The Effect of Increasing Welfare Mothers' Education on their Young Children's Academic Problems and School Readiness

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Abstract

Does an increase in a mother's education improve her young child's academic performance? Positive correlations between mothers' educational attainment and children's well being, in particular 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 (e.g., OLS) and analysis of variance (e.g., ANOVA) approaches to estimating the effect of maternal schooling on child outcomes may be biased by omitted variables, we use experimentally induced differences in mothers' education to estimate Instrumental Variable (IV) models. Our 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. We find 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. Our estimated causal effects of maternal education on children's academic school readiness and academic problems are large enough to be of considerable importance for policies that affect the work, welfare, and training of low-income mothers.

Keywords: Education, Child Well-being, Welfare Policy

Introduction

To what extent does an increase in maternal education improve children's academic performance? That is, if a program could somehow increase mothers' schooling, how much benefit would we expect to see in their children's cognitive development? Would these benefits be found if the mother obtained non-traditional forms of education such as Adult Basic Education (ABE) or vocational training? While current welfare reform programs emphasize work-first strategies, this paper considers whether states are overlooking an important opportunity to improve young children's academic problems and school readiness by providing welfare recipients with educational opportunities.

Positive correlations between mothers' educational attainment and children's well being, and particularly school outcomes and cognitive development, are among the most replicated results from developmental studies (Bee et al., 1982; Haveman & Wolfe, 1995). The processes by which maternal education affects children's development may be both direct and indirect. Direct effects may consist of enrichments to the child's home learning environment and mother-child interactions (Bee et al., 1982; Richman, Miller & Levine, 1992). Maternal education may also benefit children indirectly by increasing maternal earnings and family income. Nevertheless, surprisingly little is known about the causal nature of this relationship. Most studies estimating associations between maternal education and child outcomes are correlational and therefore likely biased by the omission of many spurious factors (e.g., maternal personal endowments such as verbal ability) that could be driving the maternal education-child development association.

We avoid these biases by using 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 maternal education on children's academic school readiness. Welfare recipients with young children residing in three sites in the NEWWS-COS sample were randomly assigned to participate in either an education-first program or a work-first program. For comparison purposes, a control group that received no additional programming was included in each site. Approximately two years after random assignment a follow-up survey collected detailed information on the program and control group members' education and employment experiences. At this time, mothers also answered questions about their children's problems in school, and the Bracken Basic Concepts Scale/School Readiness Composite was administered to a focal child between the age of five and seven years old in each family. Because conventional regression (e.g., OLS) and analysis of variance (e.g., ANOVA) methods may lead to estimates that are biased due to omitted variables, we take advantage of the random-assignment nature of our data. We use experimentally induced differences in mothers' education to estimate Instrumental Variable (IV) models. The IV approach improves our ability to draw causal inferences by eliminating biases associated with omitted variables.

We find that increases in maternal education are significantly and positively associated with children's academic school readiness, and negatively associated with children's academic problems. The IV models produce larger, although less precise, estimates compared to the OLS models. Our estimates suggest that an additional nine months of education causes a .23 standard deviation increase in children's academic school readiness, and between a .26 and .52 standard deviation decrease in school problems. These effects are large enough to be of considerable importance for the policies affecting the work, welfare and training of low-income mothers.

Background

Maternal education has been deemed by some the single best predictor of children's later intellectual functioning because of its ability to consistently predict children's cognitive and academic outcomes across different measures and populations (Bee et al., 1983; Smith et al., 1997). Duncan and Brooks-Gunn (1997) coordinated analyses among 12 groups of researchers working with different developmental datasets. Eight of the datasets included measures of both maternal education and children's academic or cognitive outcomes at various points in childhood and adolescence. Analyses conducted with all of these datasets indicated that maternal education was positively and significantly associated with children's cognitive and educational outcomes (Duncan & Brooks-Gunn, 1997). For example, Smith, Brooks-Gunn and Klebanov (1997) conducted analyses with two samples of young children from different datasets. They found that maternal education was positively associated with measures of children's intelligence at 2-, 3-, and 5- years of age in the Infant Health and Development Study sample, and children's verbal ability at 3- and 4- years of age, as well as math and reading achievement at 5- and 8-years of age in a sample drawn from the National Longitudinal Survey of Youth.

Maternal education is associated not only with children's academic achievement, but also their academic difficulties such as grade retention and special education placement. Both grade retention and special education placement in elementary school are an indication of severe academic problems (Dauber, Alexander, & Entwisle, 1993). Holloman, Dobbins and Scott (1998) found that maternal education was negatively associated with special education placement by 10 years of age, particularly for a learning disability. Children of mothers with less than a high school diploma were twice as likely to be in special education as children of mothers with a high school diploma. Byrd and Weitzman (1994) found that children of mothers who did not graduate from high school were 1.4 times more likely to repeat kindergarten or first grade compared to children of mothers who had graduated from high school. Despite being used as tools for remediation, grade retention and special education placement do not seem to improve, and may even harm, a student's subsequent educational achievement (Reynolds & Wolfe, 2000; McCoy & Reynolds, 1999).

Unfortunately, most work in this area cannot attribute the maternal education-child development correlation to mothers' education per se, as opposed to genetic differences or other characteristics that differentiate individuals who acquire different levels of schooling. We know of no studies that link experimental increases in maternal schooling and children's development. Two noteworthy non-experimental studies, both using data from the National Longitudinal Study of Youth (NLSY), take 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. To estimate whether children's achievement and behavior improved after their mothers returned to school, Kaestner and Corman (1995) associated the increases in maternal schooling to children's individual differences in scores on the Peabody Individual Achievement Tests (PIATs) when children were ages 5- and 7- years old. They found no effect of increased maternal education on children's achievement scores.

To estimate whether achievement and behavior differences between earlier- and later-born siblings is related to increases in mother's formal schooling, Rosenzweig and Wolpin (1994) associated increases in mother's educational attainment to differences in siblings' scores on achievement tests. In contrast to the Kaestner and Corman, they found that an additional year of maternal schooling had a modestly positive and marginally significant effect on children's PIAT scores for children ages 5- to 9- years old. Interestingly, they found that mothers' enrollment in school during a child's first three years had a significant and large positive effect on children's scores on the Peabody Picture Vocabulary Test (PPVT), a measure of children's receptive vocabulary, for children ages 3- to 9-years old, but no effect on children's PIAT scores.

Why might children with more highly educated mothers show higher levels of cognitive development and academic achievement as well as fewer academic problems? Higher levels of maternal education have been associated with higher quality home learning environments and mother-child interactions as well as greater maternal participation in children's schooling (Bee et al., 1982; Richman, Miller & Levine, 1992; Stevenson & Baker, 1987). The association between maternal education and teaching strategies may be of particular importance for the early school success of young children. Laosa (1980, 1983) demonstrated that a mother's educational attainment correlates with her teaching style with her preschool-age children. Specifically, a higher level of education was related to a mother's greater use of verbal reinforcement, inquiry, modeling strategies and reading to children. Although Laosa and others' work (Richman, Miller & Levine, 1992; Stevenson & Baker, 1987) provides a theoretical explanation of the effect of maternal education on children's school outcomes, it cannot rule out alternative explanations of the maternal education-child development correlation. Consequently, all told, the spare and inconsistent nature of the evidence makes it impossible to reach conclusions regarding the role of parental education *per se* in promoting the academic and cognitive development of children.

Data sources

Our data come from the National Evaluation of Welfare to Work Strategies Child Outcomes Study (NEWWS-COS). The NEWWS was an experimental evaluation of the Job Opportunity and Basic Skills Training (JOBS) program conducted by the Manpower Demonstration Research Corporation (MDRC) under contract to the 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 that they participate in work and training activities in order to receive full cash benefits. 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 aspects of client's family life and their children's 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 the age of three and five. This child was designated the "focal child," and detailed developmental data were subsequently collected about this child. Between 1991 and 1994, approximately 3,700 families were randomly selected to be enrolled in the NEWWS-COS. Once enrolled, mothers in the Child Outcomes Study sites were randomly assigned to one of two JOBS program streams or to a control group. In each COS site, one JOBS program emphasized Human Capital Development (HCD) and another focused on Labor Force Attachment (LFA).

In Riverside, pre-existing state regulations pertaining to Adult Basic Education 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 either a math or literacy appraisal exam (see Hamilton, Brock, Farrell, Friedlander, & Harknett, 1997). Mothers not in need of basic skills training were assigned to either the LFA program or to the control group. Mothers in need of basic education were randomly assigned to one of the two treatment streams or to the control group. This resulted in some non-comparability of HCD treatment groups across the three sites. 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 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 at least thirty hours a week. If they were not exempt, and did not satisfy the participation mandates, program mothers were sanctioned for non-compliance. The sanction resulted in the reduction of a client's welfare benefit in the amount of the client's portion of the welfare grant, but left the children's portion intact.[1] The 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 type and sequence of activities offered. HCD programs emphasized a long-term investment in welfare recipients' skills thought important for obtaining higher-paying jobs. Consequently, the HCD program mandated educational activities such as high school, General Education Degree (GED) preparation, Adult Basic Education (ABE), English as a Second Language (ESL), vocational training, or college.[2] 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 transition 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. 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. 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 intake staff collected information on Standard Client Characteristics (SCC) for all participants. Information was collected on mothers' prior welfare receipt, past educational attainment, current educational activities, employment history, and other 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 their math and reading skills.

Approximately two years after randomization, trained interviewers administered a follow-up survey to all control and program group members in their homes.[3] Follow-up survey data was

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[1] 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 $120 decrease in a monthly grant of $624 in Riverside.

[2] For the most part, the JOBS program did not develop new educational programs; rather participants attended already existing programs in their communities. Consequently, the type of education that mothers received in these programs was not unique to the experimental context of NEWWS.

[3] 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 Spanish.

collected for 3,194 (or 87 percent) of enrolled mothers and their children. The final sample for analyses of the two-year Child Outcomes Study and for the present study consists of 3,108 (or 94 percent) of survey respondents' and their children.[4]

The follow-up survey collected detailed information from participants about their education, employment, and job training experiences since baseline. Mothers answered questions about their family life as well as the focal child's development, behavior, school experiences, and overall health. At this time, the interviewer also assessed the focal child's academic school readiness.

Administrative data on employment, earnings and welfare receipt were collected for all participants for the year prior to and the two 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 respective states. Finally, UI records only reflect earnings from formal employment and do not include 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 two or more years, and at least a third of participants in each site did not have a high school diploma or a General Education Development (GED) degree. 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 degree or General Education Degree (GED). Finally, Grand Rapids had the highest proportion of mothers who were attending educational programs at the time of random assignment.

Measures

Our variables are organized into three sets of measures: baseline characteristics; JOBS program mediators (i.e., pathways through which JOBS may have affected children and families); and our child outcomes-- children's academic school readiness and academic problems. Except when noted, all measures were taken from the dataset created by MDRC and Child Trends for the NEWWS COS study.

Baseline Characteristics: In order to control for differences in clients' characteristics prior to random assignment we included a set of baseline variables in our estimation models as covariates.[5] Because we were missing data on some baseline measures, we constructed a set of missing data dummy variables, and replaced the missing values with a value of one. These missing data variables allowed us to include cases in our study despite missing values on baseline characteristics. Appendix Table 1 gives the means for the full set of control variables, including the missing data dummy variables.

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[4] A total of 176 respondents to the two-year follow-up survey were dropped from the Child Outcomes Study analysis sample for the following reasons: 1) the focal child was not the mother's biological or adoptive child; 2) the interviewer had to drive over 100 miles for the interview; 3) the child was older than eight and one half years old and therefore the family was ineligible for NEWWS-COS; and 4) the mother and child had not seen each other for the past three months. See Chapter 2 in McGroder et al., 2000 for greater detail.

[5] Although random assignment should result in comparable treatment and control groups, we include baseline covariates in our estimation models to enhance the comparability of the program and control groups as well as increase precision of our models' estimates. The baseline covariates that we use in estimation models differ slightly from those used in McGroder et al. (2000).

Our set of baseline covariates included dummy variables denoting the following standard client characteristics at of baseline: being currently enrolled in GED preparation, Adult Basic Education (ABE), vocational training, and 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. We included continuous measures of the mother's age and the number and ages of her children. Composite measures of the mother's depressive symptoms, locus of control, sources of social support, family barriers to employment, and cumulative number of baseline risk factors, constructed from items in the POS, were also included in the set of baseline covariates (Appendix 2 lists the items comprising these composite variables).

Our set of covariates included two continuous 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.

We included measures of mothers' basic literacy and numeracy skills as covariates in our models. In all three sites the same math test, developed by the Comprehensive Adult Student Assessment System, was used determine client's basic math skill levels. 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. MDRC converted the GAIN reading scores to be comparable to the TALS scores, in order to facilitate across site comparisons.[6] A Spanish version of the math test, but not the literacy test, was available.

Finally, in our analyses we control for the focal child's gender and the age at the time that the 24-month survey was conducted. We included a dummy variable that had a value of 1 if the focal child was a boy and 0 if the child was a girl. We created a set of eight dummy variables each of which captures a 3-month interval to control for the child's age (for details see Appendix Table 1).

<u>Education and Employment:</u> Table 2 presents the descriptive statistics, by site, for our measures of educational activity, employment, and earnings between baseline and the 24-month survey.

We constructed measures of the number of months mothers participated in each of five educational activities: high school, ABE, vocational training, ESL, and college. In the 24-month client survey mothers were asked to provide the month and year that they started and stopped attending a particular type of educational program over the two-year program period. If a mother had reported starting and stopping an activity in the same month, we assigned a value indicating that she had spent two weeks in this activity. In addition, because some mothers attended more than one type of educational program, we created a measure of the total number of months that respondents had participated in all of the five types of educational activities. The resulting values of these measures ranged 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).

A measure of the number of quarters that a client was employed during the two-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. In our sample, the number of quarters that clients were employed ranged from 0 to 8, and the average number of quarters employed ranged from 2.0 to 3.5 across sites (Table 2).

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

Outcome measures: 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 eleven subtests included in the full BBCS. The SRC is comprised of sixty-one questions that ask children about their knowledge of colors, letters, numbers/counting, comparisons and shapes. This test has high internal reliability consistency in this sample (Cronbach's Alpha=.97), and prior research has demonstrated the reliability and validity of the study with similarly disadvantaged populations (Polit, 1996).

Although the BBCS/SRC scores can be translated into age-standardized nationally normed scores, for purposes of interpretation we use the raw scores in our analyses, and include a set of dummy variables to control for the child age at the time of assessment. The scores ranged from 0 to 61, indicating the number of concepts correctly identified. The average score ranged from 45 to 48 across sites.

Measures of academic problems for both the focal child and for any child in the household 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 in the time since random assignment. Mothers then identified whether this was the focal child or another child in the family. A response of yes to either question was given a value of one, and the answers to the two questions were summed to create an index of academic problems for the focal child and for any child in the family. The resulting scores ranged from 0 to 2; the average for focal children ranged from .07 to .20 across sites, and the average for any children ranged from .23 to .37 across sites.[7]

Methodology

Our two-stage Instrumental Variable approach to the estimation of the impacts of maternal education on child outcomes requires that we first establish that assignment to one of the JOBS treatment stream affected clients' participation in educational activities. To do this, we calculate descriptive statistics of JOBS clients' participation rates in educational activity, and among those who participated, the average length of their participation. We then estimate program impacts on mothers' educational activities, employment, and children's academic school readiness and academic problems by site.[8]

We obtain program impacts by running separate OLS regressions on the JOBS program mediators and our child outcomes on program treatment status, controlling for the set baseline covariates. In the regressions the coefficient associated with the program group variable represents the experimental impact of the program treatment (HCD or LFA) on mothers' participation in educational activities.[9]

After establishing that the JOBS programs significantly affected clients' educational activity, we turn to answering our primary research question-- What is the effect of increasing maternal

[7] Our measures of academic problems differ from the dichotomous measures of academic problems presented in McGroder et al. (2000) because they are better suited to IV analyses.

[8] All analyses were weighted to adjust for cohort differences in the random assignment of clients as well as to allow generalizations to the AFDC eligible county populations. Details of the weights are reported in McGroder et al., 2000. All analyses presented in this paper were also run without weights to confirm that the weights did not substantially alter estimated coefficients.

[9] In order to distinguish between the effect of HCD and LFA programs, we estimate a different model for each experimental treatment status in each group. For example, when regressing Atlanta HCD treatment status on months in educational activities, we exclude form the analysis mothers who were assigned to Atlanta's LFA treatment group.

education on children's academic school readiness and academic problems? Here we take advantage of the experimentally induced differences in mothers' schooling to estimate a Two Stage Least Squares Instrumental Variable (IV) model. For purposes of comparison, we also estimate conventional OLS models of the relationship between maternal schooling and the child outcomes.

<u>Estimation Models</u> Our OLS model, the kind traditionally used in this type of research, is presented below:

Child's school outcome$_i$= $\lambda_0 + \lambda_1$(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 $\lambda_{1,}$ which represents the increase in a child's academic school readiness score associated with an additional month of a mother's participation in an educational activity. The OLS model provides unbiased estimates of coefficients under the assumption that the error term (ξ_{i1}) is not correlated with the independent variables. Unfortunately, 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 OLS coefficient.

Researchers typically worry that non-experimental data will impart an upward bias to estimates of the effect of maternal education on children's outcomes (Resnick, Corley, & 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 measured and included in the OLS models. For example, most studies of the effect of education lack a good measure of the mother's genetically-endowed cognitive ability. By omitting this variable, researchers may mistakenly attribute positive children's outcomes to maternal schooling rather than to mothers' personal endowments, thereby overestimating the effect of maternal schooling on children's school outcomes.

Even in the context of a random assignment 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, who complies with the participation mandate throughout the course of the program is 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, we expect that our OLS estimates of the effects of maternal education on children's school related outcomes to be *downwardly* biased. By program design, mothers with low basic skills were directed to educational activities and generally continued in these activities until their skills improved (Hamilton et al., 1997). Therefore mothers spent more time in educational activities if it took them longer to acquire the necessary skills. We can think of three reasons that some mothers took longer to acquire the necessary skills than other mothers. They might have had lower skills to begin with, been slow learners, or been unmotivated to learn. All of these characteristics are likely to have been negatively associated with children's academic school readiness and positively associated with children's academic problems. Although we have a baseline measure of numeracy and literacy skills, we have no measure of how motivated mothers were to improve their skills or whether they are relatively fast or slow learners. Therefore, we expect that omitting these variables may lead OLS models to underestimate the effect of maternal schooling on children's school outcomes.

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

(2) Mother's number of months in educational activity$_i$ = $ß_0$ + $ß_1$(Experimental status$_i$) + $ß_2$(Baseline maternal characteristics$_i$) + $ß_3$(Baseline child characteristics$_i$) + ξ_{i2}

(3) Child's school outcome$_i$ = Φ_0 + Φ_1(Predicted mother's number of months in educational activity$_i$) + Φ_2(Employment$_i$) + Φ_3(Baseline maternal characteristics$_i$) + Φ_4(Baseline child characteristics$_i$) + ξ_{i3}

In the first stage of the estimation, the cumulative number of months a mother is in educational activities is the dependent variable and is predicted by our set of baseline covariates plus the experimental status of the mother (HCD, LFA, or control group).[10] Because experimental treatment status is, by design, unrelated to mothers' characteristics 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, Φ_1 is estimated by replacing the actual number of months in education gain with the predicted number of months in education obtained in the first stage.[11]

Using both HCD and LFA assignment as instruments will provide unbiased and causal estimates under the following five assumptions outlined by Angrist, Imbens, and Rubin (1996).[12] 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 is not likely to be affected by the random assignment of other participants to HCD, LFA, and control conditions. Third, the treatments must have a non-zero average effect on the outcome of interest. That is, the effect of maternal education on children's academic school readiness cannot be positive for some children, and negative for others leading to an average effect of zero. Fourth, none of the clients must do the exact opposite of their assigned treatment, no matter what the assignment. We must assume that no one who is assigned to the HCD

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[10] The coefficients on the program status variables in the first stage of estimation are nearly identical to the experimental program impacts. Slight differences in the values are the result of differences in the size and significance of control variable coefficients in site-specific analyses compared to those for control variable coefficients for full sample.

[11]Simple algebra applied to the case in which there is only one site and one experimental program status show that the IV estimate is the ration of the program impacts on child outcomes to program impacts on maternal education. In first stage of the IV analysis, we regress mother's education on a dummy of experimental status 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) we arrive at the following:

child's outcome$_i$ = Φ_0* + (Φ_1* $ß_1$)experimental status$_I$ + ξ_{I1}*

Letting α = (Φ_1* $ß_1$) the equation can be rewritten as:

child's outcome$_i$ = Φ_0* + α experimental status$_I$ + ξ_{I2}*

By regressing experimental status on the child's outcome we obtain an OLS estimate for α. With values for both α and $ß_1$ we can easily solve for our parameter of interest ($\Phi_1 = \alpha / ß_1$). Because the estimate of α is also the program's impact on the child's outcome, the IV estimate is the program impact on the child outcome divided by the program impact on mothers' months in education. However, in IV analysis, the standard errors in the second stage are calculated with the residual sum of square using the actual value of the regressor. We use the SAS statistical package to obtain correct standard errors.

[12] For a more detailed discussion of using IV methods to estimate the mean effect of treatment on the treated see Heckman (1997).

program would participate in educational programs if assigned to the control group, but would not participate if assigned to the HCD group. Finally, we must assume that the only way in which the treatments affect the outcomes of interest (children's school outcomes) is through their effect on the mediator of interest, in this case, maternal educational activity.

The first four assumptions are easily met given our use of experimental data and our choice of instruments. However, if the HCD and LFA programs affected children through pathways other than mothers' educational activities, Angrist et al.'s fifth assumption, the "exclusion assumption," is violated. Because the JOBS program, including the HCD program, was primarily 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.[13] We try to meet the exclusion assumption by predicting employment in the first stage of our IV model, and including it in the second stage. However, if there are still other pathways by which JOBS affected children that we have not included in our IV models, we have not satisfied the exclusion assumption.[14]

For identification purposes, an instrumental variable model needs at least as many instruments as mediators (Davidson & MacKinnon, 1993). That is, if we wish to create predicted values for both education and employment, we need to have a set of at least two instruments, and preferably more, to improve identification of these program mediators. Fortunately, we have two different program streams in three different sites. We 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.[15] Taken together these variables are exogenous and meet the criteria above for causal inference if they predict the program mediators, but do not predict children's academic school readiness and problems except through the mediators.[16]

Weak instruments in IV models are problematic (Bound et al., 1995; Staiger & Stock, 1994). R-squared and F-statistics from the first stage of two-stage least squares provide an assessment of the strength of the correlations between the set of instruments and the variables they are to predict. Staiger and Stock (1994) argue that 10 is the minimum acceptable value of the F-statistic 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 we do not estimate the effect of a mother's cumulative educational attainment on a child's academic school readiness, but rather consider the effect of her involvement in educational activities only over the post-random assignment period. We do this because the IV model is based on the premise that we can predict our independent variable of interest, maternal education, with our treatment stream interaction terms. Across sites and treatment streams, mothers differed

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

[14] 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 (such as maternal employment) may lead to biased estimates of the effects of maternal schooling in both the IV and OLS models. It is not necessary to model mediators representing the hypothesized pathways through which maternal education affects children (such as the child's home learning environment and parenting).

[15] We control for site in our model, but do not use differences across sites (unrelated to program treatment streams) to identify the program mediators.

[16] The variation in program impacts on education is not surprising. Because JOBS largely utilized existing educational programs, the type of education that clients participated was in part shaped by the educational opportunities that existed in the communities. For example, in Michigan the state spent more funding on Adult High School education than it does on ABE. Similarly, Atlanta had better links to vocational education organizations and services (Hamilton et al., 1997).

more in their educational activity over the two years of the program than in their overall educational attainment. Consequently, we were better able to predict the number of months that mothers spent in education over the two-year follow-up period as opposed to mothers' overall educational attainment.

We estimate two different OLS and IV models for our measure of children's academic school readiness and academic problems. In the first model we include one program mediator, a measure of mothers' months in educational activity, along with our baseline covariates. In the second model we introduce employment as an independent variable.

Results

Program Participation and Program Impacts: The top panel of Table 3 presents the participation rates of the HCD clients in educational activities, by type of education and site, and the bottom panel presents the same information for LFA clients. 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 two-year survey. LFA clients participated in educational activities as well, but at lower rate 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 mothers in Grand Rapids' LFA program (21 percent) and HCD program (19 percent) as well as Riverside's LFA program (15 percent) reported that they had taken college classes.

Of the mothers who participated, the average number of months spent in educational activities is presented for HCD and LFA clients in Table 3. Among participating mothers in the HCD program group, the average length of participation in all educational activities was just over eight months. Mothers in Atlanta had slightly longer participation spells than did mothers in Riverside and Grand Rapids. For mothers in the LFA program group who participated in educational activities, the average number of months was slightly lower-- 7.75 months. Program mothers in Atlanta, once again, 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).

Table 4 presents the HCD and LFA experimental program impacts on maternal educational activity, as well as quarters of employment and two-year earnings. Columns one through three present the HCD program impacts and columns four through six present the LFA program impacts. For example, the first column of the first row indicates that, on average, HCD clients in Atlanta obtained a statistically significant 1.50 more months of ABE in the 24 months post-random assignment than the control clients in Atlanta did.

The HCD programs in Atlanta and Riverside had positive impacts on mothers' educational activities. HCD clients in Grand Rapids did not obtain significantly more education than their control counterparts. In contrast, only one of the LFA programs had positive impacts on mothers' education: LFA clients in Atlanta participated in significantly more ABE activities and vocational training than their control counterparts. Grand Rapids' and Riverside's LFA programs had a negative program impact on mothers' participation in some educational activities.

In general, JOBS programs also had positive impacts on participants' employment. However, the program impacts were not uniform across either program types or sites. In all of the LFA programs, experimental group mothers were employed for more quarters over the two years than controls mothers were. The strongest LFA program impacts on employment occurred in Riverside. Among the HCD programs, only the Riverside mothers were employed for significantly more quarters than their control counterparts.

Finally, there were no significant differences between the HCD and control group in children's SRC scores. Among the LFA programs, only Atlanta's program had a positive impact on children's SRC scores. Children of mothers in Atlanta's LFA clients scored 1.65 points higher on the SRC than children of mothers in the control group (an effect size of .13).[17] Assignment to either LFA or HCD program treatment groups did not appear to have affected mothers' reports of the focal child's academic problems. However, both Atlanta and Riverside HCD sites significantly reduced mothers' reports of academic problems for any child, whereas the LFA programs did not affect mothers' reports of these academic problems.

<u>OLS and IV Model Estimation</u>: Table 5 presents the first stage IV coefficients and standard errors associated with months of education and employment for our treatment status instruments.[18] 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 to the HCD treatment in Riverside predicts employment. The F-statistics and R-square values are presented in the bottom three rows of the table. Our set of instruments significantly predicts both hypothesized JOBS mediators. In the case of education, the F-statistic is above the recommended value of 10, and in the case of employment, the F-statistic is very close to 10 (F-statistic = 9.63).

The first panel of Table 6 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. 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 BBCS/SRC scores (p-value < .05). The second column shows that, by IV estimation, an additional month of a mother's education is significantly associated with a .305 increase in a child's score (p-value < .10).[19] Columns three and four show that including the predicted value of quarters of employment in the model has virtually no effect on estimated effect of maternal schooling. The OLS coefficient in the second model translates in an effect size of .07 for the average participant in HCD educational programs. The IV coefficient from the second model translates in an effect size of .23.[20]

The second panel of Table 6 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 two-year follow-up period. The OLS findings suggest that an additional month of maternal

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[17] In Atlanta, the standard deviation of the BBCS-SRC scores was 11. The effect size of .13 was calculated by dividing the LFA program impact of 1.65 by 11.

[18] The coefficients associated with the program status variables in the first stage of the IV analysis are very similar to program impacts presented in Table 4. As noted earlier slight differences are attributable to differences in the value of the coefficients associated with control variables within sites compared to across sites. For example the experimental impact of Atlanta's HCD program on months participating in any educational activity was 2.32. The coefficient in the first stage of the IV analyses for the corresponding measure is 2.36.

[19] In a simple version of this model of one experimental program in one site, footnote 8 explains that the coefficient associated with a month of education is the program impact on children's academic school readiness divided by the impact on the months of education. For example, in Atlanta the impact of the HCD program on the BBCS/SRC is .80 and the impact on months in education is 2.32, consequently the estimate of the effect of an additional month of ABE on a child's BBCS/SRC would be (.8/2.32)= .34, which is close to the .305 coefficient in Table 6.

[20] For purposes of interpretation, we translate our OLS and IV coefficients into effect sizes by dividing the estimates by the full sample standard deviation for each outcome measure.[20] In order to describe the average effect size among those who participated in HCD educational activities, we multiply these monthly effect sizes by 8.1, the average months spent in educational activity among these clients.

education is not associated with a change in the focal children's academic problems. In contrast, the IV estimate suggests that an additional month of maternal education results in .012 fewer educational problems for focal children. Columns three and four show that 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 maternal 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.

The third panel of Table 6 presents the results of the OLS and IV estimates for the effect of cumulative months of educational activity on any child's academic problems of any child in the family. The first OLS model finds that an additional month of maternal education is significantly associated with a .01 reduction in children's academic problems, and the IV estimate suggests that an additional month is associated with a .031 decrease in the academic problems of any of the mothers' children. Again, adding in the number of quarters of maternal employment as an independent variable does not change the size or significance of either the OLS or IV estimates. The OLS estimates yield an effect size of .08, while the IV models translate into an effect size of .48.

<u>IV Extension Analyses</u>: In order to test the robustness of our findings we estimated two additional types of IV models of the effect of maternal education on children' academic school readiness. We limited our analyses to children's academic school readiness because we felt that the Bracken was a more sensitive measure of children's school outcomes. First, we estimated models of the effect of maternal education on children's school readiness, controlling for three other potential pathways by which the JOBS program may have affected children. Second, we estimated models for two subgroups of the sample-- those who were considered "in need" of basic education and those who were considered not in need of basic education.

Although our earlier findings suggested to us that the effect of maternal education on children's academic school readiness was not reduced by controlling for employment, we wanted to be confident that controlling for changes mothers' economic resources did not change the association between maternal education and children's academic school readiness. We chose to include measures of the mother's two-year total earnings and welfare, as well as whether the mother was sanctioned for non-compliance with the participation mandate. Our point of departure for these analyses was the basic model in which we use program treatment status by site interaction terms to predict maternal educational activities in the first stage, and then use the predicted months of education to predict children's BBCS/SRC score in the second stage. In subsequent models, we included the measures of mothers' earnings and AFDC receipt over the two-year period and whether the mother had been sanctioned after random assignment 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 were consequently lower (4.0 and 7.4 respectively) although still significant. In contrast, sanctioning did vary across the programs and the instruments significantly and strongly predicted whether a mother was sanctioned. The F-value associated with the instruments in the first stage estimation was quite high (34.0).

In the analyses that included measures of two-year total earnings and AFDC receipt, the coefficient associated with an additional month of maternal education was similar in size and significance to the estimates presented in Table 6. In the analyses that included whether the mother had been sanctioned, the coefficient associated with months of educational activity doubled in size from approximately .31 to .62. However, the standard error associated with this coefficient increased from .24 to .59, and consequently the coefficient was no longer statistically significant. Looking across these analyses, we found that the coefficients associated with earnings, welfare receipt, and sanctioning were neither statistically significant nor consistent in size or direction.

Finally, we wanted to determine whether the effect of mothers' participation in education on children's academic school readiness differed depending on their initial level of basic skills. We split the sample into those who were considered in need of basic education (n=1398) and those who were considered not in need of basic education (n=1447). Mothers were determined to be in need if at baseline 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. We estimated an IV model of the effect of mothers' months in education on children's BBCS/SRC scores separately for the subgroup of mothers who were in need of education and for those who were not in need. In these models we controlled for the number of quarters that mothers were employed over the two-year period.

Mothers in need of education participated in significantly more months of education than those who were not in need did, and they primarily attended basic skills educational classes such as ABE or high school/GED. Among mothers who were in need of education, each of the three HCD programs had significant program impacts on the number months that mothers spent in educational activities, but no significant program impacts on vocational education. In contrast, among mothers who were considered not in need of education, only one JOBS program, Atlanta HCD, had a positive impact on the number of months that mothers spent in educational activities, and nearly all of the program impact is accounted for by months of vocational training.

Our full set of program treatment stream-by-site interaction instruments were 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 and, consequently, the strength of the F-values reflected this difference. Among mothers in need of education the F-value associated with our instruments in the first stage of the IV estimation was 22.59 for months in any educational activity and 7.45 for the number of quarters of employment. Because mothers in Riverside who were not in need of education were not assigned to the HCD program, assignment to HCD treatment group in this site could not be used at an instrument in the first stage of the IV analyses for the not in need subgroup. The F-value associated with the remaining five instruments in the first stage of the IV estimation for these mothers was only 4.39 for months in education and 4.42 for quarters of employment.

In the IV analyses of the subgroup of mothers who were in need of basic education, a month increase in mothers' education was associated with a .33 increase in children's BBCS/SRC score (standard error = .18; p-value <.10). When we added a control for the number of quarters that mothers were employed, the coefficient associated with education increased to .34 and remained statistically significant (standard error = .19; p< .10). In IV analyses of the subgroup of mothers that were considered not in need of education, the coefficient associated with our measure of months in education was .17 but the standard error was .36, and therefore the estimate was not significant. When we included our measure of employment in the IV estimation the coefficient associated with months in education decreased to .16 and remained insignificant. In both models the coefficient associated with mothers' number of quarters of employment was positive, but not significantly different than zero.

Discussion

Although many studies report correlations between maternal education and children's cognitive development, the instrumental variable approach we use provides a more convincing estimation of the *causal* effect of a mother's education on her children's academic problems and school readiness. We find evidence that increases maternal education improves children's academic outcomes. Experimental comparisons of the control and HCD program groups in each site did not yield significant program impacts on children's academic school readiness, but by pooling data across sites in an instrumental variable analysis we find that an additional month of maternal education is positively associated with children's academic problems and school readiness.

As expected our experimental findings confirmed earlier reports that assignment to the Human Capital Development (HCD) treatment of the JOBS program significantly increased mothers' participation in educational activities (McGroder et al., 2000). The relative strength of the program impacts varied by the type of educational activity, and the program treatment stream, and we use the variability in these experimentally induced differences in educational activities to estimate IV models. In the first stage of the IV estimation, we used a set of treatment status-by-site dummies as instruments to predict the pathways to economic self-sufficiency targeted by the JOBS program. In the second stage, we estimated the effect of these predicted values on our outcomes of interest, children's academic school readiness and children's academic problems. Given two randomly assigned treatment streams in three sites and variation across these six programs in impacts on educational activity and employment, we argue that we meet the set criteria for drawing causal inference from IV estimations (Angrist et al., 1996).

Although the IV models are better able to estimate causal linkages by purging our measures of 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 process results in larger standard errors than OLS models, and consequently the 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), and fortunately this did not occur in our analyses. Because the potential for omitted variable bias is often overlooked, we recommend that researchers pay more attention to the problem of endogeneity in their estimation models, and that they seek utilize a variety of strategies reduce biases associated with it (Duncan, Magnuson, & Ludwig, 1999). However, we also recommend that researchers 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 school readiness, 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. Typically, correlational research finds that an additional year of maternal education results in .15 to .25 of a standard deviation increase in child's achievement test scores. We estimate that nine months of educational activity would increase school academic readiness by .24 of a standard deviation (representing approximately three more questions answered correctly), and reduce children's academic problems by .29 to .53 of a standard deviation. In contrast, our OLS models suggest that an additional year of education would result in a .07 of a standard deviation increase in children's academic school readiness, and reduce children's academic problems by .11 of a standard deviation.

Our analyses of the subgroup of mothers that were in need of basic education at the time of random assignment provides strong evidence of an association between mothers' participation in educational activities (largely basic education classes, such as such as ABE or high school) and children's academic school readiness in this subgroup. Our analyses of the subgroup of mothers that were not in need of basic education did not provide similarly strong evidence of a association between mothers' participation in educational activities (primarily vocational training) and children's academic school readiness. There are two possible reasons that our findings of the positive effects of maternal education are more consistent among the mothers in need of basic education than the mothers not in need of basic education. First, our more limited set of program instruments was a relatively weaker predictor of mother's 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. Second, mothers who were not in need of basic skills education but did participate in educational activities were primarily enrolled in vocational training programs, and it may be that mothers' participation in vocational training has a smaller effect on children's academic school readiness than their participation in basic skills education. Although our data confirm that basic skills education has a positive effect on children's school outcomes, we are unable to determine whether or not vocational training has a positive effect on mother's who are not in need of basic skills education.

The resulting effect sizes from our IV analyses are considered large to scholars compared to the effect of other influences on children's academic school readiness (Bee et al., 1982), but are these effects policy relevant? The answer to this question ultimately rests on the extent to which school readiness as measured by the Bracken Basic Concepts scale/School Readiness Composite and early academic problems are associated with children's long term school achievement or other important outcomes. Prior research has found that school readiness and early academic problems are important predictors of future school success (Enswistle & Alexander, 1993) and this study provides compelling evidence that programs that increase maternal education will likely improve children's academic school readiness and lessen academic problems.

Current welfare-to-work strategies have eschewed the human capital approach in favor of the work first approach. However, the emphasis on moving mothers into the work force, may also limit their opportunities to improve their basic skills through education. Because maternal education benefits children, policymakers should continue to consider ways and develop ways to improve mothers' education. Our 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. Consequently, policymakers should consider a variety of ways to increase maternal education in combination with employment, but also through other program and policies.

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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 2yr 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*

Table 2: Means and Standard Deviations of Educational Measures, Employment, Earnings, School Readiness, and Academic Problem Measures

	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 Two-year Earnings	$ 5,523	$ 7,821	$ 3,824	$ 8,279	$ 6,119	$ 9,235
Dependent Variables						
Bracken Raw Score	48	11	45	13	46	12
Academic Problem-Focal child	.07	.28	.09	.33	.20	.45
Academic Problem-Any child	.23	.49	.27	.52	.37	.60
Sample Size	*1422*		*950*		*646*	

Table 3: Experimental Group Members' Participation Rates in Educational Activities

HCD Program Group	Atlanta	Grand Rapids	Riverside	All sites
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	Atlanta	Grand Rapids	Riverside	All sites
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				
Average Months if participated in …				
Adult Basic Education	6.6	4.7	3.1	5
Vocational Training	6.8	5.8	3.3	5.9
All Educational Activities	8.1	7.4	7.8	7.8

Table 4: Summary of NEWWS Impacts on Maternal Education, Employment, Children's School Readiness and Academic Problems, by Site
(Standard Errors in Parantheses)

	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)
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)
Academic problem-Any Child	-.06 ***	-.03	-.11 ***	.02	.06	.02
	(.03)	(.05)	(.04)	(.03)	(.05)	(.04)
Sample Size	*1026*	*426*	*577*	*902*	*441*	*258*

NOTES: P-values: *p<.10, ** p<.05, *** p<.01
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, mothers' 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.

Table 5: First Stage IV Coefficients, F-statistics, and R-squares (Standard Error 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-statistic 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, mothers' 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 6: IV and OLS Estimates of Months in Educational Activities on Children's Raw Bracken School Readiness Composite Scores and Academic Problems (Standard Error in Parentheses)

	Model 1: Bracken		Model 2:Bracken	
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 Problem-Focal Child		Model 2: Academic Problem-Focal Child	
Independent variables	OLS	IV	OLS	IV
Months in Education	.000	-.012 **	.000	-.011 *
	(.001)	(.001)	(.001)	(.006)
Quarters of Employment			-.001	.009
			(.002)	(.017)

	Model 1: Academic Problem-Any Child		Model 2: Academic Problem-Any Child	
Independent variables	OLS	IV	OLS	IV
Months in Education	-.005 ***	-.031 ***	-.005 ***	-.031 ***
	(.002)	(.009)	(.002)	(.009)
Quarters of Employment			.003	.029
			(.004)	(.028)

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, mothers' 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.

Appendix Table 1: Mean, Standard Deviation, Minimum, and Maximum values for all Covariates

	Mean	Std Dev	Min	Max
Baseline Covariates				
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
Child gender	.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 month	.11	.31	0	1
Focal child age between 64-67 months	.15	.35	0	1
Focal child age between 68-71 months	.16	.37	0	1
Focal child age between 72-75 months	.12	.32	0	1
Focal child age between 76-79 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

APPENDIX 2

Descriptions of Selected Baseline Covariates

Category/Variable	Description
Maternal Psychological Well-Being and Social Support	
External Locus of Control	Four items from the baseline POS were used to measure locus of control. An internal locus of control indicates that the mother generally feels that she has control over events in her life. An external locus of control indicates that the mother generally feels that events are outside of her control. The POS items asked the mother to indicate whether she agreed or disagreed with the following statements: "I have little control over things that happen to me." "I often feel angry that people like me never get a fair chance to succeed." "Sometimes I feel I am being pushed around in life." "There is little that I can do to change many of the important things in my life." Mothers indicating that they "disagreed" or "disagreed a lot" with each of the four items were coded 0, indicating a "more internal" locus of control. Mothers indicating that they "agreed" or "agreed a lot" with each of the four items were coded 2, indicating a "more external" locus of control. Mothers with both types of responses were coded 1, indicating a "neutral" locus of control.
Any of Three Sources of Social Support	Three items from the baseline POS were used to indicate whether mothers had access to any of three sources of social support. Mothers responded "agree a lot", "agree", "disagree", or "disagree a lot" to the statements: "If I got a job, I could find someone I trust to take care of my children." "When I have troubles or need help, I have someone I can really talk to." "When I have an emergency and need cash, friends and family will loan it to me." Mothers indicating that they "agreed" or "agreed a lot" with any of these three statements were coded 1, indicating access to some social support.

Category/Variable	Description
Many Depressive Symptoms	Four items from the baseline POS were used to measure the number and severity of depressive symptoms. Mothers answered “rarely”, “some/a little”, “moderate amount”, or “most or all days” to the items: “During the past week . . . I felt sad.” I felt depressed.” I felt that I could not shake off the blues, even with the help of family and friends.” I felt lonely.” Mothers answering “rarely” or “some/a little” on each item were coded 0, indicating few depressive symptoms. Mothers answering “a moderate amount” or “most/all days” on each item were coded 2, indicating many depressive symptoms. Mothers with both types of responses were coded 1, indicating “moderate” depressive symptoms.
Family Characteristics	
Barriers to Employment	Seven items from the baseline POS were used to measure the number of barriers to employment. Mothers responded “agree a lot”, “agree”, “disagree”, or “disagree a lot” to the statements: “I cannot go to a school or job training program right now because I… …have no way to get there every day.” …have a health or emotional problem.” …have a child or other family member with a health or emotional problem.” …cannot afford child care.” …already have too much to do during the day.” “My family is having so many problems that I cannot go to a school or training program right now.” “My family is having so many problems that I cannot work at a part-time or full-time job right now.” For each item, mothers indicating that they “agreed” or “agreed a lot” were coded 1, indicating the presence of this barrier. Mothers reporting 0 or 1 of these seven barriers were coded 0; mothers reporting 2 or 3 barriers were coded 1; and mothers reporting 4 to 7 barriers were coded 3.

Category/Variable	Description
Number of Baseline Risk Factors	Items from the SCC and POS, as well as scores on the literacy and numeracy tests, were used to create the following ten dummy variables, indicating the presence of the given family risk factor: • No high school diploma or GED • Mother is primary caregiver to three or more children under age 19 living in the household at baseline • Low literacy (level 1 or 2 on the TALS) • Low numeracy (level A or B on the CASAS math test) • Never having worked full-time for 6 or more months for the same employer • Five or more years on welfare • “Many” depressive symptoms • “More external” locus of control • Between 4 and 7 barriers to employment • No social support These ten dummy variables were summed to indicate the number of family risk factors, as of baseline. Mothers reporting between zero and three of these risk factors at baseline were coded 0; mothers reporting four or five of these risk factors at baseline were coded 1; mothers reporting between six and ten of these risk factors at baseline were coded 2.