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Chapter Author: Marianne Page, Ann Huff Stevens, Jason Lindo

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7

Parental Income Shocks and Outcomes of Disadvantaged Youth in the United States

Marianne Page, Ann Huff Stevens, and Jason Lindo

Government policies that increase the incomes of poor families have been promoted as a way of improving children's life chances on the grounds that children who grow up in rich families tend to have better socioeconomic outcomes as adults than children who grow up in poor families. Yet the process that generates the relationship between parents' incomes and those of their children, though well documented, is not well understood. One possibility is that differences in family income lead to differences in parents' monetary investments in their children. Another is that differences in family income reflect differences in parents' innate characteristics that are passed on to their children.

Understanding which factors contribute to the intergenerational transmission of socioeconomic status is crucial to the development of public policies that improve youths' outcomes. If, for example, disadvantaged children have poor outcomes because their parents have less money, then the effects of public policies on family income should be a central consideration when evaluating their costs and benefits. On the other hand, 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.

Marianne Page is an associate professor of economics at the University of California, Davis, and a faculty research fellow of the National Bureau of Economic Research. Ann Huff Stevens is associate professor of economics at the University of California, Davis, and a faculty research fellow of the National Bureau of Economic Research. Jason Lindo is an assistant professor of economics at the University of Oregon.

An extensive literature documents that children raised in low-income families have significantly lower levels of education, earnings, and family income as adults than children who grew up in affluent families (see, e.g., Solon's 1999 survey). However, whether these relationships result from the effects of income per se or whether they reflect other family background characteristics is a subject of debate. Although some studies have found that the association does not disappear even after controlling for a variety of parental characteristics (e.g., Corcoran et al. 1999; Hill and Duncan 1987), one cannot be sure that observable family background variables sufficiently capture all of the familial conditions that affect children's long-term outcomes. Finding a way of comparing youth whose family characteristics would be identical except for the differences in their incomes has proven to be a challenge. The handful of studies (Blau 1999; Duncan et al. 1998; Dahl and Lochner 2005; Duncan and Brooks-Gunn 1997; Haveman and Wolfe 1995; Levy and Duncan 1999; Shea 2000) that have attempted to do so have produced mixed results. Even Susan Mayer (1997), who has devoted an entire book to this question, acknowledges that none of the five empirical strategies that she uses to tease out the causal effect of income would be convincing by itself.

This research attempts to shed some light on these questions by analyzing the effects of unexpected job loss on the next generation's socioeconomic outcomes. Jacobsen, Lalonde, and Sullivan (1993) and Stevens (1997) have documented that displaced workers experience substantive long-lasting reductions in earnings, and they argue that layoffs and firm closings can be thought of as exogenous employment shocks after conditioning on predisplacement earnings. Our estimation strategy compares groups of individuals whose families had the same levels of permanent income prior to a period when some of the family heads were displaced. In order to implement this approach, we require data on the economic outcomes of both parents and their children. We use longitudinal data from the Panel Study of Income Dynamics (PSID), which contains detailed information on both generations over many years. A disadvantage of the PSID is that sample sizes are small. As a result, our estimates are often imprecise and must be interpreted cautiously.

Nevertheless, the patterns we observe generate three broad conclusions. First, estimates of the intergenerational effects of parental job loss are sensitive to the definition of displacement. Specifically, we estimate large, statistically significant effects on the next generation's income and earnings when displacements include layoffs, but not when they are restricted to firm closures. This dichotomy suggests that individuals who are selected for layoffs may have unobserved characteristics that are correlated with their children's outcomes. Second, although we find no evidence that firm closings have intergenerational effects on average, there is evidence that such events impose long-term costs on disadvantaged children. Finally, the effects of

exogenous income shocks (from business closings) are largest among children who are young at the time of the income shock.

7.1   Empirical Strategy

Our analysis is conducted in two parts. First, we use methods taken from the displacement literature to demonstrate that displacement has a substantive and long-lasting effect on a family's resources. The purpose of this part of the analysis is to make a convincing case that displacement produces a significant exogenous shock to family income over many years. Second, we estimate the effects of this shock on children's outcomes as young adults.

7.1.1   Estimating the Effect of Displacement on a Family's Resources

We begin by following the empirical strategy introduced by Jacobson, LaLonde, and Sullivan (1993)—henceforth, JLS—to demonstrate that displacement has a large, persistent effect on a family's monetary resources. For simplicity, in this section we refer to "displacement" without distinguishing between business closings and layoffs. In the results that follow we will further distinguish results by the nature of the job displacement. We start by regressing annual measures of (log) family income and head's earnings on displacement indicators, age squared,[1] and calendar year effects.

$$(1) \qquad \ln I_{it} = \mathbf{D}_{it}\delta + \beta_1 age_{it}^2 + \mathbf{Y}\gamma_t + \alpha_i + u_{it},$$

where $\ln I_{it}$ is the log of family i's resources in year t, and $\mathbf{D}_{it}$ is a vector of dummy variables indicating that a displacement has taken (or will take) place in a future, current, or previous year; $\mathbf{Y}$ is a vector of calendar year dummies. In addition, we control for family-specific fixed effect, α_i. Because the model includes fixed effects, characteristics of the family head that do not vary over time, such as race and education, are not included.

The vector of displacement indicators ($\mathbf{D}_{it}$) contains three types of variables: dummy variables that equal one in the years prior to the displacement, a dummy variable equal to one in the year that the family head loses his job, and a series of dummy variables indicating that a displacement took place in a previous year. The first set of indicator variables captures the possibility that the head's wages may begin to deteriorate prior to the actual displacement. This might happen if wages are cut when the firm hits difficult times. Failure to include these dummies would lead to a biased estimate of the effect of the displacement. Our model, therefore, includes a dummy variable for each of the two years before the job loss occurs. The dummy variable indicating the year of the displacement captures its immediate effect on family resources, whereas the coefficients on the set of variables indicating

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1. We do not control directly for the head's age because its effects on family income cannot be separately identified from year effects when family-specific fixed effects are included.

that a displacement occurred in a previous year will reflect the persistence of the displacement effect over time. We include individual post-displacement controls for five years, along with an indicator for whether a displacement occurred six or more years ago.

By including family-specific fixed effects, we control for unobserved, time-invariant characteristics of the parents in our sample (which are potentially correlated with the probability of displacement). This means that the estimated displacement effects summarize the long-term effects of an income shock on a family's resources, and are not contaminated by permanent differences in family background. In the main part of our analysis we use this displacement shock as a source of variation in children's resources.

7.1.2   Estimating the Effect of Displacement on Children's Outcomes

The next step is to regress a measure of the child's economic well-being during early adulthood on average parental income three to five years before the job loss, some additional controls for family background, and an indicator for whether the head of the child's family was ever displaced:

(2) $O_i = a + bAvgInc_{3-5} + cFamilyCharacteristics_{it} + dDisplaced_i + \varepsilon_i,$

where O_i represents an economic outcome for child i. Since layoffs and firm closures are thought to be exogenous events, the estimated coefficient on the *Displaced* dummy is not expected to reflect parental characteristics that are correlated with income. The key to this identification strategy is the assumption that, among families with similar incomes, displacement is not correlated with unobservables that could affect children's outcomes. We examine the claim that displacements are independent of family background by including in some specifications controls for observable parental characteristics. If displacements are truly random then the inclusion of family background variables should not alter the estimated effect of displacement on children's outcomes.

7.2   Data

We use data from the 1968 to 2003 waves of the Panel Study of Income Dynamics (PSID), focusing on the displacement experiences of households that include children ages fifteen and younger. We later observe these children as young adults. Displacement is initially defined as a job separation that occurs as the result of a firm closure or layoff. Because we are more confident that job losses due to business closures are uncorrelated with unobserved characteristics of the families, in subsequent specifications we eliminate children whose parents were displaced via a layoff, and focus on the effects of firm closures only. The children whose parents were displaced and are exposed to the income shock are referred to as our treatment group.

Children without a displacement in their family history serve as a control group.

Our primary treatment group consists of children whose parent, specifically the head of household, experienced a displacement sometime before the child turned sixteen. Children over age sixteen are increasingly likely to live in independent households where exposure to their parents' monetary circumstances will be mitigated. As children age, the number of years over which they are exposed to the consequences of the income shock diminish.[2] We must also eliminate children whose parents do not have income observations three, four, or five years prior to the displacement, so that we can compute a three-year average of family income prior to the job loss.

To be included in the sample for the income and earnings outcomes, each child must be observed as the head or spouse of their own household for at least three years.[3] For other outcomes, such as completed education and labor force status, we require only a single year of observation. We focus on the first displacement experienced by a family, since subsequent displacements for a given family may not be independent events. With this in mind, we do not include in the sample any children whose parent experienced a displacement prior to the child's birth. Effectively, this means that the displacements we observe occur between 1971 and 1987, with a median displacement year of 1974. The treatment group consists of individuals whose parents experienced a displacement during this period, and the control group consists of children whose parent did not experience a displacement. The children affected are observed as heads and wives between 1975 and 2003, with a median year of 1992.

Because we want to control for family income or earnings prior to a job loss, it is necessary to choose a set of years over which we will measure income prior to the shock. While this is straightforward for those individuals who are displaced, it is less clear what income observations should be used for the control group. To solve this problem while preserving the sample size, we randomly assign children in the control group a "reference age" using the distribution of ages associated with the first displacement in our treatment group.[4] This allows us to include income three to five years prior

2. We have replicated our analysis using the displacement experiences of households including children up to age eighteen to see whether there are particular effects when the displacement occurs close to the time of college attendance decisions. The results are virtually identical to those reported here.

3. This helps to reduce measurement error in the dependent variable, which is particularly likely given the young ages at which the children's earnings are observed.

4. For example, because 8.1 percent of the displaced children have a parental displacement at age eight, controls are assigned age eight as the reference age with a probability of .081. We then use the first year in which a child is observed at the reference age as the reference year. For a control child not observed at his or her assigned reference age, we use the earliest age at which he or she is observed. If a control child is not in the survey at the assigned reference age or earlier, he or she is dropped.

to either first displacement (for the treatment group) or three to five years prior to the reference year (for the control group) in our regressions.[5] Finally, to be in our analysis sample, we require children to have the same head of household in either the year of displacement (for the treatment group) or the reference year (control group) and the five preceding years. This helps ensure that displacements are exogenous conditional on predisplacement income, while maintaining consistent sample requirements for the treatment and control groups.

To ensure that our predisplacement income measure is not driven by parent's gender or age, or by business cycle effects, we adjust parental income for these characteristics prior to including it in equation (3). We regress the natural log of parent's income on dummy variables indicating the year income is observed, and the head of household's gender and age. We then use the average of the residuals from these regressions three to five years prior to displacement (or the reference year) to control for predisplacement income.

7.4   Results

7.4.1   Summary Statistics

Sample summary statistics are shown in table 7.1. Our sample contains nearly 1,800 children, 673 of whose fathers lost their jobs due to a layoff or firm closing sometime when the children were under age fifteen, and 242 of whose fathers lost their jobs due to a firm closing. We show separate statistics for our treatment and control groups. Note that the earnings and family income of heads who will eventually be displaced are somewhat lower than among parents who do not experience a job loss. Average earnings three to five years before the displacement (reference) year are $41,499 among heads in the treatment group, and $52,325 among heads in the control group. The difference in average predisplacement earnings is much smaller when heads who experienced layoffs are eliminated from the sample: $51,396 among those who will eventually lose their jobs to a firm closure, and $53,089 among those who will not. Predisplacement income is similarly lower for the treatment group. These differences highlight the fact that displacements are not randomly distributed throughout the population: instead we assume that they are random *conditional* on predisplacement income. The legitimacy of this assumption will be discussed in section 7.4.3.

There are also substantial differences in the educational distributions

5. We have also replicated our results using a simpler design, which uses average income when the child is between ages zero and four as the control variable for both groups. This results in a smaller sample since it requires all children to be observed at age one rather than requiring that they be observed five years prior to displacement or reference year. This sample produces very similar estimates.

Table 7.1   **Sample means by parent's displacement status**

	Using all displacements			Using only closures		
	Displaced	Control	All	Displaced	Control	All
Parents						
Earnings	35,379	48,193	43,778	42,956	48,762	47,872
3 to 5 years prior to displacement (reference) year	41,499	52,325	48,591	51,396	53,089	52,830
3 to 5 years after displacement (reference) year	38,540	55,490	49,722	45,976	55,329	53,891
Family income	58,242	76,510	70,216	69,313	77,623	76,349
3 to 5 years prior to displacement (reference) year	53,589	66,311	61,928	65,878	67,564	67,306
3 to 5 years after displacement (reference) year	60,989	83,462	75,893	72,378	84,146	82,374
Education < HS	0.44	0.27	0.33	0.33	0.27	0.28
Education = HS	0.25	0.29	0.28	0.29	0.29	0.29
Education > HS	0.31	0.44	0.40	0.38	0.44	0.43
Displacement (reference) year	1976	1976	1976	1977	1976	1976
Age	39.1	41.8	40.9	41.3	41.9	41.8
Kid's age	10.7	11.6	11.3	11.6	11.9	11.9
Children						
Age	28.6	28.9	28.6	29.3	29.0	29.3
Average earnings	27,374	30,868	29,668	33,161	31,116	31,430
Earnings at ages 25 to 27	22,899	25,021	24,312	26,991	24,967	25,257
Average income	48,585	54,884	52,714	59,389	54,942	55,628
Family income at ages 25 to 27	43,673	46,842	45,782	50,570	46,368	46,969
Working	0.89	0.88	0.88	0.92	0.89	0.89
Education < HS	0.39	0.10	0.13	0.13	0.10	0.11
Education = HS	0.41	0.33	0.35	0.35	0.33	0.33
Education > HS	0.35	0.57	0.51	0.53	0.57	0.56
Ever received UI	0.35	0.25	0.28	0.31	0.26	0.27
Received UI in a given year	0.08	0.06	0.07	0.07	0.06	0.06
Ever received AFDC/TANF	0.15	0.10	0.12	0.16	0.09	0.10
Received AFDC/TANF in a given year	0.06	0.04	0.04	0.05	0.04	0.04
Had teenage pregnancy	0.25	0.20	0.22	0.16	0.21	0.20
N (Number of individuals)	673	1,117	1,790	242	1,202	1,444
N (total person-years)	7,124	12,395	19,519	2,479	13,421	15,900

Notes: Means are weighted using the individual weight in last year the individual is observed with a nonzero weight. The sample of parents is defined by the children's head of household in the first displacement year or reference year. Children of interest are observed as adults if they are heads/wives in households at ages greater than age twenty-three. Money variables are measured in 2002 dollars. Means for AFDC and teen pregnancy are calculated for women only.

of parents who do and do not suffer displacements. Approximately one-third of the treatment group parents have more than a high school education, whereas 44 percent of the control parents have obtained some post-secondary schooling. These differences narrow when we drop parents who were laid off, but they are not eliminated, which suggests that while the event

of a firm closing is likely to be exogenous with respect to individual characteristics, the likelihood of working in a job that is "susceptible" to closings is higher for less skilled workers. We will address this issue in the regression analysis by including a number of family background characteristics, and by showing that once we control for predisplacement income, the additional characteristics have little impact on the estimated effect of losing a job due to a firm closure.

Table 7.1 also shows dramatic post-reference year differences in head's earnings and family income between children whose parents were displaced and those whose parents were not. Average family income three to five years after displacement is approximately $61,000, while average family income among those with no parental displacement is more than $83,000. This suggests that firm closings and layoffs produce substantial shocks to a child's financial resources.

Table 7.1 also shows that treatment and control children have somewhat different labor market outcomes as adults. For example, average family income between ages twenty-five and twenty-seven is about $44,000 among those whose parents experienced a job loss and about $47,000 for those whose parents did not. Similarly, treatment children have lower levels of education and higher rates of Unemployment insurance (UI) receipt than the controls. Given the predisplacement differences in parents' earnings, income, and other characteristics, it is impossible to say whether this reflects any causal effect of job loss on children's outcomes. This is the focus of the regression analysis in the next section.

7.4.2   The Monetary Costs of Displacement

We begin by showing that displacement leads to a substantial long-term reduction in a family's monetary resources. Table 7.2 presents the results from regressions of heads' annual earnings and family income on the displacement dummies. The left half of the table provides results for the sample that includes all displacements, and the right half of the table shows the estimates produced by the sample that restricts displacements to those resulting from firm closures. Like previous studies, we find that family resources decline substantially when a job loss occurs. For example, when all displacements are followed, head's earnings fall by 36 percent and family income falls by 21 percent[6] in the year after displacement.[7] Family resources recover somewhat over time, but even six years later head's earnings are approximately 26 percent lower than they would have been if the displacement had not occurred. Similarly, family income is reduced by 20 percent. These estimates are all

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6. The percentage effect on earnings is computed as $e^{\delta} - 1$.
7. A firm closure can occur anytime during the year. The fathers in our sample may, therefore, lose their job anytime between early January and late December. As a result, spells of unemployment and earnings losses may be larger in the year following the displacement than in the displacement year itself.

Table 7.2   **Estimated effects of first displacements on parent's log earnings and log family income**

Dependent variable	All displacements		Only closures	
	Ln earnings (1)	Ln family income (2)	Ln earnings (3)	Ln family income (4)
3 years prior to displacement	–0.039	–0.040	–0.040	–0.043
	(0.035)	(0.026)	(0.045)	(0.035)
2 years prior to displacement	–0.055	–0.052**	–0.027	–0.061*
	(0.038)	(0.026)	(0.046)	(0.036)
1 years prior to displacement	–0.122**	–0.086***	–0.141**	–0.139***
	(0.048)	(0.031)	(0.066)	(0.050)
Year of displacement	–0.314***	–0.206***	–0.242***	–0.189***
	(0.057)	(0.040)	(0.092)	(0.068)
1 year after displacement	–0.441***	–0.239***	–0.391***	–0.240***
	(0.062)	(0.041)	(0.080)	(0.060)
2 year after displacement	–0.295***	–0.202***	–0.237***	–0.182***
	(0.055)	(0.038)	(0.079)	(0.056)
3 year after displacement	–0.369***	–0.242***	–0.333***	–0.268***
	(0.070)	(0.043)	(0.092)	(0.066)
4 year after displacement	–0.331***	–0.204***	–0.319***	–0.220***
	(0.067)	(0.044)	(0.092)	(0.070)
5 year after displacement	–0.418***	–0.206***	–0.305***	–0.187***
	(0.101)	(0.046)	(0.087)	(0.063)
6 or more years after displacement	–0.295***	–0.220***	–0.262***	–0.233***
	(0.055)	(0.041)	(0.072)	(0.056)
Number of individuals	985	985	830	830
Person-year observations	20,180	20,180	17,310	17,310
R^2	0.593	0.639	0.59	0.628

Notes: Individual-clustered standard error estimates are shown in parentheses. Regressions are weighted using the individual weight in last year the individual is observed with a nonzero weight. Regression includes individual fixed effects, year dummies, and age-squared.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

statistically significant, and indicate that displacement produces substantive and persistent economic losses, similar to those produced by JLS (1993). The results based on firm closures display the same pattern.[8] The first two rows of table 7.2 also show small declines in family income even before the displacement occurs, consistent with previous work on the effects of displacement (Stevens 1997).

    8. We have also estimated the relationship between displacement and parental earnings and income including an individual-specific trend, along with the individual fixed effect. Results for fathers' earnings are similar, although results for family income produce smaller long-run effects when individual trends are included. This may partially reflect the difficulty of identifying the individual-trend model in a relatively small data set.

7.4.3   Intergenerational Effects of Displacement—Income and Earnings

Having established that displacement substantially reduces family resources, we now investigate its intergenerational effects. Table 7.3 displays the results from the second part of our analysis. The dependent variables are based on the child's responses to the survey beginning at age twenty-four and for each year thereafter. Specially, we consider the average of adjusted log earnings and the average of adjusted log family income. As in table 7.1, the left side of the table shows the results for the full sample, and the right side of the table focuses only on displacements caused by firm closures.

Beginning with the full sample, column (1) shows the results from an ordinary least squares (OLS) regression of the log of the child's income in adulthood on the log of average family income three to five years before the "displacement" year. The estimated coefficient of 0.37 is consistent with estimates from the intergenerational correlations literature, which generally finds that the correlation between fathers' and sons' earnings is about 0.40 (Solon 1992; Zimmerman 1992). Sample variation in this variable is likely to reflect variation in other family background characteristics that are correlated with income, however, so it is inappropriate to conclude that family income has a causal effect on child's future resources.

In the next column we add a dummy variable indicating whether the father lost his job due to a layoff or firm closing. This variable is clearly correlated with the next generation's income, which is 9.2 percent lower than the average family income of those whose parents were not displaced. Next, we add a set of observable family background variables to the regression, specifically measures of the head's educational attainment, state of residence, and industry. If displacements are truly random after controlling for parental income, then the inclusion of these variables should have no impact on their estimated effect. In fact, the magnitude of the estimated displacement effect declines substantially when other family background characteristics are included, suggesting that the estimate in column (2) may partly reflect the impact of other parental attributes. Most of the change in the magnitude of the point estimates is driven by the inclusion of parental education.

The last three columns of table 7.3 provide displacement estimates for a sample that restricts treatment children to those whose parents lost their job via a firm closure. Eliminating parents who experienced a layoff from the analysis has virtually no impact on the estimated income correlation, but does reduce the point estimate on the displacement dummy to nearly zero. The standard error estimates are so large that we cannot reject the possibility that plant closings have moderately sized intergenerational effects; nevertheless, the decline in the coefficient estimate is intriguing since firm closures are more likely to be exogenous with respect to other parental characteristics.

The bottom panel of table 7.3 shows the effects of parental job loss on

Table 7.3   **Estimated effects of parental displacement on family income and earnings as adults**

	All displacements			Only closures		
	(1)	(2)	(3)	(4)	(5)	(6)
Average adjusted adult family income						
Avg. adj. log parent's famioy income	0.366***	0.359***	0.176***	0.339***	0.339***	0.162***
	(0.055)	(0.054)	(0.047)	(0.059)	(0.059)	(0.051)
Displaced (= 1 if yes)		−0.097**	−0.070		−0.006	0.002
		(0.044)	(0.044)		(0.061)	(0.060)
Parent less than HS graduate			−0.406***			−0.432***
			(0.064)			(0.074)
Parent has exactly HS education			−0.137**			−0.121*
			(0.060)			(0.063)
Parent industry fixed effects			X			X
Parent state fixed effects			X			X
Observations	1,623	1,623	1,623	1,314	1,314	1,314
R^2	0.075	0.078	0.193	0.065	0.065	0.191
Average adjusted adult earnings						
Avg. adj. log parent's family income	0.342***	0.335***	0.131**	0.296***	0.296***	0.104*
	(0.063)	(0.062)	(0.058)	(0.063)	(0.063)	(0.056)
Displaced (= 1 if yes)		−0.112**	−0.095*		−0.007	0.020
		(0.050)	(0.051)		(0.072)	(0.076)
Parent less than HS graduate			−0.450***			−0.437***
			(0.071)			(0.082)
Parent has exactly HS education			−0.176***			−0.128*
			(0.068)			(0.071)
Parent industry fixed effects			X			X
Parent state fixed effects			X			X
Observations	1,597	1,597	1,597	1,301	1,301	1,301
R^2	0.048	0.052	0.148	0.038	0.038	0.150

Notes: Standard error estimates are shown in parentheses. Regressions are weighted using the individual weight in last year the individual is observed with a nonzero weight. Earnings and income are adjusted for age, year, and sex. "X" indicates that parent industry fixed effects and/or parent state fixed effects are included in the regression.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

adult earnings. As in the top half of the table, children whose parents were displaced appear to have lower earnings as adults than children whose parents were not displaced. Specifically, children whose parents experienced a job loss have adult earnings that are about 9 percent lower than children whose parents did not, even after controlling for family background characteristics other than income. The magnitude of this estimate again changes substantially (and is very close to zero) when the sample of displacements is restricted to those resulting from a firm closure, however.

What explains the difference in the magnitude of the estimates across the different definitions of displacement? One possibility is that workers who are selected for layoffs have unobserved traits that affect both their probability of being laid off and their children's outcomes. Such concerns are less likely if we focus on firm closures, since a firm shut-down leads to job losses for *all* workers at the firm.[9] The results from table 7.2 lend weight to our concern that layoffs may be correlated with unobserved parental attributes, since the magnitude of the income and earnings shocks are very similar across the different definitions of displacement; it is not the case that layoffs and firm closures have different effects on the family's financial resources. Table 7.3 also shows that the estimated displacement coefficient is more sensitive to the inclusion of additional family background variables when the treatment group includes layoffs. Because firm closures are more plausibly exogenous, the estimated coefficient on the broader definition of displacement (including layoffs) should be thought of as an upper bound on the causal effect of income, which may also contain the effects of unobservable characteristics that lead to the layoff, but which are not captured adequately by predisplacement income. For this reason, in the rest of the chapter, we focus on displacements that occur only as the result of business closings. Results including both layoffs and closures are included in the appendix for comparison. Including the layoffs consistently produces larger and more often significant effects of job loss on the children's outcomes. This may be the result of unobservable factors that both contribute to the probability of layoff and negatively affect children's outcomes. Layoffs may be associated with poorer outcomes among the affected children, but we are less comfortable in making causal statements about this connection.

Intergenerational Effects of Displacement—Other Outcomes

Table 7.4 shows the estimated relationship between parental job loss due to business closures and other measures of socioeconomic success. Specifically, we analyze indicators for whether the child had at least a high school education, or more than a high school education, dummy variables indicating whether the child filed for unemployment benefits, Aid to Families with Dependent Children (AFDC) benefits (females only), indicators for experiencing a teen pregnancy (females only), and a variable indicating the fraction of years in which the child was working when observed at age twenty-four and later. First we consider the effect of firm closures on the next generation's completed schooling. While the estimates in the first two rows of table 7.4 are not very precise, they are of the expected sign, and suggest that children whose parents lose their jobs via a firm closure are less likely to complete

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9. Firm closures do not entirely eliminate the possibility of selection based on individual characteristics, since more able workers may leave prior to the actual firm closure and possibly avoid some of the impact of an involuntary job change.

Table 7.4   **Estimated effects of firm closures during childhood on additional adult outcomes**

	(1)	(2)
Complete at least high school		
Displaced (= 1 if yes)	−0.030	−0.019
	(0.026)	(0.020)
Attended some college		
Displaced (= 1 if yes)	−0.042	−0.015
	(0.044)	(0.048)
Ever receive unemployment insurance		
Displaced (= 1 if yes)	0.068	0.056
	(0.043)	(0.045)
Ever receive AFDC (women only, N = 746)		
Displaced (= 1 if yes)	0.056	0.049
	(0.037)	(0.038)
Teen pregnancy (women only, N = 746)		
Displaced (= 1 if yes)	−0.043	−0.061
	(0.043)	(0.042)
Working		
Displaced (= 1 if yes)	0.031*	0.049***
	(0.016)	(0.018)
Observations	1,444	1,444

Notes: Standard error estimates are shown in parentheses. Estimated marginal effects are based on probit models using the individual weight in last year the individual is observed with a nonzero weight. Column (1) controls for parent's average adjusted income. Column (2) adds controls for parent's education, industry, and state.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

high school and less likely to attend college. The next row shows the estimated effect of parental displacement on the probability of receiving UI benefits, which is positive but not statistically significant. The next two rows of table 7.4 display the estimated impact of firm closings on welfare receipt and teen pregnancy. Neither set of estimates is statistically significant. The final row of table 7.4 shows the probability that the child is working after age twenty-four. Here, the surprising result is that individuals are significantly more likely to be working if their father experienced a job loss.

7.4.4   Intergenerational Effects of Displacement—by Subgroups

Our estimates provide little evidence that, on average, income shocks have long-term effects on the next generation's socioeconomic status. By focusing on average effects, however, we may miss important differences across groups. For example, the effect of a job loss on financial constraints (and associated stress) are likely to be larger for low socioeconomic status (SES) families, so we might expect the intergenerational impact on children grow-

ing up in such families to be larger.[10] In order to explore this possibility we have estimated displacement coefficients for different subgroups of children according to their poverty status, family structure (both measured prior to displacement), parental education, and race.

Table 7.5 summarizes the results of this exercise. The first column shows the average effect of displacement on the child's financial resources, and, for purposes of comparison, the first row of the table repeats estimates for the full sample from tables 7.3 and 7.4. The first column of table 7.5 summarizes the effect of the job loss on parental household income (as shown in table 7.2); subsequent columns show the effects on children's adult outcomes.

Moving from the full sample to subsamples of children who are disadvantaged prior to any job loss changes the nature of these results. Among families with income below the poverty line before the job loss, there is a somewhat larger than average impact of displacement on family income in the parent's generation. These effects on household income during childhood seem to carry over into some statistically and economically significant effects on the next generation when we focus on disadvantaged subsamples. The family incomes of poor children whose parents experienced a plant closing are 34 percent below that of the control group. While the estimate is quite imprecise, it does stand in contrast to the very small point estimates generated for the full sample. Earnings of these children are also substantially reduced, and this estimate is statistically different from zero. When we increase the sample size by including all children with household income below 1.5 times the poverty line (in the years before job loss) we continue to find strong evidence of negative effects on children's family income and earnings as young adults. Results are similar, though somewhat weaker, when we include families with incomes up to two times the poverty line.

In contrast, row 5 shows no evidence of negative effects of parental displacement on income or earnings among those with incomes well above the poverty line. This pattern suggests that the overall lack of effects of job loss summarized in tables 7.3 and 7.4 obscures substantial effects among children who are disadvantaged prior to the parental job loss. Among children whose parental financial resources are already limited, parental job loss and the associated income shock generates substantial negative effects on their earnings and income as adults. This seems to occur only among disadvantaged families, despite the fact that closures produce substantial declines in monetary resources across the income distribution.

This concentration of negative effects among those at the bottom of the family income distribution prior to displacement is confirmed when we look

10. 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. Similarly, Oreopoulos, Page, and Stevens (2006) find that the effects of firm closures on children are largely concentrated among children whose family income is in the bottom quartile.

Table 7.5   Estimated long-term effects of firm closures for specific groups

Sample	Average effect of closure on log parental income (1)	Family income (2)	Earnings (3)	Completed high school (4)	Attended some college (5)	Received unemployment insurance (6)	Received AFDC (7)	Teen pregnancy (8)	Working (9)
								Effect of closure on children's adult outcomes	
(1) Full sample	-0.222***	0.002	0.020	-0.019	-0.015	0.056	0.049	-0.061	0.049***
(N = 1,444)	(0.050)	(0.060)	(0.076)	(0.020)	(0.048)	(0.045)	(0.038)	(0.042)	(0.018)
(2) Parent's income < Poverty line	-0.289***	-0.422	-0.456*	-0.282*	-0.116	-0.218*	-0.004	-0.489***	0.004
(N = 206)	(0.108)	(0.279)	(0.263)	(0.152)	(0.108)	(0.124)	(0.212)	(0.152)	(0.073)
(3) Parent's income < 1.5x Poverty line	-0.288***	-0.496***	-0.358*	-0.360***	-0.160***	-0.031	0.285	0.104	-0.019
(N = 393)	(0.081)	(0.166)	(0.200)	(0.105)	(0.052)	(0.106)	(0.186)	(0.144)	(0.051)
(4) Parent's income < 2x Poverty line	-0.206***	-0.274**	-0.135	-0.073	-0.078	0.006	0.048	0.032	0.013
(N = 589)	(0.067)	(0.121)	(0.127)	(0.075)	(0.064)	(0.078)	(0.101)	(0.102)	(0.035)
(5) Parent's income > 2x Poverty line	-0.219***	0.136*	0.059	-0.017	0.009	0.052	-0.021	-0.048	0.065***
(N = 855)	(0.058)	(0.070)	(0.097)	(0.020)	(0.055)	(0.057)	(0.032)	(0.050)	(0.023)
(6) Single parent	-0.327*	-0.304	-0.019	0.017	0.024	0.067	0.450*	0.318	-0.088
(N = 157)	(0.169)	(0.324)	(0.314)	(0.080)	(0.152)	(0.157)	(0.270)	(0.245)	(0.111)
(7) Black	-0.131	-0.171	-0.263	-0.152**	-0.179**	0.184**	0.084	-0.036	0.032
(N = 476)	(0.082)	(0.149)	(0.179)	(0.073)	(0.080)	(0.096)	(0.115)	(0.117)	(0.076)
(8) Parent has < High school education	-0.186***	-0.158	-0.004	-0.003	0.017	0.001	0.207**	-0.085	0.014
(N = 545)	(0.060)	(0.130)	(0.171)	(0.071)	(0.077)	(0.077)	(0.098)	(0.072)	(0.036)

Notes: Standard error estimates are shown are in parentheses. Regressions are weighted using the individual weight in last year the individual is observed with a nonzero weight. Columns (4) through (8) show estimated marginal effects based on probit models. Earnings and income are adjusted for age, year, and sex. Estimated impacts on children's adult outcomes control for parent's education, industry, and state.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

at other outcomes. Children with family income below 1.5 times the poverty line and whose parents experienced a job loss are 36 percentage points less likely to have completed high school than poor children whose parents were not displaced, and 16 percentage points less likely to have attended some college. There are positive estimated effects of parental displacement on both receiving welfare and experiencing a teen pregnancy among this group, though these effects are not statistically significant. There is a surprisingly strong and negative effect on teen pregnancy among those with income below the poverty line. This is an unexpected and implausibly large result, but is based on a very small sample of females.[11]

The next row of table 7.5 summarizes results for the cohort of children who were living with a single parent prior to job loss. There are few significant effects here, probably because the sample is extremely small. Those from single parent households are the only group for which there is a positive and significant effect of displacement on being a teen parent. Again, however, the small samples suggest caution in drawing conclusions from this group, and the estimated magnitude of the point estimate and standard errors are large enough to warrant some caution.

The next row displays the estimated effects of parental job loss on black children. The point estimates for income and earnings are negative and larger in magnitude than they are when the entire sample is included, but they are not statistically significant. There are negative and significant effects of parental job losses on college attendance among black children. There is also a significantly increased probability of receiving UI among black children whose parents lost jobs.

Finally, table 7.5 provides only limited evidence that the consequences of parental displacement may be larger for children of less-educated parents than for the population as a whole. Family income of the adult children is negatively affected by parental job loss, but this estimate is not statistically significant. Only the probability of receiving welfare is significantly higher among the children of less-educated parents.

In table 7.6 we focus on a different type of heterogeneity; specifically, whether the effects of parental job loss vary depending on the age at which the income shock occurs. This exercise is motivated by recent work in both child development and economics (summarized by Cunha et al. 2006), which suggests that (a) early childhood interventions may be more effective at reducing ability gaps than interventions undertaken at later ages, and (b) credit constraints operating in early life have a larger effect on adult outcomes than credit constraints encountered when children approach their late teens. To investigate this possibility, we add an interaction term between the displacement dummy and a variable indicating whether the child was under age seven at the time of the income shock to our basic regression. While

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11. There are 112 girls in this subsample, and forty-six of them experience a teen pregnancy.

Table 7.6   Estimated long-term effects of firm closures by age at displacement

	Family income		Earnings		Completed high school		Attended some college		Received unemployment insurance		Received AFDC		Teen pregnancy		Working	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Avg. adj. log parent's family income	0.339***	0.162***	0.296***	0.104*	0.076***	0.029***	0.223***	0.112***	-0.042*	-0.051*	-0.075***	0.002	-0.128***	-0.034	0.012	0.009
	(0.059)	(0.051)	(0.063)	(0.056)	(0.017)	(0.010)	(0.050)	(0.040)	(0.023)	(0.027)	(0.023)	(0.024)	(0.033)	(0.036)	(0.012)	(0.013)
Displaced (= 1 if yes)	0.011	0.013	0.030	0.047	-0.025	-0.014	-0.030	-0.002	0.063	0.047	0.041	0.031	-0.049	-0.072*	0.033**	0.051***
	(0.065)	(0.064)	(0.077)	(0.079)	(0.028)	(0.020)	(0.047)	(0.049)	(0.045)	(0.047)	(0.038)	(0.037)	(0.047)	(0.043)	(0.017)	(0.019)
Displaced at age ≤ 6 (= 1 if yes)	-0.123	-0.077	-0.269	-0.194	-0.030	-0.037	-0.076	-0.083	0.033	0.068	0.093	0.170	0.066	0.148	-0.014	-0.017
	(0.149)	(0.151)	(0.171)	(0.173)	(0.068)	(0.060)	(0.116)	(0.129)	(0.107)	(0.121)	(0.104)	(0.147)	(0.113)	(0.156)	(0.046)	(0.051)
Parent has less than HS education		-0.432***		-0.435***		-0.142***		-0.435***		0.063		0.150***		0.107*		-0.018
		(0.074)		(0.082)		(0.031)		(0.043)		(0.043)		(0.050)		(0.057)		(0.024)
Parent has exactly HS education		-0.119*		-0.121*		-0.021		-0.172***		-0.035		0.089**		0.043		0.03
		(0.064)		(0.071)		(0.021)		(0.046)		(0.037)		(0.040)		(0.055)		(0.020)
Parent industry fixed effects		X		X		X		X		X		X		X		X
Parent state fixed effects		X		X		X		X		X		X		X		X
Observations	1,314	1,314	1,301	1,301	1,428	1,367	1,428	1,418	1,409	1,390	722	590	746	690	1,317	1,317

Notes: Standard error estimates are shown in parentheses. Regressions are weighted using the individual weight in last year the individual is observed with a nonzero weight. Columns (5) through (14) show estimated marginal effects based on probit models. Earnings and income are adjusted for age, year, and sex. "X" indicates that parent industry fixed effects and/or parent state fixed effects are included in the regression.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

none of the estimated coefficients on the interaction terms are statistically significant, their magnitudes and the overall pattern suggests that the negative effects of family income shocks may be larger when the shocks are experienced at young ages. Every one of the interaction effects between parental displacement and the child being under seven at the time of displacement have the expected sign if the impact of displacement is worse at younger ages. Nearly all of the estimated coefficients on the interaction terms are an order of magnitude greater than the estimate on the displacement dummy. For example, the point estimates suggest that children under seven whose parents have experienced a job loss have adult earnings that are 18 percent lower than older children whose parents have been displaced, and an 8 percentage point lower probability of getting post-secondary schooling. While one cannot make too much of such imprecise estimates, the systematic pattern across all outcomes is striking. Such a pattern could be explained by the fact that younger children are exposed to the income shock for a longer period of time, but would also be consistent with theories suggesting that early childhood interventions have bigger impacts.

Taken as a whole, our results have important elements in common with two previous, and closely related, studies. First, using an administrative data set from Canada, Oreopoulos, Page, and Stevens (2006) show that there are large effects of firm closures on children's adult outcomes. Like our study, it shows that virtually all of these effects are driven by families in the lowest quartile of the income distribution. While our current study is based on a much smaller sample, and therefore produces estimates that are much less precise, both studies suggest that income, if it plays a role in children's success, is probably most important among those children with other forms of disadvantage. Second, work by Shea (2000) also using the PSID finds limited evidence of effects of parental income on children's adult outcomes for the full sample, but does find effects for those in the poverty subsample. Shea uses an instrumental variable strategy, including industry, union, and firm closures as the key instruments to isolate the role of parental income. Our estimates have the advantage of controlling for income prior to job loss, and then looking at the effects of job loss, which Shea acknowledges may be more exogenous than some of his other instruments. The fact that our results are qualitatively similar to Shea's suggests that the implied effects of income disruptions hold up, even after conditioning on the prior level of income.

Before concluding, we return to the literature on intergenerational correlations in income. How do our results fit in to this literature? Solon (1992) and Zimmerman (1992) find that the correlation in earnings between fathers and sons is approximately .40. If the income shock from displacement is exogenous, and if displacement does not produce long-run effects on children's outcomes through mechanisms other than income, we can use our estimates to measure how much of this .40 correlation reflects a causal effect. The point estimates for our full sample are close to zero and this is consistent with none of the correlation being driven by a causal relationship from

parental to kids' income. On the other hand, this zero point estimate is sufficiently imprecise that we also cannot rule out the possibility that all of the .40 correlation is due to causal effects.[12]

The more important message from this chapter with respect to estimated intergenerational correlations, however, is that nonlinearities in the relationship between parental income and kids' outcomes may be very important. The pattern of results in table 7.5 suggests that disadvantaged children are far more likely to experience negative effects from parental job losses than are children from households with average or higher income. Thus, simple correlations based on the entire income distribution cannot be easily decomposed into causal and other effects for the entire sample.

7.5   Conclusions

The question that motivates this chapter is whether family income itself, as opposed to the many family characteristics that are correlated with income, plays an important role in determining disadvantaged children's long-run socioeconomic success. Our strategy for answering this question is to focus on job displacement as an exogenous income shock. We compare outcomes among children whose families are very similar except that some families experienced a job loss. When we restrict the treatment group to families who lost jobs via a firm closure, we find little evidence that such shocks have long run effects on the next generation. Evidence of long-run negative effects is stronger when we include families who experienced a layoff, but we believe that layoffs are less likely to be exogenous events, so estimates based on this sample may reflect unobserved characteristics of the parents, rather than the effect of the income loss itself.

Although we find little evidence that income shocks affect the next generation's long-run socioeconomic success, on average, we do find evidence that such shocks have negative impacts on children who were already disadvantaged. In particular, we find that displacement substantially reduces the adult income of children whose parental income was already below the poverty line. We also find suggestive evidence that children whose parents are less educated, black, or unmarried suffer negative consequences, although these estimates are not typically statistically significant. The pattern of point estimates among the less advantaged samples suggests that these groups experience larger long-run losses when a parent loses his job, in spite of the fact that the magnitude of the income shock is similar across more and less advantaged groups.

Finally, the evidence that income affects children's long-term outcomes is

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12. The lower end of a 95 percent confidence interval around our main effect on kids' income of .002 is –0.12. Dividing this effect of displacement on kids' income by the effect of displacement on parental income (–0.22) gives an estimate of the effect of parental income of –0.12/–0.22, or .54, which is slightly larger than typical estimates of the overall intergenerational correlation.

strongest when the income shocks occur early in childhood. This may simply reflect the fact that, because firm closures have permanent effects on family income, an earlier shock will result in a larger total loss in income over the course of childhood. Alternatively, it may reflect a differential importance of financial resources early in childhood, as has been suggested by James Heckman and others.

Disruptions to family income do seem to compound the difficulties disadvantaged children will face as adults. There is little evidence that such disruptions have measurable negative effects on children who are otherwise relatively advantaged. This may help to explain one reason that the causal role of income on children's well-being has been difficult to establish in empirical research; such effects may only exist for certain subpopulations who face other obstacles or barriers to economic success as adults.

Appendix

Table 7A.1   **Estimated effects of all displacements during childhood on additional adult outcomes**

	(1)	(2)
Completed at least high school		
Displaced (= 1 if yes)	–0.086***	–0.047***
	(0.021)	(0.017)
Attended some college		
Displaced (= 1 if yes)	–0.127***	–0.082**
	(0.031)	(0.034)
Ever receive unemployment insurance		
Displaced (= 1 if yes)	0.104***	0.099***
	(0.029)	(0.031)
Ever receive AFDC (women only, N = 957)		
Displaced (= 1 if yes)	0.044*	0.021
	(0.026)	(0.023)
Teen pregnancy (women only, N = 957)		
Displaced (= 1 if yes)	0.031	0.010
	(0.034)	(0.035)
Working		
Displaced (= 1 if yes)	0.005	0.007
	(0.014)	(0.014)
Observations	1,790	1,790

Notes: Standard error estimates are shown in parentheses. Estimated marginal effects are based on probit models using the individual weight in last year the individual is observed with a nonzero weight. Column (1) controls for parent's average adjusted income. Column (2) adds controls for parent's education, industry, and state.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table 7A.2   Estimated long-term effects of all displacements for specific groups

Sample	Average effect of closure on log parental income (1)	Effect of closure on children's adult outcomes							
		Family income (2)	Earnings (3)	Completed high school (4)	Attended some college (5)	Received unemployment insurance (6)	Received AFDC (7)	Teen pregnancy (8)	Working (9)
(1) Full sample	-0.218***	-0.070	-0.095*	-0.047***	-0.082**	0.099***	0.021	0.010	0.007
(N = 1,790)	(0.034)	(0.044)	(0.051)	(0.017)	(0.034)	(0.031)	(0.023)	(0.035)	(0.014)
(2) Parent's income < poverty line	-0.194*	-0.237	-0.299**	-0.217**	-0.098	-0.123	-0.112	-0.111	-0.142***
(N = 320)	(0.113)	(0.181)	(0.150)	(0.089)	(0.064)	(0.094)	(0.126)	(0.171)	(0.048)
(3) Parent's income < 1.5x poverty line	-0.283***	-0.198**	-0.004	-0.124**	-0.068	-0.005	0.162**	0.218**	-0.001
(N = 584)	(0.074)	(0.096)	(0.133)	(0.062)	(0.049)	(0.064)	(0.081)	(0.089)	(0.035)
(4) Parent's income < 2x poverty line	-0.213***	-0.134	-0.056	-0.075*	-0.066	0.026	0.097	0.130**	-0.007
(N = 846)	(0.053)	(0.082)	(0.090)	(0.044)	(0.047)	(0.048)	(0.062)	(0.065)	(0.026)
(5) Parent's income > 2x poverty line	-0.221***	0.008	-0.099	-0.031*	-0.054	0.125***	0.007	-0.060	0.021
(N = 944)	(0.040)	(0.056)	(0.069)	(0.017)	(0.044)	(0.044)	(0.038)	(0.041)	(0.018)
(6) Single parent	-0.176*	-0.223	-0.120	0.059	0.120	0.035	-0.149	0.124	-0.065
(N = 210)	(0.105)	(0.232)	(0.218)	(0.064)	(0.103)	(0.119)	(0.176)	(0.172)	(0.068)
(7) Black	-0.151**	-0.155	-0.166	-0.080*	-0.072	0.075	-0.026	-0.001	-0.037
(N = 622)	(0.074)	(0.112)	(0.122)	(0.045)	(0.059)	(0.056)	(0.074)	(0.078)	(0.045)
(8) Parent has < High school education	-0.226***	-0.167**	-0.151	-0.120**	-0.085*	0.060	0.116**	0.089	-0.021
(N = 769)	(0.051)	(0.074)	(0.100)	(0.049)	(0.045)	(0.050)	(0.056)	(0.066)	(0.026)

Notes: Standard error estimates are shown in parentheses. Regressions are weighted using the individual weight in last year the individual is observed with a nonzero weight. Columns (4) through (8) show estimated marginal effects based on probit models. Earnings and income are adjusted for age, year, and sex. Estimated impacts on children's adult outcomes control for parent's education, industry, and state.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table 7A.3   Estimated long-term effects of all displacements by age at displacement

	Family income		Earnings		Completed high school		Attended some college		Received unemployment insurance		Received AFDC		Teen pregnancy		Working	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Avg. adj. log parent's family income	0.359***	0.175***	0.335***	0.129**	0.109***	0.050***	0.277***	0.154***	-0.031	-0.033	-0.080***	-0.031	-0.158***	-0.066*	0.024*	0.014
	(0.054)	(0.047)	(0.061)	(0.058)	(0.021)	(0.015)	(0.056)	(0.045)	(0.022)	(0.028)	(0.021)	(0.022)	(0.034)	(0.037)	(0.013)	(0.013)
Displaced (= 1 if yes)	-0.081*	-0.048	-0.082	-0.059	-0.078***	-0.042**	-0.114***	-0.071*	0.117***	0.113***	0.048*	0.03	0.023	-0.01	0.002	0.004
	(0.047)	(0.048)	(0.055)	(0.055)	(0.022)	(0.018)	(0.034)	(0.036)	(0.031)	(0.033)	(0.027)	(0.025)	(0.036)	(0.037)	(0.015)	(0.015)
Displaced at age ≤ 6 (= 1 if yes)	-0.076	-0.110	-0.140	-0.177**	-0.033	-0.019	-0.053	-0.046	-0.055	-0.061	-0.018	-0.034	0.043	0.106	0.017	0.014
	(0.078)	(0.069)	(0.086)	(0.087)	(0.039)	(0.030)	(0.060)	(0.063)	(0.051)	(0.051)	(0.041)	(0.027)	(0.071)	(0.082)	(0.025)	(0.026)
Parent has less than HS education		-0.406***		-0.450***		-0.180***		-0.410***		0.066*		0.162***		0.165***		-0.031
		(0.064)		(0.071)		(0.032)		(0.040)		(0.040)		(0.042)		(0.055)		(0.021)
Parent has exactly HS education		-0.133**		-0.170**		-0.034		-0.166***		-0.023		0.138***		0.099*		0.021
		(0.060)		(0.069)		(0.026)		(0.043)		(0.037)		(0.045)		(0.059)		(0.019)
Parent industry fixed effects		X		X		X		X		X		X		X		X
Parent state fixed effects		X		X		X		X		X		X		X		X
Observations	1,623	1,623	1,597	1,597	1,772	1,728	1,772	1,764	1,748	1,725	933	825	957	891	1,626	1,626

Notes: Standard error estimates are shown in parentheses. Regressions are weighted using the individual weight in last year the individual is observed with a nonzero weight. Columns (5) through (14) show estimated marginal effects based on probit models. Earnings and income are adjusted for age, year, and sex. "X" indicates that parent industry fixed effects and/or parent state fixed effects are included in the regression.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

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