The Intergenerational Effects of Worker Displacement

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This article uses variation induced by firm closures to explore the intergenerational effects of worker displacement using a Canadian panel of administrative data that follows more than 39,000 father-son pairs from 1978 to 1999. We find that children whose fathers were displaced have annual earnings about 9% lower than similar children whose fathers did not experience an employment shock. They are also more likely to receive unemployment insurance and social assistance. The estimates are driven by the experiences of children whose family income was at the bottom of the income distribution.

It is well known that children from affluent families tend to have higher incomes as adults than children who grow up in poor families (Solon 1992; Zimmerman 1992). This pattern has convinced many social scientists

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and policy makers that family income plays an important role in determining children's life chances. Duncan and Brooks-Gunn (1997, 608), for example, suggest that raising the incomes of poor families "will enhance the cognitive development of children and may improve their chances of success in the labor market during adulthood." Policy discussions often invoke the legacy of growing up in a poor family as evidence of the potential effectiveness of income transfer programs such as Aid to Families with Dependent Children. Despite such claims, the process that generates the intergenerational income correlation is not well understood. One possibility is that differences in income lead to differences in parents' monetary investments in their children. It is just as likely, however, that differences in income reflect differences in innate parental characteristics that parents pass on to their children.

Certainly the magnitude of the intergenerational correlation is hard to ignore. Solon's (1999) survey of the intergenerational mobility literature suggests that the correlation between fathers' and sons' earnings is about 0.4. At issue is the extent to which this correlation reflects the importance of monetary versus innate family background characteristics. High-income parents are likely to have other attributes, such as high ability or motivation, that independently have a positive effect on their children's outcomes. Cross-sectional comparisons of individuals who grew up in families with different income levels are thus likely to overstate the degree to which family resources matter.

This article examines the effect of firm closings on the next generation's outcomes.[1] Jacobson, LaLonde, and Sullivan (1993) and Stevens (1997) have documented that workers displaced by such events experience substantive long-lasting reductions in earnings, and they argue that firm closings can be thought of as exogenous employment shocks after conditioning on predisplacement earnings. Our estimation strategy constructs narrow treatment and control groups of men whose fathers had the same levels of permanent income and worked in similar types of firms prior to the 1980s, when some of the fathers were displaced. Like previous studies, we find that displacement leads to permanent reductions in family income.

Comparing outcomes among sons whose fathers experienced an employment shock to outcomes among individuals whose fathers did not, we find that sons whose fathers were displaced have annual earnings that are about 9% lower than similar children whose fathers did not experience an employment shock. They are also more likely to receive unemployment insurance. These estimates are driven by the experiences of children whose family income was at the bottom of the income distribution. The results

[1] When we include mass layoffs in our treatment group, we obtain very similar results.

suggest that the long-term consequences of unexpected job loss extend beyond the effect on one's own income to the eventual labor market outcomes of one's children.

Because our identification strategy is based on an unexpected income shock, our estimates are not driven by a correlation between family income and innate family background characteristics that cannot be observed. Nevertheless, it is important to acknowledge that our estimates may reflect displacement effects beyond the strict loss of income. For example, although we find that displacement has a minimal impact on mobility, marital status, and spousal income, we cannot conclusively rule out the possibility that the stress of losing one's job (even without an associated income loss) influences family dynamics in ways that negatively impact children's economic prospects. Displacement may also affect the next generation's labor market opportunities by changing parents' employment/social networks and through role modeling; long periods of parental unemployment, for example, may affect children's own attitudes about work.

I. Background

Understanding which factors contribute to the intergenerational transmission of income is a crucial part of developing successful antipoverty policies. In the United States there are a number of programs designed to help low-income children, including Temporary Assistance for Needy Families, Medicaid, Head Start, food stamps, and public education. Some of these involve income transfers, whereas others are direct-intervention programs. Mayer (1997) notes that throughout the history of the United States social policy has swung back and forth between the belief that material deprivation is the primary reason that poor children have poor outcomes and the belief that parental characteristics that contribute to low incomes are mostly responsible for poor children's failure. Informed social policy depends critically on understanding which of these beliefs is correct. If, for example, money is a key determinant of children's outcomes, then the effects of policies on family income should be a central consideration when evaluating their costs and benefits. However, if children's outcomes are mostly determined by innate parental characteristics that are correlated with income, then social policy should be less concerned with income redistribution and focus more on addressing deficits in the other characteristics.

A number of cross-sectional studies show that positive income correlations remain even after controlling for a variety of parental characteristics (e.g., Hill and Duncan 1987; Corcoran et al. 1992), but these correlations are likely to overstate the degree to which parental income matters if some parental attributes that are positively correlated with income and children's outcomes cannot be observed. It is difficult to find compelling variation in income that is unrelated to parental characteristics that might affect child development (Haveman and Wolfe 1995; Duncan and Brooks-Gunn 1997). Shea (2000), for example, uses cross-sectional variation in fathers' earnings due to union status, industry wage differentials, and involuntary job loss to identify the effects of parental income, but other researchers (Lee 1978; Dickens and Katz 1987; Gibbons and Katz 1992) have argued that wage differences associated with these job characteristics reflect workers' innate attributes. Dahl and Lochner (2005) also create an instrument for income that includes family background characteristics (as well as changes in income induced by the Earned Income Tax Credit [EITC]).[2] Mayer (1997) controls for unobserved parental characteristics by adding to her regression a measure of parental income taken after the child's outcome is observed. She argues that future income is exogenous with respect to a previously measured outcome, so that it can serve as a proxy for the unmeasured components of family background. The success of this strategy requires that parental investment when the child is still at home is not influenced by the anticipation of future income, however. Blau (1999), Duncan et al. (1998), and Levy and Duncan (2000) all compare outcomes across siblings with different age-specific family income levels, but this approach can only identify the effect of transitory changes in family income, and it has been well documented that permanent income has a much stronger relationship with children's long-term outcomes (Solon 1999).[3] The approach used by Morris, Duncan, and Rodrigues (2004), who exploit welfare-to-work field experiments that caused some single mothers to receive more income than others, may come closest to isolating the causal effects of income. Data constraints, however, limit their analysis to a narrow subgroup of children (children of welfare participants) and short-run outcomes.

Most existing studies that explore the effects of family resources on offspring's outcomes have been based on longitudinal data sets such as

[2] The extent to which Dahl and Lochner's identification is driven by the EITC vs. these other family background characteristics is unclear to us.

[3] A very different approach is taken by Acemoglu and Pischke (2001), who use long-run trends in earnings levels at different points in the U.S. earnings distribution (and in different geographic regions) to generate exogenous income variation. They find that demographic groups with more sharply rising incomes also experienced larger increases in their children's educational attainment. One puzzle with these results is that, in their preferred specification, the return to education has no significant effect on college enrollment decisions. This may reflect the fact that their framework makes it difficult to control for both the current return to education and the change in parental income, since both are driven by the same national trends in returns to skill. The rise in return to skill should affect children's education directly, and so their strategy may not provide a valid instrument for changes in parental income.

the Panel Study of Income Dynamics and the National Longitudinal Study of Youth, which are relatively small. It has turned out to be difficult to precisely identify parental income effects with so little data. The confidence interval around Shea's instrumental variables (IV) estimate of the effect of father's earnings on children's earnings, for example, includes effects ranging from approximately −30% to 30%. Solon's (1999) comparison of sibling and intergenerational earnings correlations suggests that no more than 40% of the similarity in brothers' outcomes is likely due to factors related to their parents' income. While this leaves open the possibility that family income plays an important role in children's development, it also suggests that, when the data are limited to only a few thousand observations, the effect may not be strong enough to estimate precisely.

This article makes several contributions to the growing literature on intergenerational income mobility. First, by exploiting variation that is induced by firm closings, we can separate the effect of a long-lasting income shock from the effect of innate parental attributes. While cross-sectional differences in fathers' labor market characteristics are likely to reflect individual attributes, our longitudinal data allow us to construct narrow groups of "treatment" and "control" children whose families look identical before the period 1980–82, when some of the fathers lost their jobs. We base our analysis on a sample of children in a Canadian administrative data set whose fathers worked at the same firm in both 1978 and 1979, and we control for average family income, regional location, industry, and firm size during those years.[4] Thus, we are able to compare outcomes across children whose families would likely have had the same level of permanent income if the treatment fathers had not been displaced. A second advantage of our study is that it makes use of a longitudinal data set that contains earnings and income observations on more than 39,000 father-son pairs. The size of this data set substantially increases the precision with which intergenerational relationships can be estimated.

II. Empirical Strategy

We regress a measure of the child's economic well-being on average family income between 1978 and 1979 and a dummy variable (Shock)

[4] Our original analysis (and an earlier version of the article) was based on fathers who worked continuously at the same firm between 1978 and 1981, some of whom experienced displacements in 1982. The advantage of focusing on this sample relative to the sample used in the article is that there is a longer "before" period over which to match the treatment and control groups. The disadvantage is that there are fewer displacements with which to identify the effects, and the outcomes of the treated children are observed at slightly younger ages. The two samples produce very similar results.

indicating whether the father experienced a plant closing between 1980 and 1982:

$$O_i = a + b\text{Avg Inc}_{78-79} + c\text{Shock}_i + \varepsilon_i,$$

where O_i represents an economic outcome for child i. Thus, we compare outcomes across children whose fathers experienced a job loss to outcomes for those whose fathers did not, controlling for family income in the predisplacement years. We also include a number of additional firm, region, and industry control variables that will be discussed in the next section. The key to this identification strategy is the assumption that after conditioning on fathers' income prior to the shock, firm characteristics, industry, and region, the families that experienced a displacement were ex ante no different from those who did not.

III. Data

Our analysis is based on data from the Intergenerational Income Database (IID), which is maintained by the Family and Labour Studies Division of Statistics Canada. The IID links tax information on children born between 1966 and 1970 to data on their parents for all years between 1978 and 1999. The links were made possible using the T1 Family File (T1FF) of the Small Area and Administrative Data Division of Statistics Canada.

The T1FF is a data set of individual tax records that has been processed in a way that matches members of each tax filer's family. The primary way in which children are matched to their parents is through their name and address. In order to be identified as living in the same family, the child must file from the same address as the parent at least once during a 5-year period beginning when the child is 16–19 years of age. Evidence presented in Oreopoulos (2003) suggests that this matching process picks up most adolescents in Canada. Younger children are more likely to be living at home but less likely to file a tax return.[5] All Canadians must file a tax return if they pay income tax in that year or if they claim unemployment insurance benefits, a nonrefundable tuition tax credit, or the monthly deduction for enrollment in a full-time education program. Since a child need only file once over a 5-year period in order to be included in the sample, the vast majority of children make it into the IID. From the 1981 Canadian census, 96% of 17-year-olds lived with a parent, and 53% received nontransfer income in the previous year. Over 80% of 20-year-olds lived with a parent, and 73% of them received nontransfer income. Oreopoulos (2003) reports that the database includes 72% of

[5] Note that a child may live away from home but still file from a parent's address and thus be included in the IID. Children who are away at college fall into this category.

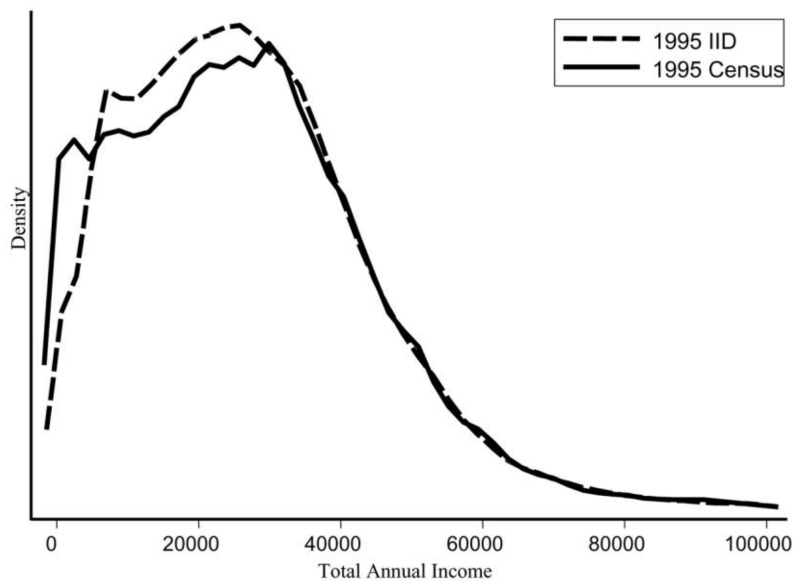

Fig. 1.—Comparison of IID sample to 1996 census: kernel densities for total income among 25–32-year-olds in 1995.

youth who were ages 16–19 in 1982, 1984, or 1986 (the years in which the matches took place).

One way of exploring the representativeness of our sample is to compare the income distribution for children later in life to the income distribution for the same cohorts in the census. Figure 1 plots 1995 income distributions for 25–32-year-olds in the IID and the 1996 census (which includes income measures for 1995). It is clear that the IID misses a fraction of children who ultimately end up at the low end of the income distribution. In Section IV, we show that this is the group for whom our results are strongest, however, so it is likely that, if anything, this omission biases our estimates toward zero.

We have also examined whether there are differences in family size between those households with fathers who worked in firms that closed versus those that did not. If there is selection into our treatment and control groups, we might expect to see a correlation between family size (based on the number of children who were matched to their parents) and the corresponding father's displacement category. We do not find this. For example, in 1982, the average number of children aged 12–19 per family is 1.81 among those whose fathers are classified as displaced and 1.82 among those whose fathers are not.

The IID provides detailed administrative data on the incomes of children and their matched parents from 1978 to 1999. It also includes information on their age, gender, marital status, family composition, and residential address, as well as an identification number for the firm at which the individual is employed. This ID number is used to match fathers in the IID to information about their firms from Statistics Canada's Longitudinal Employment Analysis Program database (LEAP). LEAP is a company-level database that includes all employers in Canada, both corporate and unincorporated. The database tracks the employment and payroll characteristics of individual firms from their year of entry to their year of exit.[6] Employers in Canada are required to register a payroll deduction account and issue a T4 slip to each employee that summarizes earnings received in a given fiscal year. The LEAP database includes every business that issues a T4 taxation slip.

Due to restrictions at Statistics Canada, only those already in the IID are matched to the LEAP firm identification number. We cannot obtain information on the total number of employees working at firms that are linked to individuals in the IID, so we approximate firm size with the number of IID fathers in the same firm in a given year. Our sample also includes a three-digit industry code for each firm and province location of the firm's head office.

The longitudinal nature of the matched IID allows firm entry and exit to be identified on an annual basis. A firm closure is assigned in a given year if there are no IID fathers working at the firm in any later year (through 1999).[7] In order to distinguish true closures from company reorganizations that lead to new identification codes, however, we do not count a firm as being closed if 35% or more of its workers move to the same "new" firm.[8]

Our main sample is limited to sons who were between the ages of 10 and 14 in 1980.[9] Information on older children is available in the IID, but we do not include these children because they are likely to have left

[6] Self-employed persons who do not draw a salary are not included in the LEAP database. In addition, businesses composed solely of individuals or partnerships who do not draw a salary are also excluded from the LEAP.

[7] This may lead to some misclassifications. For example, if a firm disappears in 1983, we will identify the closing year as 1982. In some cases, the firm may have closed early enough in 1983 that it did not file T4 slips.

[8] We examined the sensitivity of the results around this threshold using alternative values of 15% and 50%. Coefficient estimates for the main tables were similar. The standard errors were somewhat larger for the 15% threshold. We also explored displacement through mass layoffs. Identifying mass layoffs is more difficult than firm closures because we can only observe what fraction of the firm leaves in between years, and this fraction depends on the firm's size. Firms can choose who leaves, and for this reason we chose to focus on firm closures.

[9] The IID is a sample of 16–19-year-olds matched to parents in 1982, 1984, and 1986. We restrict this sample to children 14 years old or younger in 1982.

home shortly after the displacement occurred. Information on younger children is not available. Like most of the intergenerational correlations literature, we focus on sons, because daughters' earnings may not be a valid indicator of their labor market success. Children whose fathers are missing tax data are eliminated because without the tax data we cannot observe their income, place of employment, or labor market status. This restriction reduces the sample of fathers by about 11%. We also restrict the sample to sons whose fathers were between the ages of 30 and 50 in 1978. This ensures that we are focusing on fathers whose incomes would have been relatively stable: earnings of young workers tend to be more volatile than those of workers who are over age 30, and Stevens (1997) shows that the long-term effects of displacement are largest for workers with more years of tenure.

We also restrict our main sample to fathers who are initially working at firms that employed between two and 500 men (in the IID). We require that the firm employ at least two fathers because one-father firms will include self-employed fathers and, in order to reduce the possibility of mislabeling as displaced, men who voluntarily left small firms. Restricting the sample to at least five fathers in an initial firm produces very similar point estimates overall, but the standard errors are larger than those presented in the main result here. An upper bound of 500 is chosen because closures at the largest firms are extremely rare, and we were concerned that including such firms would introduce heterogeneity across the treatments and controls. Wage premiums are associated with large firms. Finally, we eliminate children whose fathers earned more than $1,000,000 (measured in 1999 dollars) in a single year, in order to be sure that our estimates are not driven by outliers.

The treatment group consists of 10–14-year-old boys whose fathers experienced a displacement between 1980 and 1982. Our primary control group consists of boys whose fathers stayed with the firm through 1979 but who may have left after that. We also require that individuals not receive unemployment insurance in 1978 or 1979, the years prior to our observed firm closures. Forty-five percent of fathers in this control group remained at the same firm until at least 1988. As part of our sensitivity analysis, we explore the consequences of including early leavers in our sample and find that including them does not change the results.

Our analysis focuses on the effects of displacements that occurred between 1980 and 1982. Choosing this period allows us to base our sample on the children of fathers who had at least 2 years of tenure at the firm, while maximizing the number of years the children would be likely to be living at home after the displacement occurred. Another advantage of focusing on displacements that occurred in the early 1980s is that it was the beginning of a substantial and prolonged recession in Canada. As a result, the number of displacement events is high.

The IID includes information on three socioeconomic outcomes: earnings, receipt of unemployment benefits, and receipt of social assistance. We use information that is available between 1995 and 1999 to create three dependent variables: the log of a 5-year earnings average, an indicator for whether the individual received unemployment insurance during the 5-year period, and an indicator for whether the individual received social assistance during the 5-year period. During this period, the sons' ages range between 25 and 33 years. Since earnings generally increase with age, we adjust our earnings measure by regressing it on a set of age dummies and use the residual as our dependent variable.

IV. Results

A. Summary Statistics

Sample summary statistics are shown in table 1. Appendix table A1 includes additional information about the firms in our sample. As described above, most of our analysis focuses on sons whose fathers were employed at firms with between two and 500 fathers. Our sample contains 39,258 fathers, 1,376 of whom experienced a firm closure between 1980 and 1982. These men worked at 13,234 different firms, 520 of which closed between 1980 and 1982.[10] Control fathers remained with the same firm between 1978 and 1979.

Table 1 shows separate statistics for our treatment and control groups. Fathers' average age, income, and earnings are initially very similar across the two groups, but by 1988, 6 years after treatment fathers have lost their jobs, the labor market characteristics of the two groups are quite different. Average earnings of displaced fathers are roughly $44,000, while the average earnings of the control fathers are approximately $49,000. Not surprisingly, the displaced fathers are also much more likely to be receiving unemployment insurance.[11] Table 1 thus provides some initial evidence that firm closings generate substantial shocks to a family's economic status. At the same time, these shocks do not appear to affect other family background characteristics: fathers' marital status and mothers' income, for example are very similar for the treatment and control groups in both 1978 and 1988.

We have estimated the effect of displacement on family income using the empirical strategy introduced by Jacobson et al. (1993). The results

[10] The control group may include fathers displaced after 1982. The fraction of displaced workers in the control group, however, is likely to be small. We also considered an alternative control group of children whose fathers remained at the same firm between 1978 and 1988, which produced similar results. The control group in our main sample is free to leave old and enter new firms after 1979, whether such a move occurs for positive or negative reasons.

[11] Information on social assistance receipt is not available prior to 1992.

Table 1
Descriptive Statistics, Fathers and Children

	Closures	Controls	Full Sample
Parents 1978:			
Father's age	37	37	37
	(5)	(5)	(5)
Father's earnings	43,407	43,741	45,068
	(16,582)	(16,258)	(16,826)
Father's total income	44,626	44,722	46,456
	(15,640)	(15,574)	(16,053)
Father on UI	.00	.00	.09
Father married	.95	.96	.95
	(.22)	(.21)	(.21)
Mother's earnings	7,250	7,715	7,984
	(10,747)	(11,465)	(12,197)
Father sample size	1,376	37,882	93,997
Parents 1988:			
Father's age	47	47	47
	(5)	(5)	(5)
Father's earnings	43,577	48,664	49,455
	(32,522)	(66,338)	(50,625)
Father's total income	44,877	49,627	50,545
	(30,520)	(64,517)	(49,003)
Father on UI	.17	.10	.10
	(.37)	(.31)	(.30)
Father married	.87	.87	.87
	(.33)	(.33)	(.34)
Mother's total income	13,996	15,060	15,624
	(14,971)	(18,421)	(18,269)
Moved from 1978 address	.52	.48	.49
	(.50)	(.50)	(.50)
Sons 1995–99:			
Child's earnings (1995–99)	24,364	25,437	24,668
	(15,620)	(20,370)	(21,479)
Child's age	30.0	30.0	30.0
	(.82)	(.82)	(.82)
Child on UI at least once (1995–99)	.24	.20	.21
	(.43)	(.40)	(.41)
Child on welfare at least once (1995–99)	.08	.06	.06
	(.27)	(.23)	(.23)
Number of children	1,411	38,334	106,339

Note.—Samples consist of all father-son pairs for which the son was between ages 10 and 14 between 1980 and 1982. The closures and controls samples consist of fathers who worked at a firm with between two and 500 fathers in the Intergenerational Income Database. Fathers must have worked at the same firm in 1978 and 1979 and received no unemployment insurance (UI) during that time. The closures sample includes fathers displaced from a firm that closed between 1980 and 1982. Standard deviations are in parentheses.

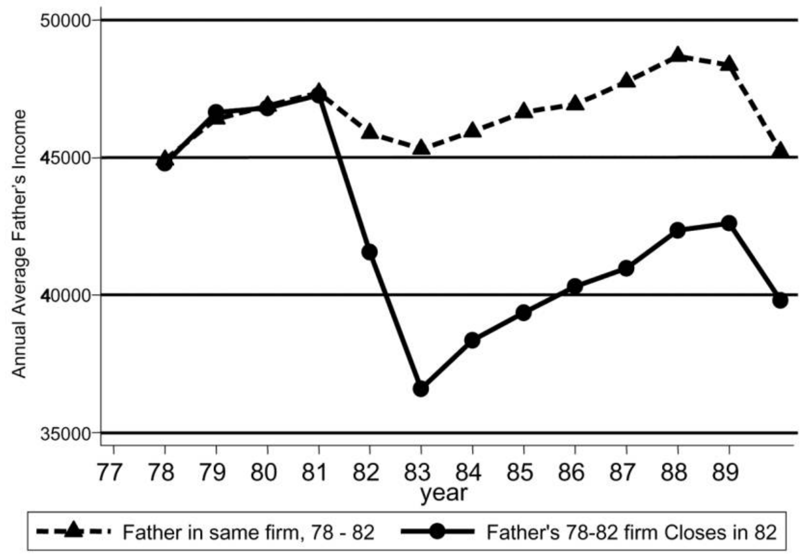

FIG. 2.—Annual average father's income by whether firm worked at between 1978 and 1982 closed in 1982.

of this analysis are detailed in appendix table A2 and are summarized in figure 2, which shows the large and lasting earnings reductions that occur following a plant closing. Here, we graph the average earnings trajectories of treatment and control fathers around the time of displacement, focusing on a set of fathers who worked continuously at the same firm between 1978 and 1981, some of whom experienced a displacement in 1982.[12] Consistent with previous studies, our analysis indicates that displaced workers experience long-term earnings reductions of about 17% (measured 8 years or more after the job loss). We find that displacement has smaller, but similar, effects on fathers' income and family income. Taken as a whole, these results confirm that our plant closings produce substantial and lasting effects on family resources.

Table 1 also shows that treatment and control children have slightly different labor market outcomes. For example, average earnings between 1995 and 1998 are about $24,000 among those whose fathers experienced a job loss and $25,000 for those whose fathers did not. Similarly, treatment children have higher rates of unemploment insurance (UI) and social assistance (SA) receipt than the controls. The last column of table 1 provides these descriptive statistics for all sons in the IID who were 12–14 years old

[12] For ease of exposition, fig. 2 eliminates fathers who were displaced in 1980 or 1981.

in 1982. Average family income is comparable, though slightly higher for these children. Otherwise, the samples are very similar.

B. Intergenerational Effects of Displacement

Table 2 summarizes our main results. Dependent variables include the log of age-adjusted average earnings between 1995 and 1999, a dummy variable indicating whether the child filed for unemployment benefits between 1995 and 1999, and a dummy variable indicating whether he filed for social assistance between 1995 and 1999. Column 1 shows the OLS estimate of the relationship between the log of son's earnings and the log of father's income averaged over 1978–79. The estimated coefficient of 0.29 is consistent with the intergenerational correlations literature. Corak and Heisz (1999), for example, find income and earnings correlations between .20 and .25 for fathers and sons in Canada. Estimates of income and earnings correlations in the United States are slightly higher, at around 0.40, but they are typically based on more years of data on fathers than we use here. The ages at which both father's and son's earnings are measured also affects these magnitudes.

In the next column we include only a dummy variable indicating whether the father lost his job due to a plant closing. This variable has a powerful effect on the child's earnings, which are 9% lower than the earnings of those whose fathers were not displaced. This estimate barely changes when we control for father's pre-1980 income (col. 3). Furthermore, the point estimate on the log of pre-1980 income is robust to the inclusion of the displacement dummy. Taken together, these results suggest that our treatment and control groups are well matched and that displacements are uncorrelated with predisplacement earnings.

Column 4 adds detailed controls for the father's initial firm to our model, including a quartic in father's firm size, 11 industry fixed effects (based on one-digit industry categories), and 36 province by urban/rural status dummy variables. If the inclusion of these controls alters the estimated displacement coefficient, then we should be concerned about the possible influence of other factors. In particular, we include dummies indicating region of residence and urban/rural status, because firm closings that occur in company towns may have long-lasting effects on local labor market conditions. If individuals tend to stay in the same location where they grew up, then the displacement coefficient may partly reflect the fact that firm closings depress wages in the local economy. The estimated displacement coefficient is not changed by the addition of these controls. While the additional controls will not necessarily reduce the potential bias in our coefficient of interest, the lack of any sensitivity of the estimates to these controls is reassuring. Results controlling for additional combinations of industry and region fixed effects are summarized

Table 2
Estimated Effects of Father's Displacement on Son's Earnings, Unemployment, and Social Assistance Receipt

	Age-Adjusted Log Earnings, 1995–99				UI Receipt		Social Assistance Receipt	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Father's log income residual	.298		.298	.269		−.045		−.064
	(.023)***		(.023)***	(.024)***		(.007)***		(.005)***
Father displaced		−.099	−.097	−.092	.04	.039	.019	.015
		(.037)***	(.037)***	(.037)**	(.013)***	(.013)***	(.009)**	(.009)*
With initial firm characteristic controls	No	No	No	Yes	No	Yes	No	Yes
Observations	38,342	38,342	38,342	38,342	39,745	39,745	39,745	39,745
R^2	.01	.00	.01	.02	.00	.03	.00	.03

Note.—The dependent variable in cols. 1-4 is son's log real earnings averaged between 1995 and 1999 after demeaning by age. The dependent variable in cols. 5 and 6 is an indicator for whether the son received unemployment insurance (UI) benefits during the same time period. The dependent variable in cols. 7 and 8 is an indicator for whether the son received social assistance benefits between 1995 and 1999. All regressions include fixed effects for birth cohort. The initial firm characteristic controls include fixed effects for regional location of firm (18 possible characters that identify provinces and smaller regions in large provinces) interacted with an indicator variable for whether the firm is in an urban or rural location. The controls also include 11 industry dummies and a quartic in firm size. Huber-White standard errors are shown, clustering by father ID.

* Denotes significance at the 10% level.
** Denotes significance at the 5% level.
*** Denotes significance at the 1% level.

in appendix table A3. The coefficients on the displacement indicator are virtually identical across specifications including anywhere from 11 to 317 industry controls and from 36 to 180 regional controls.

The remainder of table 2 shows what happens when we replace the dependent variable with indicators for whether the son received unemployment benefits or social assistance as a young adult. These specifications also suggest that boys whose parents experienced a job loss have worse economic outcomes than those whose parents did not. We find that treatment children are 4 percentage points more likely to receive unemployment benefits and 2 percentage points more likely to receive social assistance as adults than those in the control group. The mean level of social assistance receipt in the second generation is 0.06, so this point estimate is very large in percentage terms.

We have also repeated the analysis using a sample that includes both boys and girls. While the results are not shown, they are very similar to those for the full sample, although the point estimates for girls are much less precisely estimated. The noisiness of the girls' estimates is undoubtedly driven by the fact that many women in their 20s and 30s choose not to work or to work fewer hours while they are raising children and not because they have poor labor market options. Women with no earnings between 1995 and 1999 are not observed.

To put the magnitude of our displacement estimates in context, consider them in light of the literature on intergenerational income and earnings correlations. Appendix table A2 shows that the average effect of father's displacement on his own log earnings is approximately $-.14$. We estimate that the correlation between father's income and son's earnings is .298, so a naive estimate of the expected effect of displacement is $-0.04 (= 0.298 \times -0.14)$. The confidence interval around our -0.09 estimate includes -0.04; nevertheless, it is still worth considering why the magnitude of our point estimate is larger than the estimated intergenerational correlation predicts. One possibility is that our estimated intergenerational correlation is biased downward—we only have 2 years of data with which we are estimating father's income, and previous studies have shown that better measures of father's permanent income produce larger correlation estimates.[13] It is also worth noting that plant closings change income in a particular way: they generate a discrete, negative shock. If parental responses to upward versus downward shocks are asymmetric, then we should not expect the estimated effect of parental job loss to coincide with the effect that would be predicted from the intergenerational income correlation.

Another possibility is that our estimate is picking up something beyond the effect of living in a family with lower income. Because our treatment

[13] See, e.g., Solon (1992) and Zimmerman (1992).

and control groups are well matched, our estimates are unlikely to reflect the effect of innate family background characteristics; however, a job loss event could impact the family in ways other than through income. Displacement disrupts the routine of everyday family life and may increase feelings of stress and anxiety. The relevant issue for interpreting our shock variable is whether such stress and disruption are themselves a result of the income loss. If these other effects result from something such as the loss of status or position, rather than the loss of income, then the effect of displacement may be larger than the effect of more general movements in income.

Some additional effects of displacement are potentially observable with our data. Divorce, for example, has sometimes been linked to job loss (Charles and Stephens 2004). Lower income levels or subsequent reemployment could also be associated with residential moves, which are thought to have a negative impact on children (McLanahan and Sandefur 1994). We explore some of these possibilities in table 3 by estimating the effects of displacement on father's marital status and unemployment, mother's earnings, and residential mobility. We can also examine whether the inclusion of these variables affects the estimated effect of displacement on children's outcomes.

Column 1 of table 3 provides no evidence that fathers who lost their jobs are more likely to be divorced than fathers who did not lose their jobs. In the years after a firm closure, treatment fathers are no more likely to have divorced than are control fathers. We do find evidence that displacement may affect residential mobility. Compared to children whose fathers are able to keep their jobs, displaced children are about 5 percentage points more likely to have moved in the short run and about 3 percentage points more likely to move in the long run. Unemployment insurance use is clearly higher among fathers displaced in 1980–82, even 8 years later. There is no evidence of a change in mother's earnings following the father's displacement.

Table 4, however, shows that divorce and residential mobility do not seem to explain any part of the displacement effects we estimate. Here we report results of regressions of children's outcomes on family income prior to the job loss, the displacement dummy, and indicators for whether the parents divorce or move. The displacement effects are very similar to those shown in table 2, suggesting that they are not driven by either divorce or residential mobility in the aftermath of displacement.

Another step toward understanding our estimates is to control simultaneously for the shock and for postdisplacement income. We have estimated a version of our basic specification that includes the log of average earnings between 1982 and 1988:

$$O_i = \beta_1 + \beta_2 \text{AvgInc}_{78-79} + \beta_3 \text{AvgInc}_{82-88} + \beta_4 \text{Shock}_i + \varepsilon_i. \qquad (1)$$

Table 3
Estimated Effects of Father's Displacement on the Probability of Divorce, Residential Moves, and Mother's Income

	Dependent Variable			
	Ever Not Married since 1978	Moved	Mother's Earnings	Unemployment Insurance Receipt
Displacement lead or lag:				
−3	−.003	.017	148.844	−.018
	(.005)	(.012)	(163.571)	(.011)*
−2	−.004	.038	−74.955	−.001
	(.006)	(.015)**	(192.647)	(.012)
−1	−.002	.04	−276.598	−.008
	(.007)	(.015)**	(228.203)	(.012)
0	.006	.046	−320.324	.207
	(.008)	(.016)***	(246.934)	(.018)***
+1	.004	.06	−455.49	.241
	(.008)	(.017)***	(265.479)*	(.018)***
+2	−.004	.062	−297.705	.101
	(.011)	(.017***	(278.751)	(.017)***
+3	−.01	.053	−263.406	.088
	(.011)	(.017)***	(307.112)	(.016)***
+4	−.02	.049	−213.32	.049
	(.011)*	(.017)***	(333.343)	(.015)***
+5	−.008	.041	−161.119	.044
	(.012)	(.017)**	(375.072)	(.015)***
+6	−.014	.043	-240.478	.054
	(.012)	(.017)**	(420.016)	(.015)***
+7	−.015	.034	−296.754	.042
	(.012)	(.017)**	(449.384)	(.015)***
+8	−.007	.028	−463.08	.05
	(.012)	(.017)*	(459.527)	(.016)***
Observations	411,736	411,736	411,736	411,736
R^2	.5	.69	.74	.3

NOTE.—For ease of interpretation, these regressions are based only on displacements that occurred in 1982 for the sample that worked at the same firm between 1978 and 1982. The regressions include individual fixed effects and indicator variables for years since job displacement. The omitted category is never left firm or left after 1982. Standard errors are shown in parentheses.
* Denotes significance at the 10% level.
** Denotes significance at the 5% level.
*** Denotes significance at the 1% level.

If average income in the postdisplacement period is independent of other family background characteristics, then the estimates of β_3 and β_4 can help us separate the displacement effect into an income component and a part that is unrelated to income. One must interpret estimates of these coefficients cautiously, however, since earnings in the postdisplacement period (like earnings in any period) are likely to be partially determined by unobservable characteristics that also affect son's earnings.[14] Never-

[14] For example, fathers whose incomes quickly rebound following a job loss may be more motivated than fathers whose incomes rebound more slowly. At the same time, more motivated fathers may affect their sons' earnings by passing this characteristic on to them.

Table 4
Estimated Effects of Father's Displacement, Divorce, and Mobility on Son's Earnings, Unemployment, and Social Assistance Receipt

	Dependent Variable		
	Age-Adjusted Log Earnings, 1995–99	UI Receipt	Social Assistance Receipt
Log income 1978–79	.266	−.044	−.062
	(.024)***	(.007)***	(.005)***
Displacement	−.091	.039	.015
	(.037)**	(.013)***	(.009)
Father divorced	−.162	.043	.093
	(.030)***	(.012)***	(.010)***
Moved	−.045	.005	.025
	(.012)***	(.005)	(.003)***
Observations	38,342	39,507	39,507
R^2	.02	.03	.02

Note.—The dependent variables are son's log real earnings averaged between 1995 and 1999 after demeaning by age, a dummy for ever receiving unemployment insurance (UI) between 1995 and 1999, and a dummy for ever receiving social assistance between 1995 and 1999. All regressions include fixed effects for birth cohort, fixed effects for regional location of firm (18 possible characters that identify provinces and smaller regions in large provinces) interacted with an indicator variable for whether the firm is in an urban or rural location. The controls also include 11 industry dummies and a quartic in firm size. Huber-White standard errors are shown in parentheses, clustering by father ID.
** Denotes significance at the 5% level.
*** Denotes significance at the 1% level.

theless, this specification may provide suggestive evidence if a displacement effect remains when we directly control for the extent of the income shock. The results of this exercise, which are displayed in table 5, are consistent with part of the displacement effect being driven by the accompanying income loss: when postdisplacement income is included in the regression, the magnitude of the estimated displacement coefficient falls by nearly half and is no longer statistically different from zero. As expected, the estimated effect of the post-1982 income on son's earnings is positive and of a substantial magnitude.[15]

We have tried to address concerns about the endogeneity of post-1982 income by estimating equation (1) using an instrumental variables strategy where the instrument for post-1982 income is a three-way interaction between the Shock variable, region, and initial industry. The instrument is strongly correlated with post displacement income (the *F*-statistic in the first stage is 32), although we are still concerned that within-region variation in industry wages may still be correlated with parental char-

[15] Appendix table A3 shows that this specification is also unaffected by the inclusion of differing controls for initial industry and geography.

Table 5
Effects of Father's Income and Father's Displacement on Son's Earnings, Unemployment, and Social Assistance Receipt, Controlling for Father' Income, 1983–88

	Log Earnings Residual		Log Earnings Residual (IV)	UI Receipt		UI Receipt (IV)	Social Assistance Receipt		Social Assistance Receipt (IV)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Father's log income residual, 1978–81	.264	.118	.175	−.047	−.011	−.07	−.062	−.028	−.055
	(.026)***	(.029)***	(.117)	(.008)**	(.008)	(.040)*	(.005)***	(.006)***	(.037)
Father displaced	−.095	−.053	−.074	.025	.009	.031	.018	.012	.013
	(.038)**	(.038)	(.041)*	(.014)*	(.013)	(.015)**	(.009)**	(.009)	(.010)
Father's log income residual, 1983–88		.202	.127		−.05	.038		−.048	−.009
		(.017)***	(.162)		(.006)***	(.057)		(.005)***	(.054)
Detailed initial firm characteristic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	38,342	38,342	38,342	39,745	39,745	39,745	39,745	39,745	39,745
R^2	.02	.03	.04	.03	.04	.04	.03	.03	.03

Note.—The dependent variables are son's log real earnings averaged between 1995 and 1999 after demeaning by age, a dummy for ever receiving unemployment insurance (UI) between 1995 and 1999, and a dummy for ever receiving social assistance between 1995 and 1999. All regressions include fixed effects for birth cohort, fixed effects for regional location of firm (18 possible characters that identify provinces and smaller regions in large provinces) interacted with an indicator variable for whether the firm is in an urban or rural location. The controls also include 11 industry dummies and a quartic in firm size. Huber-White standard errors are shown in parentheses, clustering by father ID. IV = instrumental variables.

* Denotes significance at the 10% level.

** Denotes significance at the 5% level.

*** Denotes significance at the 1% level.

Table 6
Estimated Differences in Father's Characteristics between Early Leavers and Displaced Workers

	Log Father's Income	Mother's Earnings	Father's Age	Unmarried
Left early	−.043	1,001.9	−.369	.009
	(.025)*	(1,047.0)	.52	.02
Firm fixed effects?	Yes	Yes	Yes	Yes
Number of observations	32,450	32,450	32,450	32,450
R^2	.5713	.3933	.3726	.4137

NOTE.—The outcome variables are from 1978 and are regressed on whether a father left a firm prior to a closure or mass layoff in 1980, 1981, or 1982, and firm fixed effects. Standard errors are shown in parentheses.
* Denotes significance at the 10% level.

acteristics.[16] In any case, the second-stage estimates (shown in the third col. of table 5) are too imprecisely estimated to provide much information. The coefficient on predicted post-1982 income is positive at .13, but with an accompanying confidence interval that is so wide that we cannot rule out either that the income coefficient is zero or that it is substantially larger than the OLS estimate. The estimated coefficient on father's displacement grows more negative and again becomes marginally statistically significant.

We also consider whether our results are driven by heterogeneity across workers who leave firms before they close and those who are displaced at the time the firm shuts down. Our sample includes the latter but not the former. A concern is that those who leave a firm before it closes are more able to obtain alternative jobs than those who stay until the firm shuts down. In this case our estimates would reflect only the experiences of children whose fathers are least likely to recover from displacement. We explore this possibility in table 6 and table 7, where we extend our sample to include children whose fathers left the firm before the closure. Table 6 compares demographic characteristics in 1978 for those fathers who left the firm before it shut down to those who were displaced by the firm closure. There is no evidence that "early leavers" have higher incomes, are of different ages, or have different marriage propensities than those who remain at the firm until it closes. Table 7 shows the results of IV regressions in which we use an indicator for whether or not the firm

[16] Choice of predisplacement industry, for example, is unlikely to be independent of father's skills. While we include industry dummy variables in the earnings regression, this only controls for differences in average earnings across different types of workers. Our instrument, which is an interaction between shock × region × initial industry picks up differences in earnings recovery across different types of people. If underlying skill is correlated with initial industry/region and is also associated with the degree of recovery after displacement (and directly affects children's outcomes), then the exclusion restriction may not be valid.

Table 7
IV and Reduced from Results including Early Leavers

	Son's Log Income	Son on UI	Son on SA
Log father's income	.288	−.044	−.064
	(.030)***	(.008)***	(.006)***
Displaced	−.091	.037	.017
	(.037)***	(.013)***	(.009)*
With initial firm characteristic controls	Yes	Yes	Yes
Number of observations	32,450	32,450	32,450

NOTE.—The dependent variable in col. 1 is son's log real earnings averaged between 1995 and 1999 after demeaning by age. The dependent variable in col. 2 is an indicator for whether the son received unemployment insurance (UI) benefits during the same time period. The dependent variable in col. 3 is an indicator for whether the son received social assistance (SA) benefits between 1995 and 1999. All regressions include fixed effects for birth cohort, 11 initial industry categories, 18 regional fixed effects for location of initial firm, and a quartic for initial firm size. The sample includes fathers who left a firm prior to a closure or mass layoff (in 1980, 1981, or 1982). Displacement is instrumented with being at a firm during the year of the closure or layoff or 1–3 years earlier. Standard errors are shown in parentheses.
* Denotes significance at the 10% level.
*** Denotes significance at the 1% level.

closed in 1980–82 as an instrument for whether the father was displaced in 1980–82. As we would expect, this instrument has strong predictive power: the first stage coefficient estimate is highly significant and indicates that working in a firm that closes between 1980–82 increases the probability of leaving the firm during those years by 80%. Also as expected, the reduced form regressions (not shown) of the child's outcome on whether the firm his father worked at in 1978 closed between 1980 and 1982 produces smaller coefficient estimates than the estimated effects of displacement, but the IV estimates are of similar magnitude to those in table 2 and retain their statistical significance.

Finally, we examine how our displacement effects vary across the income distribution. Since the financial constraints and associated stress that accompany a job loss are likely to be greater for low-income families, we expect that the intergenerational displacement effects will be largest for individuals who grew up in less affluent families.[17] Economic models of intergenerational mobility developed by Becker and Tomes (1986) and further elaborated upon in Mulligan (1997) predict that in an economy with imperfect capital markets, negative income shocks will impact poor parents' investments in their children's human capital but will not impact the investment decisions of rich parents. In such models, the efficient level of human capital investment is a small fraction of rich parents' income and a large fraction of poor parents' income. Rich parents plan to leave bequests for their children, but poor parents do not, because, for these

[17] For example, Coelli (2005) finds that low-income teenagers whose parents experience a job loss are less likely to attend college, and virtually all of this affect is concentrated among parents with only a high school education or less.

Table 8
Effects of Father's Income and Father's Displacement on Son's Earnings, Unemployment, and Assistance Receipt by Father's Income Quartile in 1978

Income Quartile	(1)	(2)	(3)	(4)
Log earnings:				
Father's log income	.150	.298	.588	.219
	(.079)*	(.177)*	(.174)***	(.061)***
Father displaced	−.143	−.117	.007	−.095
	(.088)	(.070)*	(.065)	(.068)
Receipt of UI:				
Father's log income	−.135	−.096	−.073	.006
	(.078)*	(.029)***	(.076)	(.012)
Father displaced	.043	.042	.041	.023
	(.026)*	(.026)	(.027)	(.026)
Receipt of social assistance:				
Father's log income	−.015	−.099	−.161	−.04
	(.011)	(.053)*	(.047)***	(.016)**
Father displaced	.017	.004	.011	.021
	(.020)	(.018)	(.017)	(.016)

Note.—The sample of fathers is split by income quartile based on average income between 1978 and 1979. The table shows results from regressions run separately by father's income quartile. All regressions include fixed effects for birth cohort. The initial firm characteristic controls include fixed effects for regional location of firm (18 possible characters that identify provinces and smaller regions in large provinces) interacted with an indicator variable for whether the firm is in an urban or rural location. The controls also include 11 industry dummies and a quartic in firm size. Huber-White standard errors are shown in parentheses, clustering by father ID. UI = unemployment insurance.
* Denotes significance at the 10% level.
** Denotes significance at the 5% level.
*** Denotes significance at the 1% level.

families, the marginal return to human capital investment is higher than the marginal return to saving. An implication of these models is that declines in family income reduce the level of bequests left by high-income families but do not change such families' investment decisions, whereas among low-income families declines in income reduce consumption, which raises the marginal utility of consumption and the shadow price of investing in their children's human capital. As a result, parental displacement should have no impact on the eventual earnings of children growing up in high-income families but will lead to a reduction in the observed earnings of children growing up in low-income families.

An advantage of basing our analysis on such a large data set is that we can investigate the predictions of these models directly. In table 8 we present displacement estimates separately by the family's (initial) income quartile. The displacement effects appear to be concentrated among those families for whom father's earnings are in the lowest quartile. Among children in this group subsequent earnings are 13% lower than they would have been if the father had not been displaced, and the probability of UI receipt is 5 percentage points higher. In contrast, there is no evidence that there is any intergenerational effect among families in the top two quartiles. We have also investigated possible differences across the income distribution in the effects of plant closings on a father's log earnings, and we find no substantial differences across income quartiles in these effects of displacement on the first generation. Thus, the patterns in table 8 do not reflect differences in the proportional size of the income shock across columns. This finding is consistent with the predictions of the Becker and Tomes and the Mulligan models. Furthermore, it provides some insight as to why previous studies have failed to find income effects on the next generation: small sample sizes make it difficult to estimate nonlinear effects, yet virtually all of the action appears to be at the bottom of the income distribution.

V. Conclusions

This article provides new evidence on the transmission of economic status across generations. While the existence of large intergenerational income correlations has led many researchers to conclude that family income is an important determinant of children's eventual economic success, the evidence in support of this hypothesis is surprisingly limited. Previous research has been hampered by small sample sizes and the difficult task of controlling for unobserved parental attributes. Because we exploit longitudinal variation that is induced by firm closings, we are able to separate the effect of a long-lasting income shock from the effect of innate parental attributes. Our access to a data set that contains 39,000 father-son pairs aids our ability to identify this effect.

We find that the adult earnings of men whose fathers were displaced are 9% lower than earnings of similar individuals whose fathers did not experience an employment shock, even after we account for fathers' predisplacement earnings, initial region of work, industry, and firm size. Relative to men whose fathers did not lose their jobs, sons of displaced workers are also more likely to receive unemployment insurance and social assistance. Our estimates are driven almost exclusively by the experiences of individuals whose family income during childhood was in the bottom quartile of the income distribution. The results suggest that the long-term consequences of unexpected job loss extend beyond the effect on one's own income to the eventual labor market outcomes of one's children.

The interpretation of these results relies on the quality of the control group. Our analysis assumes that the labor market experiences of control fathers provide an appropriate counterfactual for what would have happened to the treatment fathers if the displacement had not occurred. Put differently, we assume that, conditional on 1978–79 earnings and other firm and region control variables, the likelihood that a job loss occurs is the same for fathers in the treatment and control groups. The fact that predisplacement labor market characteristics are virtually identical for the two groups is a promising sign that we have successfully controlled for innate family background characteristics, but if treatment and control families differ in ways that affect the second generation's economic outcomes without affecting the economic outcomes of the parents, then our displacement effects will not be identified. It is hard to imagine what such characteristics would be, however.

Finally, it is important to note that our estimation strategy captures the full effect of displacement. We have demonstrated that job loss leads to large, long-lasting reductions in a family's monetary resources, but it may also impose nonmonetary costs (such as stress) on families that affect their children's long-run outcomes. Although we find no evidence of significant displacement shocks on mobility, marital status, and spousal income, other effects from displacement, which are more difficult to measure, may play a role.

Appendix

Table A1
Descriptive Statistics, Firms

	Closures	Controls	Full Sample
Average father firm size	16	16	21
	(30)	(41)	(251)
Minimum father firm size	2	2	1
Maximum father firm size	493	496	27,824
Average median wage	40,471	39,948	37,535
	(12,170)	(13,774)	(15,322)
Province:			
Newfoundland	2.3	1.6	1.9
Prince Edward Island	.8	.6	.7
Nova Scotia	2.1	3.3	3.5
New Brunswick	4.1	2.4	2.9
Quebec	32.1	31.5	30.7
Ontario	38.1	42.0	38.6
Manitoba	2.3	3.8	4.1
Saskatchewan	2.3	3.	4.1
Alberta	11.6	7.7	8.6
British Columbia	4.3	4.2	4.9
One-digit industry:			
Missing	.2	.0	.0
Agriculture	5.8	3.5	4.4
Primary textiles and leather	2.5	5.5	3.9
Clothing and furniture	6.5	9.2	6.8
Manufacturing	11.5	14.3	10.1
Construction and transportation	22.5	22.0	21.6
Wholesale trade	9.6	12.4	10.6
Retail trade	12.7	12.2	13.6
Finance and insurance	15.4	9.3	8.9
Education and health services	4.2	4.4	10.7
Accommodation, food and beverage	9.0	7.2	9.3
Number of firms	520	12,714	22,759

NOTE.—Samples consist of all father-son pairs for which the son was between ages 10 and 14 between 1980 and 1982. The closures and controls samples consist of fathers that worked at a firm with between two and 500 fathers in the Intergenerational Income Database. Fathers must have worked at the same firm in 1978 and 1979 and received no unemployment insurance during that time. The closures sample includes fathers displaced from a firm that closed between 1980 and 1982. Standard deviations are in parentheses.

Table A2
Effects of Displacement on Father's Real Log Earnings, Log Income, and Mobility

	Dependent Variable					
	Log Father's Earnings (1)	Log Father's Income (2)	Log Parental Income (3)	Log Father's Earnings (4)	Log Father's Income (5)	Log Parental Income (6)
Displacement lead or lag:						
−3	.005	−.001	.04			
	(.016)	(.015)	(.033)			
−2	.009	.004	.027			
	(.011)	(.011)	(.035)			
−1	.007	.006	.036			
	(.012)	(.011)	(.032)			
0	−.127	−.06	−.025			
	(.018)***	(.014)***	(.033)			
+1	−.317	−.186	−.111			
	(.031)***	(.020)***	(.037)***			
+2	−.201	−.17	−.128			
	(.024)***	(.021)***	(.040)***			
+3	−.196	−.141	−.092			
	(.026)***	(.018)***	(.041)**			
+4	−.188	−.151	−.067			
	(.029)***	(.025)***	(.039)*			
+5	−.176	−.16	−.12			
	(.029)***	(.027)***	(.048)**			
+6	−.165	−.117	−.057			
	(.029)***	(.024)***	(.041)			
+7	−.182	−.159	−.106			
	(.036)***	(.032)***	(.048)**			
+8	−.188	−.166	−.122			
	(.037)***	(.033)***	(.051)**			
Log income 1978–80				1.048	.999	.808
				(.009)***	(.007)***	(.014)***
Displacement				−.139	−.107	−.092
				(.012)***	(.010)***	(.016)***
Observations	407,390	408,429	411,736	407,390	408,429	411,736
R^2	.55	.58	.31	.33	.38	.09

NOTE.—For ease of interpretation, these regressions are based only on displacements that occurred in 1982 for the sample that worked at the same firm between 1978 and 1982. In cols. 1–3 the dependent variable is the log of father's annual earnings or the log of total income (both demeaned by age and year). The regressions include individual fixed effects and indicator variables for years since job displacement. The omitted category is never left firm, or left after 1982. Columns 4–6 show results from regressing the same dependent variables on average log earnings between 1978 and 1980 and a dummy variable for whether the father was displaced in 1982. Standard error estimates are in parentheses.
* Denotes significance at the 10% level.
** Denotes significance at the 5% level.
*** Denotes significance at the 1% level.

Table A3
Sensitivity of Results to Varying Initial Industry and Region Controls

	Dependent Variable: Age-Adjusted Log Earnings, 1995–99					
Father's log income residual (pre-1982)	.269	.263	.264	.264	.122	.118
	(.024)***	(.025)***	(.025)***	(.026)***	(.028)***	(.029)***
Father displaced	−.092	−.093	−.096	−.095	−.053	−.053
	(.037)**	(.038)**	(.038)**	(.038)**	(.037)	(.038)
1983–88 average father's log income					.198	.202
					(.017)***	(.017)***
With initial firm characteristic controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of initial industry F. E.	11	81	317	317	11	317
Number of initial region F. E.	36	127	36	180	36	180
Observations	38,342	38,342	38,342	38,342	38,342	38,342

NOTE.—The dependent variable is child's log real earnings averaged between 1995 and 1999 after demeaning by age. All regressions include fixed effects for birth cohort. The initial firm characteristic controls include a quartic in initial firm size. Initial industry fixed effects are at the one-, two-, and three-digit levels, and the regional fixed effects use the first, second, or third digit of the initial firm's address (or a combination of the three). Huber-White standard errors are shown, clustering by father ID. Standard errors are shown in parentheses.
** Denotes significance at the 5% level.
*** Denotes significance at the 1% level.

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