CHILDHOOD DEPRIVATION AND ADULT WEALTH

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ABSTRACT: popular mythology holds that children growing up during the Great Depression are more frugal than subsequent generations. This paper asks whether childhood deprivation raises adult thriftiness. Specifically, I examine whether PSID children whose fathers are exogenously displaced from their jobs have more wealth as adults than children with otherwise similar fathers who do not experience job loss. I find that father's displacement does indeed raise children's net worth. This impact is most pronounced for job losses occurring when the child is an adolescent, and is concentrated on vehicles, unsecured debt and business equity. The impact of father's displacement does not appear to be driven by lower expected bequests, and I find no evidence that father's displacement forced children to become economically mature at a younger age. My results are consistent with anecdotal evidence suggesting that childhood deprivation causes subsequent aversion to debt, and are also consistent with the popular idea that post-Baby Boom generations are more entrepreneurial and independent than previous generations because of their exposure to corporate downsizing and deindustrialization.

"These kids amaze me today. I mean, they're smokin' and a bus comes, and they throw away the whole butt. I can't...Some days when I'm ridin' around in buses, I find the next day a half a dozen butts. I put 'em in the ash tray for when I run out of cigarettes."

--Frank Czerwonka, teenager during the Great Depression, quoted in Studs Terkel's *Hard Times* (page 38)

"I remember how (my father) used to scrounge and scrape to make his mortgage payments. He borrowed money from a loan company very frequently. Oh the tension in the house, when Pa used to scramble around trying to get enough money to pay that installment loan. That was the one degrading thing I remember....The worst thing I have to do every month is to pay my mortgage. I hate to think of all that interest being paid. If I had the money, I'd pay it all off. I just loathe it."

--Anna Ramsey, child during the Great Depression, quoted in *Hard Times* (page 78)

"Economic values from the Depression experience have not received their due in our analysis for lack of appropriate information. This is unfortunate since there is probably a higher ratio of speculation to evidence on this presumed legacy of the 30s than on any other 'effect'...'Depression' rationales for current economic habits in retrospective reports are no substitute for the results of a longitudinal analysis."

--Glen Elder, *Children of the Great Depression* (page 283)

I. INTRODUCTION

It is commonly believed that children exposed to economic hardship save more as adults. According to folk wisdom, for instance, children who grew up during the Great Depression learned to be more frugal and financially conservative than children who grew up during the more affluent 50s and 60s. Some economists have suggested that a generational shift in attitudes towards saving may be important to explaining the decline in personal savings rates and the rise in the ratio of consumption to GDP observed in the US during the 1980s and early 1990s (e.g Parker 1999; Browning and Lusardi 1996).

It is not hard to think of plausible mechanisms by which childhood hardship could lead to greater adult saving. Most prosaically, if unemployment or other adverse economic shocks cause parents to accumulate less wealth, children will rationally anticipate smaller bequests from their parents and will have to save more for their own retirement, all else equal. If preferences exhibit habit formation, then a period of hardship could acclimate children to low levels of consumption, lowering the marginal utility of spending as an adult. Observing adverse shocks to parents could make children more aware of economic risks, which in turn could raise precautionary savings as an adult. Difficulty repaying debts during a downturn in family fortunes may create so much stress that children become viscerally averse to debt as adults. Alternatively, the need to mobilize resources for family survival may force children to become economically mature at an earlier age, a phenomenon sociologist Glen Elder labels "the downward extension of adulthood" in his influential book *Children of the Great Depression* (1974).

While there is a large amount of anecdotal evidence linking childhood hardship, and growing up during the Great Depression in particular, to future thriftiness (see Studs Terkel's oral history *Hard Times* (1970), for instance), there is little formal evidence that growing up in hard times leads to more saving as adults. Elder (1974) examines data from the longitudinal Oakland Growth Study, which tracked a group of 167 children who were fifth graders in Oakland (CA) public schools in 1931, and reports that boys whose families were relatively deprived during the 1930s are more likely to report regular saving as adults in 1958. Elder also reports, however, that having a job as a teenager is more strongly correlated with adult saving than childhood deprivation.

Among economists, a handful of recent papers (e.g. Bosworth, Burtless and Sabelhaus (1991), Gokhale, Kotlikoff and Sabelhaus (1996), Attanasio (1998), Parker (1999)) have used panel data on consumption, income and savings at the cohort level to estimate the contributions of population aging and generational turnover to the recent decline in personal saving in the US. Results are mixed; Attanasio, for instance, finds that cohorts born between 1920 and 1939 saved *less* than postwar cohorts did at similar ages, while Parker finds that postwar generations spent more than Depression-era cohorts. While these studies are interesting in their own right, it is questionable whether consumption data by cohort over time can identify the impact of childhood hardship on adult savings (and to be fair, none of the above papers claim to identify such an impact). In the first place, such data cannot separately identify the impact of cohort, age, and time, due to linear dependence: time equals birth year plus age. Intuitively, if we observe that 70 year olds in 1990 spent more relative to income than 70 year olds in 1980, we could infer that the 1920 birth cohort is inherently less frugal than the 1910 birth cohort—or we

could infer that some sort of macroeconomic shock, such as a booming stock market, made it rational for 70 year olds to spend more relative to income in 1990 than in 1980. Attanasio and Parker identify cohort effects by assuming that time effects have zero mean and no linear trend, but this just begs the question of whether declining savings rates are driven by cohort turnover or by aggregate shocks.

Secondly, even if we accept a particular set of identifying assumptions, the link between cohort intercepts and savings preferences is unclear. If we observe cohorts over their entire lifespan, then abstracting from bequests the lifetime budget constraint must hold: cohort differences in preferences may lead to differences in the *timing* of consumption over the life cycle, but cannot cause permanent differences in the *level* of consumption relative to permanent income. Given that we do not observe cohorts over their entire lifespans, the tyranny of the lifetime budget constraint is not absolute; Attanasio and Parker, for instance, interpret their cohort effects as being due to generational differences in the ratio of observed income to permanent income, due to factors such as expected differences in the generosity of social security across generations. It is worth noting, however, that one cannot rule out perverse preference-based explanations for cohort effects—Depression-era cohorts may spend more in their 50s and 60s than surrounding cohorts precisely because they were frugal in their 20s and 30s and accumulated a larger nest egg.

Third and finally, even if we could somehow identify cohort differences in savings preferences, we would not be able to link these conclusively to childhood experiences, because different cohorts experience different shocks at all phases of the life cycle. Intuitively, if Depression-era children accumulated more savings than subsequent

generations, it may be because they grew up during the 1930s—or it may be because they faced a booming stock market, subsidies to homeownership, or unexpectedly rapid income growth during the 1950s, among other possibilities.

This paper takes an approach similar to Elder's work: rather than compare savings across cohorts, I exploit variation within a cohort. Specifically, I identify 1225 children from the initial 1968 wave of the Panel Study of Income Dynamics (PSID), and compare adult wealth holdings in 1989 of children whose fathers did and did not experience involuntary job loss. Of course, relying on within-cohort variation carries its own set of identification problems. Most fundamentally, deprivation is not randomly assigned in a cross-section. Fathers with poor work habits, low innate ability or poor self control are more likely to expose their children to deprivation, and if these attributes are transmitted across generations by genes or culture, then deprived children are likely to earn less and may have a lower willingness to save out of a given level of income as well. It was precisely concern over this type of heterogeneity that led Elder to focus on what he called "decremental deprivation" in his study of Depression-era children, classifying families according to their income change between 1929 and 1933, rather than by their income level.

Even decremental deprivation is not randomly assigned, however. Involuntary job loss and other adverse shocks that cause sudden changes in a family's income are likely to affect fathers with low human capital disproportionately. I attempt to overcome this bias by focusing on the type of involuntary job loss most likely to be exogenous--job loss due to establishment deaths, such as plant closings, plant relocations and business failures. The collective and discrete nature of establishment deaths suggests that they are

less likely to be indicative of poor work performance than, say, being fired or laid off. Table 1 presents some suggestive evidence. The table reports probit estimates, for the 667 fathers in my sample, of the marginal impact of fathers' demographic and skill covariates on the probability of reporting at least one job loss due to establishment death (or, alternatively, firing or layoff) between 1968 and 1984, with heteroscedasticity-robust standard errors in parentheses. The only significant determinant of job loss due to establishment death is fathers' age in 1968, which has a negative impact on the probability of job loss. This result is in part due to truncation bias; the PSID did not gather complete retrospective work histories in 1968, so we observe a smaller fraction of the career of workers who were older in 1968. Job loss due to firing or layoff, meanwhile, is significantly affected by age, education, region and being a laborer or service worker. The likelihood ratios for the two models confirm that fathers' observables have more explanatory power for being fired or laid off than for job loss due to establishment death. In what follows, my preferred estimates of the impact of job loss due to establishment death on children's wealth will control for fathers' demographics and observable skills; my identifying assumption will be that job loss due to establishment death is uncorrelated with unobserved factors affecting children's willingness to save, conditioning on fathers' demographics and observable skills. Still, it is comforting to note that establishment deaths are, if not completely exogenous, at least "more exogenous" than being fired or laid off.

Even if establishment deaths are exogenous, another identification problem looms: parental resources may have a direct, causal impact on children's human capital. Previous research (Ruhm (1991); Jacobson, Lalonde, and Sullivan (1993); Stevens

(1997)) has estabilshed that job loss due to establishment death has a large and persistent negative impact on family income. In my sample, experiencing at least one establishment death reduces fathers' average annual real labor plus transfer income over the period 1968-1984 by $2116 in 1982-84 dollars (with a heteroscedasticity-robust standard error of 1235). If capital markets are imperfect, parents with low income may be forced to invest suboptimally in children's skills (Loury (1981); Becker and Tomes (1986); Mulligan (1997)). In this case, children exposed to hardship are likely to earn less as adults than otherwise similar children who were not exposed to hardship; they may also have lower savings rates, if savings rates rise with income (Dynan, Skinner and Zeldes (2000)), or if education and other parental investments have a direct effect on time preferences or the ability to plan for the future (Becker and Mulligan (1997)).

Fortunately, this source of bias is unlikely to affect my results, because empirically father's job loss due to establishment death does not reduce children's human capital in my sample. Table 2 presents OLS estimates of the impact of fathers' job loss on chidren's years of schooling and permanent income, defined as the average annual real labor plus transfer income of the child and 1989 spouse, averaged over all years from 1969-1989 in which the child is a head or spouse. Standard errors are in parentheses and are robust to heteroscedasticity and cross-correlation of disturbances among siblings. The key right hand side variable is DADLOSS, which equals one if the father reports job loss to establishment death (or, in the bottom rows, firing or layoff) while the child is 18 or younger. When I control only for a constant term, establishment death has a negative and significant impact on children's income and education. However, this effect becomes insignificantly negative when I control for child's demographic variables, and becomes

insignificantly positive when I add controls for father's demographic and skill covariates. The control variable most responsible for driving down the impact of job loss on children's human capital is child's age, and this is because of the truncation bias discussed above: older children earn more and are more likely to have completed their schooling, and meanwhile we are less likely to observe displacement among fathers of older children, because we do not observe fathers' complete retrospective job histories. The bottom rows of Table 2 present analagous results for job loss due to firing and layoffs. This type of job loss has a significant negative estimated impact on children's earnings and schooling even after controlling for children's demographics, consistent with the idea that being laid off or fired is relatively endogenous with respect to inheritable endowments, although once I control for father's observables this impact becomes insignificant. The low impact of job loss on children's earnings and education is consistent with my earlier work (Shea 2000) showing that parents' money *per se* doesn't matter for children's human capital in the PSID; indeed, job loss due to establishment death is one of the instruments I use in that paper to isolate variation in parental income that is orthogonal to unobservable inheritable parental ability.

To preview my results, I do indeed find a positive and significant effect of childhood hardship on adult wealth: children of exogenously displaced fathers have significantly higher net worth as adults than children whose father does not experience establishment death. This finding is robust to alternative transformations of net worth, designed to minimize the impact of outliers, and the impact of father's job loss on children's wealth is positive even controlling only for child's demographics, although to maximize precision my preferred estimates control for father's demographics and

observable skills and measures of children's permanent income. Father's job loss due to firing or layoff has an insignificant impact on child's net worth, suggesting perhaps a downward bias due to correlation between these types of job loss and unobservable inheritable heterogeneity correlated with saving. The impact of job loss on wealth is concentrated among children aged 10 to 18 at the time of the shock, consistent with the idea that adolescent experience is critically important to forming character, and is strongest for vehicles, unsecured debt and business wealth. Lower expected bequests do not appear to be responsible for the impact of father's displacement on wealth, and contrary to Elder's findings for Depression-era children I find no evidence that children witnessing job loss become economically mature at a younger age.

II. DATA AND SPECIFICATION

The Panel Study of Income Dynamics has tracked the labor market behavior of a fixed group of families since 1968. As the children of the original PSID households have grown up and moved out, their new splitoff households have been incorporated into the PSID, making the PSID an especially valuable tool for investigating intergenerational links. The initial waves of the PSID contained two subsamples, one nationally representative as of 1968 and one designed to oversample the poor. My baseline sample consists of children of original families from the nationally representative subsample who are 18 or younger in 1968; who are household heads or spouses aged 25 or older in 1989 with no change in head or spouse in the last year; who do not have missing data on any component of 1989 wealth; and whose father is the household head in 1968 with at least

one year of occupation data available. There are 1225 from 667 families in my sample, including 646 girls from 460 families and 589 boys from 417 families. My standard errors below allow for heteroscedasticity and an arbitrary pattern of cross-correlation of disturbances among siblings.

Consumption data in the PSID is noisy and is limited to food consumption and a handful of proxies such as house value and the number of cars, making it difficult to measure saving flows. More useful for my purposes are the PSID wealth supplements, asked in 1984, 1989, 1994 and 1999. These provide information on eight categories of wealth: stocks, including stocks held in mutual funds, IRAs and investment trusts; money-like assets (cash, checking and savings accounts, money market funds, CDs, government savings bonds and treasury bills); farm and business net worth; first home equity; other real estate wealth, including equity in second homes the net value of land and rental property; vehicle net worth; miscellaneous assets, including bonds, the cash value of life insurance and the value of collections for investment purposes; and unsecured debt, including student loans, credit card debt, medical and legal bills and loans from relatives. The PSID does not provide detailed information on pension wealth; I did experiment with controlling dummy variables for pension and thrift plan participation by the head and spouse, with little impact on my results. At this stage of my research, I use 1989 and 1984 wealth data only, because most PSID data from 1994 onwards is still in a user-unfriendly early-release format with limited documentation. However, the PSID revised its 1984 and 1989 wealth data in 1994, correcting some coding errors from the original data and using more sophisticated methods for imputing missing observations, including households who refused to provide information on one or

more components of wealth and households who were willing to provide bracket information but not exact figures. I use this revised data, including households with imputed data on one or more component as long as the imputation is based on bracket information, but excluding from my sample households with any imputed wealth component not based on bracket information.

Table A.1 reports summary statistics on 1989 net worth and its components for my baseline sample of 1225 children. All figures are expressed in 1982-84 dollars. The distributions of net worth and each of its components are right skewed, with a large mass at or near zero and a long right tail; these distributions are also highly dispersed, with wide ranges and a high coefficient of variation for most components. While I report results from specifications using net worth and its components in levels as my dependent variable, these results are erratic and can be sensitive to slight changes in sample selection rules that affect whether a given outlier is in or out of the sample. Accordingly, I also report results from two specifications that are more robust to outliers: a "percentile" specification, in which the dependent variable equals the percentile of net worth or one of its components, on a scale from 1 to 100; and a "logs" specification, in which the dependent variable equals $Ln(W+1)$, where W is net worth or one of its components. This formulation allows me to include observations with zeros in the log specification, although I am still forced to exclude negative observations; for net worth, this restriction reduces the baseline sample size from 1225 to 1112.

I organize my findings around the following baseline regression model:

$$NETW_i = \alpha + \gamma * DADLOSS_i + \beta * X_i + \varepsilon_i,$$

where NETW is child I's real 1989 net worth or wealth component, entered either in levels, percentile or log format; DADLOSS is a dummy variable equal to one if child i's father loses his job due to establishment death (or, in some specifications, layoff or firing) while the child is 18 or younger; and X is a vector of control variables. From 1969 onwards, the PSID records information only on job losses occurring over the year since the previous interview; in 1968, however, the PSID asks respondents to describe their most recent job loss, even if it occurred several years ago. I use information on current job and employer tenure from the first few years of the PSID to infer the timing of job losses reported in the 1968, assuming that the previous job was lost in the same year that the current job began. All in all, there are 128 children in the sample, from 60 families, whose father loses his job to establishment death prior to age 18, and 164 children from 87 families whose father is fired or laid off. My baseline set of control variables X includes demographic variables for the child's household, demographic and skill variables for the father, and measures of the child's long run income. The first set includes child's 1989 age and age squared, dummies for race and gender, a dummy variable for the presence of a spouse in 1989 (including unmarried "spouses"), and the spouse's 1989 age and age squared; the second set includes fathers' 1968 age and age squared, dummy variables for father attending college and not completing high school; and time averages over 1968-1984 of dummy variables for one-digit occupation, living in the South and living in the city; the final set includes dummies for attending college and not completing high school for the child and spouse, and third-order polynomials in both the total and annual average real labor plus transfer income of the child and 1989 spouse; for the child, income is summed or averaged over all years from 1969-1989 in which the

child is a head or spouse, while for the current spouse, income is summed or averaged over all years from 1969-1989 in which the current spouse is present in the child's household.

III. BASELINE RESULTS

Table 3 presents estimates of the impact of father's displacement on child's 1989 net worth, under both sparse and rich sets of control variables. Standard errors are in parentheses and are robust to heteroscedasticity and cross-correlation of disturbances among siblings. The sample R-squared of each specification is reported in brackets. The column "No Controls" shows that the unconditional relationship between father's job loss due to establishment death and child wealth is negative and insignificant when net worth is expressed in levels or percentiles, and positive and insignificant when net worth is expressed in logs. However, the next column shows that merely controlling for child and spouse's demographic variables makes the impact of DADLOSS positive in all cases, and significant in the log specification. The crucial variable in this column is child's age; as discussed above, we do not observe complete retrospective job histories for fathers prior to 1968, so we have less opportunity to observe father's job loss for older children. Since older children have more wealth in 1989, omitting child's age casuses a substantial downward bias in the estimated impact of father's job loss on child's wealth.

In the next column I add controls for father's demographics and observable skills. This further increases the estimated impact of DADLOSS on net worth, which is now significant in both the percentile and log specifications. The fact that controlling for

father's observables makes some difference suggests that job loss is somewhat negatively correlated with qualities, such as intelligence or self-control, that are inheritable across generations and that are positively associated with wealth accumulation. My maintained identifying assumption in this paper is that conditioning on fathers' observables is sufficient to eliminate any correlation between job loss due to establishment death and these qualities. Note, however, that if this assumption is incorrect, the most likely direction of failure is that displaced fathers are of lower inheritable quality than nondisplace fathers with similar observables, which should create a negative bias in my estimates of the impact of DADLOSS on child's net worth.

To this point I have not yet controlled for children's permanent income. Since father's establishment death does not affect children's income in my sample, omitting income should not systematically bias my estimates of the impact of father's job loss on children's net worth. However, permanent income is surely an important source of variation in wealth, so controlling for income should improve fit and may allow us to estimate the impact of job loss more precisely. I am agnostic regarding (1) the functional form of the relationship between permanent income and wealth accumulation; (2) whether permanent income is better proxied by income or by education; and (3) whether accumulation depends on average income or the total flow of income received. Accordingly, the column "All Controls" in Table 3 includes dummies for high school completion and college attendance for child and spouse, as well as third-order polynomials in both the total and the average annual real labor and transfer income received by the child and 1989 spouse; the log specification includes polynomials in the logs of total and average income, while the other specifications use levels. Notice that I

omit asset income from my income measures, due to the endogeneity of asset income with respect to wealth. As expected, controlling for child's permanent income improves fit considerably, increasing the R-squared from 0.12 to 0.38 for the level specification, from 0.29 to 0.45 for the percentile specification, and from 0.32 to 0.48 for the log specification. This improved fit, in turn, allows for more precise estimates of the impact of job loss on children's net worth; the coefficient on DADLOSS is now positive and significant in all three specifications. Controlling for child's permanent income increases the coefficient on DADLOSS in the levels specification but reduces it in the percentile and log specifications, consistent with our prior expectation that omitting income should not systematically bias the estimated impact of father's displacement on child's net worth.

The impact of father's displacement on children's wealth is economically large. The levels specification implies that father's job loss due to establishment death increases child's 1989 net worth by $15,631 in 1982-84 dollars, which is 77.5 percent of median net worth and 28.3 percent of mean net worth among sample children. As mentioned above, however, the levels specification is sensitive to outliers, and the other specifications imply more modest but still economically important magnitudes. The percentile specification implies that father's job loss due to establishment death improves the child's standing in the 1989 wealth distribution by roughly 4 percentiles; the distance between the 52nd and 48th percentile of this distribution amounts to $4028 in real terms. The log specification lies between the level and percentile specification in terms of economic magnitude; evaluated at the sample mean of Ln(NETW+1), the coefficient on DADLOSS implies that father's displacement increases child's net worth by $10898 in real terms.

The last column of Table 3 investigates the impact on child's net worth of father's job loss due to layoff or firing, rather than establishment death, using the full set of control variables from the previous column. In all three specifications, the coefficient on DADLOSS remains positive, but is substantially smaller and statistically insignificant. One plausible interpretation of this result is that even after controlling for observable skills, fathers who are laid off or fired have lower levels of self-control, patience or some other inheritable "quality" factor that is both relevant for saving. Another related and intriguing possibility is that children take different lessons from observing different kinds of job loss. If children understand that establishment death is an exogenous event, they will learn from their father's experience that the labor market is subject to large risks beyond their control, and they will respond by insuring themselves against such risks, perhaps by increasing precautionary saving. Meanwhile, if children understand that being fired or laid off is mostly one's own "fault", then seeing their father being fired will not raise their subjective assessment of the amount of underlying risk in the labor market. Unfortunately I cannot distinguish these explanations in my data; the firm conclusion I can draw from the final two columns of Table 3 is that identifying exogenous sources of decremental deprivation is important to obtaining unbiased estimates of the true impact of childhood deprivation on adult saving.

What Are the "Formative Years?"

My baseline specification assumes that father's job loss at age 2 has the same impact on adult wealth as father's job loss occurring at age 15. If job loss affects children by affecting the way children view economic risk, however, or by accelerating the

transition from childhood to adulthood, we might expect job loss to have a greater impact if it occurs in adolescence than in early childhood, since young children have less inclination or ability to learn about economic risk or to use acute family distress as a spur towards early economic maturity. Accordingly, Table 4 re-examines the impact of father's job loss due to establishment death on child's 1989 net worth, allowing the coefficient on DADLOSS to depend whether the child is young (ages 0-9) or adolescent (ages 10-18) at the time of the shock. I also include a dummy variable for children whose fathers lose their job when the child is an adult (ages 19 and above). The vector X consists of the full set of controls used in the rightmost columns of Table 3; in some specifications I also control for the number of years separating father's job loss from 1989, in order to avoid confounding the impact of child's age with the impact of elapsed time since the shock. Elapsed time may be independently correlated with the impact of job loss on child's wealth; for instance, if job loss increases children's saving propensities, then the impact on 1989 wealth will be proportional to the number of years the child has been saving at a higher rate. For ease of interpretation, I use elapsed years minus 17.43, which is the sample mean of elapsed time among children whose father loses his job prior to age 18; thus, for instance, the reported coefficient on the age 10-18 bracket controlling for elapsed years indicates the impact on an adolescent child of father's job loss occurring in 1972, which is an "average year" for the occurrence of job loss among children in my sample.

For the percentile and log specifications, the results are clear-cut: at the sample mean of elapsed time, father's job loss has a positive and significant impact on subsequent wealth if it occurs during adolescence, but not if it occurs during early

childhood or adulthood. Job loss has its largerst impact on youth in the levels specification, but the coefficient is insignificant once I control for elapsed time; furthermore, experimentation suggested that the levels coefficients are erratic with respect to the treatment of outliers. As anticipated, the impact of job loss on wealth grows with elapsed time in all specifications, although this effect is not statistically significant. Broadly speaking, these results are consistent with the idea, familiar from the sociology and political science literatures on cohort behavior (Mannheim (1928), Ryder (1965), Inglehart (1990)), that adolescence is an especially crucial "formative" period in which experiences and observations leave an indelible mark on preferences and beliefs.

What Kinds of Wealth are Affected?

To this point I have examined only total net worth. However, the behavior of wealth broken down by asset type may yield information about the mechanisms linking childhood experience to adult wealth. Table 5 presents estimates of the impact of father's job loss on the eight components of wealth available from the PSID. These specifications use the baseline set of controls found in the rightmost columns of Table 3. In addition to the usual specifications in levels, percentiles and logs, I report probit estimates of the marginal impact of father's job loss on the probability that a given component is positive, evaluated at the mean of the control variables. These tables indicate that the impact of father's job loss is concentrated in three asset types: vehicles, unsecured debt and business equity. The probit results suggest that father's job loss due to establishment death increases the probability of having positive vehicle wealth by almost 4 percent, increases the probability of having farm or business equity by over 6 percent, and reduces

the probability of having credit card or other unsecured debt by almost 10 percent; these effects are all significant at 5 percent. Results for the log and percentile specifications are qualitatively similar; results for levels are similar in sign though not in significance.

IV. INTERPRETATION AND FURTHER RESULTS

The previous section established that the children of exogenously displaced fathers accumulate more wealth as adults than the children of otherwise similar nondisplaced fathers. There are a number of mechanisms that could explain this finding. Job loss reduces father's lifetime financial resources, which may increase children's saving by reducing expected future bequests. Father's displacement may force children to become economically mature at an earlier age, which may lead to more responsible financial behavior as adults; Elder (1974), for instance, finds that family deprivation during the Great Depression accelerated the age at which teenage boys entered the labor force, formulated career plans, and learned how to manage money. Job loss reduces family consumption, which could reduce the marginal utility of adult consumption if there is habit formation. Job loss could also affect the way children perceive economic risks; for instance, a child who sees his father thrown out of work for exogenous reasons may perceive a higher degree of labor market uncertainty as an adult, and as a result may increase precautionary saving. Finally, it is possible that the relationship between father's job loss and children's wealth is not causal at all, but rather is driven by some omitted factor.

We already have some clues about the mechanisms linking father's displacement to adult saving. The fact that father's job loss only matters if it occurs during adolescence suggests that the linkage involves some sort of active learning or response on the part of the child to the father's experiences; this result is thus arguably inconsistent with the expected bequests story, in which father's job loss affects children only through father's lifetime resources, as well as with stories relying on omitted variables bias, since the correlation between father's displacement and omitted factors relevant for saving should not depend on the child's age at the time of the shock *per se.*

The breakdown by asset type also provides tantalizing evidence. The idea that father's displacement raises children's awareness of labor income risk and precautionary savings is consistent with the results on unsecured debt, but is inconsistent with the low impact of job loss on monetary assets. Since the PSID debt measure includes loans from relatives, the debt findings may simply reflect the fact that displaced fathers can't make loans to their children; in the future I plan to look more closely at inter-vivos transfers in the PSID. Alternatively, children of displaced fathers may have a visceral aversion to debt, particularly if debts were a source of family stress during the lean years; it is easy to find examples of a visceral, almost hysterical, learned aversion to debt in the oral testimony of survivors of the Great Depression. On the other hand, the impact of job loss appears to be limited to unsecured debt; I found no impact of father's displacement on the loan-to-value ratio on the child's principal residence (results available from the author). The vehicles results are hard to square with any of the proposed linkage stories; they are perhaps least consistent with habit formation, which implies that the impact of job loss on accumulation should arguably be weakest for consumer durables.

The most surprising and intriguing results are those relating to business equity, which suggest that some children learned from their father's displacement that true job security lies in being one's own boss, rather than in working for a large company. This does not seem to be the lesson learned by children of the Great Depression, who according to popular mythology became the Organization Men of the 1950s after observing countless small business failures during the early 1930s as well as successful large-scale operations, such as the US military's performance during World War II. However, my findings are consistent with the popular mythology of post-Baby Boom generations, who are supposedly more entrepreneurial and independent than their predecessors because of their exposure to corporate downsizing and de-industrialization (these mythologies are presented in numerous popluar books about generational conflict; see Lynne Lancaster and David Stillman's *When Generations Collide* (2002) for an example hot off the presses).

While the evidence on wealth impacts by age and asset types is intriguing it is not definitive. In the rest of this section I explore some particular stories in more detail.

The Expected Bequest Effect

To an economist, the simplest explanation for the impact of father's displacement on children's wealth is the expected bequest effect: displacement reduces father's labor income sharply at the time of the shock and has lingering negative effects in the long run, so displaced fathers have lower lifetime resources, which reduces the amount of wealth they have to distribute to their children as gifts or bequests. Children of displaced fathers rationally anticipate that they will receive lower transfers from their father, and thus save

more on their own than children whose fathers are not displaced. Note that this will create a negative impact of father's job loss on children's wealth only if children's wealth is measured prior to the receipt of bequests or gifts; once transfers have been made, father's job loss may actually be associated with lower children's wealth, particularly if the offsetting impact of expected bequest on children's saving is less than one-for-one. Of the 1225 sample children, 909 have a father who is still in the PSID in 1989; the impact of father's job loss on children's wealth in this subsample is similar to the impact reported for the full sample in Table 3.

Table 6 reports some direct evidence relevant to the expected bequest hypothesis. The first column shows the impact of father's job loss due to establishment death on children's expected future inheritances as reported in 1984; the sample size for these regressions is 1050, since expected bequests are observed only for children who are heads or spouses in 1984. The results contradict the expected bequest hypothesis; father's job loss is associated with larger expected inheritances, although these coefficients are not significant. The second column replaces expected bequests with actual real inheritances received through 1989. Again the results confound expectations; father's job loss is associated with larger inheritances in two of three specifications, although again the estimates are statistically insignificant. Controlling directly for actual or expected bequests in my regressions linking father's job loss to children's wealth had little impact on the baseline findings (details available from the author).

One should not make too much of these results. The bequest questions in the PSID explicitly refer only to inheritances, excluding inter-vivos transfers but including bequests from other relatives, including spouse's relatives. Moreover, the expected

inheritance question asks only about inheritances expected in the next 10 years. Among children whose selves and fathers are both PSID respondents in 1984, the average father is 57 years old, so most sample children are presumably not expecting their father to die in the next ten years. Indeed, only 214 of 1050 sample children in the 1984 PSID report positive expected future inheritances. The PSID does ask annual questions about gifts received from all relatives, and supplemental surveys in 1980 and 1988 ask more detailed questions about intergenerational transfers; I plan to look at this information carefully in the future.

For now, note that if father's job loss affects children's wealth through expected bequests, then the impact should be mediated through father's wealth. The last column of Table 6 examines the impact of father's job loss on own real net worth in 1984. The sample size is 481, the number of fathers with complete wealth information available in 1984 (using the same restrictions on imputations discussed in Section II); the regression controls only for father's demographics and observable skills, and DADLOSS is set equal to one if the father loses his job to establishment death any time through 1984, regardless of his children's age. As expected, displacement does reduce father's net worth in 1984; the impact is statistically significant only in the log regression, although the economic magnitude is large in the levels regression.

Table 7 investigates whether controlling for father's 1984 wealth reduces the impact of father's job loss on children's 1989 net worth, using the 909 children of the 481 fathers with complete wealth information available in 1984; results using children's 1984 wealth or father's 1989 are broadly similar, but sample sizes are smaller. The first column re-estimates the baseline specification with full controls from Table 3; the

coefficients on DADLOSS differ from those reported in Table 3 because of the different sample restrictions, but are broadly similar. The remaining columns report the impact of DADLOSS and father's net worth considered simultaneously; father's net worth is expressed either in levels, percentiles or logs according to the specification of child's net worth. The OLS estimates show that controlling for father's wealth does not reduce the estimated impact of job loss on children's wealth. One reason for this is that the estimated impact of father's wealth on children's wealth is positive (and significant in the percentile and log specifications), contrary to the idea that father's wealth crowds out children's accumulation via expected bequests; Charles and Hurst (2001) also find a positive impact of parents' wealth on child's wealth in the PSID.

One way to think about the positive OLS coefficient on father's wealth in Table 7 is that it reflects an upward omitted variables bias. Standard models predict that higher father's wealth *per se* should reduce expected bequests and crowd out children's accumulation; that is, the "true" coefficient on father's wealth in the children's wealth regression is negative. However, OLS provides an upward-biased estimate of this coefficient, because of omitted factors that vary across families, that affect saving propensities and that are correlated across generations, such as patience, self-control, risk aversion or a taste for wealth in the utility function. In this case, instrumental variables can in principle allow us to uncover the true negative impact of father's wealth on children's accumulation, which in turn will allow us to ask whether this crowding out effect is strong enough to explain the impact of father's job loss on children's net worth.

In practice, I need instruments that are relevant for father's wealth but uncorrelated with omitted inheritable factors that affect saving--intuitively, I need to

isolate variation in father's wealth that is due to "luck" rather than an inherent preference for more saving. In Shea (2000) I used father's union status and industry wage premium, along with job loss due to establishment death, to isolate "exogenous" variation in father's income, arguing that a long tradition in labor economics regards union and industry status as largely a matter of luck and thus plausibly uncorrelated with unobserved heterogeneity in father's skill. While this assumption is of course controversial, I do find in Shea (2000) that the resulting "exogenous" variation in father's income, while substantial, has no impact on children's wages, earnings or years of schooling in the PSID, suggesting that it is not outrageous to assume that union and industry wage premia are both relevant for father's lifetime resources and orthogonal to omitted inheritable factors that affect saving. Accordingly, the "2SLS" rows of Table 7 present estimates of the impact of father's job loss and father's wealth on children's net worth, using time averages over the period 1968-1984 of father's union status and industry wage premium as instruments for father's wealth; see Shea (2000) for more details on how these instruments are constructed. As expected, instrumenting reduces the omitted variables bias, to the point where the estimated impact of father's wealth on children's wealth is negative, consistent with the idea that father's wealth *per se* crowds out children's accumulation. However, the impact father's wealth is estimated imprecisely and is not statistically significant, and the impact of father's job loss remains positive and significant. The 2SLS results are hampered by the fact that union status and the industry wage premium are not very relevant for father's wealth, even though they are relevant for father's long-run income; the p-value of a Wald Test for excluding union and industry from the first-stage regression is 0.11 for father's log wealth, but only 0.34 for father's wealth percentile and

0.53 for father's wealth in levels. Given the poor instrument performance, we should not interpret Table 7 as ruling out the expected bequest hypothesis decisively; at this stage, we simply do not have credible estimates of the extent to which father's wealth crowds out children's wealth, and therefore we cannot credibly say whether this crowding out is sufficient to explain the impact of father's job loss on children's wealth.

Table 8 tests yet another implication of the expected bequest hypothesis by examining whether the impact of job loss varies according to the number of children in the family. Children who have more siblings should rationally expect to get smaller financial transfers from their parents, all else equal, so that a given-sized shock to father's wealth should have a smaller impact on a given child's expected bequests if she has more siblings. If job loss affects children's net worth primarily by reducing expected bequests, then, we would expect to find that the impact of job loss on children's wealth is significantly smaller for children with more siblings. Table 8 presents estimates of the impact of job loss, controlling for the number of siblings and for the interaction between SIBS and DADLOSS. For ease of interpretation, I define SIBS as the number of siblings minus 3.29, which is the mean number of siblings among sample children; thus, the reported coefficient on DADLOSS shows the impact of job loss on a child with an average number of siblings. The results are not supportive of the expected bequest hypothesis. The number of siblings *per se* reduces child's wealth in all specifications, significantly so in the percentile case, consistent with the idea that large families make smaller intergenerational transfers per child (this can be tested directly, and I plan to do so in the future). However, the interaction between SIBS and JOBLOSS is insignificant in all cases, and has the wrong sign in the log case. The impact of father's job loss for a

child with an average number of siblings remains positive and significant. I experimented with replacing SIBS with the reciprocal of (SIBS+1); this had no qualitative impact on the results.

In summary, I do not find any direct evidence that father's job loss reduces actual or expected bequests; I do not find any evidence that father's wealth crowds out children's accumulation or that such crowding out can explain the impact of job loss on children's accumulation; and I do not find a significant negative interaction between job loss and the number of siblings. Moreover, the expected bequests hypothesis is arguably inconsistent with the fact that job loss matters only for adolescents, since if job loss only matters because of its impact on fathers' wealth, the timing of job loss in the child's life cycle *per se* should be irrelevant, although if we allow for some learning effects it is possible that children will be more *aware* of their father's weak finances if they observe a large shock in adolescence. The expected bequests hypothesis also has no power to explain variation in the impact of job loss by asset type. While none of these pieces of evidence may be enough to rule out the expected bequests hypothesis on its own, and while it may be possible to resurrect the hypothesis in the future--if we can find better instruments for father's wealth, for instance--at this point, the evidence suggests at a minimum that we should look for other explanations of the impact of father's displacement on children's accumulation.

The Downward Extension of Adulthood

One of Glen Elder's most interesting findings in his study (1974) of children growing up in Oakland during the Depression is that deprivation forced children,

especially boys, to become economically mature at a younger age. Elder finds that boys experiencing decremental deprivation were more likely than their nondeprived peers to have jobs, and that early employment caused boys to be regarded more as financially responsible by their mothers, as well as more dependable, industrious and adult-oriented by interview staff. Elder also finds that deprivation accelerated the process of "vocational crystallization" (page 165); boys from deprived families displayed mature and well-informed occupational aspirations at an earlier age, and as adults were more likely to follow a steady career path as measured by occupational continuity. While early economic maturity is most likely to be reflected in the level and stability of adult earnings, it may also cause better savings habits as adults; indeed, Elder finds that deprived boys were more likely to report regular saving as adults, especially if they worked as teenagers.

Table 9 assesses whether father's displacement caused a downward extension of adulthood in the PSID by reporting the impact of father's job loss due to establishment death on a variety of indicators of early economic maturity. I estimate both pooled regressions and regressions broken down by gender, and control for father's observable, child's age and age squared and (in pooled regressions) gender. The first row examines the impact of DADLOSS on the age at which children first report labor income in the PSID annual individual-level data. Elder's hypothesis predicts that displacement should accelerate children's entry into the labor market, but in fact I find the opposite: children with displaced fathers entering the labor force a half year later than children of otherwise similar nondisplaced fathers, and this effect is statistically significant. The next two rows examine the total real income received by children while still living at home, and the age

at which the child first established his or her own household. Again I find no evidence of early maturity on the part of the children of displaced fathers; father's job loss is associated with an insignificant decline in income received while living at home, and an insignificant delay in moving out.

The next three rows examine the impact of job displacement on the probability a child ever reports having a spouse between 1969 and 1989, the age of first "marriage" conditional on ever having a spouse, and the number of years between 1969 and 1989 that the child reports living with a spouse; I do not distinguish between legal marriage and living together. Elder finds no impact of decremental deprivation on the age or likelihood of marriage for boys, but he did find that girls from deprived families were more likely to marry young. In contrast, I find that father's displacement is significantly associated with later marriage, especially for girls. Father's job loss is associated with a significantly higher likelihood of ever marrying, mostly for boys; on net, job loss has an insignificant positive impact on years married prior to 1989 for boys and an insignificant negative impact on years married for girls.

The final two rows of Table 9 examine the impact of father's job loss on "vocational crystallization", as proxied by Hirfindahl indices for industry and occupational affiliation. I first compute the share of sample years each child spends working in each of nine one-digit occupations and nine one-digit industries; the Hirfindahl index is then defined as the sum of squared shares over the nine industry or occupation categories. Stable careers spent largely in one occupation or one industry will be reflected in higher values for the Hirfindahl index. Career stability appears to be an important correlate of children's income and wealth; controlling for father's observables

and children's demographics and education, the industry and occupation Hirfindahls have significant positive impacts on child's permanent income and net worth, and the industry Hirfindahl remains significant for net worth even after controlling for child's income as in Table 3 (results available from the author). Vocational crystillization is thus potentially a powerful mechanism for translating father's job loss into adult wealth--if job loss prods children into having more stable careers as early adults. However, this does not appear to be the case in my sample: the impact of DADLOSS on the industry and occupational hirfindahls is negative and insignificant. Not surprisingly, then, controlling for career stability has little effect on the impact of DADLOSS on children's net worth.

Omitted variables bias may be a factor here. Displaced fathers are likely to live in depressed regions with fewer job opportunities for children; in turn, this may cause children to delay labor force entry, marriage and household formation, and may hamper children's efforts to launch a stable career. Note that this bias is not present in Elder's work, since all of the Oakland Growth Study subjects were drawn from one city. Nevertheless, whether or not the impact of father's job loss on children's economic maturity is causal or not, the fact remains that in my sample, the children of displaced fathers do not reach economic maturity at an earlier age than the children of nondisplaced fathers; the downward extension of adulthood thus cannot explain the impact of father's job loss on children's wealth in my sample.

Correlated Uncertainty

Most of the mechanisms I have entertained in this paper--the expected bequest effect, the downward extension of adulthood, learning about the need for precautionary

saving, learning the virtues of being one's own boss, habit formation--are causal mechanisms, which father's displacement actually generates higher subsequent saving by children. It is more difficult to think of non-causal mechanisms, spurious reasons why father's job loss might be related to children's saving through some omitted factor. In this section I confront a particular omitted variables story: correlated uncertainty. Suppose some professions or industries have a higher inherent risk of establishment death and other shocks than others, in a way that is not captured by the one-digit occupational dummies I use as a proxy for father's skill. Suppose further that occupation and industry choice is correlated across generations, either because of preferences, skills, or local labor market opportunities. Then displaced fathers will have children who tend to work in riskier industries and occupations, who will in turn save more for precautionary reasons. Displaced fathers will then have children with higher wealth, not because displacement causes children to save more but because variation in both displacement and child saving are driven by a third factor--intergenerationally correlated exposure to economic risk.

To explore this idea, I need a measure of children's income uncertainty. For each sample child with at least five years as a head or spouse, I construct an annual time series of real labor plus transfer income of the head and 1989 spouse, covering all years between 1969 and 1989 in which the child is a head or spouse. I then regress income on a constant, a time trend and a dummy variable for presence of the 1989 spouse, with all coefficients allowed to vary by household; the spouse dummy is omitted if the child has no spouse or if the spouse is always present. My baseline measure of uncertainty is the standard deviation of the residuals from this regression, which I call SDEV. SDEV measures the time-series fluctuations of annual household income not due to a time trend

or changes in marital status; the mean SDEV over the 1114 sample children with at least five years of data is $5513 in 1982-84 dollars. Regressing SDEV on DADLOSS and the full set of controls used in Table 3 yields a coefficient on DADLOSS of 345.6, with a heteroscedasticity and cross-correlation robust standard error of 262.7. Thus, the children of displaced fathers do have more volatile incomes than the children of nondisplaced fathers, consistent with the correlated uncertainty story, although the impact is not quite statistically significant.

Table 10 explores whether correlated uncertainty can explain the apparent impact of job loss on saving, using a restricted sample of 1107 children with at least five years of income data and at least one year of occupation and industry for both head and 1989 spouse, if any. The first column re-estimates the baseline model with the full set of controls on the restricted sample. The remaining columns report the impact of DADLOSS and SDEV considered simultaneously. The coefficient on SDEV is positive and significant in all three OLS specifications; households with more volatile incomes hold more wealth, consistent with the presence of a precautionary saving motive. Controlling for income volatility, however, does not eliminate the impact of father's displacement on wealth in the OLS specifications; the coefficient on SDEV remains positive and significant in all cases.

The OLS specifications assume that ex-post observed income volatility is a reasonable proxy for the ex-ante income uncertainty faced by the household. This may not be true, particularly if observed income volatility is partly due to measurement error. Accordingly the remaining rows of Table 10 estimate the impact of DADLOSS and SDEV on child's wealth using 2SLS, instrumenting SDEV with time averages of

dummies for the one-digit industry and occupation of the child and spouse; Carroll and Samwick (1997, 1998) use similar instruments. When all occupation dummies are included in the instrument list, instrumenting dramatically increases the estimated coefficient on SDEV, and the coefficient on DADLOSS becomes insignificant in the levels and percentile specifications. Taken at face value, then, the initial 2SLS results support the idea that correlated uncertainty can explain part of the apparent impact of father's displacement on child wealth.

However, these results assume that occupation and industry have no association on child's wealth other than through their association with income uncertainty. This assumption is dubious for at least one PSID one-digit occupation: self-employed businessman. Business equity is a component of net worth, and almost by definition the self-employed are far more likely to have business equity than other households. The self-employed are likely to have more nonbusiness wealth as well, even if precautionary savings motives are absent, becuase empirically initial household wealth is an important correlate of the decision to start a new business (Hurst and Lusardi 2002), whether because of liquidity constraints or because of some omitted factor, such as drive or ambition, that drives both wealth accumulation and the desire to be an entrepreneur. Accordingly, the remaining rows of Table 10 present 2SLS estimates relocating the average self-employment dummies for child and spouse from the instrument list to the vector of controls. The results are dramatic: the coefficient on SDEV is now negative and insignificant, and meanwhile the coefficient on DADLOSS is again positive and significant in all specifications. The direct effects of self-employment on child wealth are not reported in the table, but they are enormous and highly significant: in the levels

specification, for instance, child's self-employment has a coefficient (standard error) of 127224 (39391), while spouse's self-employment has a coefficient (standard error) of 161086 (29165). Interestingly, Carroll and Samwick (1997) also note that excluding the self-employed (and farmers) from their identification strategy affects the strength of their evidence for precautionary saving, although their evidence is not as dramatic as mine; Carroll and Samwick, however, assert that the high wealth of the self-employed is entirely due to their higher income volatility and that using self-employment as an instrument is therefore valid. I find this assertion hard to defend, given that lagged wealth appears to predict future business formation in the data.

While the strongest evidence for precautionary saving and the correlated uncertainty hypothesis in Table 10 appears to be spurious, the reader should not conclude that precautionary saving is absent or that correlated uncertainty does not explain at least a modest part of the apparent impact of precautionary saving on wealth. The OLS results suggest that a precautionary savings motive exists, and the OLS results could well be downward biased by measurement error or some other gap between ex-post income volatility and ex-ante uncertainty--although in my sample of young people, one could also argue that the OLS bias is upward, since favorable permanent income shocks may be the most important source of income volatility among young people, and sudden increases in permanent income may lead to rapid increases in wealth if, for instance, consumption is slow to adjust due to habit formation. Taken at face value, the 2SLS results controlling for self-employment suggest that there is no precautionary savings motive, but these results are questionable as well, because controlling for self-employment dramatically reduces instrument relevance: the p-value for the Wald test of

the exclusion of the instruments from the first stage regression rises from 0.0000 to 0.12 when self-employment dummies are removed from the instrument list. The self-employed do have much more volatile income than every one else; aside from this, however, there is not a lot of variation in income uncertainty across one-digit industries and occupations. We are left with the same conclusion that we reached at the end of our exploration of the impact of father's wealth: we need better instruments before we can definitively gauge the importance of precautionary saving, and without such estimates we cannot definitively rule out correlated uncertainty as an explanation for the estimated impact of father's displacement on child's wealth. I would note, however, that the coefficient on DADLOSS remains positive and economically large in all cases (and statistically significant in the log case) even when the coefficient on SDEV is grossly upward-biased by inappropriate instruments, suggesting that correlated uncertainty is probably not the sole explanation of the link between father's job loss and children's accumulation.

V. CONCLUSION

This paper shows that childhood deprivation can indeed lead to greater adult saving. Specifically, I find that the children of exogenously displaced fathers in the PSID have more wealth as adults in 1989 than children of nondisplaced but otherwise similar fathers. Father's displacement affects adult wealth only if it occurs during adolescence, suggesting that children's active learning or response to the shock is an important part of the impact of the shock on adult wealth, and the impact of displacement is concentrated

on vehicles, unsecured debt, and business equity. The impact of job loss does not appear to be mediated through father's wealth and expected bequests or through the downward extension of adulthood, and it does not appear that the apparent impact of father's displacement on children's wealth is due to intergenerationally correlated uncertainty. While I have not yet conclusively pinned down the precise mechanism linking father's displacement to children's accumulation, some form of learned debt aversion and learning that being one's own boss is preferable to being downsized is likely to be at least part of the final story.

This paper fits into a small literature that argues that learning from experience may be an important source of variation in wealth accumulation. Lusardi (2000), for instance, shows that having an older sibling in financial difficulty raises the likelihood that younger adults will plan for their retirement, while Cox, Ng and Waldkirch (2000) find that PSID children whose parents experience a large drop in consumption upon retirement tend to subsequently reduce their own consumption, suggesting that their parents' experience taught them about the need to save. My work differs in its focus on learning during childhood as opposed to learning during adult life, and in its focus on learning about labor income risk rather than learning about the need to save for retirement, but it shares with these papers the idea that observing adverse shocks up close can have a corrective impact on one's own behavior.

TABLE 1. DETERMINANATS OF FATHER'S JOB LOSS: PROBIT ESTIMATES

Variable	Impact on Probability of Father's Job Loss Due To:	
	Establishment Death	Layoff or Firing
Age	-0.00466 **(0.00177)	-0.0113 **(0.0020)
Race Nonwhite	0.0353 (0.0480)	-0.0167 (0.0561)
Attended College	0.0536 (0.0572)	0.0735 *(0.0400)
Did Not Complete High School	0.0275 (0.0360)	0.1219 *(0.0693)
Living in South	0.0072 (0.0363)	-0.0910 **(0.0390)
Living in City	0.0641 (0.0348)	-0.0257 (0.0380)
Occupation: Professional	-0.085 (0.157)	-0.087 (0.126)
Administrative	0.110 (0.145)	-0.058 (0.119)
Self-Employed	0.071 (0.164)	-0.054 (0.139)
Sales/Clerical	0.048 (0.150)	0.092 (0.123)
Craftsman	0.146 (0.135)	0.122 (0.113)
Operative	0.193 (0.136)	0.147 (0.112)
Laborer/Service	0.192 (0.146)	0.282 **(0.121)
Protective	-0.007 (0.165)	0.017 (0.137)
Pseudo R-squared	0.049	0.103

NOTES: for each variable in the left column, this table reports probit estimates of the impact on the probability that a sample father reports losing his job due to establishment death (second column) or being laid off or fired (third column) between 1968 and 1984. All right-hand side variables except race and education dummies are sample averages of annual observations and are therefore continuous; the table reports the impact of an infintesimal change in each variable when all variables are at their sample mean. For race and education dummies, the table reports the impact of a change in variable from 0 to 1, holding other variables at their sample mean.   The final row reports one minus the ratio of the estimated model's log likelihood and the log likelihood using only a constant term. There are 667 observations. Standard errors are in parentheses and are robust to heteroscedasticity. (*) and (**) denote significance at 10 and 5 percent.

TABLE 2. DOES FATHER'S JOB LOSS AFFECT CHILDREN'S INCOME OR EDUCATION?

$$Y = \alpha + \gamma * DADLOSS + \beta X + \varepsilon$$

Father's Job Loss Due To:	Dependent Variable	No Controls	Add Child's Demograhpics	Add Father's Demographics and Skill
Establishment Death	Child's Permanent Income	-2076 *(1218)	-281.2 (1052)	494.4 (1004)
Establishment Death	Child's Years of Schooling	-0.412 *(0.213)	-0.261 (0.214)	0.120 (0.181)
Layoff or Firing	Child's Permanent Income	-4127 **(1216)	-2946 **(1075)	-1243 (1031)
Layoff or Firing	Child's Years of Schooling	-0.764 **(0.203)	-0.656 **(0.200)	-0.032 (0.173)

NOTES: This table presents OLS estimates of the impact of father's job loss on children's years of schooling and permanent income, defined as the average annual real labor plus transfer income of self and 1989 spouse, averaged over all years prior to 1989 in which the child was a household head or spouse. The first two rows set DADLOSS equal to one if the child's father reports job loss due to establishment death while the child is 18 or younger; the next two rows report the impact of father's job loss due to being fired or laid off. The columns correspond to progressively richer sets of control variables X. The column "No Controls" contains only a constant term; the next column controls for race and gender, a dummy variable for spouse presence, and age and age squared for the child and spouse; and the last column adds controls for father's age, age squared, education, region, urbanicity and occupation. There are 1225 observations from 667 families. Standard errors are in parentheses and are robust to arbitrary heteroscedasticity and cross-correlation of disturbances among siblings. (*) and (**) denote significance at 10 and 5 percent.

TABLE 3. FATHER'S JOB LOSS AND CHILD'S NET WORTH: THE IMPACT OF RICHER CONTROLS

$$NETW = \alpha + \gamma * DADLOSS + \beta X + \varepsilon$$

Specification	No Controls	Child's Demography	Add Father's Demography and Skill	All Controls	All Controls, Layoff or Firing
Levels	-5715 (9306) [0.0002]	6305 (8541) [0.08]	10574 (9149) [0.12]	15631 *(8191) [0.38]	7859 (6343) [0.38]
Percentile	-1.796 (2.794) [0.0004]	3.191 (2.561) [0.24]	4.512 *(2.460) [0.29]	3.804 *(2.286) [0.45]	2.118 (1.914) [0.45]
Logs	0.039 (0.178) [0.0000]	0.402 **(0.169) [0.24]	0.537 **(0.160) [0.32]	0.474 **(0.136) [0.48]	0.136 (0.154) [0.48]

NOTES: this table presents estimates of the impact of father's job loss on children's net worth as adults in the 1989 PSID. DADLOSS is a dummy variable that equals one if a child's father loses his job due to establishment death (or, in the last column, being fired or laid off) while the child is 18 or younger. Depending on row, the dependent variable is either child's net worth in levels, the percentile of child's net worth (1-100), or the natural log of child's net worth plus one. The columns correspond to progressively richer sets of control variables X; the column "No Controls" has only a constant term; the next column adds controls for child's race, gender, presence of a spouse, and age and age squared for child and spouse; the next adds controls for father's age and age squared, education, region, urbanicity and occupation; and the last two add controls for the child and spouse's education as well as third order polynomials in the total and annual average real labor and transfer income received by the child and 1989 spouse in all years in which the child is a household head or spouse. The log specification has 1112 observations from 639 families; all others have 1225 observations from 667 families. Standard errors are in parentheses and are robust to heteroscedasticity and cross-correlation of disturbances among siblings. The square brackets report the sample R-squared for each specification. (*) and (**) denote significance at 10 and 5 percent.

TABLE 4: FATHER'S JOB LOSS AND CHILD'S NET WORTH: DOES THE IMPACT DEPEND ON THE AGE OF THE CHILD?

$$NETW = \alpha + \sum (\gamma_i * DADLOSS * AGE_i) + \delta * YEARS + \beta X + \varepsilon$$

Specification	Impact of Father's Job Loss When Child is:			Years Elapsed Since Job Loss
	Ages 0-9	Ages 10-18	Ages 19+	
Levels	26472 *(15959)	6131 (9009)	2312 (8127)	---
	20712 (14338)	5919 (9319)	11973 (16303)	1111 (1200)
Percentile	1.606 (3.674)	4.377 *(2.396)	-0.041 (1.975)	---
	-0.047 (3.966)	4.316 *(2.432)	2.681 (3.303)	0.319 (0.298)
Logs	0.134 (0.234)	0.517 **(0.142)	0.236 (0.174)	---
	-0.078 (0.283)	0.508 **(0.148)	0.115 (0.263)	0.0411 (0.0272)

NOTES: this table presents estimates of the impact of father's job loss due to establishment death on children's net worth in 1989, allowing the impact of job loss to vary according to child's age at the time of loss. Alternate regressions control for the number of years elapsed between the job loss and 1989. The vector X consists of the baseline set of controls used in the rightmost columns of Table 3. Standard errors are in parentheses and are robust to heteroscedasticity and cross-correlation of disturbances among siblings. (*) and (**) denote significance at 10 and 5 percent

TABLE 5: FATHER'S JOB LOSS AND CHILDREN'S WEALTH, BROKEN DOWN BY COMPONENT

$$Y = \alpha + \gamma * DADLOSS + \beta X + \varepsilon$$

Component of Wealth	Levels	Percentile	Logs	Probit
Stocks	-57.9 (440.5)	0.465 (3.362)	0.052 (0.327)	0.0115 (0.0464)
Monetary Assets	1311 (1440)	-0.037 (2.191)	0.185 (0.238)	0.0149 (0.0177)
Business Net Worth	7993 (4912)	5.554 *(2.875)	0.646 **(0.321)	0.0639 **(0.0350)
Home Equity	1259 (2573)	1.907 (2.880)	0.138 (0.398)	0.0032 (0.0491)
Other Real Estate	2035 (1663)	1.322 (2.861)	0.132 (0.298)	0.0123 (0.0334)
Vehicles	86.3 (572.7)	4.357 *(2.262)	0.565 **(0.170)	0.0382 **(0.0099)
Unsecured Debt	-1335 (961.7)	-5.782 *(3.302)	-0.746 **(0.354)	-0.0988 **(0.0481)
Other Assets	1669 (1538)	2.690 (3.767)	0.332 (0.364)	0.0309 (0.0485)

NOTES: this table presents estimates of the impact of father's job loss due to establishment death on different components of children's adult wealth in the 1989 PSID. The column "Probit" shows the marginal impact of father's job loss on the probability of observing a positive value of a given component, evaluated at the mean of all other variables; other columns are defined in Table 3. The vector X consists of the baseline controls used in the rightmost columns of Table 3. All specifications have 1225 observations from 667 families, except for the log specification on home equity, which has 1214 observations from 662 families, and the log specification on other real estate, which has 1224 observations from 667 families. Standard errors are robust to heteroscedasticity and cross-correlation of disturbances among siblings. (*) and (**) denote significance at 10 and 5 percent.

TABLE 6: THE IMPACT OF FATHER'S JOB LOSS ON FATHER'S WEALTH AND BEQUESTS

$$Y = \alpha + \gamma * DADLOSS + \beta X + \varepsilon$$

Specification	Impact of Father's Job Loss Due to Establishment Death On:		
	Children's Expected Future Inheritances As of 1984	Inheritances Received by Children as of 1989	Father's Net Worth in 1984
Levels	949.4 (3227)	-213.9 (1203)	-46809 (43379)
Percentile	0.694 (3.905)	1.506 (2.949)	-2.840 (2.984)
Logs	0.129 (0.446)	0.181 (0.303)	-0.380 *(0.190)

NOTES: this table presents estimates of the impact of father's job loss due to establishment death on children's expected future bequests as of 1984, on children's actual bequests received through 1989, and on father's net worth in 1984. The rows correspond to different transformations of the dependent variable Y. The expected bequest regressions have 1050 observations from 605 families; this is smaller than the baseline sample due to the requirement that the child be a household head or spouse in 1984. The actual bequest regressions use the baseline sample of 1225 observations from 667 families. Both bequest regressions set X equal to the baseline controls used in the right columns of Table 3. The father's wealth regressions use the 481 fathers who have complete wealth data in 1984; in this regression, X includes only father's demographics and skill covariates. Standard errors are in parentheses and are robust to heteroscedasticity and (where appropriate) cross-correlation of disturbances among siblings. (*) and (**) denote significance at 10 and 5 percent.

TABLE 7. FATHER'S JOB LOSS, FATHER'S NET WORTH AND CHILD'S NET WORTH

$$NETW = \alpha + \gamma * DADLOSS + \delta * DADNETW + \beta X + \varepsilon$$

Specification	Father's Job Loss, No Control for Father's Wealth	Father's Job Loss, Control for Father's Wealth	Impact of Father's Wealth
Levels, OLS	16733 *(8854)	16829 *(8822)	0.00421 (0.00261)
Percentile, OLS	5.643 **(2.653)	5.388 **(2.906)	0.122 **(0.033)
Logs, OLS	0.479 **(0.150)	0.549 **(0.158)	0.150 **(0.065)
Levels, 2SLS	---	15878 *(9547)	-0.0375 (0.0453)
Percentile, 2SLS	---	5.693 **(2.851)	-0.103 (0.482)
Logs, 2SLS	---	0.467 **(0.226)	-0.027 (0.378)

NOTES: this table investigates the links between father's job loss, father's wealth and children's wealth. The second column presents the impact of father's job loss due to establishment death on child's wealth without controlling for father's wealth, in a restricted sample of 909 children from 481 families (815 from 464 in the log specifications), which is smaller than the baseline sample due to the requirement that we observe father's net worth in 1984. The third and fourth columns present the simultaneous impact of father's job loss and father's net worth on children's net worth, where father's net worth is entered in levels, percentiles or logs according to the specification of child's net worth. The 2SLS specifications instrument for father's net worth using father's union status and industry wage premium. The vector X consists of the baseline controls used in the rightmost columns of Table 3. Standard errors are robust to heteroscedasticity and cross-correlation of disturbances among siblings. (*) and (**) denote significance at 10 and 5 percent.

TABLE 8: DOES THE IMPACT OF FATHER'S JOB LOSS DEPEND ON HOW MANY SIBLINGS THE CHILD HAS? $$NETW = \alpha + \gamma * DADLOSS + \delta * SIBS + \phi * DADLOSS * SIBS + \beta X + \varepsilon$$			
Specification	Father's Job Loss	Number of Siblings	Job Loss * Siblings
Levels	16291 *(8430)	-333.5 (1773)	-970.2 (2486)
Percentile	4.340 *(2.292)	-0.957 *(0.342)	-0.061 (0.873)
Logs	0.477 **(0.135)	-0.0287 (0.0300)	0.0243 (0.0551)

NOTES: this table assesses whether the impact of father's job loss due to establishment death on child's net worth varies according to how many siblings the child has. For each specification, the column reports the coefficients on father's job loss variable DADLOSS, the number of siblings SIBS, and the interaction between DADLOSS and SIBS. The vector X consists of the baseline set of control variables used in the rightmost columns of Table 3. Standard errors are in parentheses and are robust to heteroscedasticity and arbitrary cross-correlation of disturbances among siblings. (*) and (**) denote significance at 10 and 5 percent.

	TABLE 9. THE DOWNWARD EXTENSION OF ADULTHOOD: DOES FATHER'S JOB LOSS CAUSE KIDS TO GROW UP FASTER? $Y = \alpha + \gamma * DADLOSS + \beta X + \varepsilon$		
Dependent Variable	Impact of Job Loss, Boys/Girls Pooled	Impact of Job Loss, Boys Only	Impact of Job Loss, Girls Only
Age Child First Reports Labor Y	0.509 *(0.261)	0.556 (0.393)	0.501 (0.310)
Child's Total Y Prior to Moving Out	-1634 (2333)	-1673 (4941)	-692.1 (1960)
Age Child First Moves Out	0.257 (0.250)	0.411 (0.280)	0.078 (0.452)
Probability Child Ever Has Spouse	0.0372 *(0.0196)	0.0664 **(0.0265)	0.0162 (0.0264)
Age of Child when Spouse First Present	0.617 *(0.348)	0.465 (0.571)	0.783 **(0.386)
Child's Years with Spouse Present	-0.114 (0.360)	0.174 (0.537)	-0.353 (0.548)
Child's Industry Hirfindahl	-4.25e-05 (0.0235)	-0.0470 (0.0397)	0.0380 (0.0302)
Child's Occupation Hirfindahl	-0.00269 (0.0233)	-0.0297 (0.0390)	0.0144 (0.0290)

NOTES: this table presents estimates of the impact of father's job loss on a variety of measures of economic maturity. The vector X consists of controls for child's demographics (omitting spouse's demographics) and father's demographics and observable skills. Standard errors are in parentheses and are robust to heteroscedasticity and cross-correlation of disturbances among siblings. (*) and (**) denote significance at 10 and 5 percent.

TABLE 10. CORRELATED UNCERTAINTY? NET WORTH, FATHER'S JOB LOSS AND CHILDREN'S INCOME VOLATILITY

$$\text{NETW} = \alpha + \gamma * \text{DADLOSS} + \delta * \text{SDEV} + \beta X + \varepsilon$$

Specification	Father's Job Loss, No Control for Children's Income Volatility	Father's Job Loss, Control for Children's Income Volatility	Impact of Children's Income Volatility
Levels, OLS	19251 *(9370)	16087 *(8833)	9.157 **(2.085)
Percentile, OLS	5.826 **(2.426)	5.675 **(2.445)	0.000439 *(0.000233)
Logs, OLS	0.635 **(0.150)	0.623 **(0.152)	3.82e-05 **(1.28e-05)
Levels, 2SLS	---	10280 (9897)	25.97 **(4.793)
Percentile, 2SLS	---	4.281 (2.829)	0.00447 **(0.00091)
Logs, 2SLS	---	0.551 **(0.174)	2.55e-04 **(5.31e-05)
Levels, 2SLS, Control for Child's Self-Employment	---	15878 *(8573)	-1.647 (5.352)
Percentile, 2SLS, Control for Child's Self-Employment	---	5.123 **(2.355)	-0.00004 (0.00134)
Logs, 2SLS, Control for Child's Self-Employment	---	0.598 **(0.147)	-4.55e-05 (8.51e-05)

NOTES: this table investigates the links between father's job loss, children's adult wealth and children's income volatility. The second column estimates the impact of father's job loss due to establishment death on children's wealth without controls for children's income volatility in a restricted sample of 1107 children from 629 families (1017 children from 600 families for the log specification), which is smaller than the baseline sample due to the requirement that children have five or more years of income. The third and fourth columns present the simultaneous impact of job loss and children's income volatility on children's net worth in the restricted sample, where income volatility is defined as the standard error from regressing real labor and transfer income of the child and current spouse on a constant, a dummy for spouse presence and a time trend, household by household. The 2SLS specifications instrument for income volatility using dummies for child and spouse's industry and occupation, with and without controls for self-employment of the child and spouse. The vector X consists of the baseline controls used in the rightmost columns of Table 3. Standard errors are robust to heteroscedasticity and cross-correlation of disturbances among siblings. (*) and (**) denote significance at 10 and 5 percent.

Wealth Measure	Mean	Standard Deviation	Median	Minimum	Maximum	Percent Positive
Net Worth	55145	118152	20169	-596704	1388676	89.1
Stocks	3522	18118	0	0	459067	31.2
Monetary Assets	6942	18656	1580	0	306044	87.4
Business Net Worth	10794	50386	0	0	765111	14.8
Home Equity	21170	37775	6503	-22953	382556	59.9
Other Real Estate	5599	32115	0	-7651	650344	15.0
Vehicles	6623	9217	4591	0	141545	91.2
Debt	3538	18748	765	0	596787	65.4
Other Assets	4032	27370	0	0	765111	29.1

TABLE A.1. SUMMARY STATISTICS, CHILDREN'S WEALTH
1989 DATA, IN 1982-84 PRICES