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THE EFFECT OF TEEN CHILDBEARING AND SINGLE PARENTHOOD ON CHILDHOOD DISABILITIES AND PROGRESS IN SCHOOL

Joshua D. Angrist
Victor Lavy

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ABSTRACT

Teen and out-of-wedlock child-bearing are often thought to be responsible for poor health and low levels of schooling among the children of young mothers. This paper uses special disability and grade repetition questions from the school enrollment supplement to the 1992 Current Population Survey to estimate the effect of maternal age and single parenthood on children's disability status and school progress. Our results suggest that there is little association between maternal age at birth and children's disabilities. But the children of teen mothers are much more likely to repeat one or more grades than other children, and within-household estimates of this relationship are even larger than OLS estimates. The grade repetition findings from the CPS are replicated using a smaller sample from the National Longitudinal Survey of Youth. Another finding of interest is that having a father in the household is associated with lower disability prevalence and fewer grade repetitions. But many of the effects of single parenthood on disability, as well as the effect on grade repetition, appear to be explained by higher incomes in two-parent families.

Joshua D. Angrist
Department of Economics
Massachusetts Institute of Technology
50 Memorial Drive
Cambridge, MA 02139-4307
and NBER
angrist@mit.edu

Victor Lavy
Department of Economics
Hebrew University
Mt. Scopus
91095 Jerusalem
ISRAEL

I. Introduction

Out-of-wedlock child-bearing and young maternal age are often cited as a cause of poor labor market outcomes, low levels of schooling, and poor health among the children of teen mothers. Papers studying this question include Furstenberg (1976), Furstenberg, Brooks-Gunn, and Morgan (1987), Bronars and Grogger (1994), Hofferth (1987), and the study of schooling by Olsen and Farkas (1989). The association between growing up in a single-parent family and schooling, as well as other aspects of child welfare, has been explored by McLanahan (1985) and in a recent book by McLanahan and Sandefur (1994), which provides many additional references. The major empirical problem in studies like these is that the variables of interest reflect choices made by parents using information and in circumstances not fully accounted for by the covariates commonly available in research data sets. Thus, the observed association between having a teen or single mother and worse progress in school is not necessarily caused by family structure. Rather, differences in health or school performance may primarily reflect the pre-existing characteristics of families or neighborhoods where the children of teen or out-of-wedlock mothers are most likely to grow up.

A popular statistical approach to the problem of unobserved heterogeneity in research on family effects is to use information on siblings to control for unobserved additive effects that are shared by family members. For example, Geronimus and Korenman (1993) and Geronimus, Korenman, and Hillemeir (1994) examine the impact of maternal age on children using data on the children of mothers who are sisters. They conclude that maternal family background accounts for many of the apparent disadvantages suffered by the children of teenage mothers. Rosenzweig and Wolpin (1988, 1991) also work with samples of siblings. A drawback of these and many other sibling studies are that they rely on small and sometimes unrepresentative samples.

In this paper, we use a large cross-section from the School Enrollment supplement to the October 1992 Current Population Survey to examine the relationship between children's health and schooling, maternal age at birth, father presence, and other family background variables such as income. The CPS is an attractive source of information for our purposes because it contains information on nearly 40,000 children. Moreover, we can link information on children within households to control for unobserved heterogeneity by including household and mother effects.

The 1992 October CPS is unusual in that it contains a special set of questions on the health and disability status of children aged 3-14 and young people aged 15-24. In addition, the survey contains questions on the frequency and incidence of grade repetition, providing information that we can use to construct a simple but objective measure of children's school progress. Most previous research on mothers' characteristics and child development has focused on test scores (e.g., Behrman and Lavy, 1993; Rosenzweig and Wolpin, 1992), which may not be a good predictor of children's future economic welfare. Grade repetition provides an alternative outcome. It is estimated that about 5% of all students repeat a grade each year and, in many US states, the proportion of repeaters by eighth grade approaches 50% (Shepard and Smith, 1989).[1]

Grade repetition is important because it is generally thought by educators to indicate some kind of problem in school and to be a good predictor of future problems (e.g., Gelzheiser, 1987; McLeskey and Grizzle, 1992). Recent studies have shown that grade repetition is positively associated with dropping out of school (Cairns, Cairns, and Neckerman, 1989; Grissom and Shepard, 1989), and negatively correlated with cognitive achievement (Holmes, 1989; Reynolds, 1992). This relationship has led a number of researchers to treat repetition as a policy outcome of interest. For example, Currie and Thomas (1995) look at grade repetition in their evaluation of Head Start programs. Dawson (1991) has looked at the impact of family structure on health and grade repetition, as well as other schooling outcomes, using a large cross-section of children from the child health supplement to the 1988 National Health Interview Survey.

The next section describes the October CPS data and presents some descriptive statistics. Section III discusses OLS and within-family estimates of the relationship been maternal age at birth, family background variables, and childhood disability. Section IV discusses estimates of effects on grade repetition. Section V replicates the basic grade repetition findings using a smaller sample from the National Longitudinal Survey

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[1]Repetition rates in the United States are higher than in most of the industrialized countries. For example, in Japan and most European nations fewer than 1% of the school-aged population repeat each year (Smith and Shepard, 1987).

of Youth (NLSY). Section VI offers a summary and some conclusions.

II. Descriptive Statistics

The CPS is a monthly survey of over 50,000 households which includes a different topical module each month. The October school enrollment supplement to the CPS collects information on school enrollment status for respondents aged 3 and over. The supplement questionnaire differs somewhat from year to year and in the October 1992 survey, special questions on grade repetition and disability status were asked of respondents aged 24 and under. Questionnaire items for young household members were completed by adult respondents. The sample of children analyzed here includes household members aged 3-17, pooling information from the child (aged 3-14) and adult (aged 15-17) components of the survey. The grade repetition and disability questions are reproduced in the data appendix.

Descriptive statistics for the full CPS extract appear in columns 1-2 of Table 1. This sample includes children aged 3-17 enrolled in school or a day care program for whom it was possible to link data on the mother to data on the child. The mean age in the sample is between 10-11 years. 11 percent of Nonblack children were identified by respondents as Hispanic. 83 percent of the Nonblack children had fathers in the household while only 44 percent of the Black children had fathers in the household. 37 percent of Nonblack mothers were high school graduates and 41 percent of Black mothers were high school graduates. The mean age at any birth was 27 years for white mothers and 25 for Black mothers, and 8 percent of Black mothers were aged 17 or less when the child was born, in contrast with 3 percent of white mothers. Roughly 1 percent of Black and Nonblack mothers were over 35 years old when they gave birth.

In response to a question on whether the child has *ever* repeated a grade, 9 percent of Nonblack children in the sample reported repeating a grade and 14 percent of Black children reported repeating a grade. The total number of repetitions of grades 1-12 (excluding kindergarten) is .07 for Nonblacks and .13 for Blacks. This figure is smaller than the ever-repeated figure because kindergarten is the grade most often repeated. The table also shows that 86 percent of Nonblack children in the sample were in public school and 93 percent of Black children in the sample were in public school.

The first disability question tabulated in the descriptive statistics asks whether a child has ever had a condition that affected the child's ability to learn. The response to this question was positive for 4 percent of Nonblack children and for 3 percent of Black children. The remaining disability question offers a list of 10 specific conditions. Interviewers were instructed to indicate all that apply for a given respondent. In response to this question, 4 percent of Nonblack children are reported to have had a learning disability and less than one percent of both Nonblack and Black children were reported to be mentally retarded. Sight and hearing disabilities other than deafness and blindness were reported for between 1 and 2 percent of children. Roughly 2 percent of children were reported to have had health problems lasting longer than 6 months, other than those listed in the questionnaire.

The last two columns of Table 1 report averages for the entire sample of 39,527 children aged 0-17. These averages were computed using CPS sampling weights and therefore constitute population estimates that can be compared to figures reported in research on the prevalence of specific disabilities. For example, Kiely (1987) reports that the prevalence of severe mental retardation is consistently found to be between 3 and 5 per thousand population. Estimates of the prevalence of mild retardation are considerably more variable than estimates of severe mental retardation. But most studies have shown a consistently higher rate of both mild and severe mental retardation among males. Some studies also suggest that mild retardation is correlated with social class, whereas severe retardation is thought to be evenly distributed across social classes. Finally, the prevalence of many disabilities appears to increase with age.

The descriptive statistics in the last two columns of Table 1 show population prevalence estimates of mental retardation for children in the United States of 7 and 8 per 1000 population. This low rate suggests that the October CPS questionnaire effectively labels as retarded primarily those children with relatively severe mental handicaps. The figures in the table are also consistent with disability data reported elsewhere (e.g., Miller, 1976) in showing that blindness and deafness are relatively uncommon disabilities. The less severe hearing and sight impairments appear to be relatively common, and speech problems constitute the most commonly reported impairment.

III. Disability estimates

The factors that link adolescent and single motherhood to infant health may also link disabilities in older children to maternal age at birth and family structure. We use the following statistical model to explore the links between maternal age, family structure, and disability status. Let h = 1, . . .H index households and j = 1, . . .,J index children within households. Let d_{hj} denote the disability status of child j in household h. The vector of covariates is denoted X_{hj}. The behavioral relationship of interest is

$$E[d_{hj}| X_{hj}] = \mu_{0h} + X_{hj}'\delta_0, \tag{1}$$

where μ_{0h} is household or mother-specific fixed effect and δ_0 is a coefficient vector. [2] Initially, we assume that μ_{0h} is uncorrelated with X_{hj}, in which case it can be viewed as a component of the error term. Otherwise, correlation between μ_{0h} and X_{hj} can be handled by differencing or estimation in deviations from means [i.e., Analysis of Covariance (ANCOVA)].

Table 2 presents OLS estimates of equation (1) for 7 different disability variables. For the purposes of this analysis, deafness and other hearing problems were combined in one indicator, as were blindness and other sight problems. The table reports coefficients on the main variables of interest: maternal age less than or equal to 17 (Young=1), maternal age greater than or equal to 40 (Old=1), a public school dummy, a dummy for the presence of the spouse (Father=1), dummies to indicate the mother's schooling, and dummies for Blacks and Hispanics. Other regressors included in the equation are 9 Census region dummies, dummies for center city

[2]Equation (1) can be thought of as a "household production function" for health (see, e.g., Rosenzweig and Schultz, 1983). Note that within-household estimates based on equation (1) attempt to capture effects of maternal age at *each* birth, whereas some of the literature on teen child-bearing has focused on maternal age at *first* birth (e.g., Geronimus and Korenman, 1992).

and SMSA residence, and a full set of age dummies. The estimation sample includes enrolled children matched to data on their mothers.[3]

There is no statistically significant association between young maternal age and the disability outcomes. The largest effect that is close to statistical significance is the Young coefficient in the equation for emotional disturbance, with an estimate of 75 per thousand and a standard error of 45 per thousand.[4] The estimated effect of old maternal age is also insignificant for each outcome. There is considerable medical evidence that older mothers are at increased risk of having children with Downs Syndrome (see, e.g. Kiely, 1987). The relationship estimated here between old maternal age and mental retardation is positive but little bigger than its standard error.

Boys are more likely to be reported as having any disability except sight problems. The relationship between sex and disability is strongest for speech problems. The means in Table 1 also suggest that average reported disability rates differ little by race. But controlling for covariates such as parental education leads to estimated race effects that suggest a lower rate of disability for Blacks and Hispanics. Dawson (1991) also found a lower prevalence of reported health problems for minorities.

Other covariates of interest in the table include indicators of the mothers schooling level. Increasing levels of mothers schooling are almost always negatively related to the indicators of disability. For example, children of mothers with college education have a 1.2 percentage point reduced rate of learning disability. Large and significant maternal schooling effects are also observed for speech and hearing disabilities. Table 2 also shows that living in a two-parent family is associated with a substantially and significantly lower disability prevalence. The father-effect is particularly large for learning disabilities.

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[3]The sample is also restricted to CPS-defined "own-children of the reference person" in the household. In practice, this means that the children live with a parent and that data for the children was reported by a parent.

[4]Because the dependent variables is binary, the table reports heteroscedasticity-consistent standard errors.

Table 3 reports results from the same models used to construct the estimates in Table 2, with the addition of a family income variable. The variable used here is a discrete coding of interval groupings of family income into values from 1-13. Increasing family income is negatively associated with child disability rates. Inclusion of family income as a regressor also reduces the apparently beneficial effect of a father in the household to a statistically insignificant level for every disability outcome except learning disabilities and emotional problems. The coefficient in the learning disability equation is reduced by almost half when family income is included, from 1.8 percent to 1.1 percent. This suggests that a large part of the association between the presence of a second parent and child outcomes can be accounted for by the higher incomes of two-parent families. Inclusion of family income also reduces the size and statistical significance of the coefficients on dummies for mothers' schooling.[5]

Table 4 presents within-household fixed effect estimates. Regressors common to all members of the household drop out in the fixed effects estimation because they are assumed to have the same coefficient for each sibling. Thus, the father, income, and mothers' schooling covariates do not appear in the fixed effects model. The effect of young maternal age is identified in these models by comparing a child born to a young mother with a sibling who was born to the same mother when she was older or to a stepmother who was older at the time of the sibling birth. The sample used to produce these estimates includes only members of households with more than one child in the survey. Except in column 6, the fixed effects results show somewhat larger positive associations between young maternal age and the child disability outcomes than do the OLS results. None of the estimates are significantly different from zero. There is, however, a statistically significant relationship between old maternal age and other disabilities in column 6.

We conclude this section by repeating the caution that disability and health status are often thought to be poorly reported (see, e.g., Bound, 1991 for a discussion of health status reporting among retirees). For

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[5]McLanahan and Sandefur (1994) also emphasize the importance of family income for estimates of the effect of growing up with a single parent.

example, parents may be reluctant to report that their children are mentally retarded or emotionally disturbed. In a classical measurement error model with continuous variables, measurement error or randomly misreported data does not have an effect on regression estimates when the errors are confined to dependent variables. But measurement error in binary dependent variables such as health or disability status can have different implications. In particular, the statistical appendix outlines a simple model of family reporting behavior where measurement error generates a sort of attenuation bias in within-family estimates. Thus, the finding of no effect of young maternal age on reported disability status may be because bias from misreporting pushes the coefficient estimates towards zero.

IV. Grade repetition estimates

The educational outcome of greatest interest to policy makers is probably high school graduation status. An analogous outcome for children who are still enrolled is school progress, measured by the incidence and frequency of grade repetition. One reason grade repetition is important is because students who repeat grades are further from a high school diploma at the time they reach the compulsory attendance age. Grade repetition is also widely viewed by educators as an important indicator (and consequence) of poor school performance (Jackson, 1975; Gelzheiser, 1987).

When examining the association between grade repetition and the characteristics of children families, it is important to take account of the child's age and possible secular grade effects. Among children currently enrolled in a given grade, the probably of a grade repetition caused by academic reasons may be higher for those who are younger. There is also a relationship between the number of grade repetitions and a child's current grade because children who have been in school longer have had more chances to repeat a grade. Finally, grade repetition is necessarily negatively correlated with current grade enrolled because repetition means the child is held back a grade.

The following statistical model captures these essential features of the relationship between grade repetition, age and current grade enrolled. The outcome equation is

$$n_i = X_i'\beta_0 + \beta_1 a_i + \beta_2 a_i^2 + \gamma g_i + \alpha z_i + \epsilon_i \qquad (2)$$

where n_i is the number of grade repetitions by a child, a_i is the child's age, g_i is the child's currently enrolled grade, z_i is a regressor of interest (e.g., maternal age at birth), and ϵ_i is an error term. Our interpretation of this equation is that it is a linear approximation to the counterfactual causal effect of z_i on n_i with X_i, a_i, and g_i fixed. The equation is defined so that the error term is uncorrelated with a_i, X_i and z_i, but it is necessarily correlated with g_i. To see this, note that the variables n_i, s_i, a_i, and g_i are related by the following identity:

$$g_i = a_i - s_i - n_i, \qquad (3)$$

where s_i is school start age. This biases OLS estimates of the behavioral relationship of interest in the same way that the national income identity biases OLS estimates of the Keynesian consumption function.

The problem of identifying equation (2) can be solved by using equation (3) to substitute for g_i and to get the reduced form relationship between n_i and the regressors a_i, X_i, z_i, and s_i:

$$n_i = X_i'[\beta_0/(1+\gamma)] + [(\beta_1+\gamma)/(1+\gamma)]a_i + [\beta_2/(1+\gamma)]a_i^2 - [\gamma/(1+\gamma)]s_i + [\alpha/(1+\gamma)]z_i + \epsilon_i. \qquad (4)$$

Given estimates of $\theta_1 \equiv -[\gamma/(1+\gamma)]$ and $\theta_2 = [\alpha/(1+\gamma)]$, we can then find γ and α as $\gamma = -\theta_1/(1+\theta_1)$ and $\alpha = \theta_2/(1+\theta_1)$. Equivalently, s_i can be used as an instrument for g_i in (2). In either case, it is the exclusion of s_i that provides the extra information required for identification .[6]

For purposes of estimation, we computed s_i by substituting information on age, grade and the number of grade repetitions in (3). The distribution of the number of grade repetitions and school start age is given in Table 5 for the sample used to estimate equation (4). This sample includes children enrolled in grades 1-12, with data on the mother, who were not reported to be mentally retarded, with an imputed start age between 5 and 8. There are 21,878 children in this sample, a reduction from the 25,972 observations used to compute

[6]There is a large literature on the potential importance of age at school entry for school achievement (e.g., Proctor, Black and Feldhusen, 1986). However, Angrist and Krueger (1992) provide evidence which suggests that the primary effect of age at school entry on educational attainment works through a mechanical relationship between school start age and the legal dropout age. We therefore assume that s_i can be treated as exogenous.

the OLS estimates reported in Table 3.

The number of grade repetitions is defined here as the sum of grade-repetition indicators for each grade from 1-12 for children enrolled in these grades. Kindergarten repetition is not counted. Table 5 shows that most students who repeat a grade repeat only one grade, and that Blacks are more likely to have repeated a grade than Nonblacks. The distribution of school start age shows that most students start school at age 6, although roughly 19 percent start at age 7.

Table 6 reports estimates of γ and α from regressions with a variety of covariates playing the role of z_i. The coefficients were computed from OLS and fixed effects estimates of θ_1 and θ_2 in (4). Also reported are delta-method standard errors, computed using the heteroscedasticity-consistent covariance matrix for the estimates of θ_1 and θ_2.[7] The first row of Table 6 reports the estimates of γ. The first column reports OLS estimates from a model that excludes measures of the child's disability status and family income. The estimate of γ shows how much each additional year of schooling increases the probability of repeating a grade. For example, the results in column 1 suggest that, other things equal, an additional year of schooling is associated with .066 additional grade repetitions.[8]

The second and third rows of the table report estimates of α for the effect of maternal age on grade repetition. The estimates in column 1 show that young maternal age is statistically significantly associated with additional grade repetitions while old maternal age appears to have the opposite effect. The estimated effect of young maternal age is .038 with a standard error of .015.

The presence of a father in the household is associated with a reduction in grade repetition. This finding is similar to that of Dawson (1991) using the National Health Interview Survey. Other regressors of interest include mothers' schooling dummies, which show a strong negative relationship between mothers'

[7]The heteroscedasticity-consistent standard errors for the ANCOVA estimates were computing by applying the usual 4th-moments formula to equations transformed to deviations from household means.

[8]Included regressors with coefficients not reported in the table are a full set of age dummies, 9 Census region dummies, dummies for center city and SMSA residence, and black and Hispanic dummies.

schooling and grade repetition. Finally, boys are much more likely to repeat a grade than girls.

Column 2 of Table 6 reports the results of adding a measure of family income to the basic specification reported in column 1. The addition of family income as an explanatory variable reduces the estimated father-effect to zero. This is even more pronounced than the impact of the family income regressor on the father effect in the disability equations reported in Table 3. Similarly, the addition of family income as an explanatory variable reduces the relationship between mothers' schooling and grade repetition.

Columns 3 and 4 report the results of adding indicators of disability status to the basic equations reported in columns 1 and 2. There is a strong and statistically significant relationship between each of the disability indicators and grade repetition. Children reported as having a learning disabilities are most likely to have repeated a grade, while emotional problems constitute the disability category most highly related to grade repetition other than learning disabilities. Gelzheiser (1987) and others have argued, however, that at least some of the correlation between learning disability diagnoses and school progress probably reflects excessive labeling of children who are having trouble in school for other reasons.

The results in column 4 show that the relationship between disability and grade repetition is not affected by the inclusion of the family income regressor. In other respects, the contrast between columns 3 and 4 is similar to the contrast between columns 1 and 2. Inclusion of the disability indicators does not have an effect on the estimated effect of maternal age on grade repetition. Conditional on disability status, the children of young mothers are more likely to repeat grades and the children of older mothers are less likely to repeat grades.

Columns 5 and 6 report within-household estimates of equation (10). As in Table 4, the sample used to produce these estimates includes only members of households with more than one child in the survey. Regressors that do not vary within households do not appear in the models used to produce the within-household estimates.

The most important result of the fixed-effects estimation is that the effect of young maternal age is much larger than the OLS estimates. For example, the estimate in column 5 shows that the children of young mothers have an expected .096 more grade repetitions than other children. The standard error associated with this estimate is .019, not much larger than the OLS standard errors for the same coefficient. The within-household estimates of the effect of old maternal age are also larger than the OLS estimates, but no longer significantly different from zero. This suggests that the negative association between old maternal age and grade repetition may be largely attributable to family background. Finally, we note that within-household estimates of the relationship between disability status and grade repetition are similar to the OLS estimates.

V. Grade-repetition estimates using the NLSY

The NLSY is a panel survey that began in 1979, consisting of a base random sample augmented with non-random samples of Blacks, Hispanics, poor whites, and members of the armed forces. By 1992, most of the military people and all of the poor whites in the non-random part of the NLSY had been dropped, leaving 4,941 women for possible interviews. Our extract is drawn from the children of those women with non-missing responses to the supplement on school and family background given in the 1992 NLSY interview. In particular, for children aged 10 and older in 1992, the NLSY provides information on whether the child has ever repeated a grade.[9] Of the 2,079 children eligible for interview in the 1992 supplement, 1,658 had the information needed to estimate equation (4) using a dummy dependent variable indicating whether the child has ever repeated a grade.

Table 7 provides descriptive statistics for Black and Nonblack children in our NLSY extract. Note that this extract is not representative of either children or mothers because of the non-random sample design, attrition, the omission of poor whites, and the fact that the women being interviewed are younger than a random sample of American women with children. The NLSY mean age in 1992 is 32 while in the CPS it is

[9]The repetition question appears in the Mother Supplement, Section 5: School and Family Background, documented in Center for Human Resources Research (1994).

37. The mean child age in the extract is just over 12, almost two years older than in the CPS sample. Another difference between the two samples is the distribution of mothers' schooling: only 4.2 percent of the NLSY Nonblack mothers are college graduates, compared to 21 percent in the CPS sample. It is also noteworthy that only 43 percent of the NLSY Nonblack children had fathers in the household, half of the rate of the CPS sample. Roughly half of the Nonblack sample is Hispanic. Finally, the NLSY mean age at birth is about 20 years, 7 less than in the CPS sample. As a result, almost half of the children in the NLSY sample were born to teen mothers.

The NLSY outcome measure is an indicator of the child ever repeating a grade, with 1992 means of 22 percent for Nonblacks and 26 percent for Blacks. These rates are much higher than the CPS rates of 8.9 for Nonblacks and 14.9 for Blacks. The incidence of disabilities reported in Table 7 is very low, and much lower than reported in Table 1. We found a number of inconsistencies in the disability data as well, such as implausible changes in disability status, and therefore decided to omit the disability variables from our analysis of data from the NLSY.

Table 8 reports estimates of the grade repetition equation, (2). In this case, we report OLS estimates and IV estimates, using 3 quarter of birth dummies to instrument for the endogenous current grade enrolled variable.[10] Within-mother fixed-effects estimates (OLS and IV) are also reported.

The estimates in columns 1-4 of Table 8 can be compared to those reported in columns 1-2 of Table 6. The effect of young maternal age on grade repetition is positive and qualitatively similar to the estimates of Table 6, though estimated less precisely.[11] The IV estimates of this effect are also not very different from the OLS estimates. The IV model, however, leads to a reversal in the sign of the current grade enrolled

[10]Angrist and Krueger (1992) discuss the relationship between quarter of birth and children's age at school entry. The first stage equations show the largest quarter of birth effects for children born in the last quarter of the year, the second largest for those born in the third quarter and so on, relative to the omitted group, children born in the first quarter of the year.

[11]The NLSY sample cannot be used to estimate the effect of old maternal age on school progress.

variable, from highly significantly negative to positive, but very imprecise.

Column 5 reports within-household estimates of equation (8) and column 6 report the within-household IV estimates. The most striking thing about the NLSY fixed effects estimates of the effects of teen-age motherhood is that they are very similar to those obtained using the CPS. On the other hand, OLS estimates using the NLSY data still show evidence of an association between fathers' presence and school progress, even after a family income variable is added to the regression.

VI. Summary and conclusions

We used two data sets to estimate the effect of maternal age and single parenthood on measures of disability and grade repetition. OLS and fixed effects estimates using CPS data offer little evidence of a significant relationship between maternal age at birth and measures of child disability. These findings are generally similar to the regression estimates reported in other recent studies examining the impact of teen motherhood on the health status of infants and birth weight. On the other hand, misreporting of disability status could account for these low estimates. The OLS results do show an association between father's presence and disabilities. But father effects on disabilities other than learning disabilities and emotional disturbances disappear when family income is included as a regressor.

In contrast with the disability results, teen childbearing appears to have a strong effect on the incidence and frequency of grade repetition. Moreover, the fixed effect estimates show a larger impact of maternal age at birth on grade repetition than do the OLS estimates. Fixed effects estimates of the relationship between young maternal age and grade repetition suggest that children of young mothers have an average .09-.1 more grade repetitions than other children. This is slightly larger than the mean number of grade repetitions among enrolled children. The finding of a strong relationship between young maternal age and grade repetition contrasts with Olsen and Farkas' (1989) results, which show little relationship between dropout rates and mother's age at birth. On the other hand, as in some of the disability equations, the father-effect in grade repetition equations can be explained by higher incomes in two-parent households. This is consistent with the view taken by McLanahan and Sandefur (1994), who also find that a good part of the measured benefits of growing up in a two-parent family is attributable to the higher income of two-parent families.

Table 1: Descriptive statistics

Variable		Sample*		Population Weighted	
		Nonblack (1)	Black (2)	Nonblack (3)	Black (4)
C_age	(child age)	10.397	10.555	8.230	8.180
C_boy	(child is male)	0.512	0.504	0.513	0.503
Hispanic	(child is Hispanic)	0.107	0.015	0.136	0.013
Cntrcity	(household is in central city)	0.169	0.478	0.198	0.478
Insmsa	(household is in an SMSA)	0.351	0.196	0.395	0.211
Father	(father is present in household)	0.833	0.443	0.800	0.361
M_hsgrad	(mother is high school graduate)	0.371	0.412	0.366	0.417
M_someco	(mother has some college)	0.278	0.259	0.270	0.250
M_coll	(mother college graduate)	0.213	0.095	0.207	0.090
Birthage	(mother age when child was born)	26.740	24.824	26.980	25.090
Young	(mother age at birth=<17)	0.026	0.080	0.024	0.075
Old	(mother age at birth>35)	0.010	0.015	0.011	0.016
Mothage	(mother's age)	37.169	35.519	35.330	33.650
Ingrade	(grade enrolled for kids age>=6)	6.112	6.139	6.110	6.070
Repeated	(ever repeated)	0.089	0.149	0.093	0.160
Nrep	(number of repetitions)	0.066	0.133	0.071	0.144
Public	(child goes to public school)	0.855	0.936	0.855	0.939
Anylrnds	(child has any learning disability)	0.049	0.039	0.041	0.036
Lrndis	(child has a learning disability)	0.042	0.039	0.033	0.032
Mentalrt	(child is mentally retarded)	0.007	0.007	0.007	0.008
Speech	(child has a speech problem)	0.026	0.021	0.023	0.023
Emotion	(child is emotionally disturbed)	0.0076	0.0090	0.0085	0.0092
Deaf	(child is deaf)	0.0040	0.0040	0.0043	0.0048
Blind	(child is blind)	0.0029	0.0040	0.0033	0.0042
Other Hearing	(nondeaf hearing problem)	0.0135	0.0950	0.0122	0.0092
Other Sight	(nonblind vision problem)	0.0187	0.0173	0.0156	0.0135
Orthopedic	(orthopedic disability)	0.0105	0.0069	0.0096	0.0078
Other Health		0.0193	0.0161	0.0178	0.0185
Sample size:		22837	3354	33870	5657

*Data from the October 1992 Current Population Survey. The full sample includes 39,507 children aged 0-17. The working sample includes 26,191 enrolled children aged 3-17 successfully matched to data on the mother. Weights used to compute the averages in columns 3 and 4 are the CPS final weights and not the supplement weights.

Table 2: Disability Equations

	Dependent Variable						
	Lrndis (1)	Speech (2)	Emotion (3)	Hearing (4)	Sight (5)	Other (6)	Mentalrt (7)
Young	.0012	.0067	.0075	-.0005	.0052	-.0004	.0041
	(.0077)	(.0061)	(.0045)	(.0042)	(.0056)	(.0054)	(.0037)
Old	.0174	.0028	.0033	.0034	.0191	.0097	.0073
	(.0136)	(.0102)	(.0063)	(.0081)	(.0112)	(.0107)	(.0072)
Public	.0082	.0127	-.0002	.0038	-.0017	.0012	-.0004
	(.0032)	(.0026)	(.0015)	(.0021)	(.0024)	(.0029)	(.0016)
Boy	.0027	.0166	.0044	.0043	-.0014	.0060	.0020
	(.0025)	(.0019)	(.0011)	(.0015)	(.0017)	(.0019)	(.0010)
Father	-.0182	-.0069	-.0078	-.0033	-.0030	-.0091	-.0042
	(.0036)	(.0027)	(.0018)	(.0021)	(.0023)	(.0027)	(.0016)
M_hsgrad	-.0098	-.0039	-.0023	-.0049	-.0029	-.0005	-.0004
	(.0043)	(.0034)	(.0020)	(.0026)	(.0029)	(.0031)	(.0019)
M_someco	-.0072	-.0077	-.0031	-.0033	-.0028	.0039	-.0030
	(.0045)	(.0035)	(.0021)	(.0028)	(.0031)	(.0034)	(.0018)
M_coll	-.0120	-.0082	-.0038	-.0055	-.0029	.0018	-.0016
	(.0048)	(.0037)	(.0021)	(.0029)	(.0032)	(.0037)	(.0020)
Black	-.0136	-.0110	-.0042	-.0085	-.0047	-.0102	-.0029
	(.0041)	(.0032)	(.0020)	(.0024)	(.0028)	(.0031)	(.0018)
Hispanic	-.0289	-.0146	-.0056	-.0094	-.0072	-.0080	-.0007
	(.0042)	(.0035)	(.0018)	(.0024)	(.0029)	(.0035)	(.0023)
Dependent Mean	.0421	.0257	.0077	.0146	.0191	.0248	.0067

Notes: The table reports OLS estimates of linear probability models for disability indicators. The sample includes 25,972 observations. The sample is reduced from that in columns 1-2 of Table 1 by the restriction that data for children reported by a parent. Heteroscedasticity-consistent standard errors are reported in parentheses.

Table 3: Disability Equations with Family Income

	Dependent variable						
	Lrndis (1)	Speech (2)	Emotion (3)	Hearing (4)	Sight (5)	Other (6)	Mentalrt (7)
Young	.0002	.0056	.0070	-.0009	.0054	-.0015	.0042
	(.0080)	(.0064)	(.0047)	(.0044)	(.0059)	(.0056)	(.0039)
Old	.0222	.0055	.0041	.0046	.0218	.0083	.0084
	(.0146)	(.0110)	(.0068)	(.0088)	(.0121)	(.0109)	(.0078)
Public	.0062	.0107	-.0010	.0031	-.0024	.0010	-.0008
	(.0034)	(.0027)	(.0016)	(.0022)	(.0026)	(.0030)	(.0017)
Boy	.0274	.0162	.0044	.0041	-.0012	.0061	.0020
	(.0025)	(.0020)	(.0011)	(.0015)	(.0018)	(.0020)	(.0010)
Father	-.0106	-.0004	-.0045	-.0006	-.0018	-.0033	-.0030
	(.0040)	(.0031)	(.0019)	(.0023)	(.0026)	(.0031)	(.0018)
M_hsgrad	-.0046	.0005	-.0010	-.0032	-.0022	.0018	.0003
	(.0045)	(.0035)	(.0021)	(.0027)	(.0030)	(.0033)	(.0020)
M_someco	.0000	-.0019	-.0007	-.0005	-.0012	.0077	-.0021
	(.0050)	(.0038)	(.0022)	(.0030)	(.0033)	(.0036)	(.0020)
M_coll	-.0019	.0010	-.0006	-.0017	-.0015	.0083	-.0002
	(.0054)	(.0042)	(.0024)	(.0033)	(.0037)	(.0041)	(.0024)
Black	-.0161	-.0129	-.0053	-.0099	-.0058	-.0118	-.0032
	(.0044)	(.0034)	(.0021)	(.0025)	(.0030)	(.0032)	(.0019)
Hispanic	-.0324	-.0168	-.0073	-.0102	-.0076	-.0105	-.0013
	(.0043)	(.0036)	(.0018)	(.0026)	(.0031)	(.0036)	(.0023)
H_faminc	-.0022	-.0018	-.0008	-.0007	-.0004	-.0014	-.0003
	(.0005)	(.0004)	(.0002)	(.0003)	(.0003)	(.0004)	(.0002)
Dependent Means	.0427	.0261	.0080	.0151	.0195	.0253	.0069

Notes: The sample includes 24,667 observations. The sample is reduced from that in table 2 by missing values for the income variable. The table reports OLS estimates of linear probability models for disability indicators. Heteroscedasticity-consistent standard errors are reported in parentheses.

Table 4: Fixed Effects Estimates of Disability Equations

	Dependent Variable						
	Lrndis (1)	Speech (2)	Emotion (3)	Hearing (4)	Sight (5)	Other (6)	Mentalrt (7)
Young	.0104	.0091	.0058	.0072	.0039	-.0027	.0017
	(.0090)	(.0061)	(.0048)	(.0048)	(.0054)	(.0064)	(.0039)
Old	-.0154	-.0362	-.0086	-.0015	.0095	.0223	.0145
	(.0189)	(.0195)	(.0077)	(.0018)	(.0122)	(.0103)	(.0103)
Public	.0198	.0135	-.0030	.0112	.0013	-.0000	-.0024
	(.0069)	(.0062)	(.0032)	(.0037)	(.0039)	(.0144)	(.0036)
Boy	.0283	.0136	.0034	.0031	-.0002	.0054	.0027
	(.0027)	(.0021)	(.0010)	(.0015)	(.0016)	(.0019)	(.0010)
Dependent Mean	.0406	.0258	.0067	.0136	.0173	.0225	.0061

Notes: The table reports within-household ANCOVA estimates of linear probability models for disability indicators. The equations also include black and hispanic dummies (relevant for a small number of mixed-race/ethnic households). Heteroscedasticity-consistent standard errors are reported in parentheses. The sample includes 21,254 observations. The sample is reduced from that in Table 3 because it is limited to children from households where more than one child was interviewed.

Table 5: Distribution of Grade Repetition and School Start Age

Race	variable	0	1	2	3
nonblack	repetitions	17,882 (93.7)	1,142 (6.0)	54 (0.3)	3
black		2,456 (87.8)	317 (11.3)	23 (0.8)	1

Race	variable	5	6	7	8
nonblack	start age	987 (5.2)	14,287 (74.9)	3,531 (18.5)	276 (1.5)
black		216 (7.7)	1,978 (70.7)	526 (18.8)	77 (2.8)

Notes: Sample is restricted to own-children of the household reference person, enrolled in grades 1-12, with data on the mother, who are not reported to be mentally retarded, with an imputed start age between 5 and 8. Start age is imputed using the identity in the text. The number of repetitions is the sum of grade repetition indicators for grades 1-12. Percent distributions in parentheses.

Table 6: Grade Repetition Equations -- CPS sample

	OLS/Delta-method estimates				Fixed Effects	
	(1)	(2)	(3)	(4)	(5)	(6)
Startage	.0662	.0686	.0722	.0743	.0798	.0827
	(.0044)	(.0463)	(.0045)	(.0047)	(.0059)	(.0058)
Young	.0383	.0318	.0394	.0332	.0964	.0957
	(.0154)	(.0159)	(.0153)	(.0158)	(.0194)	(.0193)
Old	-.0550	-.0519	-.0560	-.0539	-.0725	-.0601
	(.0164)	(.0180)	(.0165)	(.0182)	(.0457)	(.0448)
Public	.0097	.0025	.0070	.0005	-.0334	-.0407
	(.0052)	(.0054)	(.0052)	(.0054)	(.0145)	(.0141)
Boy	.0427	.0438	.0354	.0365	.0490	.0422
	(.0039)	(.0040)	(.0039)	(.0039)	(.0045)	(.0045)
Father	-.0323	-.0020	-.0268	.0011		
	(.0056)	(.0065)	(.0056)	(.0064)		
M_hsgrad	-.0866	-.0707	-.0833	-.0690		
	(.0086)	(.0088)	(.0084)	(.0087)		
M_someco	-.1120	-.0887	-.1108	-.0897		
	(.0085)	(.0089)	(.0084)	(.0088)		
M_coll	-.1389	-.1034	-.1350	-.1026		
	(.0085)	(.0091)	(.0084)	(.0090)		
H_faminc		-.0078		-.0071		
		(.0008)		(.0008)		
Lrndis			.2368	.2367		.2234
			(.0178)	(.0182)		(.0187)
Speech			.0562	.0481		.0215
			(.0215)	(.0218)		(.0227)
Emotion			.1192	.1162		.1218
			(.0520)	(.0521)		(.0627)
Hearing			.0766	.0692		.1009
			(.0282)	(.0282)		(.0338)
Sight			.0586	.0584		.0815
			(.0212)	(.0218)		(.0265)
Other			.0550	.0535		.0781
			(.0198)	(.0201)		(.0241)
N	21,878	20,748	21,878	20,748	17,785	17,795

Notes: The table reports estimates of γ and α in equation (4) in the text. The equations also include black and hispanic dummies. Sample size reported in bottom row. The sample in column 1 is the same as described in the notes to Table 5. Delta-method standard errors based on equation (4) and the OLS heteroscedasticity-consistent covariance matrix are reported in parentheses.

Table 7: Descriptive Statistics -- NLSY sample

Variable		Nonblacks	Black
C_age	(child age)	12.293	12.063
C_boy	(child is male)	0.506	0.534
Hispanic	(child is Hispanic)	0.527	0.000
Cntrcity	(household is in central city)	0.146	0.166
Insmsa	(household is in an SMSA)	0.732	0.861
Father	(father is present in household)	0.412	0.480
M_hsgrad	(mother is high school graduate)	0.431	0.315
M_someco	(mother has some college)	0.208	0.134
M_coll	(mother colllege graduate)	0.042	0.029
Birthage	(mother age when child was born)	19.653	19.744
Young	(mother age at birth<=17)	0.481	0.478
Old	(mother age at birth<=35)	0.000	0.000
Mothage	(mother age in 1992)	31.911	31.788
Ingrade	(grade enrolled for kids)	5.905	5.778
Repeated	(ever repeated)	0.216	0.261
Lrndis	(child has a learning disablity)	0.019	0.010
Mentalrt	(child is mentally retarded)	0.000	0.000
Speech	(child has a speech problem)	0.002	0.000
Emotion	(child is emotionally disturbed)	0.003	0.002
Hearing	(child has a hearing problem)	0.002	0.002
Sight	(child has a vision problem)	0.002	0.000
Other	(child has another health problem)	0.072	0.076
Sample size		1248	410

Notes: Sample of children of NLSY respondents.

Table 8: Grade Repetition Equations -- NLSY Sample

	OLS (1)	IV (2)	OLS (3)	IV (4)	Fixed Effect (5)	IV FE (6)
Boy	0.0765	0.1048	0.0750	0.1078	0.0684	0.1196
	(0.0181)	(0.0223)	(0.0198)	(0.0242)	(0.0210)	(0.0250)
Ingrade	-0.2424	0.0949	-0.2395	0.0141	-0.2816	-0.0053
	(0.0122)	(0.0536)	(0.0133)	(0.0258)	(0.0171)	(0.0513)
Young	0.0257	0.0228	0.0188	0.0813	0.1253	0.0867
	(0.0198)	(0.0240)	(0.0216)	(0.0546)	(0.0319)	(0.0363)
Father	-0.0608	-0.0871	-0.0544	-0.0815		
	(0.0193)	(0.0238)	(0.0212)	(0.0257)		
M_hsgrad	-0.0254	-0.0726	-0.0202	-0.0620		
	(0.0215)	(0.0270)	(0.0235)	(0.0288)		
M_someco	-0.0550	-0.1294	-0.0620	-0.1414		
	(0.0268)	(0.0344)	(0.0295)	(0.0374)		
M_coll	-0.0585	-0.1538	-0.0397	-0.1305		
	(0.0494)	(0.0615)	(0.0522)	(0.0639)		
HH_size	0.0269	0.0556	0.0303	0.0585		
	(0.0108)	(0.0138)	(0.0118)	(0.0148)		
Black	0.0453	0.0335	0.0553	0.0428		
	(0.0259)	(0.0314)	(0.0282)	(0.0336)		
Hispanic	-0.0163	0.0131	-0.0086	0.0067		
	(0.0233)	(0.0286)	(0.0252)	(0.0301)		
H_faminc			-0.0002	-0.0002		
			(0.0001)	(0.0001)		
N	1658	1658	1403	1403	1041	1041

Notes: Estimates of equation (4) in the text. Standard errors in parentheses.

Data Appendix

A. School progress and disability questions by questionnaire item number
(children's item number/adult item number).

62/48. Since starting school, has . . . ever repeated a grade? If yes, which one?, Any others? (Interviewer is to check each grade that applies from K-12).

67/52. Has . . . ever had a physical, mental, or other health condition that *adversely affected their ability to learn*?

68/53. Has . . . had any of the following conditions? (Read and check *all* that apply) Learning disability, mental retardation, speech impairment, serious emotional disturbance, deafness, other hearing impairment, blindness, other vision impairment, orthopedic impairment, other health impairment (lasting 6 months or more), none of the above.

B. Linking data on members of the same household

The October 1992 CPS contains 159,439 person-records, including 39,527 records on children aged 0-17. Records are uniquely identified by the record type, the household identifier, and the line number within the household. Information on children aged 0-14 is reported on record type 5 (child records) and information on respondents aged 15 and over is reported on record type 1 (adult records). Records for both adults and children include a variable indicating one parent's line number when there is a parent in the same household as the child. Adult records include a variable showing the spouse's line number when the spouse is in the same household as the first adult parent. There are 37,430 records for adults with children under 18 (not necessarily in the same household). Of these, 28,663 have a spouse line number.

The household identifier and parent's line number were used to link observations on children 0-17 to a parent (usually the mother). The parent-child match was then linked to information on spouses using the first parent's spouse line number. At least one parent was found for 35,811 out of 39,527 children. A second parent was found for 27,093 of the initial parent-child matched pairs.

Fixed effects estimates in this paper are within-household and not necessarily within family. In practice, however, the fixed effects estimates are computed for a subsample limited to children for whom information was reported by a parent. This means that any two children within the same household will usually have had information reported by the same parent and will therefore be siblings.

The CPS includes different survey weights for supplement and regular questions. These are post-stratification weights; the CPS sample is actually self-weighting. In practice, weighted and un-weighted estimates differed little. Except for the population prevalence estimates reported in Section II, the results reported here are unweighted. The weights used to compute the estimates in columns 3 and 4 of table 1 are the child and adult "final weights" for the main CPS questionnaire.

Statistical Appendix

The implications of reporting error are outlined in a model that relates children's recorded disability status to actual disability status.[12] We use this model to show that as long as there is a degree of within-household homogeneity in reporting behavior, then mis-reporting of disability status leads to an attenuation bias in fixed effects estimates of the effect of covariates on disability status.

As before, let d_{hj} denote the actual disability status of child j in household h, and let r_{hj} denote reported disability status for the same individual. The following identity relates reported and actual disability status:

$$P[r_{hj}=1 | X_{rj}] = \{P(r_{hj}=1 | X_{hj}, d_{hj}=1)P(d_{hj}=1 | X_{hj})\} + \{P(r_{hj}=1 | X_{hj}, d_{hj}=0)[1-P(d_{hj}=1 | X_{hj})]\}$$

A key identifying assumption used here is that the probability of mis-reporting, while a function of the child's actual disability status, is otherwise determined by the reporter and not the child. Assuming data for all children in a family are reported by the same person, we can write:

$$P(r_{hj}=1 | X_{hj}, d_{hj}=1) \equiv \pi_{1h}$$

$$P(r_{hj}=1 | X_{hj}, d_{hj}=0) \equiv \pi_{0h}$$

Second, we assume a linear conditional expectation for actual disability status:

$$E[d_{hj} | X_{hj}] = \mu_{1h} + X_{hj}'\beta_0,$$

where μ_{1h} is a household effect. Since most of the regressors used here are dummy variables, the linearity restriction is not very limiting.

Define $\pi_h = \pi_{1h} - \pi_{0h}$. Thus, π_h indicates how much more likely a truly disabled child is to be reported as disabled than is a child who is not disabled. For example, in the case of accurate reporting, $\pi_h = 1$. In the case of "perverse reporting", where non-disabled children are always reported as disabled and disabled children as non-disabled, $\pi_h = -1$. Finally, if there is no relationship between actual disability status and reported disability status then $\pi_{1h} = \pi_{0h}$ and $\pi_h = 0$.

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[12]Krueger and Summers (1988) discuss the consequences of measurement error in longitudinal models with dummy regressors.

Substituting, we have

$$E[r_{hj}| X_{rj}] = \{\pi_{0h} + \mu_{1h}\pi_h\} + X_{hj}'\beta_0\pi_h.$$

Now suppose that β_0 is estimated in a model with fixed effects. In particular, suppose that the equation above is differenced across sibling pairs (indexed by j and k) and, for illustration, that X_{hj} is a scalar. Differencing eliminates $\{\pi_{0h} + \mu_h\pi_h\}$, so that

$$E[r_{hj} - r_{hk}| X_{rj}, X_{rk}] = [X_{hj} - X_{hk}]\beta_0\pi_h.$$

OLS estimates of the differenced equation have a probability limit equal to the probability limit of

$$\frac{\beta_0\{\Sigma[X_{hj} - X_{hk}]^2\pi_h\}}{\Sigma[X_{hj} - X_{hk}]^2},$$

which is β_0 times a weighted average of π_h with positive weights $[X_{hj} - X_{hk}]^2/\Sigma[X_{hj} - X_{hk}]^2$ that sum to 1. Suppose also that a truly disabled child is always more likely to be reported as disabled than a non-disabled child. This means that $\pi_{1h} \geq \pi_{0h}$ so that $1 \geq \pi_h \geq 0$ for all h. In this case, as in classical errors-in-variables models, the first-differences estimate of β_0 is shrunk towards zero, away from the true β_0. In the extreme case where there is no relationship between reported and actual disability status, the estimated β_0 will be zero because π_h will be zero.

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