Institute for Research on Poverty
Discussion Paper no. 1274-03

The Effect of Increases in Welfare Mothers' Education on Their Young Children's Academic and Behavioral Outcomes: Evidence from the National Evaluation of Welfare-to-Work Strategies Child Outcomes Study

Katherine Magnuson
Columbia University School of Social Work
E-mail: kam2101@columbia.edu

September 2003

This manuscript is based on research supported by the Young Scholars Small Grants Program for Research using Welfare Outcomes Data, from the Office of the Assistant Secretary for Planning and Evaluation, U.S. Department of Health and Human Services (DHHS), and administered through the Institute for Research on Poverty. The opinions and conclusions expressed herein are solely those of the author and should not be construed as representing opinions or policy of DHHS.

Abstract

Does an increase in a welfare mother's education improve her young child's academic performance or behavior? Positive correlations between mothers' educational attainment and children's well being, particularly children's cognitive development and academic outcomes, are among the most replicated results from developmental studies. Yet, surprisingly little is known about the causal nature of this relationship. Because conventional regression approaches to estimating the effect of maternal schooling on child outcomes may be biased by omitted variables, this study uses experimentally induced differences in mothers' education to estimate instrumental variable (IV) models. Data come from the National Evaluation of Welfare-to-Work Strategies Child Outcomes Study—an evaluation of mandatory welfare-to-work programs in which welfare recipients with young children were randomly assigned to either an education- or work-focused program group or to a control group that received no additional assistance. Findings suggest that increases in maternal education are positively associated with children's academic school readiness, and negatively associated with mothers' reports of their children's academic problems, but with little to no effect on children's behavior. Analyses were not able to determine whether the benefits of maternal education persisted over time, although they were able to test whether mothers' returns to schooling during their children's preschool years were more beneficial than returns during later years. Weak evidence indicates that mothers' reentry into school when children are young will have a lasting effect on children's academic problems.

The Effect of Increases in Welfare Mothers' Education on Their Young Children's Academic and Behavioral Outcomes: Evidence from the National Evaluation of Welfare-to-Work Strategies Child Outcomes Study

INTRODUCTION

Because children learn both at home and in school, it is not surprising that family characteristics, such as socioeconomic status and parental education, are important predictors of children's school success (Hess et al., 1984; Lee and Croninger, 1994; Scott-Jones, 1984). Students from economically disadvantaged families enter school already academically far behind and continue to perform less well than their more advantaged peers (Entwisle and Alexander, 1993; Stipek and Ryan, 1997). Programs to improve economically disadvantaged children's school readiness typically intervene directly with the child, for example, by providing enriching preschool experiences. However, another potential route of intervention targets parents' education (Magnuson and Duncan, forthcoming).

Although close to half of economically disadvantaged mothers return to school sometime after the birth of their children without any intervention (Astone et al., 2000; Furstenberg, Brooks-Gunn, and Morgan, 1987; Love et al., 2002; Rich and Kim, 1999), numerous programs have been undertaken to promote employment among disadvantaged adults, by offering them education and training. Evaluation studies typically find modest positive effects of education and training programs on adult women's subsequent earnings (Heckman, Lalonde, and Smith, 1999). Rarely have studies of these programs considered whether the children benefited from their parents' participation in basic skills and training programs. Even in the absence of large gains in employment or income, these programs may have a positive effect on children if they improve the amount of stimulation in children's home learning environments or improve the quality of their interactions with their mothers (Magnuson and Duncan, forthcoming). By overlooking these possible benefits, we may be underestimating the effects of basic education and job training to improve the lives of poor families, and neglecting an opportunity to more firmly establish whether maternal education improves children's lives.

In this study, I use data from a welfare-to-work experiment, the National Evaluation of Welfare-to-Work Strategies Child Outcomes Study (NEWWS COS), to estimate the effect of an increase in welfare mothers' education on their children's outcomes. Welfare recipients with young children in the NEWWS COS sample were randomly assigned to participate in either an education-first or a work-first program. For comparison purposes, a control group that received no mandated programming was included in each site. Approximately 2 and 5 years after random assignment, follow-up surveys collected detailed information on the program and control group members' education and employment experiences, as well as family life and child well-being.

Identifying causal associations between mothers' education and children's outcomes is difficult because mothers who participate in education and training programs are likely to differ in unobservable ways from mothers who do not. Consequently, conventional regression (e.g., OLS) and analysis of variance (e.g., ANOVA) methods may lead to estimates that are biased by omitted variables. This study minimizes the threat of omitted variable biases by using experimentally induced differences in mothers' education to estimate instrumental variable (IV) models of the effects of mothers' participation in educational activities on children's outcomes measured 2 years after random assignment. If key assumptions are met, IV methods eliminate omitted variable biases, thus improving causal inference by reducing the plausibility of alternative explanations for the associations between maternal education and children's outcomes. Using data from the 5-year follow-up survey, analyses were not able to determine whether the benefits of maternal education persisted over time, although they were able to test whether mothers' returns to schooling during their children's preschool years were more beneficial than returns during later years.

The paper proceeds with a discussion of the previous literature on the effects of maternal education on children's academic and behavioral outcomes, and possible pathways by which it exerts an influence. Next, the data and methods used in the study are described. Finally, results are presented and discussed.

BACKGROUND

Higher levels of maternal educational attainment have been consistently linked to their children's better academic achievement (Haveman and Wolfe, 1995). Positive associations have been found between maternal education and children's performance on cognitive tasks as early as 3 months of age (Roe and Bronstein, 1988), young children's school readiness (Christian, Morrison, and Bryant, 1998; Seefeldt et al., 1999), school grades (Conger, Conger, and Elder, 1997; Smith, 1989), tests of academic achievement (Alexander, Entwisle, and Bedinger, 1994; Alwin and Thornton, 1984; Davis-Kean and Eccles, 2003; Mercy and Steelman, 1982), and educational attainment (Alwin and Thornton, 1984; Ensminger and Slusarick, 1992; Mare, 1980; Rumberger, 1983; Sewell, Haller, and Ohlendorf, 1970). Lower levels of academic difficulties such as grade retention and special education placement are also associated with higher levels of maternal education (Byrd and Weitzman, 1994; Holloman, Dobbins, and Scott, 1998).

Research on the association between maternal education and children's behavior is less common, and the evidence from accumulated studies does not point to a consistent association between children's behavior and their parents' education (Bradley and Corwyn, 2002). Nevertheless, when associations are found, higher levels of education predict fewer behavioral problems in children (Dearing, McCartney and Taylor, 2001; Velez, Johnson, and Cohen, 1993).

Maternal education may indirectly benefit children because more highly educated parents earn more than less educated parents, more educated mothers are likely to marry highly educated fathers, and household income may have a positive effect on children (Duncan and Brooks-Gunn, 1997). Although the indirect pathways may contribute to a substantial portion of the overall effect of maternal education on children, the direct effects may be equally important. Both the time mothers spend pursuing education and the skills they learn in school may directly affect children in several ways. Parents serve as role models for their children and, therefore, mothers who spend time in education or attain a higher level of education may entice their children to do the same (Cohen, 1989). In addition, the skills that a mother gains in a classroom may generalize to other areas of her life, including perhaps most important for children, the home.

What types of skills learned in the classroom are likely to translate into the parenting arena for the benefit of children? First, mothers with greater educational attainment generally have parenting and teaching styles that promote children's positive development (Bee et al., 1969; Fox, Platz, and Bentley, 1995; Harris, Terrel, and Allen, 1999). Second, mothers with higher levels of education typically engage their children in higher quality verbal interactions, perhaps as a result of their experiences learning through verbal interactions between experts and pupils (Richman, Miller, and Levine, 1992; Uribe, Levine, and Levine, 1993). Third, mothers with higher levels of education on average provide cognitively stimulating learning environment and literacy activities in the home (Davis-Kean and Eccles, 2003; Kohl, Legua, and McMahan, 2000). Finally, mothers who spend more time in school may be comfortable interacting with teachers and educational institutions, which enables them to be effectively involved in their children's schooling (Stevenson and Baker, 1987).

Despite the consistent evidence of a strong correlation between maternal education and children's well-being across a variety of samples and ages, and several theoretical models that explain these associations, most of this research cannot rule out alternative explanations for the presumed effect, a necessary conditions to establish a causal effect (Shadish, Cook, and Campbell, 2002; Sobel, 1998). This is problematic, because it is quite likely that a characteristic associated with both mothers' schooling and children's academic achievement may explain the association (Sobel, 1998). Research on women's educational attainment suggests that mothers' family background characteristics, such as family structure, and individual level characteristics, such as intellectual ability, predict women's educational attainment and schooling (Astone et al., 2000; Mare, 1980; Rumberger, 1983; Rumberger et al., 1990).

One way to reduce omitted variable biases is to measure the typically unmeasured characteristics that are likely to be related both to mothers' educational attainment and their children's academic ability and include them in the estimation model (Duncan, Magnuson, and Ludwig, 1999; Sobel, 1998). In an effort to do this, several studies have included measures of maternal intelligence and other family characteristics as covariates in their estimation models. In general, these studies find a smaller association between maternal education and children's academic outcomes than studies that do not control for maternal intelligence (Rosenzweig and Wolpin, 1994; Yeates et al., 1983; for an exception see Bee et al., 1982). Similarly, analyses of children's behavioral, but not academic, outcomes find little effect of maternal education when family economic characteristics are included as controls (e.g., Yeung, Linver, and Brooks-Gunn, 2000).

Experimental design studies that manipulate maternal education provide one of the best ways to obtain unbiased estimates of the effects of maternal education on children's outcomes (Shadish et al., 2002). However, to date, experimental educational programs have succeeded in producing only very modest increases in maternal education (McGroder et al., 2000; Quint, Bos, and Polit, 1997).[1] The evaluation studies have not found significant differences between the children of experimental and control group mothers on academic and behavioral outcomes, but program effects on children were unlikely in the absence of substantial gains in maternal education.

Several studies have tried to account for the confounded correlations between parental genes, parental educational attainment, children's genes, and children's academic outcomes by using behavioral genetic research designs, but they have come to opposing conclusions. Using a sample of adopted adolescents and matched biological children in two-parent families, Neiss and Rowe (2001) estimated the proportion of the association between parental education and children's verbal IQ that was attributable to genetic influences and parental education. They found that maternal education was significantly, but modestly, associated (.16) with adolescents' verbal IQ.

Behrman and Rosenzweig (2002) differentiated between mothers' genetic endowments and their educational attainment by comparing the children of monozygotic twin mothers. Using a sample of adult

---

[1]Other experimental designs have combined parent and child educational programming (e.g., Even Start; Gamse, Conger, Elson, and McCarthy, 1997).

children born to twin mothers between 1936 and 1955, the authors found that paternal, but not maternal, schooling had a beneficial effect on children's educational attainment. More recent research employing adoption as a natural experiment to untangle the relative contribution of genes and environment has come to the same conclusion. Maternal education has an association with educational attainment, but its influence is channeled through paternal education (Plug, 2002).

Two noteworthy fixed-effects studies, both using data from the National Longitudinal Study of Youth Child Supplement, took advantage of the fact that young mothers often acquire more formal schooling after the birth of a child, or between the births of first and subsequent children. Kaestner and Corman (1995) found no effect of increases in maternal education on changes in children's academic achievement test scores over a 2- and 4-year period. Rosenzweig and Wolpin (1994) associated increases in mothers' educational attainment to differences in siblings' scores on achievement tests. In contrast to Kaestner and Corman (1995), they found that an additional year of maternal educational attainment before a child's birth had a modestly positive effect on children's academic achievement. The difference in the timing of the increases in education may account for the discrepancy in the results from these studies. In Rosenzweig and Wolpin's study, mothers obtained additional education prior to the child's birth, whereas in Kaestner and Corman's study, the education was completed after the child's birth. If the timing of education matters, perhaps because increases in education after birth necessitate time away from the child and place children in lower quality child care, then this may explain the discrepancy in findings.

A large amount of research has linked maternal education to children's cognitive development and school outcomes, and a multitude of theories suggest pathways by which maternal education directly affects children's school outcomes. Less research has established links between maternal education and children's behavior. However, the bulk of this research has failed to provide a basis for causal inference, because it has been unable to rule out plausible alternative explanations for the maternal education-child development correlation by accounting for systematic differences between mothers who obtain higher levels of education and those who do not. The studies that have attempted to use methods to correct for omitted variable biases have led to contradictory conclusions. Some studies have found a positive but small association, whereas others have found no association. In sum, the spare and inconsistent nature of the evidence makes it impossible to reach conclusions about the role of maternal education per se in promoting children's academic achievement and positive behavior.

This study considers the effects of increases in welfare mothers' education on their young children's behavior and academic outcomes. This is the first study to rigorously consider the effects of maternal education across academic and behavioral domains among a large sample of educationally and economically disadvantaged families in the United States. Unlike much of the previous research, this study employs a research design that is uniquely suited to establishing causal associations by ruling out other potential explanations. By taking advantage of the experimentally induced increases in maternal education to estimate IV models of the effect of mothers' education, the threat of bias resulting from omitted mother and child characteristics is eliminated.

DATA AND SAMPLE

The data come from the National Evaluation of Welfare-to-Work Strategies Child Outcomes Study (NEWWS COS). NEWWS was an experimental evaluation of the Job Opportunities and Basic Skills Training (JOBS) program conducted by the MDRC under contract to the U.S. Department of Health and Human Services. Established by the Family Support Act of 1988, the JOBS program was intended to move welfare recipients toward economic self-sufficiency by requiring participation in work and training activities. NEWWS was undertaken to determine if the JOBS program affected clients' education, employment, and income. The NEWWS COS was embedded within three of the NEWWS sites and was designed to determine if the JOBS program affected noneconomic aspects of family and child well-being. Child Trends, under subcontract to MDRC, conducted the NEWWS COS.

Approximately 5,900 families in the Atlanta, GA, Grand Rapids, MI, and Riverside, CA, NEWWS sites were eligible to be included in the Child Outcomes Study, in that they had at least one child between 3 and 5 years old at study entry. This child was designated the "focal child," or the child for whom detailed developmental data were subsequently collected. Between 1991 and 1994, approximately 3,700 families were randomly selected to be enrolled in the NEWWS COS. Once enrolled, mothers were randomly assigned to one of two JOBS program streams or to a control group. In each site, one JOBS program emphasized human capital development (HCD) and another focused on labor force attachment (LFA).

In Riverside, pre-existing state regulations required that only welfare recipients determined to be in need of basic education could be assigned to the HCD program. Mothers were considered "in need" of education if they were not proficient in English, did not have a high school diploma or GED, or if they scored below a given cutoff on a math or literacy assessment (Hamilton et al., 1997). Mothers in need of basic education were randomly assigned to one of the two treatment streams or to the control group. Mothers not in need of basic skills training were assigned to either the LFA program or to the control group. This resulted in some noncomparability of HCD treatment groups across the three sites.[2] Mothers in Riverside's HCD program were relatively less educated than the mothers in Atlanta and Grand Rapids HCD programs.

Mothers assigned to the JOBS program streams were mandated to participate in work-related activities or educational programs. Program mothers received enhanced case management to direct and monitor their progress through work-preparation activities. These mothers were exempt from the mandated JOBS program (HCD or LFA) activities only if they left welfare or were employed for at least 30 hours a week. If they were not exempt, and did not satisfy the participation mandates, program mothers were sanctioned for noncompliance. The sanction resulted in the reduction of a client's welfare

[2]This was handled by controlling for Riverside participants' in-need status in all analyses, and using sample weights to correct for the differential probability of HCD group members being selected into the HCD treatment stream in Riverside.

benefit in the amount of the client's portion of the welfare grant, but left the children's portion intact.[3] Sanctions continued until clients complied with the participation mandate.

Although the HCD and LFA program shared the goal of reducing welfare dependency, they differed in the underlying philosophy of how this was best achieved, and, consequently, in the type and sequence of activities offered. HCD programs emphasized an investment in welfare recipients' skills thought important for obtaining higher-paying jobs. Consequently, the HCD program mandated educational activities such as high school, GED preparation, Adult Basic Education (ABE), English as a Second Language (ESL), vocational training, or college.[4] Mothers continued with their education until they demonstrated basic job skill competencies, at which point they were expected to find employment. In contrast, the LFA program sought to move welfare recipients into the work force as rapidly as possible. In this work-first approach, mothers were mandated to participate in work or work-related activities, and only if they were unable to find employment after participating in job search and job clubs were they assigned to either vocational skills training or other short-term educational activities.

It is important to note that over the course of the study the two streams of the JOBS program became similar. In particular, as it progressed, the HCD program became more employment focused, with mothers who received welfare being exposed to employment-oriented messages and services (e.g., job search) (Hamilton et al., 2001). This was consistent with the HCD program goals, in that educational activities during the beginning of the program were considered investments that would lead to employment in subsequent years.

Control group members were free to seek out any educational or employment services they wished, but were given neither mandates nor incentives to do so with a few exceptions. By the very end of the study, welfare reform legislation had been passed and in Atlanta and Grand Rapids this resulted in

---

[3]For a three-person family this amounted to a $45 decrease in monthly grant of $280 in Atlanta, an $88 decrease in a monthly grant of $474 in Grand Rapids, and a $120 decrease in a monthly grant of $624 in Riverside.

[4]For the most part, the JOBS program did not develop new educational programs; rather participants attended already existing programs in their communities.

a very small proportion of the control groups being exposed to work oriented mandates and services, although more controls may have been exposed to employment-oriented messages and rhetoric (Hamilton et al., 2001). Both experimental and control group members were offered a package of welfare-to-work transitional benefits that included child care subsidies for educational or work-related activities and Medicaid benefits.

Over the course of the evaluation, data on clients and their families were collected from several sources. Prior to random assignment, welfare staff collected information on Standard Client Characteristics (SCC) forms for all participants. Information was collected on mothers' prior welfare receipt, past educational attainment, current educational activities, employment history, and demographic characteristics. Mothers were also asked to fill out a Private Opinion Survey (POS). This survey asked respondents about their attitudes toward work and welfare, barriers to employment, and mental health. Finally, at baseline, mothers completed direct assessments of math and reading skills.

Approximately 2 and 5 years after random assignment, trained interviewers administered follow-up surveys to all control and program group members in their homes.[5] Follow-up survey data were collected for 3,194 (87 percent) of enrolled mothers and their children during the 2-year survey. Response rates for the 5-year survey were lower, just over 70 percent in Atlanta and Grand Rapids, but lower in Riverside (Hamilton et al., 2001). To maximize the sample in analyses, we use all available cases at each time point. In the case of 2-year data, the sample consists of 3,108 of mothers and their children, and for the 5-year data consists of 2,332 mothers and children.

During the 5-year follow up survey, mothers were asked if they would allow the study to contact the focal child's school to conduct a teacher survey by mail. In general, response rates were much lower for the teacher sample, and again, rates were higher in Atlanta and Grand Rapids (over 50 percent) than in Riverside (about 40 percent).

---

[5]All interviewers were female, and an effort was made to match the race/ethnicity of the interviewer to the race/ethnicity of the respondent. Bilingual interviewers administered the survey if clients preferred to conduct the survey in Spanish.

The 2-year and 5-year follow-up surveys collected detailed information from program participants about their educational, employment, and job training experiences after random assignment. Mothers answered questions about their family life as well as the focal child's development, behavior, school experiences, and overall health. During the 2-year follow-up survey, the interviewers also assessed the focal child's academic school readiness and during the 5-year follow-up survey they assessed the child's academic achievement. The 5-year teacher survey collected information about children's academic achievement and classroom behavior.

Administrative data on employment, earnings, and welfare receipt were collected for all participants for the year prior to and the 5 years after random assignment. Information on mothers' AFDC benefits was obtained from state and county AFDC records. In Atlanta and Grand Rapids, AFDC records were not available for participants who moved out of the state. In Riverside, records were not available for participants who moved outside of the county. Data on clients' earnings were obtained from state Unemployment Insurance (UI) administrative records. UI records were not available for participants who moved or worked outside of their state. Finally, UI records only reflected earnings from formal employment, not earnings from "off-the-books" employment.

Table 1 presents the characteristics of this ethnically diverse and economically disadvantaged study sample at baseline. Over 60 percent of participants in each site had been on welfare for 2 or more years, and at least a third of participants in each site did not have a high school diploma or a GED. Atlanta had primarily black clients, whereas Grand Rapids had primarily white clients. Riverside had a substantial proportion of Hispanic, black, and white clients as well as the highest proportion of mothers without a high school diploma or GED. Finally, Grand Rapids had the highest proportion of mothers who were attending educational programs at the time of random assignment.

TABLE 1
NEWWS COS Sample Characteristics at Baseline

Baseline Characteristics	Atlanta	Riverside	Grand Rapids
Demographic Information			
Currently married	1%	2%	2%
Never married	72%	44%	59%
Age	29.0	29.2	26.7
Number of children	2.3	2.2	2.1
Black	95%	18%	39%
Hispanic	1%	35%	6%
Earnings in prior year	$995	$1,404	$1,964
Number of months receiving AFDC in prior year	10	7	8
Average monthly AFDC payment in prior year	$262	$501	$349
Focal child age	4.4	4.2	4.3
Educational Attainment			
No educational degree	37%	55%	39%
Highest degree - GED	5%	7%	8%
Highest degree - HS diploma	51%	35%	49%
Highest degree - vocational or 2-yr degree	6%	3%	4%
Current Educational Activity			
Currently enrolled in GED class	1%	1%	5%
Currently enrolled in ABE class	1%	1%	3%
Currently enrolled in vocational training	6%	7%	10%
Currently enrolled in college	2%	6%	12%
Sample Size	*1422*	*950*	*646*

MEASURES

Variables are organized into four sets of measures: baseline characteristics, JOBS program mediators (i.e., pathways through which JOBS may have affected children and families), 2-year child outcomes, and the 5-year child outcomes. Means of all key explanatory variables and child outcomes are presented in Table 2. Except when noted, all measures were taken from the data set created by MDRC and Child Trends for the NEWWS COS study.

Baseline Characteristics

To control for differences in clients' characteristics prior to random assignment, analyses include a set of baseline variables as covariates.[6] Because some baseline measures were missing values, missing data dummy variables were constructed (1=missing; 0=not missing), and the missing values were replaced with zeros. These missing data variables allow cases to be included in the study despite missing values on baseline characteristics. Appendix Table A1 presents the means for the full set of control variables, including the missing data dummy variables.

Baseline covariates include dummy variables denoting the following standard client characteristics at baseline: being currently enrolled in GED preparation, ABE, vocational training, or college courses; having a GED or high school diploma; having an educational degree higher than a GED or high school diploma; being black; being Hispanic; and having ever been married or living with a spouse. Continuous measures of the mother's age, number of her children, and ages of her youngest child as well as composite measures of the mother's depressive symptoms, locus of control, sources of social

[6]Although random assignment should result in comparable treatment and control groups, baseline covariates were included in the estimation models to enhance the comparability of the program and control groups as well as to increase precision of the models' estimates. McGroder et al. (2000) report that differences between control and experimental groups on baseline characteristics were not systematic, and occurred less often then would be expected by chance. Results from analyses were not sensitive to the inclusion of these baseline covariates.

TABLE 2
Means and Standard Deviations of 2-Year Education, Employment, Earnings, School Readiness, Academic Problem Measures, Behavior, and Cognitive Stimulation

	Atlanta		Riverside		Grand Rapids	
Variables Measured at 24 months	Mean	SD	Mean	SD	Mean	SD
Independent Variables						
Total months in ABE	.93	3.38	.76	2.50	.90	3.20
Total months in vocational training	1.00	3.25	.38	1.60	.68	2.35
Total months in all education activities	2.35	5.27	2.70	5.23	3.97	6.03
Quarters of employment	3.07	2.88	2.00	2.69	3.47	2.65
Total 2-year earnings	$ 5,523	$ 7,821	$ 3,824	$ 8,279	$ 6,119	$ 9,235
Two-Year Dependent Variables						
Bracken raw score	48	11	45	13	46	12
Academic problem - focal child	.07	.28	.09	.33	.20	.45
Positive behavior	10.54	2.96	11.31	2.62	11.29	2.50
Problem behavior	2.13	1.87	1.97	1.71	2.35	1.89
Cognitive stimulation	2.34	.78	2.28	.75	2.34	.67
Sample Size	*1422*		*950*		*646*	
Five-Year Dependent Variables						
Woodcock Johnson-Reading	94.79	17.09	94.92	17.10	99.05	17.61
Woodcock Johnson-Math	99.88	19.07	97.82	18.16	99.15	18.29
Academic problems - focal child	.19	.45	.19	.45	.36	.56
In remedial reading group	.42	.49	.48	.50	.48	.50
In math remedial group	.36	.48	.38	.49	.43	.50
Sample Size	*921*		*661*		*581*	

Note: Sample sizes vary due to missing data.

support, family barriers to employment, and cumulative number of baseline risk factors were also included in analyses.

Covariates included two measures of welfare receipt from the administrative data—the number of months that the mother had received welfare and the average amount of welfare payment in the year prior to random assignment. Taken from UI data, a measure of mothers' total earnings in the year prior to random assignment was also included as a covariate.

Baseline measures of mothers' basic literacy and numeracy skills were included in analyses as covariates. In all three sites, the same math test, developed by the Comprehensive Adult Student Assessment System (CASAS), was used to determine clients' basic math skill levels. A Spanish version of the math test was available. The CASAS test measures math skills that are likely to be used in the work place.

In Atlanta and Grand Rapids, the Test of Applied Literary Skills (TALS) was administered to assess mothers' literacy. However, in Riverside the state-mandated Greater Avenue to Independence (GAIN) literacy test was used to assess literacy. To facilitate across-site comparisons, MDRC converted the GAIN reading scores to make them comparable to the TALS scores.[7] The TALS questions were designed to measure skills in understanding and using print material in everyday life.

Finally, all of the analyses included a set of dummy covariates for the focal child's gender and age at the time of the child assessment. The gender dummy variable had a value of 1 if the focal child was a boy and 0 if the child was a girl. For the 2-year estimation models, eight dummy variables, each of which captures a 3-month interval, control for the child's age. For analyses with the 5-year outcomes, a set of 12 dummy variables, capturing 3-month intervals, are included as covariates (for details see Appendix Table A1).

---

[7]This conversion of scores is explained in Haney et al. (1996).

Education and Employment

Table 2 presents the descriptive statistics, by site, for the measures of educational activity, employment, and earnings between baseline and the 24-month survey.[8] Measures of education and employment over the first 24 months of mothers' participation in the program are the main independent variables of interest in analyses of the two- and 5-year child outcomes.

Measures of the number of months mothers participated in each of five educational activities (high school, ABE, vocational training, ESL, and college) over the first 2 years of the program were constructed. In the 24-month survey, mothers were asked to provide the month and year that they started and stopped attending a particular type of educational program over the 2-year program period. If a mother had reported starting and stopping an activity in the same month, a value indicating that she had spent 2 weeks in this activity was assigned. Because some mothers attended more than one type of educational program, a measure of the total number of months that respondents had participated in all of the five types of educational activities was created. The resulting values of these measures range from 0 to 27 months. On average mothers spent less than 1 month in any given type of educational program, but between 2 and 3 months in all educational programs (Table 2).

During the 5-year survey mothers were asked if they had participated in four types of educational programs after baseline (ESL, ABE, vocational training, or college). From these data, a dichotomous measure of whether mothers had ever participated in educational programs during the 5 years of the study was created.

Data on the number of quarters that a client was employed during the 2-year program period were constructed from UI data. A mother was considered employed during a quarter if UI records showed any earnings during the quarter. The number of quarters that clients were employed range from 0 to 8, and the average number of quarters employed ranged from 2.0 to 3.5 across sites (Table 2).

---

[8]Appendix Table A2 presents the means and standard deviations for the education and employment variables for subsample of respondents who completed the 5-year survey.

2-year Child Outcome Measures

The 2-year child outcome measures include measures of the focal child's school readiness, academic problems, and problem and positive behavior, as well as a measure of the cognitive stimulation in the child's home environment.

Administered approximately 24-months after random assignment, the Bracken Basic Concept Scale/School Readiness Composite (BBCS/SRC) directly assessed the focal child's academic school readiness. The SRC consists of five of the 11 subtests included in the full BBCS. The SRC consists of 61 questions that ask children about their knowledge of colors, letters, numbers/counting, comparisons, and shapes. In the NEWWS COS study, the BBCS/SRC has high internal reliability consistency (Cronbach's Alpha=.97). In addition, prior research has demonstrated the reliability and validity of the instrument with similarly disadvantaged children (Polit, 1996).

For purposes of interpretation, raw scores were used in the analyses, and a set of dummy variables was included to control for the child's age at the time of assessment. The Bracken (BBCS/SRC) raw scores ranged from 0 to 61, indicating the number of concepts correctly identified. The average scores range from 45 to 48 across sites.[9]

Measures of academic problems for the focal child were created from survey data. Mothers were asked if any of their children had received any special help in school for a learning problem, and if any of their children had repeated a grade since random assignment. A response indicating that the focal child had experienced either of these academic problems was given a value of 1, and both of these problems a value of 2. The resulting scores ranged from 0 to 2; the mean for this variable varied from .07 to .20 across sites.[10]

---

[9]Analyses were also conducted with the standardized measure of the BBCS-SRC and results did not differ from those reported for the BBSC-SRC raw score.

[10]Using a composite measure of academic problems that ranges from 0–1 does not change reported results.

The measure of school readiness is correlated very modestly with the measures of academic problems (r = -.03, p<.10). As expected, having academic problems was associated with lower school readiness.

The measure of children's externalizing behavior captures whether the focal child exhibited problem behaviors such as lying, bullying, and cheating. Mothers were asked to indicate how true statements were of their child's behavior. Responses varied from not true (0) to often true (2). Responses to five items were summed, and the resulting measure ranges from 0 to 10, with a mean across sites of 1.97 to 2.3. The reliability of the measure is adequate (Cronbach's Alpha=.61).

Mothers reported on the focal child's positive social behavior, including how well the child got along with others and how helpful and cooperative the child was. Again, mothers were asked to indicate whether statements about the child were not true (0), sometimes true (1), or often true (2). The seven items were summed to create a score that ranged from 0 to 14, with means across sites ranging from 10.5 to 11.3. The scale was highly reliable (Cronbach's Alpha=.77). As expected, problem behavior is negatively correlated with positive behavior, but only moderately (r=-.19, p<.01).

Cognitive stimulation in the home was measured by mothers' reports of how often they engaged in nine stimulating activities with the focal child such as playing guessing games, playing puzzles, or going to the library. Responses ranged from never (0) to almost everyday (5). Scores were averaged across the nine items to create a score that ranged from 0 to 5. The average score ranged from 2.28 to 2.34 across sites. The scale is adequately reliable (Cronbach's Alpha=.68).

<u>5-year Child Outcome Measures</u>

Child outcomes from the 5-year data include assessments of children's academic achievement and survey measures of children's academic problems and school engagement.

Interviewers directly assessed the focal child's academic achievement with the Woodcock Johnson Tests of Achievement-Revised. Passage comprehension and letter-word identification subtests combined to create a Broad Reading score, and calculation and applied problem subtests combined to a

Broad Math score. Both composite scores were age-standardized according to national norms, with mean of 100 and a standard deviation of 15. Broad Reading scores were lower in Atlanta and Riverside (about 95) than Grand Rapids (99), whereas Broad Math scores were more comparable across all three sites (ranging from 97 to 100).

During the 5-year follow-up survey, mothers were asked if the focal child had ever been held back, and had ever received special educational services. A response of yes to either question was given a value of 1. To create a measure of academic problems comparable to the one created with 2-year data, the two questions were then summed. Values ranged from 0 to 2, and means ranged from .38 to .39 across sites.

Three measures from the teacher survey were used as dependent variables in analyses—whether the child was in a remedial reading group or in a remedial math group, and the child's school engagement. Teachers reported the focal child's ability level separately for reading and math, and two dichotomous measures were created to indicate whether the student was in a remedial reading group and remedial math group. Rates were higher for remedial reading, .42 to .48 across sites, than math, .38 to .43 (Table 2).[11]

Teachers reported on the focal child's school engagement by rating how true statements were about the student's behavior in the classroom. These statements included, "This student comes to class unprepared" and "In this class my student seems a million miles away." Responses to 11 items were coded from 0 (very true) to 3 (not true at all) and then summed to create a scale that ranged from 0 to 33, with higher scores reflecting higher levels of engagement. The scale has high internal consistency (Cronbach's Alpha=.92) and the mean across sites ranged from 18.77 to 19.68.

The child-report composite of school engagement included questions similar in content to those included in the teacher measure of the child's school engagement. Children were asked whether seven statements were true of their behavior, including "When I am in class, I try very hard" and "When I am in

---

[11]Teachers also reported information about whether the child was below grade level for reading and math. Results from analyses with these measures parallel those reported for children's placement in remedial reading and math groups.

school, I pay attention in class." Responses were coded from 0 (never true) to 3 (very often true) and summed across the items. Again, higher scores reflect higher levels of school engagement. Resulting scores ranged from 0 to 21, with means across sites ranging from 18.77 to 19.68. The measure had adequate internal consistency (Cronbach's Alpha=.75).

METHODOLOGY

The analyses proceed in three parts. First, experimental patterns of educational activities during the first 3 years of the program are explored to determine whether the HCD and LFA program increased mothers' education. Participation rates, and for those who attended programs, the average length of participation, are provided separately for several types of educational programs, as well as for all types of educational programs. Second, OLS and IV analyses of the effects of maternal education participation on children's outcomes are conducted with the 2-year survey data. In addition, some exploratory subgroup analyses and robustness checks are conducted with the 2-year data.

Finally, after a brief consideration of experimental patterns of educational activities during the final 3 years of the program, OLS and IV estimates of the effects of maternal educational participation in the first 2 years of the program on children's outcomes measured during the 5-year survey are conducted. Because patterns of educational participation differed in the last 3 years of the program, these results reflect whether obtaining education when children are younger is more beneficial than obtaining when they are older. The explanation for this interpretation is discussed further in the next two sections of the paper.

Estimation Models

A conventional OLS model of the association between maternal educational activity and child outcomes is presented below:

(1) Child outcome$_i$= λ_0 + λ_1(Mother's number of months in educational activity$_i$) + λ_2(Employment$_i$) + λ_3(Baseline maternal characteristics$_i$) + λ_4(Baseline child characteristics$_i$) + ξ_{i1}

The coefficient of interest is λ_1, which represents the change in a child outcome associated with an additional month of a mother's participation in educational activities during the first 2 years of the program. The OLS model provides unbiased estimates of coefficients under the assumption that the error term (ξ_{i1}) is not correlated with the independent variables. Omitting a variable that is correlated with both the dependent and an independent variable causes a spurious correlation between the error term and the independent variable, and this may bias the coefficient.

Researchers typically worry that nonexperimental data will impart an upward bias to estimates of the effect of maternal education on children's outcomes (Resnick, Corley, and Robinson, 1997). This is because more highly educated mothers are likely to be advantaged in other ways that may positively affect their children's development, but that are not typically included in OLS models. For example, most studies of the effect of maternal education lack a good measure of the mother's genetically endowed cognitive abilities. By omitting this variable, researchers may mistakenly attribute positive child outcomes to maternal schooling rather than mothers' personal endowments, thereby overestimating the effect of maternal schooling on children's school outcomes (Behrman and Rosenzweig, 2002).

Even in the context of an experiment, OLS models do not provide unbiased estimates of the effect of maternal education on children's school outcomes. Although experimental status is unrelated to children's academic school readiness at the time of random assignment, whether mothers complied with the participation mandate was not randomly determined. Consequently, OLS models that do not account for all of the measured and unmeasured differences between mothers who do and do not participate in the JOBS activities may provide biased estimates of the effect of maternal schooling on children's school outcomes.

It is important to note that, contrary to the usual case, OLS estimates of the effects of maternal education on children's school-related outcomes are expected to be biased *downward*. By program design, mothers in the HCD program were directed to educational activities and generally continued in these activities until their skills improved (Bos et al., 2002; Hamilton et al., 1997). Therefore mothers spent more time in educational activities if it took them longer to acquire the necessary skills. Three reasons might explain why some mothers took longer to acquire the necessary skills than other mothers. They might have had lower levels of skills to begin with, been slow learners, or been unmotivated to learn. All of these characteristics were likely to have been negatively associated with children's outcomes and positively associated with children's academic problems. Although data included a baseline measure of numeracy and literacy skills, available data did not include a measure of how motivated mothers were to improve their skills or whether they were relatively fast or slow learners. Therefore, I expected that omitting these variables would cause OLS models to underestimate the effects of maternal schooling.

An IV approach provides a way to estimate the model without omitted variable bias (Foster and McLanahan, 1996). IV estimation amounts to estimating a two-equation system:

(2) $$\text{Mother's number of months in educational activity}_i = \beta_0 + \beta_1(\text{Experimental status}_i) + \beta_2(\text{Baseline maternal characteristics}_i) + \beta_3(\text{Baseline child characteristics}_i) + \xi_{i2}$$

(3) $$\text{Child's outcome}_i = P_0 + P_1(\text{Predicted mother's number of months in educational activity}_i) + P_2(\text{Employment}_i) + P_3(\text{Baseline maternal characteristics}_i) + P_4(\text{Baseline child characteristics}_i) + \xi_{i3}$$

In the first stage of the estimation, the cumulative number months a mother participated in educational activities is the dependent variable and was predicted by the set of baseline covariates plus the experimental status of the mother (HCD, LFA, or control group). Because experimental treatment status is unrelated to mothers' characteristics, by design, at baseline, the measure of predicted months in education is purged of any correlation with unobserved maternal characteristics, and therefore is also purged of any spurious correlation with the error term in Equation 3. In the second stage, P_1 is estimated by replacing the actual number of months in education with the predicted number of months in education obtained in the first stage.[12] Statistical packages automatically make the necessary corrections to the standard errors.

Using both HCD and LFA assignment as instruments provides unbiased and causal estimates under the following five assumptions outlined by Angrist, Imbens, and Rubin (1996). First, the treatment must be randomly assigned. Second, the outcome of a treatment for an individual must be unrelated to the treatment status of other individuals. In the case of JOBS, this means that effect of the HCD program on a client's child was not likely to be affected by the random assignment of other participants to HCD, LFA, and control conditions. Third, the treatments must have a nonzero average effect on the outcome of interest. That is, the JOBS programs must have increased maternal education. Fourth, none of the clients must have done the exact opposite of their assigned treatment, no matter what the assignment. The method assumes that no one who was assigned to the HCD program would have participated in educational programs if assigned to the control group, but would not have participated if assigned to the HCD group. Finally, the only way in which the treatments could have affected the outcomes of interest (children's outcomes) was through their effect on the mediator of interest, in this case, maternal education.

The first four assumptions are easily met given the choice of random assignment variables as instruments. However, if the HCD and LFA programs affected children through pathways other than

---

[12]Simple algebra applied to the case in which there is only one site and one experimental program status shows that the IV estimate is the ratio of the program impacts on child outcomes to program impacts on maternal education. In first stage of the IV analysis, mothers' education is regressed on an experimental status dummy variable to obtain an OLS estimate of $ß_1$, the program's impact on mothers' education. By substituting the predicted value of mother's education in the second stage of the IV equation (Equation 3) I arrived at the following:

child's outcome$_i$ = $\Phi_0{}^* + (\Phi_1{}^* \, ß_1)$experimental status$_{I} + \xi_{I1}{}^*$

Letting $\alpha = (\Phi_1{}^* \, ß_1)$ the equation can be rewritten as:

child's outcome$_i$ = $\Phi_0{}^* + \alpha$ experimental status$_{I} + \xi_{I2}{}^*$

By regressing experimental status on the child's outcome, I obtained an OLS estimate for α, the program's impact on child outcomes. With values for both α and $ß_1$ it is easy to solve for the parameter of interest ($\Phi_1 = \alpha / ß_1$). This solution demonstrates that the IV estimate amounts to the program impact on the child outcome divided by the program impact on mothers' months in education. Appendix Table A3 presents experimental impacts on maternal education and employment as well as the two- and 5-year child outcomes.

mothers' education, Angrist and colleague's fifth assumption, the "exclusion assumption," is violated and the resulting estimate may be biased.[13] Because the JOBS program, including the HCD program, was designed to move welfare recipients into economic self-sufficiency through employment, the program may have affected children by increasing mothers' participation in the labor market.[14] I try to meet the exclusion assumption by predicting employment in the first stage of the IV model, and including it in the second stage. However, if there were still other pathways by which JOBS affected children that were not included in the IV models, the exclusion assumption is not satisfied.[15]

For identification purposes, an IV model needs at least as many instruments as mediators (Davidson and MacKinnon, 1993). That is, if I wish to create predicted values for both education and employment, I need to have a set of at least two instruments. Fortunately, there are two different program streams in three different sites. For IV analyses, I take advantage of the variation in experimental treatments by using each program approach (LFA or HCD) in each site (Atlanta, Grand Rapids, and Riverside) to create a set of six instrumental variables. Site is controlled for in the models and therefore differences across sites (unrelated to program treatment streams) are not used to identify the program mediators.[16] Taken together these variables are exogenous and meet the criteria above for causal inference if they predict the program mediators, but do not otherwise affect children's outcomes.

Weak instruments in IV models are problematic (Bound, Jaeger, and Baker, 1995; Staiger and Stock, 1994). R-squared and F-values from the first stage of two-stage least squares provide an

---

[13]The extent of the bias is determined by the relative association of the omitted pathway with the outcome of interest.

[14]It is possible that JOBS may also have affected children through their experiences in child care, but I do not have data on the mothers' use of child care over the 2 years of the program.

[15]It is important to note that the exclusion assumption pertains only to mediators of program impacts hypothesized to operate independently of maternal schooling; failure to control for other pathways through which the programs had impacts on children (e.g., maternal employment) may lead to biased estimates of the effects of maternal schooling in both the IV and OLS models. However, it is not necessary to include measures of the hypothesized pathways through which maternal education affects children (e.g., child's home learning environment or parenting).

[16]Using just two instruments, HCD and LFA status, does not significantly change the reported findings. Using site as instruments in addition to experimental treatment status also does not change the reported findings.

assessment of the strength of the correlations between the set of instruments and the variables they are to predict. Staiger and Stock (1994) argued that 10 is the minimum acceptable value of the F-value associated with the hypothesis that the coefficients of the instruments in the first-stage regression are jointly equal to zero.

It should be noted that the models do not estimate the effects of a mother's cumulative educational attainment on a child, but rather the effects of her involvement in educational activities over the first 2 years of program. The IV model is based on the premise that the independent variable of interest, maternal education, can be predicted by the treatment stream variables. Across sites and treatment streams, mothers differed more in their educational activity over the 2 years of the program than in their overall educational attainment. Consequently, the instruments are better able to predict the number of months that mothers spent in education over the 2-year follow-up period than mothers' overall educational attainment or activity. In addition, the 5-year survey did not ask in detail about mothers participation in educational programs over the last 3 years of the program, so a measure of the number of months mothers spent in education over the 5 years of the program could not be created.

With the full sample pooled across the three sites, two different OLS and IV models were estimated for each of the 2- and 5-year child outcome variables. In the first model, one program mediator, a measure of mothers' months in educational activity during the first 2 years of the program, was included along with the baseline covariates. In the second model, employment during the first 2 years of the program was introduced as an independent variable.

The interpretation of the estimates of the effect of maternal education on children's 2-year outcomes is straightforward. The coefficients represent the effect of an additional month of maternal schooling during the first 2 years of the program on children's outcomes at 2 years. However, interpreting results from the analyses of the 5-year outcomes is more complicated. These analyses estimate the effect of maternal education over the first 2 years of the program on child outcomes during the fifth year of the study. If the treatment patterns of mothers' educational activity did not change between the 2- and 5-year surveys, then the estimates would capture the persisting effect of mothers' education on children 3 years later. However, if the patterns of education did change, as might be expected from the increasing employment focus of the HCD program, then the estimates have a different interpretation. More specifically, if the HCD's program's treatment effect on mothers' educational activities diminished over time, because mothers in the control of LFA groups obtain additional schooling in the last years of the program whereas control mothers did not, then the OLS and IV analyses do not estimate whether children of mothers' who obtain more education continue to perform better than children whose mothers do not. Rather, in this case, the analyses indicate whether *early* increases in maternal education are more beneficial than *later* increases in maternal education. That is, coefficients represent the estimated effect of an additional month of education when children are young, irrespective of mothers' educational activities during the last 3 years of the study.

RESULTS

Program Participation

The first panel of Table 3 presents the participation rates of the HCD clients in educational activities, by type of education and site. The second panel presents the same information for LFA clients, and the third panel presents this information for the control group. Although participation rates were far from universal, close to half the HCD participants in each site participated in some type of educational activity between random assignment and the 2-year survey. LFA and control group members participated in educational activities as well, but at lower rates than the HCD clients did. By far the most common types of education across all programs were ABE and vocational training, although substantial proportions of clients in all of the education treatment streams as well as in Grand Rapids and Riverside's LFA program (15 percent) reported that they had taken college classes.

Among participating mothers in the HCD program group, the average length of participation in all educational activities is just over 8 months (Table 3). Mothers in Atlanta and Grand Rapids had

TABLE 3
Participation Rates in Educational Activities

	Atlanta	Grand Rapids	Riverside	All Sites
HCD Program Group				
Percent participating in				
High school	0%	7%	5%	3%
Adult basic education	22%	22%	43%	28%
English as a second language	0%	1%	3%	2%
Vocational training	21%	19%	9%	17%
College	4%	19%	9%	9%
All educational activities	43%	59%	55%	51%
Sample Size	520	205	256	981
Average months if participated in				
Adult basic education	7.5	5.8	4.7	6.7
Vocational training	7.6	5	5.4	6.7
All educational activities	8.3	8.2	7.3	8.1
LFA Program Group				
Percent participating in				
High school	0%	2%	2%	1%
Adult basic education	10%	12%	4%	9%
English as a second language	0%	2%	1%	1%
Vocational training	11%	8%	7%	9%
College	2%	21%	15%	11%
All educational activities	23%	40%	25%	28%
Sample Size	411	235	238	884
Average months if participated in				
Adult basic education	6.6	4.7	3.1	5.0
Vocational training	6.8	5.8	3.3	5.9
All educational activities	8.1	7.4	7.8	7.8
Control Program Group				
Percent participating in				
High school	1%	4%	3%	2%
Adult basic education	5%	12%	6%	6%
English as a second language	0%	0%	2%	1%
Vocational training	10%	10%	13%	9%
College	5%	21%	13%	11%
All educational activities	18%	42%	27%	26%
Sample Size	524	232	510	1,266
Average months if participated in				
Adult basic education	5.0	7.1	3.2	5.0
Vocational training	6.4	5.4	4.9	5.6
All educational activities	7.5	8.8	7.6	7.9

slightly longer participation spells than did mothers in Riverside. For mothers who participated in educational activities in the LFA and control groups, the average number of months is slightly lower, just under 8 months. Mothers in Atlanta's LFA program and Grand Rapid's control group participated for longer spells than mothers in the other sites (see Hamilton et al., 1997, for a further discussion of site differences in program participation).

OLS and IV Model Estimation 2-year Outcomes, Full Sample

Table 4 presents results from the first stage of the IV analyses, the coefficients and standard errors for the treatment status instruments predicting education and employment.[17] Overall the set of treatment stream variables performs very well as a set of instruments. Assignment to the HCD treatment in each site and to the LFA treatment in Atlanta predicts educational activity, whereas assignment to LFA treatment in each site and HCD treatment in Riverside predicts employment. The F-values and R-squares are presented in the bottom three rows of the table. In the case of education, the F-value is above the recommended value of 10, and in the case of employment, the F-value is very close to 10 (F-value = 9.63).

Turning to the OLS analyses and the second stage of the IV analyses, findings suggest that increases in maternal education have positive and significant effects on children's school readiness and academic outcomes 2 years after baseline. However, the estimated effects are much larger in IV analyses than in OLS analyses. The first panel of Table 5 presents the results for the OLS and IV estimations of the effect of a month of mothers' educational activities on children's academic school readiness (BBCS/SRC).[18] For example, the first column of the first row shows that, by OLS estimation, an additional month of mothers' education is significantly associated with a .089 higher score on children's

---

[17]The coefficients associated with the program status variables in the first stage of the IV analysis are very similar to program impacts presented in Appendix Table A3. Slight differences are attributable to differences in the value and significance of the coefficients associated with control variables within sites compared to across sites.

[18]Appendix Table A4 shows the coefficients and standard errors for all control variables in the OLS estimation model of Children's Bracken Score.

TABLE 4
First Stage IV Coefficients, F-values, and R-squares for 2-Year Survey Sample
(Standard Errors in Parentheses)

Instruments	Months of Education	Quarters of Employment
Atlanta HCD	2.36 ***	.25
	(.34)	(.17)
Atlanta LFA	.60 *	.43 **
	(.34)	(.17)
Grand Rapids HCD	.96 *	.00
	(.50)	(.25)
Grand Rapids LFA	-.98 *	.96 ***
	(.50)	(.25)
Riverside HCD	2.94 ***	.68 ***
	(.43)	(.21)
Riverside LFA	-.36	1.22 ***
	(.44)	(.22)
F-value for instruments	20.90 ***	9.63 ***
Full model R-square	.17 ***	.21 ***
Increase in R-square associated with instruments	.040 ***	.015 ***

Notes: P-values: *p<.10, ** p<.05, *** p<.01.

Covariates included for educational attainment and participation at baseline, prior earnings, prior welfare reciept, numeracy, literacy, depressive symptoms, mother's and focal child's age, number of baseline risk factors, family barriers to employment, race, marital status, number of children, locus of control, sources of social support, and child gender.

TABLE 5
IV and OLS Estimates of the Effect of Mothers' Months in Educational Activities on Children's 2-Year Survey Outcomes
(Standard Errors in Parentheses)

	Model 1: Bracken School Readiness		Model 2: Bracken School Readiness	
Independent Variables	OLS	IV	OLS	IV
Months in education	.089 ***	.305 *	.098 ***	.311 *
	(.035)	(.168)	(.035)	(.169)
Quarters of employment			.134 *	.671
			(.070)	(.493)

	Model 1: Academic Problems		Model 2: Academic Problems	
Independent Variables	OLS	IV	OLS	IV
Months in education	.000	-.012 **	.000	-.011 *
	(.001)	(.006)	(.001)	(.006)
Quarters of employment			-.001	.009
			(.002)	(.017)

	Model 1: Home Env.- Cognitive Stimulation		Model 2: Home Env.- Cognitive Stimulation	
Independent Variables	OLS	IV	OLS	IV
Months in education	.003	.024 *	.003	.024 *
	(.003)	(.013)	(.003)	(.013)
Quarters of employment			.005	-.011
			(.005)	(.038)

	Model 1: Externalizing Behavior		Model 2: Externalizing Behavior	
Independent Variables	OLS	IV	OLS	IV
Months in education	-.011	-.022	-.013	-.023
	(.007)	(.034)	(.007)	(.034)
Quarters of employment			-.027 *	.066
			(.014)	(.100)

	Model 1: Positive Behavior		Model 2: Positive Behavior	
Independent Variables	OLS	IV	OLS	IV
Months in education	.018 *	.009	.020 **	.008
	(.010)	(.049)	(.010)	(.049)
Quarters of employment			.036 *	.048
			(.021)	(.146)

Notes: P-values: *p<.10, ** p<.05, *** p<.01.

Covariates included for site, ineed-status, educational attainment and participation at baseline, prior earnings, prior welfare reciept, numeracy, literacy, depressive symptoms, mother's and focal child's age, number of baseline risk factors, family barriers to employment, race, marital status, number of children, locus of control, sources of social support, and child gender.

BBCS/SRC scores ($p < .05$). The results in the second column show that, by IV estimation, an additional month of maternal education is significantly associated with a .305 increase in a child's score ($p < .10$). Including the predicted value of quarters of employment in the IV model did not change the estimated effect of maternal schooling on children's school readiness (columns 3 and 4). The resulting effect sizes from both the OLS and IV models for an average HCD participant are displayed in Figure 1. [19]

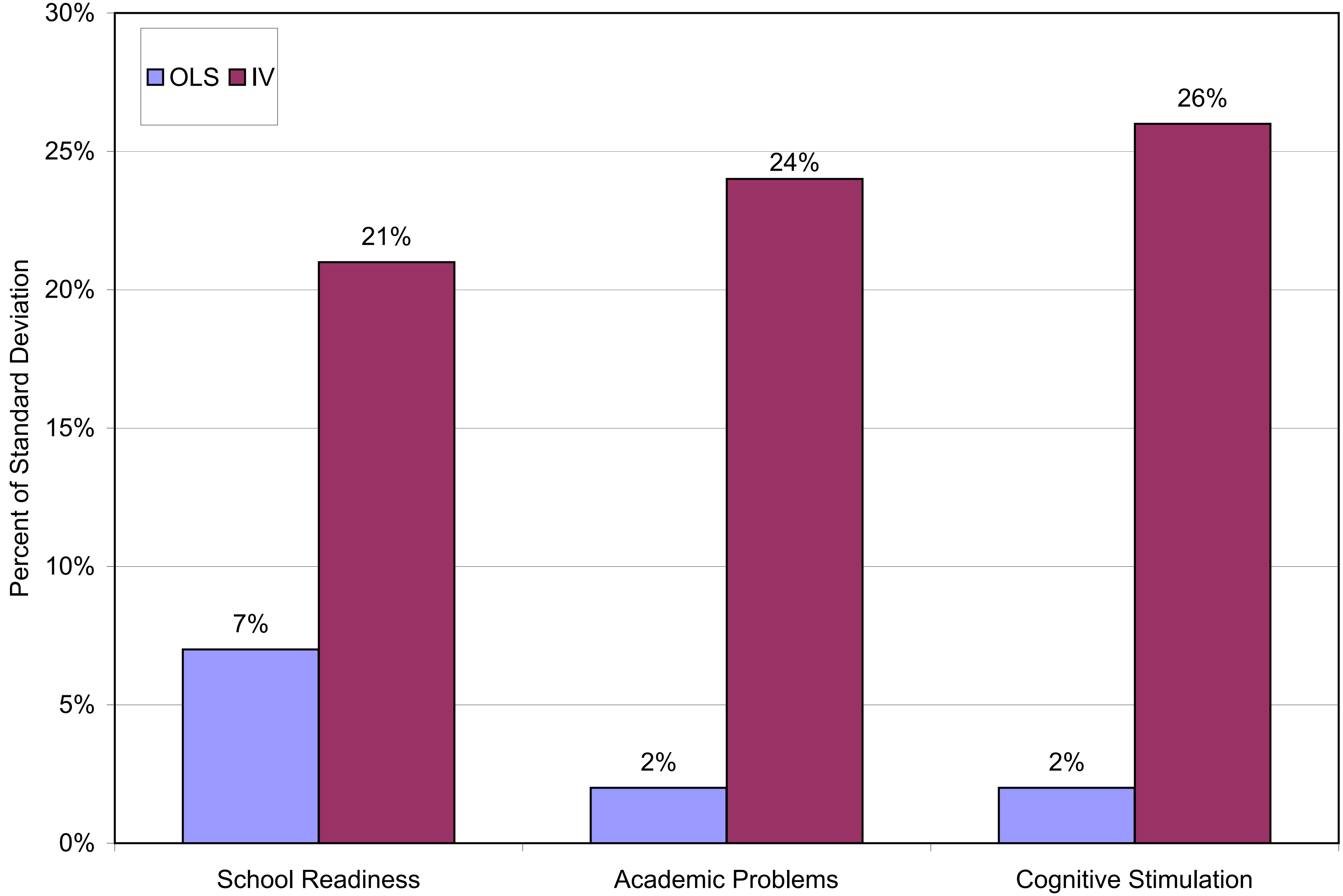
The second panel of Table 5 presents the results of the OLS and IV estimates for the effect of months in educational activity on whether the focal child experienced any academic problems during the 2-year follow-up period. The OLS findings suggest that an additional month of maternal education is not associated with children's academic problems. In contrast, the IV estimate suggests that an additional month of maternal education results in .012 fewer educational problems. Columns 3 and 4 show that, again, introducing the number of quarters of maternal employment as an independent variable, for the most part, does not change the size or significance of the IV and OLS coefficients. The OLS results suggest that education does not affect focal children's academic problems, and the IV coefficients translate into an effect size of .26 for the child of the average participant in HCD educational activities (Figure 1).

The third panel of Table 5 presents the results of the OLS and IV estimates for the effect of cumulative months of educational activity on cognitive stimulation in children's environments. Again, a similar pattern emerges such that the effect of maternal education is small and not significant in the OLS models, but larger and statistically different from zero in the IV models. In Figure 1, the discrepancy in the resulting effect sizes is apparent.

In contrast to the first three panels of Table 5, the fourth and fifth panels suggest that mothers' participation in educational activities is not strongly related to children's behavioral outcomes. In the case

[19]For purposes of interpretation, I translate the OLS and IV coefficients into effect sizes by dividing the estimates by the full sample standard deviation for each outcome measure. To describe the average effect size among those who participated in HCD educational activities, I multiply these monthly effect sizes by 8, the average number of months spent in educational activity among HCD mothers.

FIGURE 1
Estimated Effects of 8 Months of Maternal Education on Children's Outcomes, 24 Months



of externalizing behavior, the OLS and IV estimates are similar, but both are rather small and not statistically different from zero. In the case of positive behavior, the OLS and IV estimates are again small, but in this case the OLS estimate is larger than the IV estimate. In addition, the OLS estimate is significant whereas the IV estimate is not. In sum, the estimated effects of mothers' participation in education activities on children's behavior are small (Figure 2).

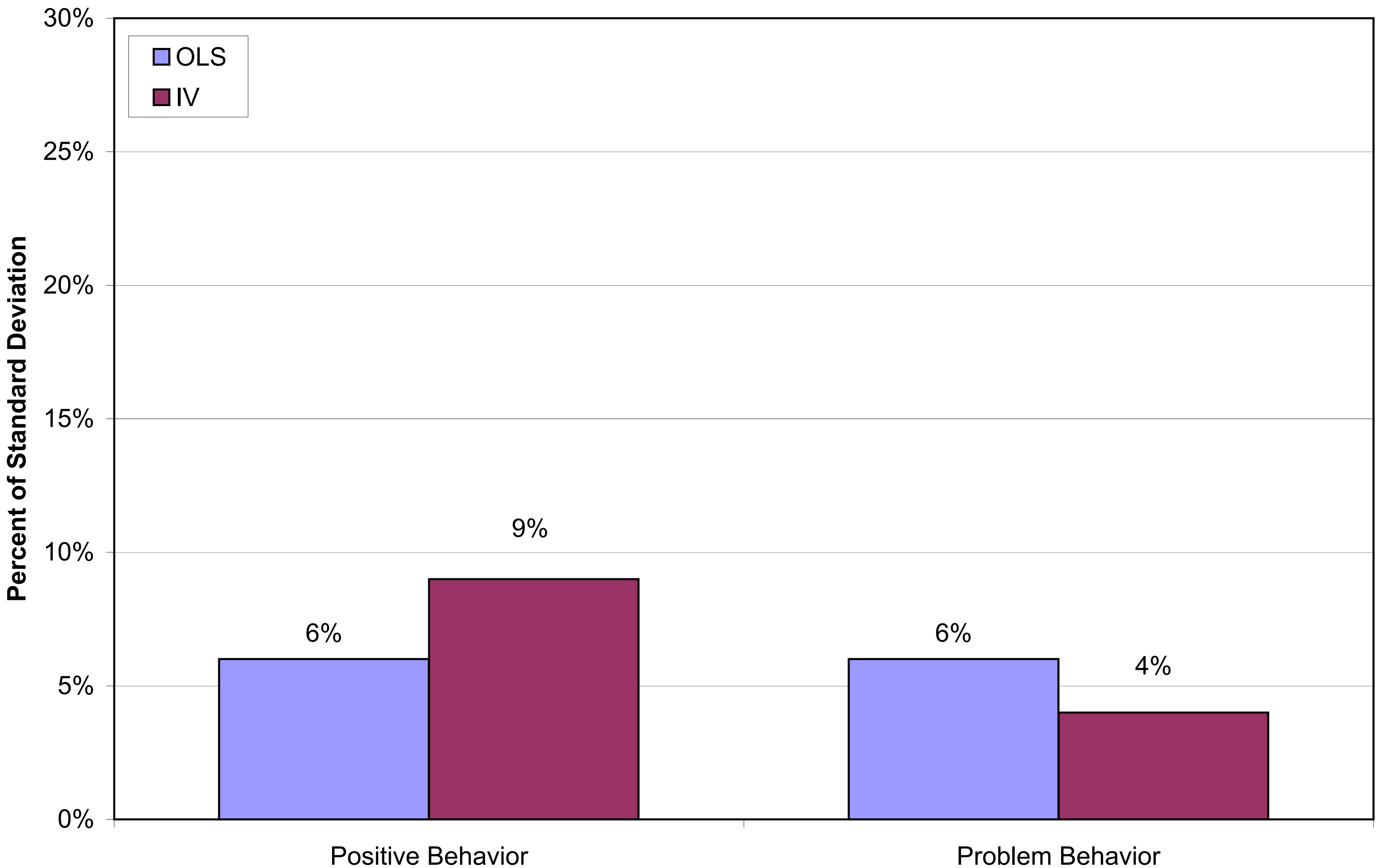
IV Sensitivity Analyses 2-year Outcomes, Full Sample

To test the robustness of the findings, I estimate the effects of maternal education on children's school readiness (BBCS/SRC), controlling for three other potential pathways by which the JOBS program may have affected children. Although the earlier findings suggested that the effect of maternal education on children's academic school readiness was not reduced by controlling for employment, I want to be confident that controlling for changes in mothers' economic resources did not change the association between maternal education and children's outcomes. I include measures of the mother's 2-year total earnings and welfare receipt, as well as whether the mother was sanctioned for noncompliance with the participation mandate.

The point of departure for these analyses was the IV model in which JOBS treatment status, the instruments, predict maternal educational activities in the first stage, and the predicted months of education predict children's Bracken score in the second stage. In subsequent models, the measures of mothers' earnings and AFDC receipt over the 2-year period, and whether the mother had been sanctioned after random assignment, are included in the first and second stages of IV estimation.

Mothers' earnings and AFDC receipt varied less across the six JOBS programs than did employment, and the F-values associated with strength of the instruments in the first stage of the IV estimation are consequently lower, although still significant (4.0 and 7.4 respectively, not shown in tables). In contrast, sanctioning did vary across the programs, and the instruments significantly and strongly predicted whether mothers reported that they had been sanctioned. The F-value associated with the instruments in the first stage estimation of sanctioning is quite high (34, not shown in tables).

FIGURE 2
Estimated Effect of 8 Months of Maternal Education on Children's Behavior, 24 Months



Results from the second stage of the IV analyses with 2-year total earnings and AFDC receipt as independent variables are similar to those presented in Table 5. In the analyses that included whether the mother had been sanctioned, the coefficient associated with months of educational activity doubles from approximately .31 to .62 (not shown in tables). However, the standard error associated with this coefficient increased from .24 to .59, and consequently the coefficient is no longer statistically significant. Looking across these analyses, the coefficients associated with earnings, welfare receipt, and sanctioning is neither statistically significant nor consistent in the magnitude or direction of effects (not shown in tables).

Did the Effect of Maternal Education on School Readiness Differ According to Mothers' Initial Need for Education?

To determine whether the effects of mothers' participation in education on children differed depending on their initial level of basic skills, IV models are estimated for two subgroups of mothers—those who were considered in need of basic education (n=1,398) and those who were considered not in need of basic education (n=1,447). At baseline, mothers were determined to be in need of education if they scored below a cutoff on either the math or reading skills test, if they had limited English proficiency, or if they did not have a high school diploma or GED. IV models of the effects of mothers' months in education on children's school readiness scores are estimated separately for these two groups of mothers. The IV models controlled for the number of quarters that mothers were employed over the 2-year period (model 2, Table 5).

Not surprisingly, the HCD and LFA programs had a larger impact on the educational activities of mothers who were in need of basic education than of those who were not. Among mothers who were in need of education, each of the three HCD programs significantly predicts the number of months that mothers spent in educational activities. In contrast, among mothers who were considered not in need of education, only one JOBS program, Atlanta HCD, predicts the number of months that mothers spent in educational activities. Consequently, instruments are better able to predict months of educational activity and months of employment for those who were in need of educational activities than for those not in need. The strength of the F-values reflects this difference. Among mothers in need of education, the F-value associated with the instruments in the first stage of the IV estimation is 22.59 for months in any educational activity and 7.45 for the number of quarters of employment (not shown in tables). Because mothers in Riverside who were not in need of education were not assigned to the HCD program, assignment to HCD treatment cannot be used as an instrument in the first stage of the IV analyses for this subgroup of mothers. The F-value associated with the remaining five instruments in the first stage of the IV estimation is only 4.39 for months in education and 4.42 for quarters of employment (not shown in tables).

Results from the subgroup analyses, presented in Table 6, suggest that the effects of maternal education on children's school readiness are concentrated among mothers in need of basic education at the time of random assignment. The results suggest that for these mothers a month increase in mothers' education is associated with a .34 increase in children's Bracken score ($p < .10$). For mothers not in need of education, the coefficient associated with the measure of months in education is .159, but the standard error is more than twice as large as the coefficient, and therefore the estimate is not significant.

In contrast, the estimated effects of mothers' education on children's academic problems and cognitive stimulation in the home environment, although not statistically significant, are larger among the more highly educated mothers. For example, an additional month of education is associated with a .023 increase in the stimulation of the home learning environment among the not-in-need subgroup of mothers, but an increase of .017 among those in need of education.[20]

[20]The means for these outcomes differed across the groups. Among those in need of education the mean BBCS/SRC score was .45, for academic problems the mean was .13, and for cognitive stimulation the mean was 2.22. Among those not in need of education the mean for the BBCS/SRC was 2.41, the mean for academic problems was .09 and for cognitive stimulation the mean was 2.41.

TABLE 6

IV and OLS Estimates of the Effect of Mothers' Months in Educational Activities on Children's Raw Bracken School Readiness Composite Scores and Academic Problems, by Educational Need Status (Standard Errors in Parentheses)

	In Need of Education Subgroup					
	Bracken School Readiness		Academic Problems		Home Environment-Cognitive Stimulation	
Dependent Variables	OLS	IV	OLS	IV	OLS	IV
Months In education	.097 ***	.344 *	.000	-.007	.003	.017
	(.035)	(.186)	(.001)	(.006)	(.002)	(.013)
Quarters of employment	.129	.544	.001	-.012	.005	.053
	(.069)	(.672)	(.002)	(.022)	(.005)	(.048)

	Not In Need of Education Subgroup					
	Bracken School Readiness		Academic Problems		Home Environment-Cognitive Stimulation	
Dependent Variables	OLS	IV	OLS	IV	OLS	IV
Months In education	.095 ***	.159	-.005 ***	-.017	.003	.023
	(.035)	(.373)	(.002)	(.014)	(.003)	(.031)
Quarters of employment	.139 ***	1.094	-.036	.034	.006	-.080
	(.069)	(.724)	(.024)	(.027)	(.003)	(.061)

OLS and IV Model Estimation 5-Year Outcomes, Full Sample

Data collected on mothers' educational participation in the 5-year survey was limited to whether mothers had ever participated in particular types of education after random assignment, and the number of months participated in each type of education over the final year of the program. The focus of the HCD programs became more employment-oriented as the study progressed, and this is apparent in the pattern of participation in educational activities at the 5-year survey. Whereas the percentage of mothers participating in education over the first 2 years of the program was twice as high in HCD group as in LFA and control groups (53 percent vs. 27 percent in the control group and 28 percent in the LFA groups, Table 3), by the time of the 5-year survey, the discrepancy in education among groups was much smaller—approximately 42 percent of mothers in the control group and LFA group reported ever having participated in educational programs versus 56 percent in the HCD group (not shown in tables). This suggests that very few mothers in the HCD group entered education in the last 3 years of the program, whereas substantial proportions of mothers in the LFA and control groups did. Given the increases in LFA and control group mothers' educational participation during the last 3 years of the study, using estimates of mothers' education from *only* the first 2 years of the study in analyses yields estimates that reflect whether the early timing of maternal schooling is particularly beneficial to children, in comparison to later maternal schooling. The OLS and IV analyses do not, and the data cannot, model the effects of cumulative educational participation or whether children of mothers' who obtain more education continue to perform better than children whose mothers do not.

Because the sample has changed, Table 7 presents the first-stage results from the IV analyses with the 5-year sample. Although the pattern of associations is nearly identical to that reported for the 2-year sample in Table 4, the sample is smaller so the F-statistics are somewhat lower for the measure of months of maternal education (17.78) and quarters of employment (8.23).

The estimates presented in Table 8 mirror those presented in Table 6. In the case of children's academic achievement as measured by the Woodcock Johnson Achievement Tests-Revised, although the

TABLE 7

First-Stage IV Coefficients, F-values, and R-squares for 5-Year Survey Sample

(Standard Errors in Parentheses)

Instruments	Months of Education	Quarters of Employment
Atlanta HCD	2.58 ***	.23
	(.45)	(.23)
Atlanta LFA	.62	.66 ***
	(.45)	(.23)
Grand Rapids HCD	1.08 ***	.12
	(.50)	(.25)
Grand Rapids LFA	-.95	.88 ***
	(.48)	(.25)
Riverside HCD	3.43 ***	.77 ***
	(.52)	(.27)
Riverside LFA	-.08	1.03 ***
	(.41)	(.21)
F-value for instruments	17.78 ***	8.23 ***
Full model R-square	.22 ***	.20 ***
Increase in R-square associated with instruments	.038 ***	.017 ***

Notes: P-values: *p<.10, ** p<.05, *** p<.01.

Covariates included for educational attainment and participation at baseline, prior earnings, prior welfare reciept, numeracy, literacy, depressive symptoms, mother's and focal child's age, number of baseline risk factors, family barriers to employment, race, marital status, number of children, locus of control, sources of social support, and child gender.

TABLE 8
IV and OLS Estimates of the Effect of Mothers' Months in Educational Activities on Children's Raw Bracken School Readiness Composite Scores and Academic Problems (Standard Errors in Parentheses)

	Model 1: WJ Reading		Model 2: WJ Reading	
Independent Variables	OLS	IV	OLS	IV
Months in education	.077	.169	.094	.181
	(.072)	(.330)	(.073)	(.331)
Quarters of employment			.240	.436
			(.144)	(.890)

	Model 1: WJ Math		Model 2: WJ Math	
Independent Variables	OLS	IV	OLS	IV
Months in education	.039	.104	.066	.137
	(.080)	(.363)	(.080)	(.366)
Quarters of employment			.394 ***	1.180
			(.159)	(.938)

	Model 1: Academic Problem-Focal Child		Model 2: Academic Problem-Focal Child	
Independent Variables	OLS	IV	OLS	IV
Months in education	-.002	-.013	-.002	-.013
	(.002)	(.009)	(.002)	(.009)
Quarters of employment			.001	.003
			(.004)	(.027)

	Model 1: Remedial Group Reading		Model 2: Remedial Group Reading	
Independent Variables	OLS	IV	OLS	IV
Months in education	-.003	-.015	-.003	-.012
	(.002)	(.011)	(.003)	(.012)
Quarters of employment			-.003	.027
			(.005)	(.029)

(table continues)

TABLE 8, continued

Independent Variables	Model 1: Remedial Group Math		Model 2: Remedial Group Math	
	OLS	IV	OLS	IV
Months in education	-.002	-.009	-.003	-.008
	(.003)	(.011)	(.003)	(.011)
Quarters of employment			-.011 **	.011
			(.005)	(.026)

Independent Variables	Model 1: Teacher Report School Engagement		Model 2: Teacher Report School Engagement	
	OLS	IV	OLS	IV
Months in education	.054	.340 *	.063	.290
	(.042)	(.193)	(.043)	(.190)
Quarters of employment			.114 *	-.550
			(.080)	(.472)

Independent Variables	Model 1: Child Report School Engagement		Model 2: Child Report School Engagement	
	OLS	IV	OLS	IV
Months in education	-.016	.037	-.014	.035
	(.017)	(.073)	(.017)	(.074)
Quarters of employment			.034	-.081
			(.034)	(.207)

Notes: P-values: *p<.10, ** p<.05, *** p<.01.

Covariates included for educational attainment and participation at baseline, prior earnings, prior welfare reciept, numeracy, literacy, depressive symptoms, mother's and focal child's age, number of baseline risk factors, family barriers to employment, race, marital status, number of children, locus of control, sources of social support, and child gender.

estimates from the IV models are larger than those from the OLS models, resulting estimates are small and nonsignificant. For example, the OLS results (model 2) suggest that an additional month of maternal education is associated with a .094 higher reading score, whereas the IV results suggest that it is associated with a .181 higher score. Figure 3 presents the resulting effect sizes for the average HCD participant who attended an educational program. The estimated effects of an additional month maternal education on children's math achievement are even smaller, and thus translate into smaller effect sizes (Figure 3).

In contrast, results from IV analyses of children's academic problems and placement in a remedial reading group yield substantial, although nonsignificant, estimated effects of mothers' participation in education. An additional month of maternal education reduces children's academic problems by -.013 (model 2, column 4), and reduces the probability of being in a remedial reading group by .012 (model 2, column 4). The OLS model results suggest a much smaller reduction in the case of academic problems (-.002) and remedial reading placement (.003). The discrepancies in the effect sizes resulting from the OLS and IV models are considerable. The OLS estimates suggest effect sizes of .03 for academic problems and .05 for remedial reading, whereas the IV models suggest effect sizes of .21 for academic problems and .20 for remedial group reading (Figure 3). In both IV and OLS models, the estimated effects of maternal education are smaller on children's math ability grouping than reading ability grouping.

The effects of maternal education on children's school engagement are presented in the last two panels of Table 8. In both sets of analyses, the IV estimates are larger than the OLS estimates, but they differ a great deal in size and are not statistically significant. The IV estimates translate into effect sizes of .30 for teachers' reports of children's school engagement but only .08 for children's reports. The OLS models yield smaller effect sizes—.06 for teachers' reports of school engagement and -.03 for children's reports (Figure 4).

FIGURE 3
Estimated Effects of 8 Months of Maternal Education on Children's Academic Achievement, 60 Months

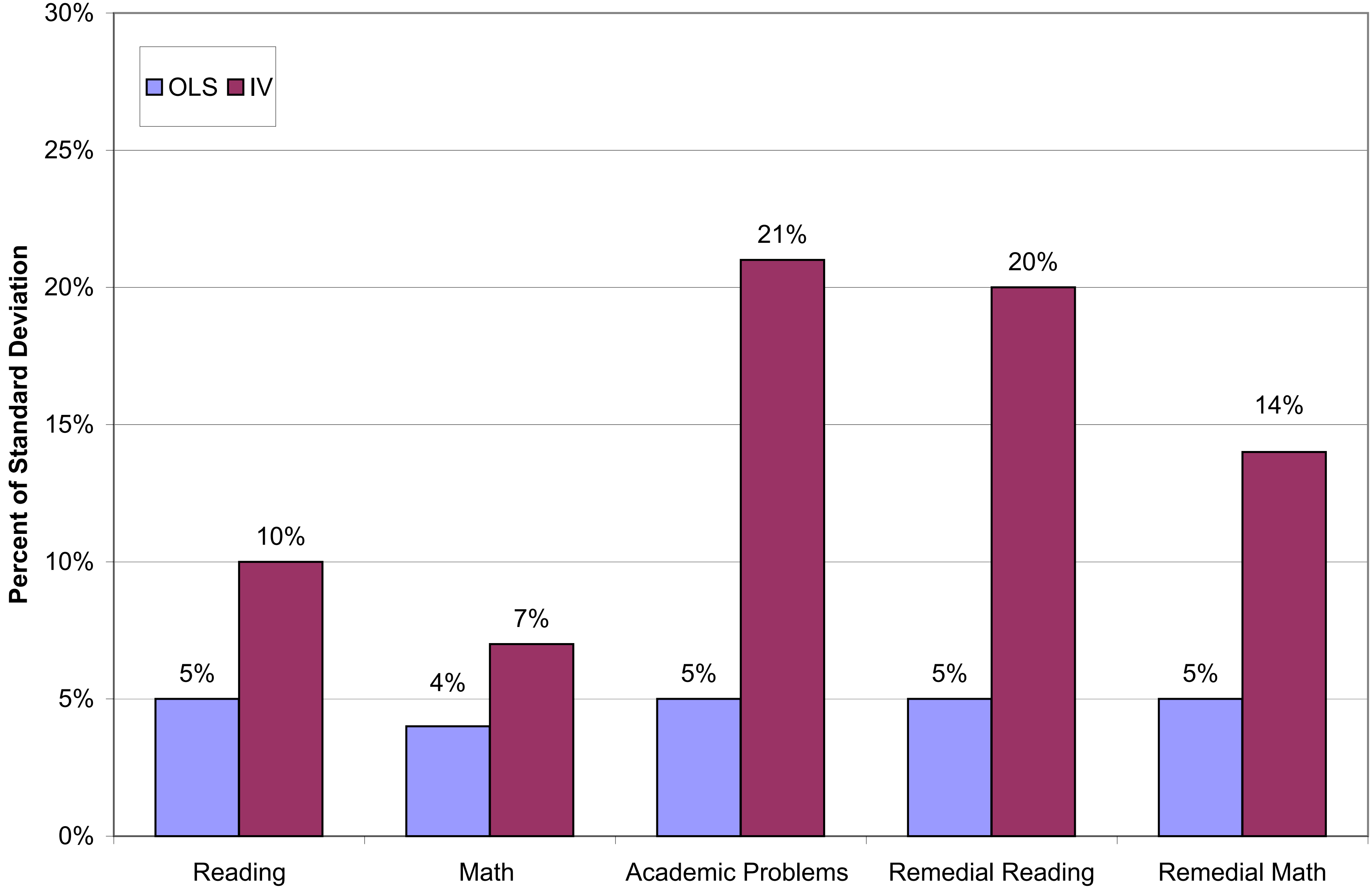
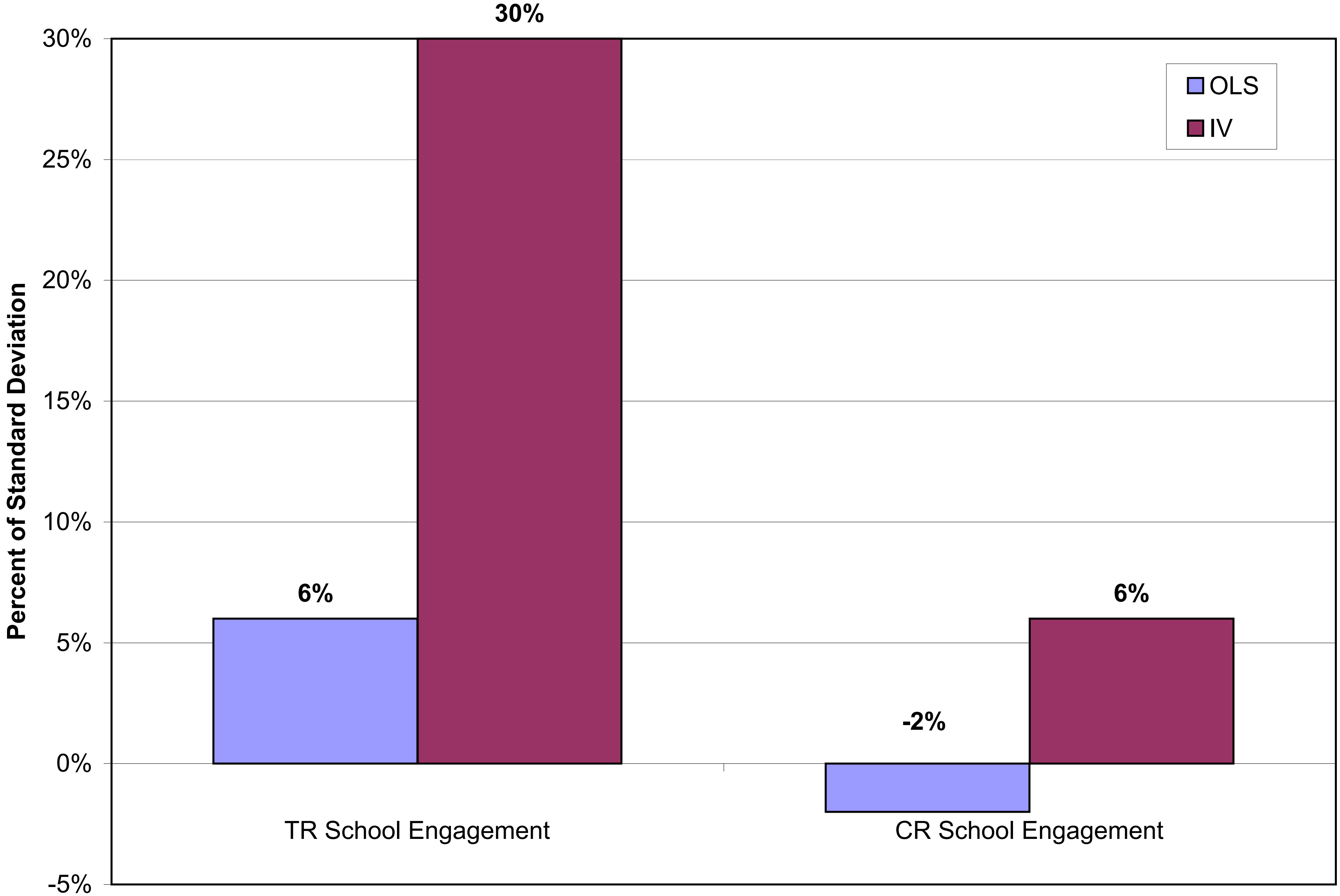


FIGURE 4
Estimated Effects of 8 Months of Maternal Education on Children's School Outcomes, 60 Months



DISCUSSION

Although many studies report correlations between maternal education and children's cognitive development, the instrumental variable analyses in this paper provide a more convincing estimation of the causal effect of a mother's education on her child's developmental outcomes. Results suggest that increases in welfare mothers' education improved their children's academic, but not behavioral, outcomes. Both the OLS and IV analyses indicate that mothers' educational participation led to reductions in children's academic problems as well as improvements in school readiness and the quality of home learning environments. Increases in maternal education seemed to have little or no effect on mothers' reports of their children's behavior.

As expected the experimental findings confirmed earlier reports that assignment to the Human Capital Development (HCD) stream of the JOBS program significantly increased mothers' participation in educational activities during the first 2 years of the program (McGroder et al., 2000). The relative strength of the program impacts varied by the type of educational activity mothers participated in, the program treatment stream they were assigned to, and the site. I used the variability in these experimentally induced differences in educational activities to estimate IV models. In the first stage of the IV estimation, treatment status-by-site dummy variables were used as instruments to predict mothers' education and employment. In the second stage, children's outcomes at 24 and 60 months were regressed on the predicted values of mothers' education and employment. Given two randomly assigned treatment streams in three sites and variation across these six programs in impacts on educational activity and employment, the established criteria for drawing causal inference from IV estimations were likely met (Angrist et al., 1996).

Although the IV models are better able to estimate causal linkages by purging measures of maternal education of endogenous influences, they do not offer precise estimates of the effect of maternal education on children's academic school readiness. The IV estimation results in larger standard errors than OLS models, and consequently in larger confidence intervals. Large confidence intervals are most problematic when they result in type II errors (not rejecting the null hypothesis when it is false). Because the potential for bias in estimates is often overlooked, researchers should pay more attention to the problem of omitted variables in their estimation models and employ a variety of strategies to reduce it (Duncan, Magnuson, and Ludwig, 1999). However, researchers should also remain alert to the possibility of type II errors that results from imprecise estimation, as well as the assumptions and limitations associated with their chosen method of analysis.

In addition to providing evidence of a causal relationship between maternal education and children's academic outcomes, the IV model results suggest that the size of the effect of maternal education on children's academic school readiness and academic problems is substantial. The IV results suggest that 8 months of educational activity increase children's academic school readiness by .21 of a standard deviation (representing approximately three more questions answered correctly) and reduce children's academic problems by .20 of a standard deviation. These estimates are just slightly larger than estimates from previous studies. Typically, correlational research finds that an additional year of maternal education results in approximately .19 of a standard deviation increase in children's achievement test scores (White, 1982). In contrast, the results from OLS models are half the size of those suggested by the literature. The results suggest that an additional year of education results in a .07 of a standard deviation increase in children's academic school readiness, and reduces children's academic problems by .02 of a standard deviation.

The analyses with the subgroup of mothers who were in need of basic education at the time of random assignment provide mixed evidence as to whether the association between mothers' participation in educational activities differed according to their level of prior academic skills. The effects of maternal education on children's academic school readiness was concentrated among mothers who were in need of education. On the other hand, effects on children's academic problems and stimulation in the home learning environment were not concentrated in the group of mothers not in need of education. However, the more limited set of program instruments is a relatively weaker predictor of participation in educational activities for mothers not in need of basic education, and consequently the IV estimation may have been less precise for this subgroup. In addition, mothers who were not in need of basic skills education enrolled primarily in vocational training programs rather than basic education, and it may be that mothers' participation in vocational training had different effects on children's outcomes than did their participation in basic skills education. Prior research has not explored the differential effects of vocational education on children's academic outcomes, and this would be a fruitful avenue for future research.

Although the analyses offer little evidence that maternal education affected children's behavior, it is important to note that the measures of behavior available in the NEWWS data, maternal reports, may not be sensitive or accurate enough to capture important differences in children's behavior. These maternal reports may also be subject to reporter bias in that they may reflect mothers' own mental health or experiences, rather than children's actual behavior. In addition, the measures in this data may not capture the types or range of behavior that maternal education is likely to change.

The analysis of educational participation among mothers who were assigned to the NEWWS education treatment stream suggests that increasing a mother's education may be more difficult than previously considered. More than half of the mothers in the study did not participate in training even when mandated to do so. On the other hand, just less than half of the mothers in the control group did participate in educational programs without any programmatic intervention. There are several reasons why this might have happened. First, the HCD program may have increased mothers' education only by pushing mothers who were likely to return to school to do so more quickly than they might have otherwise. If this were the case, as the study continued, mothers in the control group would have continued to seek out schooling and this would explain the decreasing difference in experimental/control education rates. Second, welfare reform in 1996 changed the conditions of the control group, although only a small proportion of women in the program and control groups faced newly mandated welfare work requirements and time limits. Nevertheless, the rhetoric and debate surrounding the welfare changes may have motivated control group mothers in all sites to seek out additional education or employment in anticipation of the policy changes (Hamilton et al., 1997). Future research should seek to better understand the reasons why low-income mothers choose to pursue additional education.

Because of control and LFA mothers' participation in educational activities during the last 3 years of the program, the OLS and IV results from analyses with 5-year child outcomes should be interpreted as an indication of whether maternal education during children's preschool years, as compared with maternal education in early school years, had an effect on children's academic outcomes. The analysis does not indicate whether the children of mothers who returned to school had better outcomes than children of mothers who did not return to school. Results suggest that the earlier timing of mothers' education did not have a lasting benefit children's reading or math achievement. However, the results provide some inconclusive evidence that maternal education during children's preschool years may have had a lasting effect on teachers' reports of children's school engagement and educational tracking, such as being placed in a remedial reading ability group. The resulting effect sizes for the child of the average HCD mother who obtained more schooling were nearly as large as those found in analyses with the 2-year data. The sample size was a third smaller, however, so it is not surprising that the estimates do not reach statistical significance.

Why might the early timing of mothers' educational activity affect children's academic problems? Rates of special education placement and grade retention are highest in the first year of school. Similarly, ability group placement occurs early in a child's schooling (Entwisle and Alexander, 1993). Early childhood education programs that boost children's academic achievement during the early school years have been found to have lasting effects on children's special education placement, even after the program's effects on achievement have faded (Barnett, 1995). If an increase in mothers' education promotes a child's early academic performance, as suggested by results from this study, then it might also prevent the child from being retained or placed in special education. That is, later maternal schooling may benefit children's achievement, but may occur too late in a child's education to prevent academic problems. Additional research with more detailed measures of the timing of children's academic trajectories would shed light on these questions.

Why might children's math skills or ability grouping be less effected than their reading? Several studies have found that young children's calculation skills are not sensitive to variations in their social background, including parental education (Entwisle and Alexander, 1990; Jordan, Huttenlocher, and Levine, 1992). This may be due to one of two reasons. First, home environments may not differ in the opportunities that they provide for learning math as compared with literacy. Or it may be that math concepts are primarily learned in school through formal instruction rather than at home, and that schools do a particularly good job in teaching math to young children, especially socioeconomically disadvantaged children (Entwisle and Alexander, 1990). If classroom experiences play a strong compensating role in children's math achievement, then the advantages bestowed by increases in mothers' education may not matter much.

Policymakers should continue and strengthen efforts to promote basic education among current and former welfare recipients because it benefits children. Current welfare-to-work strategies emphasize a work-first approach. Federal guidelines allow only limited amounts education to count toward states' work participation requirements. In 2000, 39 states had guidelines that allowed all welfare recipients to participate in at least some basic education to meet work requirements, although restrictions and limitations vary greatly across states. At least one study suggests that educational activities have decreased dramatically among welfare recipients (U.S. General Accounting Office, 1998). However, recent data also suggest that rates of educational participation among less educated adults increased during the 1990s (U.S. Department of Education, 2001). Consequently, additional detailed analyses of educational participation patterns are necessary to better understand whether welfare reform has improved or limited educational participation among some groups. Finally, future evaluations of adult education and training programs should include improvements in children’s academic outcomes as a possible benefit of these programs. Because special educational services and grade retention are costly interventions, the finding that increases in maternal education prevent these problems is particularly noteworthy.

APPENDIX TABLE A1

Mean, Standard Deviation, Minimum, and Maximum Values for All Baseline Covariates

	Mean	Std Dev	Min	Max
Baseline Covariates (in all analyses)				
Atlanta	.47	.50	0	1
Grand Rapids	.22	.41	0	1
Inneed in Riverside	.22	.42	0	1
In GED Preparation Classes	.02	.14	0	1
In Post-Secondary Education	.05	.22	0	1
In ABE	.01	.12	0	1
In Vocational Training	.07	.26	0	1
Has Higher than HS Diploma	.57	.49	0	1
Has HS Diploma or GED	.52	.50	0	1
Ever Been Married or Lived with Spouse	.39	.49	0	1
Mom Age	28.56	5.52	20	52
Age of Youngest Child	3.64	1.36	1	18
Number of Children	2.21	1.09	1	6
Black	.59	.49	0	1
Hispanic	.12	.33	0	1
Prior Year Earnings	1329.97	3304.77	0	44823
Number of Months of Welfare Reciept	8.75	4.58	0	12
Average Monthly AFDC	356.56	224.07	0	1280
Male Child	.49	.50	0	1
Reading Skills	203.33	49.60	0	249
Math Skills	269.15	72.32	0	390
Missing Literacy Skills	.04	.20	0	1
Missing Math Skills	.04	.20	0	1
Depressive Symptoms	.47	.75	0	2
Missing Depressive Symptoms	.16	.37	0	1
Locus of Control	1.09	.80	0	2
Missing Locus of Control	.15	.36	0	1
Sources of Social Support	.07	.26	0	1
Missing Sources of Social Support	.18	.39	0	1
Number of Baseline Risk Factors	.40	.65	0	2
Missing Baseline Risk Factors	.15	.36	0	1
Family Barriers to Employment	.85	.65	0	2
Missing Family Barriers to Employment	.17	.37	0	1
24-month survey covariates				
Focal Child Age Less Than 64 months	.11	.31	0	1
Focal Child Age Between 64-66 months	.15	.35	0	1
Focal Child Age Between 67-70 months	.16	.37	0	1
Focal Child Age Between 71-74 months	.12	.32	0	1
Focal Child Age Between 75-78 months	.12	.32	0	1
Focal Child Age Between 80-83 months	.10	.30	0	1
Focal Child Age Between 84-87 months	.09	.28	0	1
Focal Child Age Between 88-91 months	.07	.26	0	1

(table continues)

APPENDIX TABLE A1, continued

	Mean	Std Dev	Min	Max
60-month survey covariates				
Focal Child Age Less Than 99 months	.04	.20	0	1
Focal Child Age Between 99-100 months	.06	.23	0	1
Focal Child Age Between 101-103 months	.10	.29	0	1
Focal Child Age Between 104-106 months	.12	.33	0	1
Focal Child Age Between 107-109 months	.12	.32	0	1
Focal Child Age Between 110-112 months	.08	.27	0	1
Focal Child Age Between 113-115 months	.09	.28	0	1
Focal Child Age Between 116-118 months	.07	.25	0	1
Focal Child Age Between 119-121 months	.08	.27	0	1
Focal Child Age Between 122-124 months	.07	.25	0	1
Focal Child Age Between 126-128 months	.07	.26	0	1
Focal Child Age Between 127-131 months	.06	.23	0	1

APPENDIX TABLE A2

Means and Standard Deviations of 2-Year Education, Employment, Earnings, School Readiness, Academic Problem Measures, Behavior, and Cognitive Stimulation

	Atlanta		Riverside		Grand Rapids	
Variables Measured at 24 Months	Mean	SD	Mean	SD	Mean	SD
Independent Variables						
Total Months in ABE	1.04	3.56	.72	2.48	.95	3.27
Total Months in Vocational Training	.90	2.96	.42	1.72	.63	2.21
Total Months in All Education Activities	2.36	5.21	2.68	5.36	3.86	5.95
Quarters of Employment	2.98	2.84	1.97	2.67	3.46	2.67
Total 2-Year Earnings	$ 5,158	$ 7,332	$ 3,509	$ 6,820	$ 6,102	$ 9,387
Sample Size	*921*		*661*		*581*	

APPENDIX TABLE A3

Summary of NEWWS Impacts on Maternal Education, Employment, Children's School Readiness and Academic Problems, by Site (Standard Errors in Parentheses)

	Human Capital Development			Labor Force Attachment		
	Atlanta	Grand Rapids	Riverside	Atlanta	Grand Rapids	Riverside
Months in ABE	1.50 ***	.39	1.88 ***	.30 **	-.24	-.07
	(.22)	(.36)	(.26)	(.13)	(.29)	(.09)
Months in Vocational Training	.95 ***	.37	.15	.39 *	-.12	-.25 *
	(.23)	(.25)	(.14)	(.21)	(.23)	(.14)
Months in All Education	2.32 ***	.71	2.52 ***	.55 *	-.95 **	-.24
	(.34)	(.60)	(.40)	(.28)	(.53)	(.38)
Quarters of Employment	.24	-.14	.63 ***	.49 **	.91 ***	1.24 ***
	(.17)	(.25)	(.18)	(.19)	(.25)	(.22)
24-Month Outcomes						
BBCS/SRC Raw Score	.80	.09	1.42	1.65 ***	.27	.08
	(.58)	(.94)	(.92)	(.64)	(.91)	(.90)
Academic Problem-Focal Child	-.02	.05	-.04	.00	.01	.00
	(.02)	(.04)	(.03)	(.02)	(.04)	(.03)
Problem Behavior	-.13	.20	.05	-.21	.40 **	.25
	(.16)	(.22)	(.19)	(.17)	(.19)	(.16)
Positive Behavior	.14	-.06	.20	.23	-.22	-.05
	(.24)	(.29)	(.28)	(.27)	(.26)	(.24)
Cognitive Stimulation	-.05	.03	.12	.12 *	-.09	-.03
	(.06)	(.07)	(.08)	(.07)	(.07)	(.07)
Sample Size	*1026*	*426*	*577*	*902*	*441*	*258*

(table continues)

APPENDIX TABLE A3, continued

	Human Capital Development			Labor Force Attachment		
	Atlanta	Grand Rapids	Riverside	Atlanta	Grand Rapids	Riverside
5-Year Outcomes						
WJ Broad Reading	.39	-1.23	1.26	-.65	1.15	-.01
	(1.27)	(1.91)	(1.74)	(1.35)	(1.82)	(1.61)
WJ Broad Math	2.08	-2.01	1.32	.39	1.36	1.88
	(1.49)	(2.00)	(1.88)	(1.55)	(1.89)	(1.70)
Academic Problems	-.06	.05	-.07	.02	.05	.05
	(.04)	(.07)	(.05)	(.05)	(.07)	(.05)
Remedial Group Reading	-.02	-.08	.00	-.01	-.08	.12 **
	(.05)	(.07)	(.07)	(.05)	(.06)	(.06)
Remedial Group Math	-.06	-.09	-.03	-.07	-.02	.02
	(.05)	(.06)	(.06)	(.05)	(.06)	(.05)
Teacher Report School Engagement	1.75 **	-1.05	1.11	.77	-.64	-1.38
	(.83)	(1.02)	(.96)	(.92)	(.93)	(.86)
Child Report School Engagement	-.12	-.54	.80 *	-.11	-.64	.20
	(.30)	(.38)	(.45)	(.34)	(.40)	(.41)
Sample Size	605	344	360	530	359	428

Notes: P-values: *p<.10, ** p<.05, *** p<.01; Sample sizes may vary due to missing data on outcome variables.

Covariates included for educational attainment and participation at baseline, race, marital status, number of children, prior earnings, prior welfare reciept, numeracy, literacy, depressive symptoms, locus of control, sources of social support, number of baseline risk factors, family barriers to employment, mother's and focal child's age, and child's gender. These program impacts may differ slightly than those reported in McGroder et al. (2000) due to differences in baseline covariates.

Appendix Table A4

Coefficients and Standard Errors for Covariates of IV Regression Model of Maternal Education on Children's BBCS-SRC scores

Atlanta	1.02 (.74)		Missing Math Skills	-1.16 (6.59)	
Grand Rapids	-.80 (.66)		Depressive Symptoms	.49 (.28)	
In Riverside in Need of Basic Education	-1.32 (.81)		Missing Depressive Symptoms	1.07 (1.35)	
In GED Preparation Classes	-.32 (1.55)		Locus of Control	.45 (.32)	
In Postsecondary Education	-.03 (1.66)		Missing Locus of Control	3.12 (1.63)	
In ABE	-5.00 (1.75)	***	Sources of Social Support	.65 (.75)	
In Vocational Training	-.26 (.81)		Missing Sources of Social Support	-.95 (1.02)	
Has Higher than HS Diploma	.59 (1.14)		Number of Baseline Risk Factors	-.33 (.44)	
Has HS Diploma or GED	-.38 (.86)		Missing Baseline Risk Factors	-.97 (2.07)	
Ever Been Married or Lived with Spouse	-.64 (.43)		Family Barriers to Employment	-.45 (.40)	
Mom Age	.02 (.04)		Missing Family Barriers to Employment	-.19 (1.38)	
Age of Youngest Child	.10 (.16)		Focal Child Age Less Than 64 months	-18.95 (.86)	***
Number of Children	-.61 (.21)	***	Focal Child Age Between 64-67 months	-15.90 (.81)	***
Black	-.04 (.57)		Focal Child Age Between 68-71 months	-10.25 (.81)	***
Hispanic	-1.04 (.63)	**	Focal Child Age Between 72-75 months	-7.59 (.82)	***
Prior Year Earnings	.00 (.00)		Focal Child Age Between 76-79 months	-4.42 (.83)	***
Number of Months of Welfare Reciept	-.07 (.06)		Focal Child Age Between 80-83 months	-3.07 (.87)	***
Average Monthly AFDC	.00 (.00)		Focal Child Age Between 84-87 months	-2.03 (.88)	***
Male Child	-1.13 (.37)	***	Focal Child Age Between 88-91 months	-.24 (.91)	
Reading Skills	.02 (.01)	**			
Math Skills	.03 (.01)	***			
Missing Literacy Skills	10.08 (6.72)				

Notes: P-values: *p<.10, ** p<.05, *** p<.01.

All covariates were entered in the same estimation model with predicted months of months of maternal education and quarters of employment from the first stage of the IV estimates.

Bibliography

Alexander, K. L., D. Entwisle, and S. D. Bedinger. 1994. "When Expectations Work: Race and Socioeconomic Differences in School Performance." *Social Psychology Quarterly* 57: 283–299.

Alwin, D. F., and A. Thornton. 1984. "Family Origins and the Schooling Process: Early Versus Late Influence of Parental Characteristics." *American Sociological Review* 49: 784–802.

Angrist, J., Imbens, G., and D. R. Rubin. 1996. "Identification of Causal Effects Using Instrumental Variables." *Journal of the American Statistical Association* 90: 444–455.

Astone, N. M., R. Schoen, M. Ensminger, and K. Rothert. 2000. "School Reentry in Early Adulthood: The Case of Inner-City African Americans." *Sociology of Education* 73: 133–154.

Barnett, W. S. 1995. "Long-Term Effects of Early Childhood Programs on Cognitive and School Outcomes." *The Future of Children* 5: 25–50.

Bee, H. L., K. E. Barnard, S. J. Eyres, C. A. Gray, M. A. Hammond, A. L. Spietz, C. Snyder, and B. Clark. 1982. "Prediction of IQ and Language Skill from Perinatal Status, Child Performance, Family Characteristics, and Mother-Infant Interaction." *Child Development* 53: 1134–1156.

Bee, H. L., L. F. Van Egeren, A. P. Streissguth, B. A. Nyman, and M. S. Leckie. 1969. "Social Class Differences in Maternal Teaching Strategies and Speech Patterns." *Developmental Psychology* 1: 726–734.

Behrman, J. R., and M. R. Rosenzweig. 2002. "Does Increasing Women's Schooling Raise the Schooling of the Next Generation? *American Economic Review* 92: 323–334

Bos, J., S. Scrivener, J. Snipes, and G. Hamilton. 2002. *Improving Basic Skills: The Effect of Adult Education in Welfare-to-Work Programs*. NY: Manpower Demonstration Research Corporation.

Bound, J., D. A. Jaeger, and R. Baker. 1995. "Problems with Instrumental Variables Estimation When the Correlation between the Instruments and the Endogeneous Explanatory Variable is Weak." *Journal of the American Statistical Association* 90: 443–450.

Bradley, R. H., and R. F. Corwyn. 2002. "Socioeconomic Status and Child Development." *Annual Review of Psychology* 53: 371–99.

Byrd, R. S., and M. L. Weitzman. 1994. "Predictors of Early Grade Retention among Children in the United States." *Pediatrics* 93: 481–487.

Christian, K., F. J. Morrison, and F. B. Bryant. 1998. "Predicting Kindergarten Academic Skills: Interactions among Child Care, Maternal Education, and Family Literacy Environments." *Early Childhood Research Quarterly* 13: 501–521.

Cohen, J. 1989. "Parents as Educational Models and Definers." *Journal of Marriage and the Family* 49: 339–351.

Conger, R. D., K. J. Conger, and G. H. Elder. 1997. "Family Economic Hardship and Adolescent Adjustment: Mediating and Moderating Processes." In *The Consequences of Growing Up Poor*, ed. G. Duncan and J. Brooks-Gunn. New York: Russell Sage Foundation, pp. 288–310.

Davis-Kean, P. E., and J. S. Eccles. 2003. "The Influence of Parent Education and Family Income on Child Achievement: The Indirect Role of Parental Expectations and the Home Environment." Paper presented at the Society for Research on Child Development, Tampa, FL.

Dearing, E., K. McCartney, and B. A. Taylor. 2001. "Change in Family Income Matters More for Children with Less." *Child Development* 72: 1779–1793.

Duncan, G., and J. Brooks-Gunn, Eds. 1997. *The Consequences of Growing Up Poor*. New York: Russell Sage Foundation.

Duncan, G., K. Magnuson, and J. Ludwig. 1999. "The Endogeneity Problem in Developmental Studies." Mimeo, Northwestern University.

Ensminger, M., and A. L. Slusarick. 1992. "Paths to High School Graduation or Dropout: A Longitudinal Study of a First-Grade Cohort." *Sociology of Education* 65: 95–113.

Entwisle, D. R., and K. L. Alexander. 1990. "Beginning School Math Competence: Minority and Majority Comparisons." *Child Development* 61: 454–471.

Entwisle, D., and K. L. Alexander. 1993. "Entry into School: The Beginning School Transition and Educational Stratification in the United States." *Annual Review of Sociology* 19: 401–423.

Foster, E. M., and S. McLanahan. 1996. "An Illustration of the Use of Instrumental Variables: Do Neighborhood Conditions Affect a Young Person's Chances of Finishing High School." *Developmental Methods* 1: 249–260.

Fox, R. A., D. L. Platz, K. S. Bentley. 1995. "Maternal Factors Related to Parenting Practices, Developmental Expectations, and Perceptions of Child Behavior Problems." *Journal o f Genetic Psychology* 156: 431–441.

Furstenberg, F. F., J. Brooks-Gunn, and S. P. Morgan. 1987. *Adolescent Mothers in Later Life*. New York: Cambridge University Press.

Gamse, B. C., D. Conger, D. Elson, and M. McCarthy. 1997. *Follow-Up Study of Families in the Even Start in Depth Study*. Cambridge, MA: Abt Associates.

Hamilton, G., T. Brock, M. Farrell, D. Friedlander, and K. Harknett. 1997. *Evaluating Two Welfare-to-Work Program Approaches: Two Year Findings on the Labor Force Attachment and Human Capital Development Programs in Three Sites*. Washington, DC: United States Department of Health and Human Services Office of the Assistant Secretary for Planning and Evaluation, Administration for Children and Families.

Haney, W., L. Ludlow, A. Raczek, S. Stryker, and A. Jones. 1996. "Calibrating Two Tests of Adult Literacy: An Equating Study of the Test of Adult Literacy Skills (TALS) and the Comprehensive Adult Student Assessment System (CASAS) GAIN Appraisal Reading Test." Report prepared for Manpower Demonstration Research Corporation by the Program in Educational Research, Measurement and Evaluation, Boston College.

Harris, Y. R., D. Terrel, and G. Allen. 1999. "The Influence of Education Context and Beliefs on the Teaching Behavior of African American Mothers." *Journal of Black Psychology* 25: 490–503.

Haveman, R., and B. Wolfe. 1995. "The Determinants of Children's Attainments: A Review of Methods and Findings." *Journal of Economic Literature* 23: 1829–1878.

Heckman, J., R. LaLonde, and J. Smith. 1999. "The Economics and Econometrics of Active Labor Market Programs." In *Handbook of Labor Economics, Volume 3*, ed. O. Ashenelter and D. Card. New York: Elsevier Science, pp. 1865–2095.

Hess, R. D, S. D. Holloway, W. P. Dickson, and G. G. Price. 1984. "Maternal Variables as Predictors of Children's School Readiness and Later Achievement in Vocabulary and Mathematics in Sixth Grade." *Child Development* 55: 1902–1912.

Hollomon, H. A., D. R. Dobbins, and K. G. Scott. 1998. "The Effects of Biological and Social Risk Factors on Special Education Placement: Birth Weight and Maternal Education as an Example." *Research in Developmental Disabilities* 19: 281–294.

Jordan, N. C., J. Huttenlocher, and S. C. Levine. 1992. "Differential Calculation Abilities in Young Children from Middle and Low-Income Families." *Developmental Psychology* 28: 644–653.

Kaestner, R., and H. Corman. 1995. "The Impact of Child Health and Family Inputs on Child Cognitive Development." Cambridge, MA: National Bureau of Economic Research Working Paper #5257.

Kohl, G. O., L. J. Lengua, and R. J. McMahon. 2000. "Parent Involvement in School Conceptualizing Multiple Dimensions and Their Relations with Family and Demographic Risk Factors." *Journal of School Psychology* 38: 501–523.

Lee, V. E., and R. G. Croninger. 1994. "The Relative Importance of Home and School in the Development of Literacy Skills for Middle-Grade Students." *American Journal of Education* 102: 286–329.

Love, J. M., E. E. Kisker, C. M. Ross, P. Z. Schochet, J. Brooks-Gunn, D. Paulsell, K. Boller, J. Constantine, C. Vogel, A. S. Fuligni, and C. Brady-Smith. 2002. *Making a Difference in the Lives of Infants and Toddlers and Their Families: The Impacts of Early Head Start. Volume 1: Final Technical Report*. Princeton, NJ: Mathematica Policy Research.

Mare, R. D. 1980. "Social Background and School Continuation Decisions." *Journal of the American Statistical Association* 75: 295–305.

Magnuson, K., and G. Duncan. Forthcoming. "Parent- vs. Child-Based Intervention Strategies for Promoting Children's Wellbeing." *Monographs in Parenting*.

McGroder, S. M., M. J. Zaslow, K. A. Moore, and S. M. LeMenestrel. 2000. *National Evaluation of Welfare-to-Work Strategies Impacts on Young Children and Their Families Two Years After Enrollment: Findings from the Child Outcomes Study*. Washington, DC: United States Department of Health and Human Services Office of the Assistant Secretary for Planning and Evaluation Administration for Children and Families.

Mercy, J. A., and L. C. Steelman. 1982. "Familial Influence on the Intellectual Attainment of Children." *American Sociological Review* 47: 532–542.

Neiss, M., and D. Rowe. 2001. "Parental Education and Child's Verbal IQ in Adoptive and Biological Families in the National Longitudinal Study of Adolescent Health." *Behavior Genetics* 30: 487–495.

Plug, E. 2002. "How Do Parents Raise the Educational Attainment of Future Generations?" Mimeo, Department of Economics, University of Amsterdam.

Polit, D. F. 1996. "Self-Administered Teacher Questionnaire in the New Chance 42-Month Survey." New York: MDRC.

Quint, J., J. Bos, and D. Polit. 1997. *New Chance: Final Report on a Comprehensive Program for Young Mothers in Poverty and Their Children*. NY: MDRC.

Resnick, S., R. Corley, and J. Robinson. 1997. "A Longitudinal Twin Study of Intelligence in the Second Year." *Monographs of the Society for Research in Child Development, 62*. Chicago: University of Chicago Press.

Rich, L. M., and S. Kim. 1999. "Patterns of Later Life Education among Teenage Mothers." *Gender and Society* 13: 798–817.

Richman, A. L., P. M. Miller, and R. A. Levine. 1992. "Cultural and Educational Variation in Maternal Responsiveness." *Developmental Psychology* 4: 614–621.

Roe, K., and R. Bronstein. 1988. "Maternal Education and Cognitive Processing at Three Months as Shown by the Infants' Vocal Response to Mother vs. Stranger." *International Journal of Behavioral Development* 11: 389–395.

Rosenzweig, M. R., and K. I. Wolpin. 1994. "Are There Increasing Returns to the Intergenerational Production of Human Capital? Maternal Schooling and Child Intellectual Development." *Journal of Human Resources* 29: 670–693.

Rumberger, R. W. 1983. "Dropping Out of High School: The Influence of Race, Sex, and Family Background." *American Educational Research Journal* 20: 199–220.

Rumberger, R. W., G. Poulos, R. Ghatak, P. L. Ritter, and S. M. Dornbusch. 1990. "Family Influences on Dropout Behavior in One California High School. *Sociology of Education* 63: 283–299.

Scott-Jones, D. 1984. "Family Influences on Cognitive Development and School Achievement." In *Review of Research in Education*, ed. E. Gordon. Washington, DC: American Educational Research Association, pp. 259–304.

Seefeldt, C., K. Denton, A. Galper, and T. Younszal. 1999. "The Relation between Head Start Parents' Participation in a Transition Demonstration, Education, Efficacy, and Their Children's Academic Abilities." *Early Childhood Research Quarterly* 14: 99–109.

Sewell, W. H., A. O. Haller, and G. W. Ohlendorf. 1970. "The Educational and Early Occupational Status Attainment Process: Replication and Revision." *American Sociological Review* 35: 1014–1027.

Shadish, W. R., T. D. Cook, and D. T. Campbell. 2002. *Experimental and Quasi-Experimental Designs for Generalized Causal Inference*. Boston: Houghton Mifflin.

Smith, T. E. 1989. "Mother-Father Differences in Parental Influence on School Grades and Educational Goals." *Sociological Inquiry* 59: 88–91.

Sobel, M. E. 1998. "Causal Inference in Statistical Models of the Process of Socioeconomic Achievement." *Sociological Methods and Research* 27: 318–348.

Staiger, D., and J. H. Stock. 1994. "Instrumental Variables Regression with Weak Instruments." Cambridge, MA: National Bureau of Economic Research Technical Working Paper #151.

Stevenson, D. L., and D. P. Baker. 1987. "The Family-School Relation and the Child's School Performance." *Child Development* 58: 1348–1357.

Stipek, D. J., and R. H. Ryan. 1997. "Economically Disadvantaged Preschoolers: Ready to Learn but Further to Go." *Developmental Psychology* 33: 711–723.

U.S. Department of Education, National Center for Education Statistics. 2001. *The Condition of Education 2001*. Washington, DC: U.S. Government Printing Office.

U.S. General Accounting Office. 1998. *Welfare Reform: States are Restructuring Programs to Reduce Welfare Dependence*. Washington, DC: U.S. General Accounting Office, Health, Human Services, and Education Division.

Uribe, F. M. T., R. A. Levine, and S. E. Levine. 1993. "Maternal Education and Maternal Behavior in Mexico: Implications for the Changing Characteristics of Mexican Immigrants to the United States." *International Journal of Behavioral Development* 16: 395–408.

Velez, C. N., J. Johnson, and P. Cohen. 1993. "A Longitudinal Analysis of Selected Risk-Factors for Childhood Psychopathology." *Journal of American Academy of Child and Adolescent Psychiatry* 33: 861–864.

White, K. 1982. "The Relation between Socioeconomic Status and Academic Achievement." *Psychological Bulletin* 91: 461–481.

Yeates, K. O., D. MacPhee, F. A. Campbell, and C. T. Ramey. 1983. "Maternal IQ and Home Environment as Determinants of Early Childhood Intellectual Competence: A Developmental Analysis." *Developmental Psychology* 19: 731–739.