

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

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Abstract: This paper uses layoffs and business closings to identify the effect of a permanent parental income shock on children's long-run socioeconomic outcomes. We find that estimates of the intergenerational effects of parental job loss are sensitive to our definition of job displacement. Focusing on measures of displacement that are most likely to be exogenous, we find suggestive evidence that the long-term consequences of parental job loss are substantive for children growing up in low-income families, and largest for young children.

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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 that 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.

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, for example, 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, 2004; 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 pre-displacement 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 not very precise and must be interpreted cautiously.

Nevertheless, the patterns we observe generate three broad conclusions: 1) 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. 2) Although we find no evidence that firm closings have an intergenerational effect on average, there is evidence that such events impose long-term costs to disadvantaged children, and 3) the effects of

exogenous income shocks are largest for young children. We discuss these findings in more detail in the remainder of this chapter.

I. 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.

I.A. Estimating the Effect of Displacement on a Family's Resources

We begin by following the empirical strategy introduced by Jacobson, LaLonde and Sullivan (JLS 1993), 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.

$$\ln I_{it} = D_{it}\delta + \beta_1 age_{it}^2 + Y\gamma_t + \alpha_i + u_{it} \quad (1)$$

where $\ln I_{it}$ is the log of family i 's resources in year t , and 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. In addition, we control for family-specific fixed effect, α_i . Since the model includes fixed effects, characteristics of the family head that do not vary over time, such as race and education, are not included.

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.

The vector of displacement indicators (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 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 6 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 differences in family background. In the main part of our analysis we use this shock as a source of variation in children's resources.

I.B. 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:

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

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 their estimated effect on children's outcomes.

II. Data

We use data from the 1968-2003 waves of the Panel Study of Income Dynamics (PSID), focusing on the displacement experiences of households that include children ages 15 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. 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 16. Children over age 16 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. 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, each child must be observed as the head or spouse of their own household for at least 3 years. 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.² This allows us to include income 3 to 5 years prior to either first displacement (for the treatment group) or 3 to 5 years prior to the reference year (for the control group) in our regressions.³ 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 5 preceding

² For example, because 8.1 percent of the displaced children have a parental displacement at age 8, controls are assigned age 8 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 her assigned reference age, we use the earliest age at which she is observed. If a control child is not in the survey at the assigned reference age or earlier, she is dropped.

³ We have also replicated our results using a simpler design which uses average income when the child is between ages 0 and 4 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.

years. This helps ensure that displacements are exogenous conditional on pre-displacement income, while maintaining consistent sample requirements for the treatment and control groups.

To ensure that our pre-displacement 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 pre-displacement income.

IV. Results

IV.A. Summary Statistics

Sample summary statistics are shown in Table 1. Our sample contains over 1600 children, 610 of whose fathers lost their jobs due to a layoff or firm closing sometime when the children were under age 15, and 222 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 3-5 years before the displacement (reference) year are \$42,000 among heads in the treatment group, and \$53,000 among heads in the control group. The difference in average pre-displacement earnings is much smaller when heads who experienced layoffs are eliminated from the sample: \$51,000 among those who will eventually lose their jobs to a firm closure, and \$53,000 among those who will not. Pre-displacement 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 pre-displacement income. The legitimacy of this assumption will be discussed in Section IV.C.

There are also substantial differences in the educational distributions 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% 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 pre-displacement income, the additional characteristics have little impact on the estimated effect of losing a job due to a firm closure.

Table 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 3-5 years after displacement is approximately 62,000, while average family income among those with no parental displacement is more than \$85,000. This suggests that firm closings and layoffs produce substantial shocks to a child’s financial resources.

Table 1 also shows that treatment and control children have somewhat different labor market outcomes as adults. For example, average family income between ages 25 and 27 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 UI receipt than the controls. Given the pre-displacement 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.

IV.B. 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 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 37% and family income falls by 21%.^{4 5} Family resources recover somewhat over time, but even 6 years later head's earnings are approximately 27% lower than they would have been if the displacement had not occurred. Similarly, family income is reduced by 19 percent. These estimates are all 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.

The first 2 rows of Table 2 also show what happens to family resources in the years prior to the displacement. Here we see that these resources begin to deteriorate slightly even before the displacement occurs. For example, two years prior to the displacement, family income is five percent lower than it would have been if the displacement had not occurred. Previous work has found this pattern as well (Stevens, 1997).

IV.C. 1 Intergenerational Effects of Displacement—Income and Earnings

Having established that displacement substantially reduces family resources, we now investigate its intergenerational effects. Table 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 24 and for each year thereafter. Specially, we consider the log of average earnings and the log

⁴ The percentage effect on earnings is computed as $e^{\delta} - 1$.

⁵ 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.

of average family income. As in Table 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 OLS regression of the log of the child's income in adulthood on the log of average family income 3-5 years before the "displacement" year. The estimated coefficient of 0.35 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, 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% 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 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 the sample restriction reduces 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 3 shows the effects of parental job loss on 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% lower than children whose parents did not, even after controlling for family background characteristics other than income. The magnitude of this estimate 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.⁶ The results from Table 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 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. Since 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 pre-displacement income. For this reason, in the rest of the paper, we focus on displacements that occur only as the result of business closings.

⁶ 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.

IV.C.2. Intergenerational Effects of Displacement—Other Outcomes

Table 4 shows the estimated relationship between parental job loss and other measures of socioeconomic success. Specifically, we include a dummy variable indicating whether the child filed for unemployment benefits, a dummy variable indicating whether she filed for AFDC (females only), and indicators for whether the child had at least a high school education, or more than a high school education. 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 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 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 and statistically significant (approximately 10%) regardless of whether additional family background characteristics are included in the regression. The fact that we see increased UI receipt among children whose parents faced a firm closure is somewhat surprising, given the absence of effects on their earnings or income. One possibility is that this primarily reflects higher take-up of UI among children whose parents may have been more likely to have experience with the UI system.

The last two rows of Table 4 display the estimated impact of firm closings on welfare receipt and teen pregnancy. Neither set of estimates is statistically significant, except for the last row and column of Table 4, where the sign of the estimate is the opposite of what one would expect if family income shocks negatively impact children.

IV. D. 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 SES families, so we might expect the intergenerational impact on children growing up in such families to be larger.⁷ 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 four years prior to displacement), parental education, and race.

Table 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 shows estimates for the full sample. Among those children whose parental income was below the poverty line four years before the job loss, there is a larger than average impact of displacement on family income in the parent's generation.

The large monetary effects seem to carry over into some statistically and economically significant effects on the next generation. The family incomes of poor children whose parents experienced a plant closing are 40 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, but this estimate is not statistically different from zero. For comparison, the next line of Table 5 shows estimated effects for children from families who were living well above the poverty line prior to displacement. Here, the estimated effects on children's eventual earnings and income are small and positive. These results suggest that the apparent negligible effect of firm closings on children overall masks substantial heterogeneity. This seems to occur despite the fact that closures produce substantial declines in monetary resources across the income distribution.

Looking at other outcomes, poor children whose parents experienced a job loss are 17 % less likely to have completed high school than poor children whose parents were not displaced,

⁷ 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.

although the effect is not statistically different from zero. There is no evidence of negative effects on college attendance, UI or AFDC receipt, or teen pregnancy among this group.

The next line of Table 5 summarizes results for the cohort of children who were living with a single parent prior to job loss. Here there is only limited evidence of effects on family income; the point estimate is large and negative, but far from statistical significance. Interestingly, this group is the only group for which the income shock has a positive effect on the likelihood of teen pregnancy.

The next row displays the estimated effects of parental job loss on black children. The point estimates for income and earnings are larger than they are when the entire sample is included, but they are not statistically significant. There is a large negative effect on college attendance among black children whose parents faced plant closings, which is significant at the 10% level. The increased probability of receiving UI is particularly large for this group.

Table 5 also provides suggestive evidence that the consequences of parental displacement may be larger for children of less-educated parents than for the population as a whole. The estimated coefficient on the displacement dummy is not statistically different from zero, but it is large and negative.

In Table 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, Heckman, Lochner and Masterov, 2006) which suggests that 1) early childhood interventions may be more effective at reducing ability gaps than interventions undertaken at later ages, and 2) credit constraints operating 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 7 to our basic regression. While almost none of the estimated coefficients on the interaction terms are statistically significant, their magnitudes suggest that the

negative effects of family income shocks may be substantially larger when the shocks are experienced at young ages. Nearly all of the estimated coefficients on the interaction terms are an order of magnitude greater than the estimate on the displacement dummy, and in the expected direction. For example the point estimates suggest that children under 7 whose parents have experienced a job loss have adult earnings that are 18% lower than older children whose parents have been displaced, and a 13% 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 dataset 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 sub-sample. 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.

V. Conclusions

This paper was motivated by the question of whether family income itself, as opposed to the many family characteristics that are correlated with income, plays an important role in the long-run outcomes of disadvantaged children. Our strategy for answering this question was to focus on job displacement as an exogenous income shock and examine how children subjected to such a shock compare to children from otherwise similar families. Our overall results suggest that, when restricting the treatment group to children affected by firm closings, there is little evidence that even the very large negative income shocks experienced by the affected families result in negative outcomes for the children when followed-up as young adults. We do show negative effects on children whose parents were laid off, but believe that such results may reflect unobserved characteristics of the parents, rather than the resulting income loss itself. For firm closings, there is less concern that omitted parental characteristics are driving the results.

This finding of no average affect of income shocks, however, may obscure important effects of negative income shocks for children who already face some level of disadvantage. In particular, in our sample of children who have incomes below the poverty line prior to any parental displacement, there is evidence of negative effects of the displacement on children. Poor children whose parents were displaced have family incomes as adults that are reduced by more than 40 percent, and these reductions are statistically significant. These same children have estimated earnings reductions that are also quite large, but these are not statistically significant. When we focus on groups of children whose parents are less educated, black or unmarried, there is again suggestive evidence that firm closures have negative effects on a variety of outcomes, although the reduced sample sizes mean that most of these effects are not statistically significant. The pattern of point estimates, however, is consistent with stronger evidence of effects among all of the disadvantaged groups, when compared with the full sample of children across the income distribution and across demographic characteristics. This is true despite the finding that the

magnitude of the income shock from displacement is quite similar across the different subgroups; thus, the finding of more negative effects among disadvantaged children does not simply reflect a larger shock to their family resources.

Finally, the evidence that income affects children's long-term outcomes is 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 less 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 sub-populations who face other obstacles or barriers to economic success as adults.

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Table 1
Sample Means By Parent's Displacement Status

	Using All Displacements			Using Only Closures		
	Displaced	Control	All	Displaced	Control	All
Parents						
Earnings	35,608	49,276	44,758	42,366	49,087	48,068
3-5 years prior to displacement (reference) year	41,813	52,945	49,207	50,788	53,285	52,906
3-5 years after displacement (reference) year	3,885	56,612	50,733	45,477	56,229	54,598
Family Income	58,659	78,213	71,651	68,350	78,163	76,676
3-5 years prior to displacement (reference) year	53,935	66,899	62,549	64,870	67,751	67,314
3-5 years after displacement (reference) year	61,551	84,738	77,137	72,039	85,007	83,084
Education < HS	0.43	0.27	0.32	0.33	0.27	0.28
Education = HS	0.26	0.28	0.27	0.30	0.29	0.29
Education > HS	0.31	0.44	0.40	0.37	0.44	0.43
Displacement (reference) year	1976	1976	1976	1977	1976	1976
Age	39.2	41.8	41.0	41.2	42.1	41.9
Kid's age	10.9	11.7	11.42	11.7	12.0	12.0
Children						
Age	30.5	30.7	30.5	30.7	30.7	30.7
Average Earnings	27,374	31,145	29,883	33,161	31,106	31,417
Earnings at ages 25-27	22,899	25,012	24,320	26,991	25,120	25,384
Average Income	51,574	59,401	56,774	60,213	59,226	59,376
Family Income at ages 25-27	43,932	46,828	45,877	50,576	46,742	47,288
Education < HS	0.19	0.11	0.14	0.12	0.11	0.11
Education = HS	0.39	0.32	0.34	0.34	0.33	0.33
Education > HS	0.42	0.57	0.51	0.53	0.57	0.56
Ever received UI	0.33	0.23	0.26	0.30	0.23	0.24
Received UI in a given year	0.08	0.06	0.06	0.07	0.06	0.06
Ever received AFDC/TANF	0.14	0.08	0.10	0.12	0.08	0.09
Received AFDC/TANF in a given year	0.05	0.03	0.04	0.05	0.03	0.04
Had Teenage Pregnancy	0.26	0.20	0.22	0.16	0.20	0.20
N (Number of individuals)	610	1,044	1,654	222	1,115	1,337
N (total person-years)	5,560	10,243	15,803	2,004	11,027	13,031

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 23. Money variables are measured in 2002 dollars. Means for AFDC and teen pregnancy are calculated for women only.

Table 2
Estimated Effects of First Displacements on Parent's Log Earnings and Log Family Income

Dependent Variable:	All Displacements		Only Closures	
	Ln Earnings	Ln Family Income	Ln Earnings	Ln Family Income
	(1)	(2)	(3)	(4)
3 years prior to displacement	-0.049 (0.038)	-0.021 (0.026)	-0.040 (0.047)	-0.035 (0.035)
2 years prior to displacement	-0.068* (0.041)	-0.046* (0.028)	-0.049 (0.048)	-0.074** (0.037)
1 years prior to displacement	-0.135*** (0.051)	-0.077** (0.033)	-0.155** (0.070)	-0.144*** (0.052)
Year of displacement	-0.331*** (0.061)	-0.193*** (0.043)	-0.264*** (0.098)	-0.200*** (0.072)
1 year after displacement	-0.463*** (0.065)	-0.234*** (0.044)	-0.412*** (0.083)	-0.252*** (0.062)
2 year after displacement	-0.308*** (0.058)	-0.185*** (0.040)	-0.231*** (0.082)	-0.166*** (0.057)
3 year after displacement	-0.398*** (0.074)	-0.229*** (0.045)	-0.353*** (0.097)	-0.275*** (0.069)
4 year after displacement	-0.365*** (0.071)	-0.200*** (0.047)	-0.336*** (0.096)	-0.229*** (0.074)
5 year after displacement	-0.441*** (0.107)	-0.196*** (0.047)	-0.302*** (0.089)	-0.172*** (0.060)
6 or more years after displacement	-0.308*** (0.056)	-0.214*** (0.042)	-0.271*** (0.074)	-0.242*** (0.058)
Number of individuals	928	928	780	780
Person-year observations	19,183	19,183	16,417	16,417
R-squared	0.601	0.643	0.588	0.634

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.

- * denotes significance at the 10% level
- ** denotes significance at the 5% level
- *** denotes significance at the 1% level

Table 3
Estimated Effects of Parental Displacement on Family Income and Earnings As Adults

	All Displacements			Only Closures		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Average adjusted adult family income</i>						
Avg. adj. log parent's family income	0.350*** (0.053)	0.343*** (0.052)	0.160*** (0.045)	0.352*** (0.059)	0.352*** (0.059)	0.187*** (0.053)
Displaced (=1 if yes)		-0.096** (0.044)	-0.061 (0.044)		-0.003 (0.061)	-0.002 (0.060)
Parent less than HS graduate			-0.416*** (0.062)			-0.409*** (0.073)
Parent has exactly HS education			-0.131** (0.058)			-0.108* (0.063)
Parent Industry fixed effects			X			X
Parent State fixed effects			X			X
Observations	1654	1654	1654	1337	1337	1337
R-squared	0.069	0.073	0.188	0.070	0.070	0.191
<i>Average adjusted adult earnings</i>						
Avg. adj. log parent's family income	0.332*** (0.060)	0.323*** (0.059)	0.132** (0.056)	0.311*** (0.063)	0.311*** (0.063)	0.114** (0.056)
Displaced (=1 if yes)		-0.115** (0.050)	-0.098** (0.050)		-0.003 (0.072)	0.012 (0.075)
Parent less than HS graduate			-0.438*** (0.070)			-0.440*** (0.081)
Parent has exactly HS education			-0.147** (0.066)			-0.131* (0.071)
Parent Industry fixed effects			X			X
Parent State fixed effects			X			X
Observations	1,624	1,624	1,624	1,321	1,321	1,321
R-squared	0.047	0.051	0.149	0.042	0.042	0.145

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.

* denotes significance at the 10% level

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*** denotes significance at the 1% level

Table 4
Estimated Effects of Firm Closures During Childhood on Additional Adult Outcomes

	(1)	(2)
<i>Complete at least high school</i>		
Displaced (=1 if yes)	-0.020 (0.027)	-0.012 (0.021)
<i>Attended some college</i>		
Displaced (=1 if yes)	-0.037 (0.046)	-0.013 (0.049)
<i>Ever receive unemployment insurance</i>		
Displaced (=1 if yes)	0.077* (0.044)	0.081* (0.046)
<i>Ever receive AFDC</i>		
Displaced (=1 if yes)	0.038 (0.034)	0.035 (0.033)
<i>Teen Pregnancy</i>		
Displaced (=1 if yes)	-0.048 (0.044)	-0.072* (0.041)
Observations	1,337	1,337

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 parents average adjusted income. Column 2 adds controls for parent's education, industry, and state.

* denotes significance at the 10% level

** denotes significance at the 5% level

*** denotes significance at the 1% level

Table 5
Estimated Long-term Effects of Firm Closures For Specific Groups

	Effect of Closure on Children's Adult Outcomes							
	Average Effect of Closure on Log Family Income	Family Income	Earnings	Completed High School	Attended some college	Received Unemployment Insurance	Received AFDC	Teen Pregnancy
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:								
Full Sample (N=1,337)	-0.137*** (0.008)	-0.002 (0.060)	0.012 (0.075)	-0.012 (0.021)	-0.013 (0.049)	0.081* (0.046)	0.035 (0.033)	-0.072* (0.041)
Parent's Income < Poverty Line (N=170)	-.156*** (0.024)	-0.541* (0.313)	-0.327 (0.385)	-0.190 (0.147)	0.233 (0.233)	0.079 (0.214)	-0.054 (0.249)	0.000 (0.177)
Parent's Income > 2x Poverty line (N=729)	-.120*** (0.008)	0.090 (0.077)	0.053 (0.087)	-0.004 (0.020)	0.022 (0.060)	0.129* (0.066)	0.083 (0.087)	-0.063 (0.064)
Single Parent (N=141)	-.146*** (0.013)	-0.263 (0.338)	0.098 (0.327)	0.012 (0.122)	-0.098 (0.138)	0.084 (0.181)	0.206 (0.355)	0.535** (0.210)
Black (N=372)	-.133*** (0.019)	-0.098 (0.154)	-0.120 (0.183)	-0.092 (0.078)	-0.175* (0.092)	0.296*** (0.105)	-0.054 (0.128)	-0.031 (0.129)
Parent has < High School Education (N=498)	-.150*** (0.035)	-0.174 (0.133)	-0.007 (0.172)	0.031 (0.072)	0.032 (0.085)	0.025 (0.081)	0.101 (0.082)	-0.140** (0.065)

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.

* denotes significance at the 10% level

** denotes significance at the 5% level

*** denotes significance at the 1% level

Table 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	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Avg. adj. log parent's family income	0.352*** (0.059)	0.187*** (0.053)	0.311*** (0.063)	0.114** (0.056)	0.088*** (0.020)	0.037*** (0.012)	0.244*** (0.057)	0.132*** (0.044)	-0.034 (0.023)	-0.042 (0.028)	-0.065*** (0.022)	0.004 (0.023)	-0.141*** (0.035)	-0.044 (0.038)
Displaced (=1 if yes)	0.015 (0.065)	0.009 (0.063)	0.035 (0.077)	0.039 (0.079)	-0.012 (0.029)	-0.004 (0.020)	-0.019 (0.048)	0.008 (0.051)	0.067 (0.046)	0.065 (0.047)	0.025 (0.035)	0.018 (0.031)	-0.058 (0.047)	-0.088** (0.041)
Displaced at age <=6 (=1 if yes)	-0.127 (0.149)	-0.080 (0.150)	-0.277 (0.170)	-0.198 (0.174)	-0.052 (0.082)	-0.071 (0.083)	-0.117 (0.124)	-0.136 (0.139)	0.072 (0.117)	0.132 (0.135)	0.112 (0.117)	0.159 (0.148)	0.104 (0.126)	0.241 (0.205)
Parent has less than HS education		-0.409*** (0.073)		-0.438*** (0.081)		-0.141*** (0.032)		-0.441*** (0.045)		0.068 (0.043)		0.090** (0.040)		0.112** (0.056)
Parent has exactly HS education		-0.105* (0.063)		-0.125* (0.070)		-0.018 (0.023)		-0.166*** (0.048)		-0.053 (0.037)		0.042 (0.034)		0.046 (0.055)
Parent Industry fixed effects		X		X		X		X		X		X		X
Parent State fixed effects		X		X		X		X		X		X		X
Observations	1337	1337	1321	1321	1321	1264	1321	1311	1290	1270	693	544	698	646

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.

* denotes significance at the 10% level

** denotes significance at the 5% level

*** denotes significance at the 1% level