample are non-employed at $t^* = 1$. The displaced also have lower employment rates at $t^* < -4$. Note that the method we use to impute spells of unemployment (filling in gaps) means that employment rates at $t^* = -8$ and $t^* = 5$ are 1 by definition.

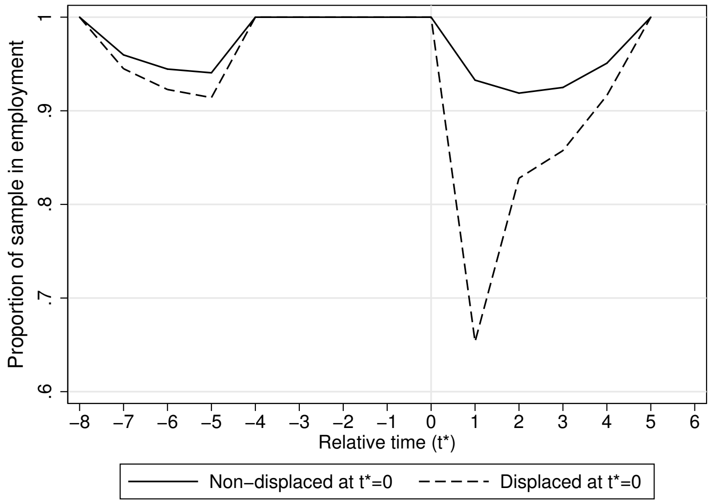

Figure 3: Proportion of sample observed

Figures 2 and 3 illustrate that the post-displacement difference in earnings between the treatment and control groups is largely due to different employment rates. There is some evidence at $t^* = 5$, however, that the treatment group have lower earnings despite all being in the sample.

To check the robustness of these results we plot the difference in earnings between the treatment and control groups under a number of different assumptions, shown in Figure 4. The solid line plots the gap in earnings between the two lines shown in Figure 2. We then compare this with a sample which has no pre-displacement restriction on employment. This has the effect of slightly increasing the gap in earnings before displacement because the displaced have lower employment probabilities at $t^* \leq 0$, but has very little effect on the gap after displacement. One advantage of this sample is that we can follow earnings for up to nine years after displacement. It is interesting to note that the earnings gap has completely disappeared by $t^* = 9$.

We then consider the impact of our method of creating unemployment spells. The third line in Figure 4 shows the effect of assuming that permanent exits from the NES sample are unemployed for the remaining sample period. Unsurprisingly, this increases the earnings loss substantially at $t^* = 1$ because a large proportion of displaced workers disappear from the NES and do not reappear. Estimated earnings losses still reduce and after five or six years are

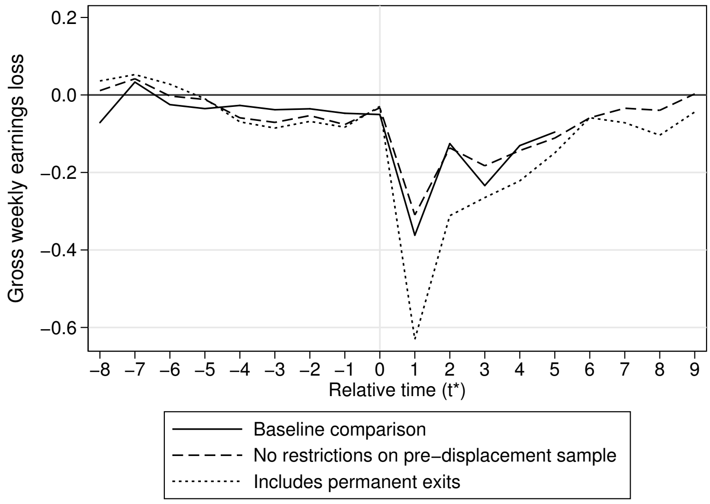

Figure 4: Average earnings loss: alternative sample definitions

only slightly larger than under the alternative assumption.

4.2 Comparisons of pure wage effects

As noted, earnings losses are driven mainly by the increased rates of non-employment in the displacement sample. This is in contrast to the results of Jacobson *et al.*, who claim large earnings losses even among those who are re-employed after employment. To examine this issue more closely, we restrict the sample to those individuals who have a wage recorded in the NES and are therefore definitely in employment. We split the sample according to the length of the "gap" between the displacement event and the subsequent observation in the NES. Thus an individual who was displaced in 1998 and first observed subsequently in 2000 would have a gap of one year. In Figure 5 we plot average wage losses relative to a control group who do not experience displacement and who do not have a gap.

It is striking that displaced workers who are observed in the NES in the year after displacement (those with no gap) experience *no* additional wage loss in the year after displacement, although their wages are about 5% lower before displacement. Individuals who are not observed in the NES in the years after displacement do tend to have lower post-displacement wages, but they also tend to have lower pre-displacement wages as well, so there is no clear evidence of wage losses if we look only at workers who are in employment (and hence observed in the NES). In fact, Figure 5 is more consistent with models of selection rather than models of wage loss due to the loss of firm-specific human capital. When a firm closes the

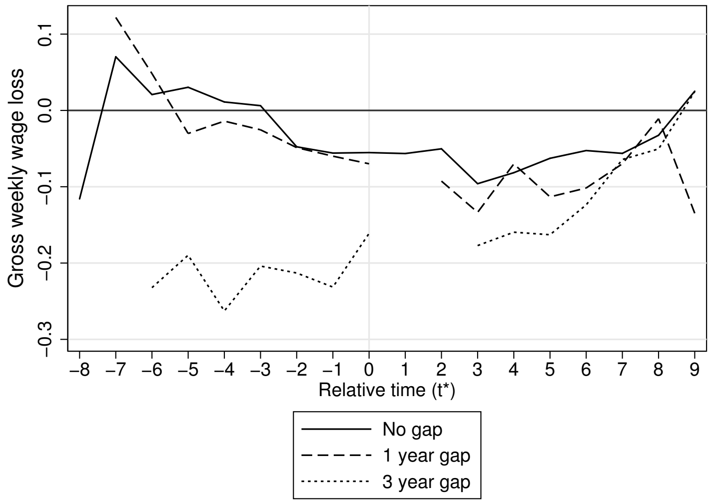

Figure 5: Average wage loss by length of gap

workers with the highest earnings ability are employed more quickly, while those with lower earnings ability experience periods of unemployment.

4.3 Matched average earnings comparisons

The treatment and control groups used to calculate mean earnings in Figures 2 to 5 were not matched, apart from the requirement that both groups be in employment for the five year prior to displacement. One possibility, therefore, is that the difference in post-displacement earnings between the two groups is not due to the displacement itself, but rather to differences in the characteristics of the two groups. In this case the earnings of the control group are not a good estimate of the counterfactual earnings w_{it}^0.

We therefore use propensity-score techniques to match individuals in the treatment group with individuals in the control group. The relevant samples resulting from the matching process are shown in Table 7. The top panel shows how the unmatched sample comprises five years of data which is balanced from $-4 \leq t^* \leq 0$. The large size of the control group means that everyone in the treatment group is successfully found a "neighbour" after matching on wages (panel 2), while almost all are found a neighbour after matching on $\mathbf{x}_{it}$ (panel 3).

The resulting earnings differences between the treatment and control groups are shown in Figures 6 and 7. Unsurprisingly, matching on pre-displacement wages almost entirely eliminates the pre-displacement difference in wages between the treatment and control groups observed in the raw sample. The matched sample still experiences large earnings falls at $t^* = 1$ of

	Control group (non-displaced)					Treatment group (displaced)				
t*	1998	1999	2000	2001	2002	1998	1999	2000	2001	2002
(a) Unmatched samples										
-8	0	0	0	0	35,794	0	0	0	0	450
-7	0	0	0	37,470	40,227	0	0	0	564	514
-6	0	0	38,630	41,985	43,128	0	0	277	643	548
-5	0	42,617	43,138	44,994	45,214	0	388	299	695	580
-4	47,241	48,708	46,836	47,867	48,367	475	436	321	740	627
-3	47,241	48,708	46,836	47,867	48,367	475	436	321	740	627
-2	47,241	48,708	46,836	47,867	48,367	475	436	321	740	627
-1	47,241	48,708	46,836	47,867	48,367	475	436	321	740	627
0	47,241	48,708	46,836	47,867	48,367	475	436	321	740	627
1	44,293	45,285	44,186	43,005	40,187	338	309	202	488	245
2	41,441	42,315	40,262	36,344	0	317	289	190	393	0
3	38,586	38,547	34,039	0	0	294	254	156	0	0
4	35,043	32,395	0	0	0	256	208	0	0	0
5	29,190	0	0	0	0	216	0	0	0	0
(b) Samples matched on pre-displacement wages										
-8	0	0	0	0	470	0	0	0	0	450
-7	0	0	0	601	526	0	0	0	564	514
-6	0	0	240	659	569	0	0	277	643	548
-5	0	388	282	705	593	0	388	299	695	580
-4	475	436	321	740	627	475	436	321	740	627
-3	475	436	321	740	627	475	436	321	740	627
-2	475	436	321	740	627	475	436	321	740	627
-1	475	436	321	740	627	475	436	321	740	627
0	475	436	321	740	627	475	436	321	740	627
1	445	411	301	675	528	338	309	202	488	245
2	404	377	270	578	0	317	289	190	393	0
3	366	340	223	0	0	294	254	156	0	0
4	330	295	0	0	0	256	208	0	0	0
5	269	0	0	0	0	216	0	0	0	0
(c) Samples matched on pre-displacement characteristics										
-8	0	0	0	0	452	0	0	0	0	450
-7	0	0	0	559	520	0	0	0	559	514
-6	0	0	269	630	549	0	0	276	637	548
-5	0	362	294	693	577	0	383	298	689	578
-4	475	428	320	734	625	475	428	320	734	625
-3	475	428	320	734	625	475	428	320	734	625
-2	475	428	320	734	625	475	428	320	734	625
-1	475	428	320	734	625	475	428	320	734	625
0	475	428	320	734	625	475	428	320	734	625
1	434	400	299	638	503	338	302	201	483	243
2	406	382	265	529	0	317	283	189	388	0
3	372	349	226	0	0	294	248	155	0	0
4	326	285	0	0	0	256	202	0	0	0
5	275	0	0	0	0	216	0	0	0	0

Table 7: Sample sizes

around 30%, but the recovery in earnings appears faster for the matched sample, with the earnings loss almost disappearing by $t^* = 4$. Matching on $\mathbf{x}_{it}$ produces very similar results. Pre-displacement earnings losses are almost eliminated by using only the matched sample, and post-displacement earnings losses are somewhat smaller.

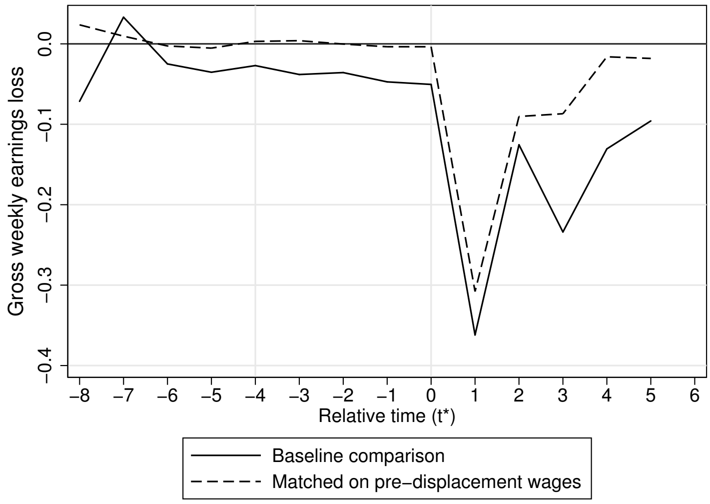

Figure 6: Average earnings loss: matched on pre-displacement wages

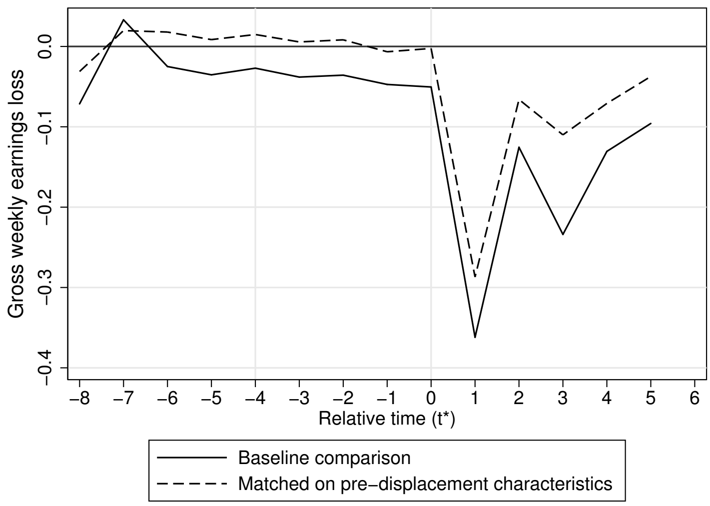

Figure 7: Average earnings loss: matched on pre-displacement values of $\mathbf{x}_{it}$

4.4 Regression results

In this section we use regression methods to estimate the earnings loss experienced by displaced workers. The data used are identical to those used to draw the graphical comparisons. As before, treatment and control groups are defined for each year and then stacked.

Table 9 reports some baseline estimates of the impact of displacement which are directly comparable to the graphical comparisons shown in the previous sections, with the addition of estimated standard errors. OLS estimates on the unmatched sample (column 1) show that although mean wages are lower in the periods preceding displacement, none of these estimates are significantly different from zero. There is, however, a constant effect of being in the displacement group of -0.0365 which is just insignificant at 5%. Wage losses in the periods following displacement are initially large (0.513 log points equates to a fall of 40%), but decrease in size and are insignificantly different from zero after five years.

OLS estimates of Equation 2 are potentially biased because they treat the individual fixed effect α_i as part of the error term. We therefore then estimate Equation 2 using within-i mean deviations, which sweeps out any term which is fixed for an individual over time, including any unobservable. The results are shown in the second column of Table 9. Post-displacement wage effects now diminish more quickly and also tend to be smaller, suggesting that some of the raw difference in post-displacement wages is due to a negative correlation between α_i and $\mathbf{d}_{it}$. It is interesting to see that some estimated differentials are actually positive, including that at $t^* = 5$. This is partly a result of sample selection at the beginning and end of the sample period. Due to the way in which unemployment spells are created, at $t^* = 5$ only those in employment are included in the sample (see Figure 3). If displacement serves to remove workers with low earning potential from the NES sample, we might observe wages of those who remain in the sample actually increasing.

We then repeat these regressions, but use the matched samples described in Section 4.3, shown in columns (3) and (5). The main effect of using the matched sample is to eliminate the constant difference in wages between the two groups (the coefficient on d_i). The post-displacement fall in wages is very similar for $1 \leq t^* \leq 3$, but is insignificant for $t^* \geq 4$. Columns (4) and (6) also use the matched comparisons but estimate differences using the within-i transformation. This does not significantly alter the result.

In Table 10 we repeat the fixed-effects estimates of Equation 2 separately for each year of separation. Wage loss at $t^* = 1$ (the year after displacement) varies from -0.76 log points for those displaced in 2000 to -0.53 for those displaced in 2001. Note that the estimates for

Variable name	*Description*	*Non-displaced, t*=0*		*Displaced, t*=0*	
		Mean	S.D.	Mean	S.D.
lnrw	Log real weekly earnings	5.743	(0.613)	5.676	(0.626)
female	Female	0.395	(0.489)	0.413	(0.492)
age	Age in years	42.967	(10.709)	43.683	(11.280)
age2	Age-squared in years	1960.844	(936.019)	2035.347	(1004.713)
region_1	North East	0.052	(0.222)	0.034	(0.180)
region_2	North West	0.120	(0.325)	0.098	(0.297)
region_3	Yorkshire & Humberside	0.101	(0.301)	0.083	(0.275)
region_4	East Midlands	0.084	(0.277)	0.084	(0.277)
region_5	West Midlands	0.105	(0.306)	0.109	(0.312)
region_6	South West	0.079	(0.270)	0.078	(0.269)
region_7	East	0.091	(0.288)	0.101	(0.302)
	London (base)	0.118	(0.323)	0.157	(0.364)
region_9	South East	0.125	(0.331)	0.130	(0.336)
region_10	Wales	0.041	(0.199)	0.044	(0.206)
region_11	Scotland	0.084	(0.277)	0.083	(0.275)
	Managers and administrators (base)	0.150	(0.358)	0.176	(0.381)
soc_2	Professional	0.113	(0.317)	0.061	(0.239)
soc_3	Associate professional	0.071	(0.257)	0.099	(0.299)
soc_4	Administrative and technical	0.174	(0.379)	0.172	(0.377)
soc_5	Skilled trades	0.139	(0.345)	0.164	(0.371)
soc_6	Personal service	0.064	(0.244)	0.062	(0.240)
soc_7	Sales and customer service	0.076	(0.265)	0.064	(0.245)
soc_8	Process, plant & machine operatives	0.153	(0.360)	0.167	(0.373)
soc_9	Elementary occupations	0.060	(0.238)	0.036	(0.186)
sic_1	Agriculture, forestry & fishing	0.001	(0.026)	-	-
sic_2	Mining & quarrying	0.004	(0.060)	-	-
sic_3	Food	0.031	(0.173)	0.018	(0.134)
sic_4	Textiles	0.015	(0.123)	0.045	(0.207)
sic_5	Leather	0.002	(0.045)	0.007	(0.086)
sic_6	Wood	0.006	(0.075)	-	-
sic_7	Pulp & paper	0.032	(0.176)	0.034	(0.181)
sic_8	Fuels	0.004	(0.060)	-	-
sic_9	Chemicals	0.022	(0.145)	0.009	(0.096)
sic_10	Rubber & plastics	0.018	(0.132)	0.021	(0.143)
sic_11	Non-metallic mineral products	0.012	(0.108)	0.013	(0.112)
sic_12	Basic and fabricated metals	0.043	(0.203)	0.070	(0.256)
sic_13	Machinery & equipment	0.035	(0.184)	0.035	(0.185)
sic_14	Electrical and optical equipment	0.039	(0.193)	0.039	(0.194)
sic_15	Transport equipment	0.038	(0.190)	0.019	(0.137)
sic_16	Other manufacturing	0.012	(0.108)	0.022	(0.146)
sic_17	Electricity, gas and water	0.012	(0.111)	0.005	(0.071)
sic_18	Construction	0.040	(0.195)	0.039	(0.193)
	Wolesale & retail trade (base)	0.185	(0.388)	0.148	(0.355)
sic_20	Hotels & restaurants	0.016	(0.127)	0.019	(0.135)
sic_21	Transport, storage & communication	0.088	(0.284)	0.052	(0.222)
sic_22	Financial intermediation	0.003	(0.058)	0.080	(0.271)
sic_23	Real estate, renting and business servi	0.104	(0.305)	0.135	(0.342)
sic_24	Public admin & defence	0.038	(0.191)	0.060	(0.238)
sic_25	Education	0.141	(0.349)	0.021	(0.143)
sic_26	Health & social work	0.030	(0.169)	0.077	(0.267)
sic_27	Other community, social and personal :	0.031	(0.174)	0.025	(0.157)
public	Public sector	0.204	(0.403)	0.101	(0.301)
trainee	Trainee	0.003	(0.052)	-	-
cov	Union agreement	0.609	(0.488)	0.461	(0.499)
parttime	Part-time employment	0.157	(0.364)	0.168	(0.374)
lop	Loss of pay marker	0.051	(0.221)	0.062	(0.241)
firmsize	Employment/1000	13.184	(32.166)	3.148	(16.746)
t_5	1998	0.200	(0.400)	0.169	(0.375)
t_6	1999	0.203	(0.402)	0.171	(0.377)
t_7	2000	0.193	(0.395)	0.134	(0.341)
t_8	2001	0.200	(0.400)	0.288	(0.453)
t_9	2002	0.205	(0.404)	0.237	(0.425)

Table 8: Sample means at $t^* = 0$

	Unmatched, conditional on covariates(1)				Matched on w_{it}				Matched on x_{it}			
	OLS		FE		OLS		FE		OLS		FE	
displaced	-0.0365	[0.054]			0.0335	[0.409]			-0.0196	[0.633]		
d(-7)	0.0094	[0.667]			-0.0323	[0.386]	-0.0591	[0.081]	0.0468	[0.232]	0.0084	[0.809]
d(-6)	0.0037	[0.868]	0.0062	[0.684]	-0.0286	[0.478]	-0.0283	[0.389]	0.0464	[0.266]	0.0144	[0.669]
d(-5)	-0.0192	[0.397]	-0.0003	[0.983]	-0.0411	[0.314]	-0.0405	[0.206]	0.0263	[0.524]	0.0062	[0.850]
d(-4)	0.0035	[0.853]	0.0393	[0.005]	-0.0285	[0.466]	-0.0210	[0.504]	0.0396	[0.316]	0.0311	[0.334]
d(-3)	-0.0048	[0.798]	0.0293	[0.035]	-0.0223	[0.569]	-0.0149	[0.637]	0.0216	[0.584]	0.0131	[0.685]
d(-2)	0.0006	[0.975]	0.0329	[0.018]	-0.0327	[0.403]	-0.0253	[0.423]	0.0205	[0.605]	0.0120	[0.709]
d(-1)	-0.0078	[0.683]	0.0252	[0.070]	-0.0283	[0.471]	-0.0209	[0.508]	0.0103	[0.795]	0.0018	[0.955]
d	-0.0148	[0.447]	0.0199	[0.151]	-0.0356	[0.368]	-0.0281	[0.372]	0.0126	[0.751]	0.0041	[0.898]
d(+1)	-0.5132	[0.000]	-0.4867	[0.000]	-0.5388	[0.000]	-0.5627	[0.000]	-0.4717	[0.000]	-0.5092	[0.000]
d(+2)	-0.2069	[0.000]	-0.1643	[0.000]	-0.2220	[0.000]	-0.2248	[0.000]	-0.1394	[0.008]	-0.1526	[0.000]
d(+3)	-0.1401	[0.000]	-0.0400	[0.048]	-0.1680	[0.003]	-0.1267	[0.001]	-0.1275	[0.026]	-0.1001	[0.008]
d(+4)	-0.0914	[0.025]	0.0200	[0.401]	-0.0638	[0.292]	-0.0169	[0.676]	-0.0629	[0.305]	-0.0275	[0.507]
d(+5)	0.0161	[0.711]	0.1001	[0.002]	-0.0480	[0.484]	0.0133	[0.792]	-0.0319	[0.635]	0.0345	[0.501]
N*	1,692,802		1,692,802		45,948		45,948		45,461		45,461	
N	63,984		63,984		5,108		5,108		5,043		5,043	
R-squared	0.5092		0.412		0.0266		0.0236		0.0275		0.0246	

Notes
(1) Regression includes full set of controls listed in table of means
(1) All regressions include dummies for relative time t*
(2) P-values in brackets. OLS standard errors are robust to within-i clustering.

Table 9: Baseline regression results

2002 rely on a sample who are all employed in 2003, and this estimate is actually positive, albeit insignificantly different from zero. Again, this shows that a sample comprising only those who find work after displacement is probably not representative of all those who are displaced.

	Displaced in 1998		Displaced in 1999		Displaced in 2000		Displaced in 2001		Displaced in 2002	
	Coeff.	P-value	Coeff.	P_value	Coeff.	P_value	Coeff.	P_value	Coeff.	P_value
d(-6)									0.0087	[0.678]
d(-5)							-0.0406	[0.020]	-0.0338	[0.102]
d(-4)					-0.0051	[0.843]	-0.0013	[0.939]	0.0310	[0.126]
d(-3)			0.0083	[0.740]	-0.0332	[0.193]	0.0116	[0.501]	0.0165	[0.416]
d(-2)	0.0018	[0.948]	-0.0146	[0.560]	-0.0127	[0.617]	0.0119	[0.489]	0.0201	[0.321]
d(-1)	0.0032	[0.909]	-0.0456	[0.068]	-0.0097	[0.704]	0.0063	[0.713]	0.0106	[0.599]
d	-0.0240	[0.393]	-0.0277	[0.267]	-0.0360	[0.158]	0.0010	[0.952]	0.0269	[0.185]
d(+1)	-0.6564	[0.000]	-0.5883	[0.000]	-0.7643	[0.000]	-0.5336	[0.000]	0.0193	[0.515]
d(+2)	-0.2884	[0.000]	-0.3837	[0.000]	-0.2633	[0.000]	-0.0084	[0.695]		
d(+3)	-0.0879	[0.011]	-0.1445	[0.000]	-0.0850	[0.012]				
d(+4)	-0.0783	[0.032]	-0.0138	[0.684]						
d(+5)	0.0327	[0.396]								
N*	331,849		341,405		329,687		342,158		347,703	
N	36,745		37,295		35,484		37,011		37,875	
R-squared	0.3016		0.3856		0.4456		0.4743		0.4657	

Notes
1. All regressions are within-i fixed-effects
2. All regressions include full set of controls in baseline regressions

Table 10: Fixed-effect estimates by year of displacement

The costs of displacement are unlikely to be homogenous across all individuals. In Table 11 we therefore split the sample by various characteristics at $t^* = 0$, the period immediately before displacement.

	Age <= 25		*Age 26-40*		*Age> 40*	
	Coeff.	P-value	Coeff.	P_value	Coeff.	P_value
d(-6)	0.0917	[0.690]	0.0150	[0.556]	0.0007	[0.972]
d(-5)	0.0260	[0.906]	-0.0021	[0.932]	0.0007	[0.967]
d(-4)	0.0096	[0.965]	0.0565	[0.015]	0.0317	[0.065]
d(-3)	-0.0616	[0.776]	0.0348	[0.136]	0.0314	[0.067]
d(-2)	-0.0127	[0.953]	0.0447	[0.056]	0.0279	[0.104]
d(-1)	-0.0002	[0.999]	0.0329	[0.158]	0.0212	[0.216]
d	-0.0060	[0.978]	0.0274	[0.241]	0.0155	[0.366]
d(+1)	-0.3581	[0.101]	-0.4533	[0.000]	-0.5235	[0.000]
d(+2)	-0.1456	[0.508]	-0.1406	[0.000]	-0.1864	[0.000]
d(+3)	0.0600	[0.790]	0.0157	[0.653]	-0.0887	[0.000]
d(+4)	-0.1273	[0.580]	0.0883	[0.029]	-0.0132	[0.665]
d(+5)	0.0222	[0.928]	0.1549	[0.006]	0.0726	[0.079]
N*	65,285		655,656		971,861	
N	7,805		70,908		105,697	
R-squared	0.3837		0.3443		0.4399	

	Private sector		*Public sector*	
	Coeff.	P_value	Coeff.	P_value
d(-6)	-0.0041	[0.803]	0.0761	[0.075]
d(-5)	-0.0088	[0.571]	0.0562	[0.163]
d(-4)	0.0310	[0.038]	0.0954	[0.015]
d(-3)	0.0210	[0.159]	0.0797	[0.041]
d(-2)	0.0258	[0.084]	0.0703	[0.072]
d(-1)	0.0170	[0.253]	0.0856	[0.028]
d	0.0114	[0.443]	0.0786	[0.044]
d(+1)	-0.5194	[0.000]	-0.1836	[0.000]
d(+2)	-0.1727	[0.000]	-0.0494	[0.295]
d(+3)	-0.0463	[0.033]	0.0499	[0.391]
d(+4)	0.0138	[0.591]	0.1076	[0.112]
d(+5)	0.0916	[0.008]	0.1745	[0.075]
N*	1,346,342		346,460	
N	147,097		37,313	
R-squared	0.3755		0.5286	

	Skilled		*nskilled occupations (*	
	Coeff.	P_value	Coeff.	P_value
d(-6)	0.0108	[0.689]	0.0038	[0.834]
d(-5)	-0.0061	[0.812]	-0.0004	[0.981]
d(-4)	0.0543	[0.028]	0.0274	[0.101]
d(-3)	0.0452	[0.067]	0.0166	[0.320]
d(-2)	0.0454	[0.066]	0.0202	[0.225]
d(-1)	0.0403	[0.103]	0.0111	[0.504]
d	0.0224	[0.364]	0.0105	[0.528]
d(+1)	-0.4845	[0.000]	-0.4959	[0.000]
d(+2)	-0.1552	[0.000]	-0.1803	[0.000]
d(+3)	-0.1316	[0.001]	-0.0136	[0.560]
d(+4)	-0.0286	[0.534]	0.0307	[0.266]
d(+5)	0.1805	[0.004]	0.0516	[0.171]
N*	569,905		1,122,897	
N	61,671		122,739	
R-squared	0.2030		0.4002	

Notes
(1) 1-digit SOC 1-3
(2) 1-digit SOC 4-9

	Union agreement (1)		*No union agreement*	
	Coeff.	P_value	Coeff.	P_value
d(-6)	0.0245	[0.270]	-0.0121	[0.567]
d(-5)	0.0096	[0.647]	-0.0125	[0.535]
d(-4)	0.0502	[0.012]	0.0260	[0.179]
d(-3)	0.0435	[0.031]	0.0101	[0.601]
d(-2)	0.0455	[0.024]	0.0137	[0.480]
d(-1)	0.0343	[0.088]	0.0078	[0.689]
d	0.0294	[0.144]	0.0004	[0.983]
d(+1)	-0.5138	[0.000]	-0.4668	[0.000]
d(+2)	-0.1725	[0.000]	-0.1638	[0.000]
d(+3)	-0.0474	[0.101]	-0.0399	[0.166]
d(+4)	-0.0053	[0.875]	0.0360	[0.289]
d(+5)	0.0883	[0.061]	0.0984	[0.031]
N*	1,032,462		660,340	
N	112,029		72,381	
R-squared	0.4444		0.3583	

Notes
(1) Major agreement as defined by NES

	Manufacturing		*Services*	
	Coeff.	P_value	Coeff.	P_value
d(-6)	0.0022	[0.934]	0.0127	[0.503]
d(-5)	0.0083	[0.743]	0.0020	[0.913]
d(-4)	0.0416	[0.087]	0.0426	[0.014]
d(-3)	0.0268	[0.270]	0.0374	[0.031]
d(-2)	0.0312	[0.199]	0.0434	[0.012]
d(-1)	0.0140	[0.565]	0.0427	[0.014]
d	-0.0005	[0.984]	0.0401	[0.021]
d(+1)	-0.7275	[0.000]	-0.3579	[0.000]
d(+2)	-0.3026	[0.000]	-0.0871	[0.000]
d(+3)	-0.0937	[0.004]	0.0081	[0.762]
d(+4)	-0.0603	[0.108]	0.0919	[0.004]
d(+5)	-0.0088	[0.863]	0.1997	[0.000]
N*	522,190		1,076,044	
N	56,664		117,392	
R-squared	0.2099		0.4668	

Notes
(1) SIC codes 3-16
(2) SIC codes 19-27

Table 11: Fixed-effect estimates by characteristics at time of displacement

There is some evidence here that older workers experience greater earnings losses in the short term, particularly at $t^* = 1$, but there is no evidence that these losses continue for more than two or three years, and in fact differentials are positive in the final year of the sample (when the whole sample is employed).

There are large differences in the extent of earnings losses between the public and private sector, as might be expected. Workers in the public sector whose establishment closes are less likely to be subsequently unemployed and as a result their earnings losses are much smaller: 16% compared to 40% at $t^* = 1$.

Differences in earnings losses according to occupation at $t^* = 0$ and union coverage are small, but the final panel of Table 11 suggests a substantial difference between those employed in manufacturing industries and those employed in services. Losses for all periods after $t^* = 0$ are greater in manufacturing.[12]

5 Conclusions

We provide the first estimates of the earnings losses associated with enterprise closure in the UK. Our estimates are robust to different definitions of the sample used and to different estimation methods. Our key finding is that earnings losses are primarily associated with periods of non-employment (as defined by absence from the NES) rather than with falls in wages for those who are re-employed. This is at odds with the findings from the US, but consistent with the only other UK study on worker displacement (Borland *et al.* 2002).

Our second key finding is that earnings losses do *not* appear to be particularly long-lived. After controlling for observable characteristics (either by matching or by using linear regression) displaced workers earnings are not lower than non-displaced workers five years after displacement. A caveat to this finding is that it partly reflects the methods we have used to construct the sample, because permanent exits from the NES are not included.

These findings are preliminary. A key difficulty with the NES is that it is a sample of employees. In addition, workers who change employer may be missing from the sample for a short period. Both of these facts suggest that non-appearance in the NES does not necessarily imply periods of non-employment with associated large earnings losses. In this sense our estimates of earnings losses may in fact be overstated. We are currently working on identifying spells of unemployment more precisely using data on unemployment claimant recipients which can also be linked to the NES.[13]

[12] This may reflect the fact that all manufacturing enterprises are in the private sector; these effects need to be disentangled.

[13] See Gregory & Jukes (2001).

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