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JOB LOSSES AND CHILD OUTCOMES

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Job losses and child outcomes*

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Abstract
Based on matched employer-employee data from Norway, we analyze the effects of worker displacement in 1986-1987 on their children's earnings in 1999-2001. Using displacement of fathers to indicate an exogenous earnings shock we seek to identify whether family resources have a direct effect on children's economic outcome. As in previous Scandinavian studies, we find the intergenerational earnings mobility to be fairly high compared to the U.S. and the U.K. Job losses appear to have a negative effect on earnings and employment of those affected, while we find no significant effects on offspring.

Keywords: Displacement, intergenerational earnings correlations.
JEL classification: J62, C23

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1. Introduction

It is well established that the economic success of children is to some extent related to the incomes of their parents, see Solon (1999) for an overview. The positive correlation between parents' and children's earnings is consistent with a model where utility-maximizing families invest a part of their income in the human capital of children; see Becker and Tomes (1979, 1986). Even so, it may be argued that the mechanisms behind intergenerational mobility (or lack of such) are not fully understood. One argument is that conventional intergenerational earnings elasticities confound several deeper parameters. For instance, Goldberger (1989) notes that the empirical observation of father-child correlations may also be explained without appealing to utility maximization: "..suppose that intergenerational links are stronger for occupation or socioeconomic status than for income or earnings. Then restricting attention to the monetary measures could lead an economist to understate the influence of family background on inequality." Also recent empirical contributions to the literature aspire to disentangle mechanisms behind the intergenerational earnings correlation. Grawe (2004) notes that observed non-linearities in intergenerational earnings correlations do not necessarily support the credit constraints that seem necessary for the story of earnings transmission through investment in education, but rather low market ability and low preferences for education etc. being passed on from their parents to their children.

Oreopolous et al. (2005) also note that income differences might be result of parental income differences leading to differences in monetary investments in children or, just as likely, a reflection of the passing on of parental characteristics. They go on to argue that comparing outcomes of children from families with different income levels may overstate the importance of economic resources, as high income parents also may have high motivation and ability that affect outcomes of the next generation. Their approach is to use firm closures in

Canada to indicate exogenous income shocks. They find that worker displacement has detrimental effects on children's labour market outcomes. This finding is taken as evidence that, indeed, family income is an important factor in determining the economic outcome of the children.

Huttunen et al. (2006), using the same Norwegian data source as in the present study, find that worker displacement has negative effects, but mainly through employment effects. Rege et al. (2005) find that plant downsizing substantially increases permanent withdrawal from the labour force by increasing the disability entry rate of workers in the affected plants. Despite of the potential importance of displacement for children's outcomes, there are very few studies on the topic. The present paper tries to close this knowledge gap. Applying a similar approach as Oreopolous et al. (2005) we investigate whether their conclusions hold confronted with a large sample of Norwegian father-child pairs. Our data includes earnings information for fathers from the period 1982-1985 and for the children from the period 1999-2001 when they are at age 26-30, together with relevant plant level information.

Previous research has found lower intergenerational earnings correlations in non-Anglo-Saxon Europe, in particular Scandinavia, than in the US and UK, see Björklund and Jäntti (1997, 2000), Solon (2002), Bratberg et al. (2005, 2007), Bratsberg et al. (2007).[1] Even though the intergenerational earnings correlations are low in Norway, parental income could have a causal effect, albeit small in size. It could also be the case that an employment shock to the family affected the cognitive environment of children or youths and thus had a "nurture effect" that could show up at a later age. An exogenous income shock would help to identify such effects. On the other hand, if no intergenerational effects of a displacement are found, it adds to the evidence that the Scandinavian egalitarian welfare model reduces the effects of family background and potential credit constraints that seem to be more important in North

[1] The analysis by Oreopolous et al. (2005) using Canadian data finds correlations closer to European results. This fact they attribute to their conditioning on stable workers before firm closings.

America. We do not find any statistically significant effect of fathers' displacement on the intergenerational earnings correlation.

The rest of the paper proceeds as follows. Section 2 presents the empirical strategy. Section 3 describes the data used. The results are presented in Section 4, while Section 5 offers some concluding remarks.

2. Empirical approach

The common approach in studies of intergenerational income mobility is to run a regression of some measure of the young generation's earnings on a measure of their parents' earnings. The interpretation is that the stronger the effect of parent earnings is, the less is the intergenerational mobility. The earnings measures are meant to be approximations of lifetime income, and the underlying economic model is often thought of as utility-maximizing families that invest parents' earnings in children's human capital. Estimating a relationship that is clearly related to such an underlying model is not straight-forward, however. First, the young generation's outcome is not related to investment in education, but also to unobservable endowments ('nature and nurture'), in addition to random shocks. Second, in practice only a few years of earnings are available. When income from one or a few periods is interpreted as a measure of permanent income, a classical error in variables problem arises because each period contains random variation around the permanent part, causing attenuation bias. From the early nineties, following the work of Solon (1992) and Zimmerman (1992), much attention has been focused on how to alleviate this bias, typically by using income averages over several years or instrumental variables. Third, even if these problems were solved, the parent-child regression is a reduced form, and a positive correlation does not necessarily have a causal interpretation. As noted in the introduction, the focus in this paper is on the latter

problem, and we use a shock to family income as a source of exogenous variation to explore the effect on the next generation.

We proceed in two steps: first we consider the economic effects of displacement for those affected. Then, contingent on identifying such effects, we assess the impact on the next generation. The idea is that displacement represents an exogenous shock to family economy, and if this shock affects the children of those displaced it is evidence that economic resources have a direct effect on children's economic outcome. This empirical strategy follows Oreopolous et al. (2005). In the first step, we take advantage of having access to a long earnings panel together with a rich set of background characteristics and compare earnings trajectories for workers who were displaced to workers who were not. The motivation for using displaced workers, and not workers who quit voluntarily or were fired, is to avoid selection on unobservable abilities. But this is no guarantee: in downsizing firms the least productive workers may have to leave first (or the most productive may leave voluntarily), and workers with good alternative job prospects may leave firms where closure threatens. If we confuse low worker productivity with a shock to the firm, we may overstate the effects of the shock. To reduce this possibility, we pick workers with a stable relationship to the firm. Furthermore, we include pre-displacement observations in the analysis. We consider workers who were – or were not – displaced in 1986-1987, and use 1982-1994 as our observation period. The estimated equation is

$$Y_{it} = \alpha + \sum_{t=1982}^{1994} \gamma_t D_{it} + \varepsilon_{it} \,, \tag{1}$$

where D_{it} is an indicator of whether displacement took place interacted with year dummies; i.e. leads and lags of the displacement dummy. Y_{it} is father's log earnings, demeaned by first regressing log annual earnings on year, age, and industry dummies and using the residual

from this regression in equation (1).[2] We expect γ_t to be negative in the years after displacement if there is a negative income effect, but if γ_t is different from zero also in the years before displacement, it indicates that the negative effect after displacement may be due to selection. OLS and fixed effect (FE) results will be reported. The advantage of FE is that time constant unobserved heterogeneity, e.g. differences in worker productivity, is swept out of the model.

An empirical finding in a previous Norwegian study (see Huttunen et al., 2006) is that the employment effects of displacement may be more important than the earnings effects. Therefore, we also estimate similar equations for non-employment and recorded unemployment. However, as we condition on workers with a stable relationship to the firm before 1986, these regressions are only for the post-displacement years.

In the second step of the analysis, we include a dummy for fathers' displacement in a regression of child earnings on father earnings. It is well known that using single year earnings of parents may seriously bias the estimates, see Solon (1992) and Zimmerman (1992), thus typically averages over several years are used. We follow that practice. There is also a growing understanding that using child outcomes when they are too young may induce life cycle bias, see Haider and Solon (2006), Grawe (2006). We then face a trade-off: on the one hand we wish the children to be young enough to potentially be affected by a shock to family economy and employment; on the other hand we want to avoid observing their own labour marker outcomes at a too early stage in their career. But if the life-cycle bias is the same for displaced and non-displaced workers, we can still get an unbiased estimate of the difference in the intergenerational mobility between the two groups. We estimate the following equation,

[2] Alternatively, the controls could be included in equation (1), but the industry controls would have to be dropped in a fixed effect regression.

(2) $$\bar{Y}_i^{c99-01} = \alpha + \beta \bar{Y}_i^{f82-85} + \delta D_i^{f86} + \theta X_i + u_i .$$

Here, $\bar{Y}_i^{c99-01}$ is average log earnings of the child 1999-2001, $\bar{Y}_i^{f82-85}$ is fathers' average log earnings 1982-1985, and β is the intergenerational earnings elasticity. D_i indicates that the father was displaced in 1986-87, and X_i is a vector of dummy variables controlling for gender and birth cohort of the child, and father's industry in 1986, before potential displacement. δ measures the effect of this childhood shock to the family economy on the next generation as young adults.

Our data permits us to follow child earnings until 2001. We have chosen to work with the 1971-74 cohorts, who were 12-15 in 1986, and observe their earnings in 1999-2001, when they were 25-30 years old. The next section explains data in more detail.

3. Data and sample

Our data source is a full population database of matched employer-employee data, constructed by merging several administrative registers. The core is individual background information for the years 1986-2001, moreover, gross earnings based on tax records are available from 1967 on.[3] For individuals in the labour force, the data contains identifiers that make it possible to merge firm information at the plant level. For our purposes, it is convenient that this information includes the date a job started. Furthermore, it is possible to link parents and children by personal identifiers.[4]

In the present study, we wish to construct a sample of fathers with a stable attachment to the labour force, who were susceptible to displacement, with children young enough to be

[3] In addition to earned income, the earnings measure includes unemployment insurance, disability benefits, and sick pay, but not means tested social assistance.
[4] See Møen et al. (2003) for a closer account of the data.

affected by this possible event, and old enough to be observed with earnings in the sample period. 1986 is the first year for which we have information on firms, thus we can only identify downsizing and closure from 1987 on. These concerns lead us to extract the following sample. We include men born 1930-1950, with children born 1971-1974. Furthermore, fathers are only included if they have a valid plant identifier in 1986, are working full time, have tenure with the plant since at least 1983,[5] and if that plant had at least five employees. Finally, we have excluded individuals from the petroleum industry because of the volatility of the sector, with a multitude of births and deaths of firms compared to the other sectors. Moreover, the average wage level in this sector is so high that it would affect the average wage level of displaced workers in our analysis.

With 1986 as base year we identify plants that have a reduction in their labour stock of at least 30% from 1986 to 1987 or closed down. A plant is defined as closed if the plant identifier is no longer present in the data.[6] Fathers are then classified as displaced if the plant has closed down or if the plant downsizes and an individual is no longer with the plant in 1987. It should be noted that the data are updated yearly by the end of May, thus plants downsize or close down between May 31, 1986 and May 31, 1987. Our treatment group consists of fathers who were displaced in that period. As noted in the previous section, in the father-child regressions, we condition on fathers' earnings measured as the average of the years 1982-85, excluding years with zero earnings.[7] We use log earnings in the estimations, and the averages are over the logs, i.e. not log of averages The year 1986 is avoided, as the plant may have closed down sometime after May 31 in that year.

[5] The data contains job start dates, thus we may compute tenure for jobs that started before 1986.

[6] If the plant reappears in later years we consider it a data error and do not classify the plant as closed. Also, if a majority of the workers from a disappeared plant show up with the same plant id, we interpret this as reorganization and do not count the plant as closed.

[7] Given that we focus on the difference between displaced and non-displaced workers, and assuming that the occurrence of zero earnings is distributed randomly between the two groups, the point that the estimated intergenerational mobility is sensitive to the selection rule (see Couch and Lillard (1998) and Corak and Heisz (1999)) should not affect the estimate of the displacement

In addition to earnings effects, we explore the effects of displacement on non-employment and registered unemployment. Non-employment is defined simply as having no valid plant identifier in the current year. Unemployment is months of registered unemployment in the current year.[8] Obviously, our definition of non-employment encompasses individuals who are registered as unemployed, but it also includes individuals who are outside the labour force. Furthermore, as our data are updated in the end of May, an individual who is classified as non-employed in a given year may have found work later in that year.

As noted above, we consider average earnings 1999-2001 for children, computed in the same way as for fathers. Because children are still of an age – 25-30 – where a certain fraction may be undertaking education, we condition on not being in that category. This finally gives us a sample of 58,853 father-child pairs.

(Table 1 about here)

The descriptive statistics for fathers in Table 1 are based on data from 1986, which was is first year with complete background characteristics for the fathers. Non-displaced workers have higher education than those displaced. Earnings are somewhat lower for the two treatment groups. However, when we compare with the earnings trajectories in Figure 1 below, this appears to be a case of "Ashenfelders dip": there are hardly any sign of earnings differences before 1985. The main difference between the samples is the sectors of which they are employed. Employees who experience displacement have to a much higher degree their background in the manufacturing sector, while the share of public sector employees is more than four times as high for the non-displaced compared to the displaced. This reflects two

[8] It is necessary to register as unemployed to obtain unemployment benefits. This variable is only available from 1988.

central features of the labour market in the period under study: a massive downsizing of the manufacturing sector due to globalisation, outsourcing, etc, and a significant labour protection against displacement in the public sector. The descriptives for children show that children of displaced workers have less education and lower earnings than children of workers who did not offer a job loss. The analysis in the next section will explore whether this earnings difference may be attributed to income shocks following their fathers' displacement in 1986-87.

4. Results

(Figure 1-3 about here)

We start by inspecting descriptive evidence on the effects of displacements on fathers' outcomes. Figure 1 shows earnings trajectories 1982-1994 of non-displaced and displaced workers. We also show separately workers displaced due to closure. The trajectories start out quite similar, but displaced workers experience a drop starting in 1986. Earnings of displaced workers are slightly below those of the non-displaced in the beginning of the observation period. Workers who were displaced because of closure are, on the other hand, more similar to non-displaced. The difference is modest – 1991 earnings of displaced workers are about 7% below 1986 earnings – but it also seems that displacement gives a lasting negative effect compared with the rest. In 1994 the earnings difference is about 9%. We also note that the trajectories follow the business cycle: a recession started in 1996 which lasted until 1993. Average earnings for all groups start to catch up from 1992 for all groups but faster from the displaced. One explanation may be that unemployed individuals get jobs and move from unemployment benefits into paid work.

Figure 2 shows non-employment shares. As expected, for the displaced this share is well above the others in 1987, 45% vs. 3%. The difference decreases, but in 1994 it is still 10 percentage points and another 4 points for workers from closed plants. The trajectory for non-displaced workers shows a positive trend which reflects ageing of the sample, leading to a larger fraction of retirement. Figure 3, which traces average weeks of unemployment (including those with no unemployment), tells a similar story, and again, it is workers from closed plants who fare the worst.

(Table 2 about here)

As we argued in Section 2, there may be systematic selection of workers with low earnings capabilities into plants that are in danger of closing down. Therefore, we estimate equation (1), which is a regression of yearly earnings 1982-1994 on leads and lags of the displacement dummy. We use two versions of the displacement dummy: one for all displaced, and one for displaced workers from closed plants. Table 2 reports the results. The OLS results for all displaced workers indicate a negative and statistically significant effect of displacement for all years after 1987 (period 0 in the table). The negative effect begins in 1986; this is as expected because the observation period for plants is May to May. The FE results are similar, but with a significant coefficient also in 1985, raising suspicions of pre-displacement differences. Turning to workers who were displaced from closing plants, there are no significant differences from other workers before 1996, but significant negative effects in the years thereafter. The effects last through the observation period, in accordance with the descriptive evidence in Figure 1. The effects are modest in size, though, largest in 1991 with -0.163 (FE), according to a negative effect of 15%.

(Table 3 and 4 about here)

Table 3 reports results for non-employment.[9] As a criterion for inclusion in the sample is stable labour force attachment before 1986, thus we can only estimate this outcome from 1987. Displacement is associated with increased risk for non-employment, for workers displaced due to closure as well as the broader group which includes workers from downsizing plants. However, the FE estimates are not significant in all years. We cannot conclude that the increased non-employment risk is because of displacement and not because of unobserved properties, but the results from the earnings regressions suggest that the latter is not the case. Table 4 shows the same exercise for months of unemployment.[10] OLS estimates show an increase in unemployment after displacement. For the largest displacement group the effect vanishes in the FE results. This tendency is less clear-cut for workers from closing plants.

Summing up this part of the analysis, we have found that displacement is followed by reduced earnings and unstable employment. Even though selection problems cannot be completely ruled out, we find the evidence is quite convincing that these unfavourable outcomes are caused by displacement. Reasonably, this evidence of exogeneity is clearest for workers who experience that their plants close down.

(Table 5 about here)

Table 5 finally shows results from regressing child outcomes on fathers' earnings, for the full sample and by gender. For each group, the first column shows the intergenerational earnings elasticity without the displacement dummy, column 2 introduces the dummy for all

[9] As this is a linear probability model, the coefficients may be interpreted directly as marginal effects.
[10] No correction has been made for the large number of zeros.

displacement, and column 3 replaces this with the closure dummy. We first note that the elasticities are fairly low: 0.12 on average, but only 0.05 for men, and 0.19 for women. Even for individuals in their late twenties these are low numbers – Oreopolous et al. report 0.383 for individuals of comparable age. Several previous Scandinavian studies, like those mentioned in the introduction, find small elasticities, however, and our particularly low numbers may probably be attributed to the low young age at which the children are observed in this study.[11] Our main interest is in the displacement dummies, however. As it turns out, neither of the displacement dummies has any effect. The elasticities remain unchanged, and no estimates of δ are anywhere near being significant.[12] This is clearly contrary to Oreopolous et al., who find large and significant effects in Canada. One potential explanation for these diverging results may be higher female labour force participation in Norway. This will *ceteris paribus* give a smaller effect of reduced income caused by displacement, since men's earnings as share of household income might be smaller in Norway than in Canada.

The conclusions in this section are i) that displacement have negative effects on earnings and employment, ii) it is possible to argue that these effects are not due to unobservables, iii) this exogenous shock has no effects on the earnings of the next generation. Thus this study does not provide evidence that family income as a child is an important determinant of earnings as an adult.

[11] Bratberg et al. (2005) find somewhat lower elasticities for women but higher for men at age 30 for the Norwegian 1965-cohort. In addition to cohort differences the discrepancy may also be due to the conditioning on stable workers.

[12] We have checked for nonlinearities by including second and third order terms in fathers' earnings (see for instance the discussion in Solon (1992)). It turns out that a third order polynomial function of fathers' earnings is significant. This is in line with the findings of Bratsberg et al. (2007). More important, it does not affect the insignificant results of father's disclosure on children's earnings.

5. Concluding remarks

Using matched employer-employee data from Norway, we analyse the effects of worker displacement in 1986-87 on children's earnings and labour force attachment in 1999-2001 for more than 47,000 father-child pairs. Seven years after displacement, average earnings of those affected are still below their pre-closure earnings. There is also a gap between displaced and non-displaced workers in the share of non-employment. However, it turns out that this has no discernible effect on their children when they are in their late twenties: regressing an indicator of this event, together with fathers' earnings and other controls, on children's earnings or non-employment yields no effect. This result deviates from the findings of Oreopolous et al. (2005), who find clear effects using similar Canadian data. Rigorous testing of the differences between the two countries is beyond the scope of the present paper. The following heuristic arguments might, however, shed some light on these findings: First, our estimate of the intergenerational earnings elasticity is low, a fact that may be attributed both to the generally high intergenerational earnings mobility in Scandinavia and the relatively young age of which the children in our sample are observed.[13] Second, as for the (part of the) parent-child correlations in earnings that is attributed to parents investments in the offspring's education, it should be noted that education in Norway is free at all levels, and the government provides student loans and scholarships with favourable conditions. Thus, financial constraints are less important in educational choices. Furthermore, for those who do invest, the returns to education in Norway are low compared to most other countries.

As earlier mentioned, the actual transmission mechanism between generations is not well known. In addition to the effect through education, economists realise that there are mechanisms that work through "nature and nurture." For example, it could be the case that an

[13] It is well known that earnings at young ages may be poor indicators of lifetime earnings (see for instance Haider and Solon 2006, Grawe 2006; Lee and Solon 2006). The children in our sample are on average slightly younger than in the sample of Oeropolous et al.

employment shock to the family affected the cognitive environment of children or youths and thus had a “nurture effect” that could show up at a later age. However, our results do not indicate any such effects.

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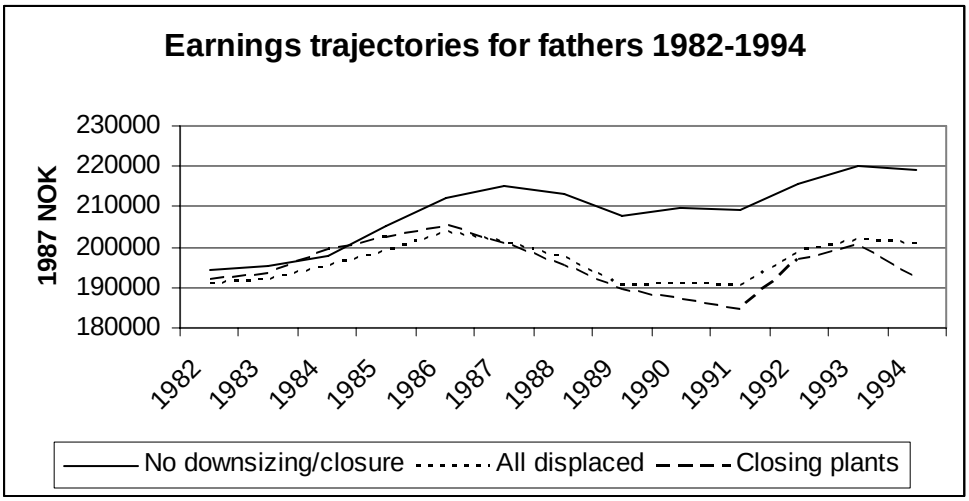

Figure 1

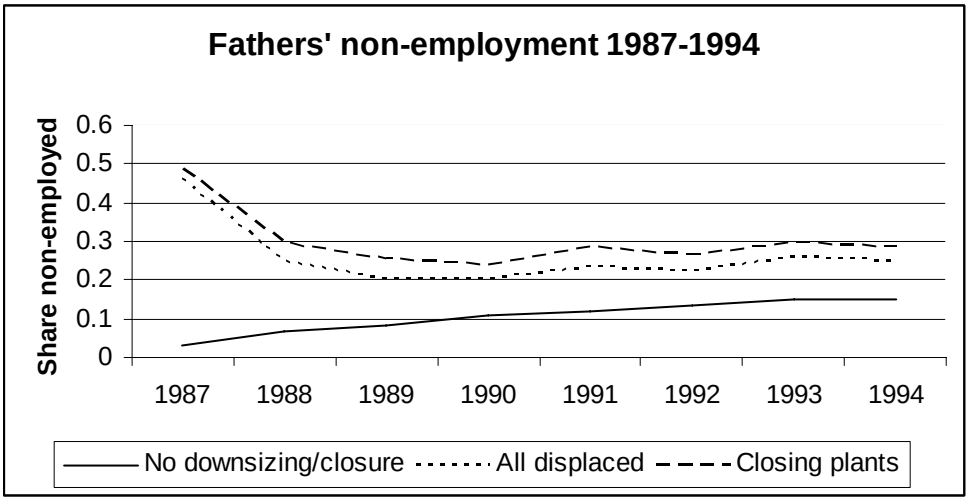

Figure 2

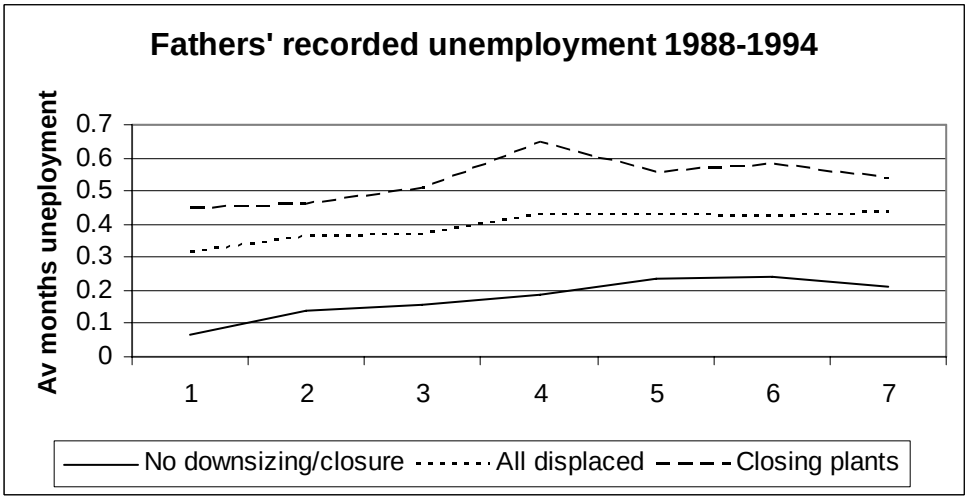

Figure 3

Table 1 Descriptive statistics

	Fathers in 1986					
	Non-displaced		All displaced		Displaced from closing plant	
	Mean	Std.Dev	Mean	Std.Dev	Mean	Std.Dev
Age	43.0	4.9	42.9	4.9	43.4	4.9
Earnings	212112	88898	203722	79880	205255	86694
Education (years)	11.3	3	10.4	2.5	10.4	2.6
Sector						
Manufacturing	0.35	0.48	0.45	0.5	0.44	0.5
Electricity	0.02	0.14	0.01	0.1	0.03	0.18
Construction	0.09	0.28	0.19	0.39	0.13	0.34
Wholesale	0.13	0.34	0.13	0.33	0.12	0.32
Transport	0.07	0.26	0.11	0.31	0.12	0.33
Finance	0.08	0.27	0.05	0.22	0.1	0.3
Public	0.26	0.44	0.06	0.25	0.06	0.23
Plant size	57.1	119.5	70.8	104.3	38.9	63
N	45089		2052		572	
	Children in 2001					
	Mean	Std.Dev	Mean	Std.Dev	Mean	Std.Dev
Age	28.6	1.1	28.6	1.1	28.6	1.1
Earnings	254525	139143	251891	131271	252630	142342
Education (years)	12.6	2.9	12.3	2.9	12.1	3.0
Female	0.48	0.50	0.49	0.50	0.48	0.50
N	56367		2486		720	

Table 2 Effects of displacement on fathers' log earnings 1982-1994

All displaced					Displaced from closing plants			
	OLS		FE		OLS		FE	
Years since displacement	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
-5	0.008	0.010			0.004	0.018		
-4	0.009	0.010	-0.001	0.009	0.001	0.018	-0.001	0.017
-3	0.010	0.010	0.000	0.009	0.008	0.018	0.004	0.017
-2	-0.012	0.009	-0.022	0.009	-0.006	0.018	-0.011	0.017
-1	-0.029	0.009	-0.038	0.009	-0.041	0.018	-0.043	0.017
0	-0.070	0.009	-0.084	0.009	-0.096	0.018	-0.105	0.017
+1	-0.069	0.010	-0.092	0.009	-0.097	0.018	-0.114	0.017
+2	-0.067	0.010	-0.094	0.009	-0.079	0.018	-0.104	0.017
+3	-0.064	0.010	-0.096	0.009	-0.120	0.018	-0.145	0.017
+4	-0.088	0.010	-0.121	0.009	-0.163	0.018	-0.197	0.018
+5	-0.057	0.010	-0.073	0.009	-0.081	0.018	-0.095	0.017
+6	-0.052	0.010	-0.071	0.009	-0.101	0.018	-0.113	0.017
+7	-0.066	0.010	-0.084	0.009	-0.108	0.018	-0.122	0.017
N				47230				
NxT				605658				

Dependent variable is father's log earnings demeaned by age, year, and pre-displacement industry. Regressors are leads and lags of dummy for displacement in 1987. 'All displaced' refers to displacement due to plant closure or downsizing. Years = 0 in 1987

Table 3 Fathers' non-employment 1987-1994

	All displaced				Displaced from closing plants			
	OLS		FE		OLS		FE	
Years since displacement	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
0	0.412	0.007	0.324	0.007	0.437	0.013	0.318	0.014
1	0.173	0.007	0.085	0.007	0.220	0.013	0.101	0.014
2	0.117	0.007	0.029	0.007	0.165	0.013	0.046	0.014
3	0.088	0.007	0.001	0.007	0.129	0.013	0.010	0.014
4	0.107	0.007	0.020	0.008	0.156	0.013	0.036	0.014
5	0.087	0.007	-0.001	0.008	0.124	0.013	0.004	0.014
6	0.102	0.007	0.014	0.008	0.135	0.013	0.015	0.014
7	0.096	0.007			0.129	0.013		
N				47230				
NxT				377840				

Linear probability model where dependent variable indicates no valid firm id in current year. Demeaned by age, year, and pre-displacement industry. Regressors are lags of dummy for displacement in 1987. 'All displaced' refers to displacement due to plant closure or downsizing.

Table 4 Fathers' recorded unemployment 1988-1994

	All displaced				Displaced from closing plants			
	OLS		FE		OLS		FE	
Years since displacement	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
1	0.196	0.023	0.028	0.027	0.347	0.044	0.058	0.050
2	0.177	0.023	0.008	0.027	0.289	0.044	-0.003	0.051
3	0.166	0.023	-0.002	0.027	0.319	0.044	0.027	0.051
4	0.191	0.023	0.022	0.027	0.422	0.044	0.128	0.051
5	0.149	0.023	-0.022	0.027	0.286	0.044	-0.008	0.051
6	0.128	0.023	-0.042	0.027	0.304	0.044	0.009	0.051
7	0.175	0.023			0.294	0.044		
N				47230				
NxT				330610				

Dependent variable is months of recorded unemployment in current year. Demeaned by age, year, and pre-displacement industry. Regressors are lags of dummy for displacement in 1987. 'All displaced' refers to displacement due to plant closure or downsizing.

Table 5 Effects of fathers' displacement and log earnings 1982-1985 on children's average log earnings 1999-2001

	All			Men			Women		
Father's log earnings	0.118 [0.015]	0.118 [0.015]	0.118 [0.015]	0.052 [0.020]	0.053 [0.020]	0.053 [0.020]	0.187 [0.023]	0.187 [0.023]	0.188 [0.023]
Father displaced due to downsizing or closure	-	-0.004 [0.019]	-	-	0.003 [0.023]	-	-	-0.011 [0.031]	-
Father displaced due to closure	-	-	-0.012 [0.034]	-	-	0.031 [0.040]	-	-	-0.065 [0.057]
N	58853			30397			28456		

Standard errors in brackets. Age adjusted earnings measures. Controlled for father's industry in 1986 and child's cohort (1971-1974)

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