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Identifying Effects of Income on Children's Development: Integrating an Instrumental Variables Analytic Method with an Experimental Design

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The authors welcome comments and discussion.

The Next Generation Project

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Introduction

Between 1975 and 1993 the percentage of children under age 6 living in families with incomes below the poverty line rose from 18 to 23 percent; while child poverty rates have declined in the post-1993 period, child poverty rates are still very high (Bennett & Lu, 2000). Past research suggests that reducing or eliminating the time a child lives in poverty may have large and lasting benefits. Children in poverty are more likely to experience poor health, to score lower on standardized IQ and achievement tests, and to be retained in grade or drop out of school (Haveman & Wolfe, 1994; Smith, Brooks-Gunn, & Klebanov, 1997). These effects are especially strong for children who persistently live in poverty, who experience poverty during the early childhood years, and who live in the deepest poverty (Duncan & Brooks-Gunn, 1997a; Duncan, Brooks-Gunn & Klebanov, 1994; Smith et al., 1997).

Is this documented relation between income and children's development causal and reversible? Research on the causal and reversible effects of poverty on children is still inconclusive. First, identifying causal relations under most commonly used empirical techniques is difficult. To understand this difficulty, consider the research that finds that children in poverty are more likely to experience poor health, to score lower on standardized IQ and achievement tests, and to be retained in grade or drop out of school (Haveman & Wolfe, 1994; Smith, Brooks-Gunn, & Klebanov, 1997; Duncan & Brooks-Gunn, 1997). Based on these studies, one might conclude that reducing or eliminating the time a child lives in poverty would have large and lasting benefits, and that interventions which aim to move families out of poverty benefit children. However, because parents in poor and nonpoor families may differ in a variety of unmeasured characteristics in addition to their income level, the effects of poverty on children may reflect these unmeasured differences, rather than their income level. Second, research to date has not attempted to identify the effect of increases in income on child well-being. Even if poverty were causally related to children's outcomes, the relation between income and children's outcomes may not be reversible. That is, if children are negatively affected by poverty, increasing income may not remove the previous disadvantage they have experienced and thus may not put them on the same footing as children in higher income families.

In this paper, we describe an analytic approach that can provide an unbiased estimate of the causal effect of income on children, yielding conclusions that can inform both theoretical and intervention research. The effect of poverty on children in the extant research is typically estimated using path analysis (regression analysis or structural equation modeling) using ordinary least squares (OLS), techniques that may provide biased estimates of causal relations.[1] The technique we describe in this paper, one that combines the use of random assignment design with an instrumental variables strategy, can better estimate the causal effect of income The key element that makes this technique especially successful is that random assignment of an individual or family to a treatment group or to a control group is uncorrelated with any observable or unobservable factors that may confound estimated effects on children's development.

This paper builds on earlier work (e.g. see James and Singh, 1978 and Foster and McLanahan, 1996) by describing the advantages of including an experimental design along with instrumental variables estimation. This combined approach is not new—others have written about the use of naturally occurring random experiments along with an instrumental variables method (see, for example, Angrist, Imbens, & Rubin, 1996). However, the recent emergence of data on children from randomized experimental trials aimed at parents' employment and income has made this technique a potential avenue to address questions of interest to developmental psychology. Because instrumental variables estimation is rarely used in empirical research in psychology, the first part of this paper discusses in detail the need for such a technique, how it can be implemented, and how, along with an experimental design, it may contribute to our understanding about causal processes. The second part of this paper presents findings from an analysis utilizing this technique, and discusses the implications of these findings.

Biases in correlational analyses

Three sources of bias may underlie correlational associations: omitted variable bias, simultaneity bias and errors in variables bias.[ii] These are well known to psychologists, but techniques to address these biases are rarely employed. To understand why correlational relations estimated by ordinary least squares regressions may be biased, consider the equation:

$$Y_i = \alpha + X_i\beta + \varepsilon_i \qquad (1)$$

where Y_i is the dependent variable of interest (e.g. a cognitive test score); X_i represents the independent variable of interest (e.g. poverty); and ε_i is the error term.

The ordinary least-squares slope estimator is:. $\hat{\beta} = \frac{\sum X_i Y_i}{\sum X_i^2} = \beta + \frac{\sum X_i \varepsilon_i}{\sum X_i^2}$. If $E(\sum X_i \varepsilon_i) = 0$ then $E(\hat{\beta}) = \beta$. Otherwise the least-squares estimator may be biased.

A relation between the independent variable and the error term may arise from several sources. First, for any two variables X and Y, there may be a third variable, Z, which is related to both X and Y and may make it appear that there is a relation between X and Y that does not exist (i.e. omitted variable bias). Second, bias can arise by attributing all of the correlational relation between an independent variable X on a dependent variable Y to the effect of X and Y and none to the effect of Y on X (i.e., simultaneity bias or reverse causality). Third, if there is error in the measurement of either the independent variable X or the dependent variable Y, then the relation between X and Y may be biased (measurement error, or errors-in-variables bias). These three sources of bias are presented in simplified form in Figure 1.

Selected empirical methods to identify causal effects

Some design and analytic techniques have been used to attempt to control for the biases addressed above in research estimating the effects of poverty on children. The most popular of these include (1) models with controls for family and child level characteristics and (2) fixed effects models. While both of these techniques have their own merits, neither may sufficiently estimate unbiased causal relations.

Models that Control for Family- and Child-Level Characteristics. Many studies add independent variables as covariates as a way to control statistically for observed differences among families (and sometimes even unobserved differences; see Mayer, 1997). For example, adding family structure in an analysis examining the relation between income and children's outcomes statistically controls for the higher incidence of single parenthood among poor families and for the confounding effect of family structure on children's well-being. Without controls for family or child characteristics the relation between X and Y is likely to be overestimated. The relation between income level and children's outcomes is reduced with the inclusion of family-level controls, such as family structure (Duncan & Brooks-Gunn, 1997). In theory it seems straightforward to include additional control variables. However, in practice, it is difficult to include all "omitted variables" that may confound the relation under study, either because not all "omitted variables" are readily available, are not easily observed or are presumed to be important. Although these models are an improvement over models that do not add control variables, least-squares estimates obtained from these models may still be subject to the biases discussed above.

Fixed Effects Models[iii]. Fixed effects models are used to control for unobserved characteristics in equations examining the relation between family circumstances and child outcomes by estimating the difference in the effects of an independent variable for two children in the same family (family fixed effects) or on a single child at two points in time (child fixed effects). The family fixed effects models estimate the difference between siblings' outcomes as the dependent variable. Then, the difference between siblings in their independent variable (for example, poverty) at the time of the assessment of the outcome variable is used as the independent variable. Because the unobserved family effects are assumed to be the same across siblings, they can be "subtracted out" of the difference equation.[iv]

Fixed effects models have been conducted to better estimate the effects of poverty on children using sibling data (Duncan, Yeung, Brooks-Gunn and Smith, 1998) and using multiple observations of the same child over time (Blau, 1999). In the study of the effects of income on children, the effects of income in fixed effects models are much smaller and often insignificant relative to those found in models which include only controls for family and child characteristics (Blau, 1999; Duncan, Yeung, Brooks-Gunn and Smith, 1998), implying that the fixed effects, or characteristics of families or children that do not change over time, and that have been "subtracted out," are very important to the model and are unmeasured in the models including controls for family characteristics. However, fixed effects models provide unbiased estimates *only* under the assumption that the unobserved effect is static across siblings or across time. This assumption implies, for example, that unobserved family effects, such as marital conflict, have a similar effect on girls and their brothers; or that child characteristics, such as delinquency, are similar from one developmental stage to another. If this assumption is violated, then the unobserved family- or child-level effect cannot be "subtracted out" of the equation, and a bias in the estimate still exists. Furthermore, fixed effects estimates of income may only be identified if and only if siblings within a family (for the family fixed effects model) or if a child over time (for a child fixed effects model) experienced different levels of income.

Using instrumental variables along with experimental designs

As we have indicated, there are a number of potential biases in the research aimed at addressing the question of the effects of family income on children. Here we describe an integrated design and analysis strategy that combines the use of an experimental design with an instrumental variables analytic method. In order to understand the technique, we first describe its two components: Instrumental Variables (IV) Models and Experimental Designs. Then we discuss the advantage of their integration.

Estimating Instrumental Variables Models. The technique of instrumental variables estimation can address some of the potential bias in estimating the causal effects of income on children. This estimation technique can be conducted using two-equation or simultaneous equation approaches. However, we focus here on the two-equation approach, as the estimates produced by simultaneous equation approaches are somewhat more sensitive to specification errors than those of two-stage least squares (this is because errors in one part of the model may bias estimates in another part of the model, see Schmitt & Bedeian, 1982 for further discussion).

Instrumental variables estimation requires the availability of a new variable(s), for simplicity let's call it P, which is correlated with the independent variable X and is uncorrelated with the error term, or ε_i. Implementing instrumental variables estimation consists of two stages. In the first stage, an equation is estimated in which the independent variable of interest, such as income, is predicted by an instrument or a set of instruments (variable(s) correlated with income) and a set of control variables. Suppose that our equation of interest is:

$$Y_i = \alpha + X_i\beta + Z_i\beta_z + \varepsilon_i \qquad (2)$$

where Z is our independent variable of interest and potentially subject to bias if Y_i were to be estimated by ordinary least squares regression and X_i are the covariates included in the model. The first stage equation of two-stage least squares takes the form of:

$$Z_i = \alpha_o + X_i\beta + P_i\gamma + \mu_i \qquad (3)$$

where P_i serves as the instrument, or as the variable which will isolate the variance in Z_i that is unrelated to $Y_{i,;}$ X_i are the covariates included in the model and the error term is denoted as μ_i. From the coefficient estimates from this equation we can derive $\hat{Z}_i$. This predicted measure is then used to *replace* the actual measure, Z_i, in a second-stage equation. The second stage estimating equation then becomes:

$$Y_i = \alpha + X_i\beta + \hat{Z}_i\beta_z + \varepsilon_i \qquad (4)$$

The standard errors of this second-stage equation are then corrected to allow for statistical inference on the coefficient of the income measure.[v] With the standard error correction, the predicted measure estimates the relation between Z and Y free of the biases previously discussed. If the instrument, P, is not associated with any omitted variables that affect Y, and if Y cannot predict P (which has been achieved through the specification of the model), then the factors that bias Z will be encompassed in the error term of the first-stage equation (μ_i), along with any measurement error in Z. One may manually estimate two-stage least squares and make the standard error correction, or one may use "pre-programmed" procedures available in a number of statistical packages that automatically correct the standard errors.[vi]

The biggest challenge in implementing this procedure to obtain correct estimates of the effect of income on child outcomes is finding an appropriate instrument. To estimate an instrumental variables model there must exist a variable (i.e., the instrument) that meets several key assumptions.[vii] First, the instrument, Z, such as program treatment assignment, must be random.[viii] Second, the effect of Z on any one individual will not be influenced by the effect of Z on any other individual. This assumes that there are no community effects due to Z, and that there are no displacement effects due to Z. Third, it must be assumed that Z has the intended effect on individual behavior. That is, if Z is assignment to a program, then it is assumed that sample members in a program group will not avoid the treatment, and at the same time, that sample members in a control group will not be induced to take the treatment.

Finally, the only way that the instrument, Z, must affect Y, child development, is via its effects on the independent variable of interest, in this case income. As previously stated, a valid instrument must significantly predict Z and must be uncorrelated with the error term, ε_i. Another way to think about this is to consider that any variable that serves as an instrument must be included in the equation predicting the independent variable of interest (income), but excluded from the equation predicting the dependent variable of interest (child outcomes). For example, if we were examining the relation between income and children's outcomes, we might consider using teenage motherhood as an instrument, because it is associated with income level. But teenage motherhood may also affect children's development because teenage mothers are often not well educated and may be psychologically unprepared to parent (and thus teenage motherhood is associated with the error term in the equation). Since teenage motherhood should be included in both the income and the child outcomes equations, it cannot be used separately to identify the prediction of income.

To make this effort even more difficult, there must exist at least one instrument for each potentially biased variable in the second-stage equation. If equation (2) were expanded further to include Z_2 and both Z_1 and Z_2 were potentially biased in a least-squares estimation technique then two instruments are necessary, P_1 and P_2, in order to estimate two-stage least squares. For example, employment is intricately linked to the incidence of poverty because income from earnings partly determines the level of family income. Therefore, in estimating the effects of poverty, the second-stage equation should likely include measures of both predicted income and predicted employment. Two unique instruments are required to identify the effects of these two measures on child outcomes.

Using Data from Experimental Designs. Experimental designs provide an additional source of methodological leverage for identifying the effects of changes in family circumstances on children. With a correctly implemented random assignment experiment, unobserved and observed characteristics are similar for individuals in the control group and the treatment group. Thus, any differences in outcomes between individuals in these two groups that are observed after random assignment can be attributed solely to the treatment (i.e. $E(\hat{\beta}) = \beta$ by design if β is the coefficient on a 0-1 variable indicating program group status).[ix]

One early example of an evaluation that had the potential to try to identify the effects of income on children was the Negative Income Tax (NIT) experiment conducted in four cities from 1968 to 1982 (Munnell, 1986; Office of Income Security Policy, 1983). The NIT experiment targeted working poor families and was designed to examine the labor force participation response to a guaranteed level of family income. Although the NIT maintained or increased income level, it also decreased work effort. In addition, some evidence suggests that the NIT increased marital dissolution and other aspects of family behavior (Munnell, 1986; Office of Income Security, 1983). More notably for the context of this paper, in selected sites, the NIT improved measures of child nutrition, early school achievement, and high school completion (Mallar & Maynard, 1981; Salkind & Haskins, 1982).

Although the NIT analyses demonstrate a means of reducing bias in estimating the relation between social programs and children's functioning, these analyses are limited in informing us about the effects of changes in family income on children. In particular, we cannot definitively identify all of the ways in which these programs affected children. That is, the NIT may have affected child functioning because of the changes in family income, the parental work reduction, the changes in family structure, or a combination of all these factors. If some of the effect of the NIT on children is related to work reduction rather than family income changes, then a comparison of child outcomes in the program group and in the control group does not solely identify the effects of income. Because programs may change multiple aspects of family behavior, estimates of program impacts on child well-being in any evaluation suffer from this same limitation in their use for identifying the effects of income, as opposed to the program, on children.

Combining Instrumental Variables Estimation with an Experimental Design. Instrumental variables estimation with experimental data of an adult-focused intervention strategy can help to isolate the extent to which changes in family income affect child functioning. As we indicated earlier in the discussion about instrumental variables methods, it is challenging to find valid instruments that predict the independent variable in the first-stage equation (and, this is only related the dependent variable of interest through its effects on the independent variable)

By examining these same relations in the context of an experimental design, we potentially have access to a valid instrument. First, by using random assignment status as an instrument, the biases associated with correlational research may be overcome. Omitted variables (contained in the error term of the child outcomes equation) are not correlated with the predicted variable used in the second stage equation because treatment is random and uncorrelated with observed and unobserved family and child characteristics. The predicted value of income is free of simultaneity bias because random assignment status is not determined according to characteristics of the child. The predicted value of the independent variable includes only the variance of the independent variable accounted for by random assignment and therefore is free of unexplained measurement error. Finally, random assignment status must be correlated with the independent variable of interest. This is clear in this case as income is one of the key targets of many adult-focused interventions aimed to move families out of poverty. In summary, assignment to the treatment group guarantees that $E(\sum P_i \varepsilon_i) = 0$ and the observed intended effect of the treatment (e.g. actually altering income) guarantees that $E(\sum P_i X_i) > 0$.

There is one complication, however. Programs may influence many aspects of family behavior. In estimating the effects of one independent variable on children, we need to be aware of other aspects of family behavior for which there is a program impact and that may confound the total effect of the independent variable on child outcomes.[x] For example, in examining the relation between income and children, factors such as employment are likely important to consider, as one way in which programs may change family income is by altering parental employment behavior. Since both employment and income affect child well-being, both should be included in the equation, because it is important to understand the effect of income, separate from that of employment, on children. Now the child outcomes equation will contain more than one potentially biased variable. Consequently, at least two instruments are required to estimate a unique solution to the instrumental variables model. Assignment to the intervention group provides only one instrument.

One way to find more than one valid instrument is to use a multiple-group random assignment design that could allow for more than one indicator variable representing assignment to the intervention group(s). In the previous discussion this implies that both P_1 and P_2 are available as instruments by construction of the random assignment design.

What if there are additional ways in which an intervention affects children than that through employment and income? This would mean that the model would be incorrectly specified and would bias the estimates of the effects of income from the Instrumental Variables models. In the case of adult-focused welfare interventions aimed at employment and income, it is more likely that there are only two paths—employment and income—by which the intervention affected children; this case may be much harder to make in a multi-faceted intervention program.

To take an example, consider programs that target adult employment and income and not other aspects of family or child well-being directly. Such programs include those which have a "make work pay" design—typically they offer welfare recipients a financial incentive to go to work, either by allowing them to keep more of their welfare dollars as they go to work or by supplementing their earnings outside the welfare system. Other aspects of children's daily experiences, like child care or family functioning, can still be affected by these programs—what is critical to the model specification is that any effects on child care or family functioning are likely to be the *result* of the effects on employment and income, and thus encompassed in their total effect.

Some of these "make work pay" strategies offer more to parents than simply the financial incentive to work; some offer more one-on-one case management as well. One might argue that the case management provided by such programs may have benefited maternal self esteem, or otherwise affected parents, and, in this way, benefited children. If this were the case, however, other service programs with strong case management, like that provided in the New Chance Demonstration (Quint, Bos, & Polit, 1997) would have had stronger effects on children. (New Chance had very few effects on families and children). Similarly, one would expect positive effects from home visiting programs—which have much stronger case management than welfare programs. However, a recent review of home visiting programs shows that, except in rare cases, this program model tends to have few effects on families and children (Gomby, Culross, & Behrman, 1999). Therefore, we suspect that it is a reasonable assumption that any effects on children of welfare programs aimed at employment and income are fully mediated by the program's effects on employment and income, and that any other mediating processes that are not encompassed in the total effect of employment and income are at most relatively small. Thus, two instruments might be adequate for identifying the effects of these factors on child outcomes. However, this is a key assumption of the IV models we have estimated in this paper, and, if this assumption is violated, would bias the estimates attained.

Illustration to estimate the effects of income on children

The Minnesota Family Investment Program.

Policy efforts to alter the welfare system are faced with the competing goals of alleviating poverty among welfare recipients and encouraging employment. The Minnesota Family Investment Program (MFIP) was designed to address this dilemma by making employment pay more than welfare. Unlike programs that provide or mandate education and employment services for parents receiving welfare, this program attempted to increase employment and family income through the use of financial incentives. Early results suggest that this is a very promising strategy for reducing poverty among single parents (Lin et al., 1998; Miller et al., 1997).

This evaluation includes extensive data collection at the time of random assignment and during a number of follow-up periods. Administrative records provide data on unemployment and public assistance receipt, and surveys gather additional data on education, employment, family income, and family functioning. Surveys also collect data on parental functioning, parenting, and children's outcomes three years after random assignment. Measures of child functioning include maternal report of children's health, behavior and academic functioning.

In MFIP, single-parent families receiving welfare were assigned to one of three research groups: (1) Full MFIP, (2) MFIP Incentives Only, or (3) AFDC (the control group). Whereas under AFDC welfare payments were reduced dollar for dollar with earnings, families assigned to both the Full MFIP and MFIP Incentives Only were able to keep more of their welfare income as their earnings increased. In addition, families in the MFIP group were required to participate in employment and training services if they were on welfare for 24 of the prior 36 months (or else they faced sanctions), while those in the Incentives Only group did not face any of these employment and training mandates. Families assigned to the AFDC group received the benefits of Minnesota's AFDC program.

Evaluation results conducted at 18 and 36 months after random assignment show that MFIP increased both employment and total family income (Miller et al., 1997; Miller, Knox, Gennetian, Dodoo, Hunter, & Redcross, 2000). Over the three-year follow-up period, MFIP increased employment 13 percentage points and reduced poverty by 10 percentage points among single parent, urban, long-term welfare recipients (recipients who had been receiving welfare for at least 2 years when they entered the study; See Table 1.) These findings suggest that the random assignment status variables from MFIP satisfy the conditions necessary for estimating an instrumental variables model to examine the effects of income on children. In addition to its large effects on poverty reduction, MFIP's unique three group research design may be used to create multiple instruments.

Evaluations such as MFIP provide the type of data particularly suitable for examining the effects of income on children. First, MFIP uses a random assignment design and is an intervention that *targets* income; therefore, assignment to the intervention group offers an ideal "random event," or instrument, to estimate the effects of income. Second, MFIP increased income fairly substantially (by $1300/year for the Full MFIP program and $1100/year for the MFIP Incentives Only program), without which it would be impossible to estimate the relation between increases in income on child outcomes (because random assignment status would no longer be an effective instrument). Third, MFIP has multiple measures of child functioning collected post-baseline, including those analyzed in previous studies of the effects of income on children. Finally, MFIP offers ways to estimate income effects controlling for program effects on employment, and employment effects controlling for income because of its a multiple-treatment design, which allows for more than one valid instrument.

Sample and Procedures

This study focuses on single-mother families with a child aged 2 to 9. The families were long-term recipients of welfare (on welfare for at least 24 of 36 months prior to random assignment) and who lived in urban counties (Hennepin, Anoka or Dakota). These single-mother welfare recipients were randomly assigned to one of three research groups at the time of their interview for re-certification of welfare. The sample targeted for this study entered the program between April 1994 and October 1994. The final analysis sample of 879 families for this paper was obtained after imposing a few additional restrictions, as was done in previous analyses conducted on this sample (Gennetian & Miller, 2000). First, we limit our analysis to focal children who were at least 5 years old and who were less than 13 years old at the time of the three-year follow-up interview. Some children who were interviewed were out of the age range for the analysis because the interview took place earlier or later than anticipated relative to their birth date. Second, because the focal child in each household was chosen before the interview, based on the child's age at random assignment, some "pre-determined" focal children were not in the household at the time of the survey, either because they had moved to another residence or because the designation at random assignment was based on incorrect information. For these cases, another focal child was randomly chosen at the time of the interview. The final analysis sample excludes sample children who were not the pre-determined focal child.

Data Sources. Data for this study were compiled from a variety of sources. Basic demographic information is available for all sample members from a baseline information form completed just prior to random assignment. Staff in the financial offices interviewed each welfare recipient and collected important demographic information, such as the sample member's age, race, educational attainment, prior work history, and prior welfare receipt.

Data from state administrative records are used to track families' benefit receipt and employment during the follow-up period. *Public assistance benefits records* include monthly information on public assistance benefits (including MFIP, AFDC, Food Stamps, and Family General Assistance) provided to each member of the research sample. *Unemployment Insurance earnings records* provide quarterly earnings information for each sample member, as reported by employers to the Unemployment Insurance (UI) system. These data exclude earnings that are not covered by or not reported to the UI system — for example, jobs in the informal economy. Earnings and benefit data are available for each sample member for a minimum of one year prior to random assignment and three years following random assignment.

Finally, information was collected from a survey administered to each family three years after random assignment. The child section of the survey took 45 minutes to administer and contained a range of questions designed to measure children's environments and a number of child outcomes. The survey achieved a response rate of 80.3 percent. Although this is a reasonably high response rate, there is the possibility that analyses using the survey sample will suffer from non-response bias. Results from an analysis of non-response bias suggest that any bias is minimal (Gennetian & Miller, 2000).

Measures

Employment. Data from unemployment insurance records were used to create two measures of employment—employed in the first year post random assignment, and the average employment rate in the first three years post random assignment.

Income. Income was computed as the combination of earnings, food stamps and welfare benefits as collected from unemployment insurance and public assistance benefit records. As with employment, two measures of income were created—income in the first year post random assignment and average income in the first three years post random assignment.

Academic Achievement. Mothers were asked to gauge their children's performance in school by responding to the following question: "Based on your knowledge of the child's schoolwork, including report cards, how has he or she been doing in school overall?" Responses could range from 1 ("not well at all") to 5 ("very well"; mean = 4.06; sd = 1.10)

Engagement in School. Mothers were also asked four questions about their child's level of engagement in school (e.g., "My child cares about doing well in school"). Their responses could range from 1 ("not true") to 3 ("often true"). The child's engagement in school is measured by the sum of the mother's responses. This sum can range from 4 to 12, with a higher number indicating a higher level of engagement (alpha = .60; mean = 10.10, sd = 1.82).

Problem Behavior. Mothers responded to a series of questions designed to measure aspects of problem behavior by the focal child. The 28-item scale includes questions such as "my child is disobedient at home" and "my child is too fearful or anxious," and responses can vary from 0 ("not true") to 2 ("often true"). See Peterson and Zill (1986) for details. A total score was created as the sum of responses to all 28 questions. The total score can range from 0 to 56, with higher values indicating more behavioral problems (alpha = .92; mean = 11.69, sd = 9.20).

Positive Behavior Scale. Mothers were asked a series of questions designed to measure positive aspects of the child's behavior. This 25-item scale, developed by Polit (1996), includes questions such as "my child is helpful and cooperative" and "my child is cheerful and happy," and responses can range from 0 ("not at all like my child") to 10 ("completely like my child"). A total score was created as the sum of responses to the 25 questions (alpha = .95; mean = 196.16, sd = 37.5).

Results

For comparison, we first examined the effects of income on four child outcomes using a model similar to that used in other studies examining the effects of income on children (Duncan & Brooks-Gunn, 1997). For these analyses, we examined the effects of parents' income (measured over the first three years of follow-up) on the four child outcomes, with and without controlling for the following variables all measured at random assignment: parent is never married (vs. divorced or separated), parent had no high school degree, parent is Black, parent was other minority, gender of child, child's age). The results of this analysis are presented in Table 2 (in these and subsequent models, two-tailed hypothesis tests with a two-tailed alpha, Type I error rate = 0.10, are conducted on the relation between income and child outcomes, because the unbiased effects of increasing income could be either positive or negative). Surprisingly, unlike other studies that have found a positive relation of income to children's cognitive outcomes, in this sample of long-term welfare recipients there is a negative relation, with children in higher income families performing more poorly in school. This somewhat puzzling result is something we turn to in the Discussion section. However, there was no effect on children's engagement in school. Income affected behavior problems in the expected direction (reducing behavior problems) but had no effect on positive behavior. Also, measuring income for only one-year post random assignment or as a three-year income average did not affect the results appreciably, nor did including covariates in the model.

Turning to the results of the Instrumental Variables estimation, the first-stage equation predicting income included the two instruments (P_1 and P_2), representing assignment to each of two research groups, and a set of baseline characteristics hypothesized to affect income and employment (included in the model are the following variables, all measured at baseline: parent received welfare for 5 or more years, parent is never married (vs. divorced or separated), the number of children in the family, the presence of a child under the age of 6, parent has no high school degree, parent is Black, parent is other minority (non-Black and non-White), parent had her first child as a teenager, earnings in the year prior to random assignment, and two variables indicating the quarter of random assignment). A similar first-stage equation predicting employment was also estimated. Two models were tested: one examining the effects of income (the combination of earnings, AFDC and Food Stamp payments in the first year post-random assignment) and employment (employed in at least one of the first 4 quarters) in the first year post-random assignment, and a second examining the effects of income and employment in the first three years post-random assignment (using the variables representing the average of the two variables described above over three years). While the latter may be a more stable estimate of the increase in income due to MFIP on children, MFIP's effects on employment and income were strongest in the first year compared with later in the follow-up period (Miller et al., 2000). Therefore, random assignment to the MFIP program may be a more powerful instrument for the effects of income in the first year as compared with the effects of income throughout the follow-up period.

The results of the first stage equations are presented in Table 3. As is evident from the Table, the dummy variables representing Full MFIP and MFIP Incentives Only were associated with employment and income, both in the first year and across the three-year follow-up, a necessary condition for the IV strategy. A test of the effects of the instruments suggests that these variables are strong predictors of both employment ($F = 13.38$, $p < .001$ for employment in the first year and $F = 15.13$, $p < .001$ for employment over the three years) and income ($F = 14.65$, $p < .001$ for income in the first year and $F = 6.89$, $p < .01$ for income over the three years). Notably, while the instruments are significant predictors of both first year income and the three-year average measure, the F-value of the effect of the instruments on the three year average measure is considerably smaller than that for the year one measure, suggesting that the instruments may not be strong predictors of income measured over three years. This may limit the power to detect significant effects of income using this latter definition in the second stage IV models.

The second-stage equation used predicted income and employment (e.g. $\hat{Z}_{1i}$ and $\hat{Z}_{2i}$) along with the same set of baseline characteristics (excluding the instruments) to predict the child outcomes. See equation (4) for a simplified version. This differs from the treatment/control comparison in that the predicted income and employment variables, and not the treatment indicator itself, are used to predict the child outcome measure. The estimate of the income effect obtained from this second-stage equation (with the standard error correction) will provide an unbiased estimate of the relation between income and child outcomes, assuming the model is correctly specified, and are much more plausible as estimates of the causal effects of income than those from models using OLS estimates.

The results of this second stage equation are presented in Table 4. For comparison, the results of analogous OLS estimates are also provided (in the OLS models, the same covariates are used as in the second stage IV estimate, and income and employment are both included in the equation (rather than the predicted value of income and employment as in the IV models). In the OLS models, small, insignificant effects of income are found, using income measured both in the first year and all three years post-random assignment. However, in the IV models using the first year of post-random assignment income, significant positive effects of income are found, predicting engagement in school and positive social behavior. The effects on the other two variables (and for all of the variables using the three-year measure of income) are in the expected direction (favorable effects of income) but are not statistically significant. Notably, the parameter estimates are larger using the three-year average measure of income, but so is the standard error, resulting in no significant effects of income on child outcomes. As expected, the lack of power in the first stage equation limited our ability to find significant effects in the second stage in these models using three-year average income. In none of the models did employment have a significant effect (although, interestingly, the coefficients on the employment measures are always opposite to those of the income measures).

Although theory may suggest or dictate that an effect of a particular variable of interest estimated via OLS may be confounded by other biases, it is not always clear that the bias is serious enough to try to control for it. If not serious enough, OLS may be a preferred method of estimation since it will yield more efficient estimates. The Durbin-Wu-Hausman test, popularly referred to as the Hausman test, is one way to examine whether or not IV estimation provides better estimates than OLS estimation. This test, which has a chi-squared distribution, basically compares the consistency of the OLS and IV estimates (a “vector of contrasts,” see Davidson & MacKinnon, 1993 for a discussion). The test statistic may also be computed by means of artificial regressions.[xi] Using this method of artificial regressions, a Hausman test indicated that, for most of the child outcome measures except for behavior problems, the OLS estimates are not consistent, i.e. they are biased.

Discussion

These findings suggest that increasing income improves children’s engagement in school and positive social behavior for long term welfare recipients. The IV estimates of the relation between income measured in the first-year post-random assignment and child outcomes find that increased income results in higher engagement in school and more positive social behavior. The effects of income measured throughout the three year follow-up period are similar though weaker, partly due to the diluted influence of the program on long term income. These findings are exciting in showing a causal and reversible effect of income for long term welfare recipients not found using typical OLS estimation techniques. However, they are consistent with the conclusions drawn from research that generally does show that income benefits children’s development (Duncan & Brooks-Gunn, 1997).

Confidence in our findings stem from two sources. First, there is consistency between the effects from the IV estimation with the hypothesized and likely pathways that led to MFIP's effects on children's development relying purely on the experimental impact findings (Gennetian & Miller, 2000). As discussed in detail in Gennetian and Miller (2000), compared to outcomes for children and families on AFDC, MFIP's incentives only program increased income and consistently showed a pattern of improving children's development. The Full MFIP program, which increased effects on employment more so than income, generated no additional positive effects on children. These experimental impacts suggest a strong link between increases in income and improved child well-being. In addition, further analyses (available from the authors) were conducted using interaction terms between program group status and exogenous baseline characteristics as instruments (this approach is discussed in further detail below). These models yielded results similar to those presented here, suggesting that the findings are robust across different model specifications.

The effects of income in the IV estimates are quite large, one-quarter to one-third of a standard deviation effect on school engagement and positive behavior. These effects are larger than one might expect given the extant literature on the effects of income on children. It is indeed possible that income has such a large effect on child outcomes for children in families who were long term recipients of welfare. It is also possible that in this study, unlike other studies relying on nonexperimental data, some of the effect of income is capturing the small effects of case management, or other components of the MFIP program not assumed to be mediators of the effects of the program on child outcomes. This may overstate the causal effects of income on children.

Why do the IV models find a positive causal effect of income that is not consistently found in the OLS estimates? OLS estimates, as mentioned earlier, may be biased by other omitted variables, by simultaneity bias or by errors-in-variables bias. This means that the OLS estimates may be capturing other effects besides the pure effect of income on children. Interestingly, the OLS estimates in this sample are not similar to those in previous work—but this may reflect the fact that these estimates are obtained in a long-term welfare recipient sample. Families with higher incomes in this study's sample may be more likely to have larger families, thus making them qualify for larger welfare grants (while number of children is controlled in the examination of the effect of income, family size may affect income differently depending on the amount of income families receive from welfare). Therefore, high income in this sample may be associated with characteristics like large family size and greater welfare stigma in a way not typically found in a broader low-income sample. Or families with higher incomes may work very long hours in poor quality jobs, which may inhibit the positive effects of income in this very disadvantaged sample. The IV models reduce these biases associated with the OLS estimates, and thus the estimates of the effects of income are estimates of the pure causal effect of increasing income on children.

MFIP is but one example of an evaluation that is available to implement an instrumental variables technique in examining the effects of family circumstances on child outcomes. Before the passage of the welfare reform in 1996, many states applied for waivers to try out innovative alternatives to the traditional AFDC program. With receipt of the waiver, states were required to run evaluations of their welfare programs. Five states were later selected as part of a competitive grant process to include measures of child functioning in their follow-up surveys. MFIP, described above, is one of these five. These evaluation studies will allow for analyses of the type described above regarding the effects of changes in income and employment on children's development across a wide variety of welfare and employment studies. This kind of replication is important, especially for generalizing the findings presented in this paper. Because MFIP's incentives were tied to employment, using MFIP treatment status as an instrument identified income gains that occurred to families only in the context of more employment. The effects of income generated by a pure income transfer may or may not have the same effect on children's outcomes.

These experimental studies can also be used to examine the effects of other aspects of child and family environments that are affected by these types of intervention programs, although adding further mediators does require more instruments to identify the equations. Other instruments can be obtained by interacting the random assignment dummy variable with one or more exogenous baseline characteristics likely to significantly moderate the effects of the intervention on income and employment.[xii] This latter technique provides multiple instruments: (1) treatment status and (2) the interaction of the baseline variable and treatment status. Recall that for this expanded set of variables to be valid instruments they must significantly predict both parental employment and family income, and they must affect child outcomes only via parental employment and family income (in addition to meeting the other assumptions outlined earlier).

A couple of studies have used such an approach to examining the effects of child care and parents' education on children. In one study, the sites of the treatment were interacted with the program dummy and included, along with the program dummy itself, as instruments for the effects on child care on children's cognitive school readiness (Bos & Granger, 2001). The other study used a three-group research design, along with interactions of the program dummies and sites, as instruments in predicting mothers' education (Magnusson & McGroder, 2001). Both of these approaches yield estimates of causal processes that are less biased than those attained using OLS estimates.

Furthermore, with the IV approach in mind, studies can be designed explicitly to obtain valid instruments that can then be used to assess causal relations between family circumstances and children's development. Such studies would require a random assignment design, along with multiple treatments or baseline covariates and follow-up measures of family circumstances and children's functioning. These studies could also target broader populations of families, and families in multiple settings. As data become available, instrumental variables analysis along with an experimental design is a practical and effective method to address a number of questions about the true effects of environmental-, family- or child-level factors on children's development.

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Figure 1

Bias arising from Omitted Variables, Simultaneity and Measurement Error

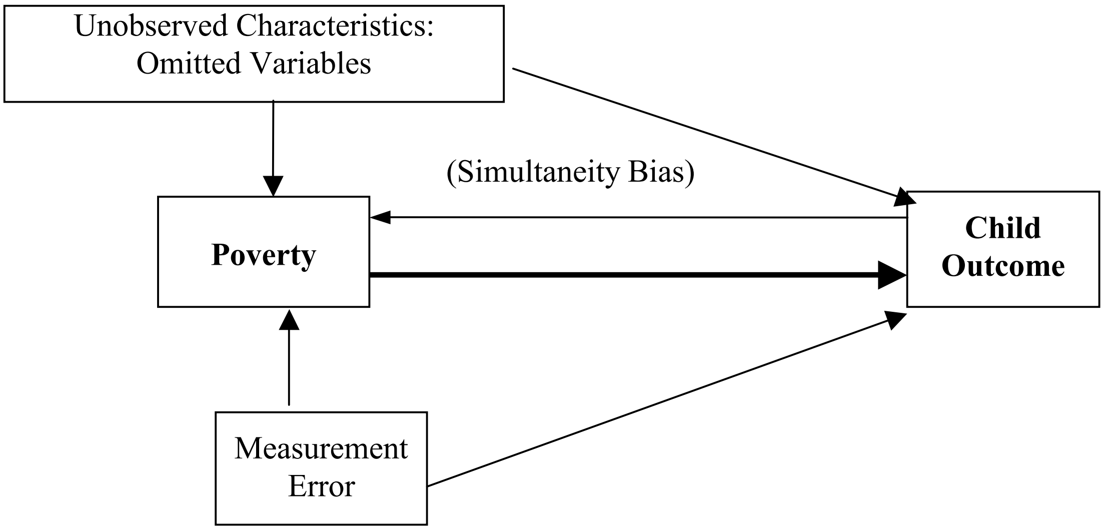

Table 1

MFIP's Impacts on Employment, Earnings,Welfare, and Income for Long-term Recipients in Urban Counties

	Average Outcome Levels			MFIP vs. AFDC	MFIP Incentives Only vs. AFDC	MFIP vs. MFIP Incentives Only
Outcome	MFIP	MFIP Incentives Only	AFDC	Impacts of Full MFIP Program	Impacts of Financial Incentives Alone	Impacts of Adding Mandatory Services and Reinforced Incentive Messages
<u>Employment and earnings</u>						
Average quarterly employment rate (%)						
Year 1	0.71	0.63	0.53	0.18 ***	0.10 ***	0.08 **
Year 2	0.74	0.64	0.58	0.15 ***	0.05	0.10 ***
Year 3	0.74	0.71	0.62	0.12 ***	0.10 ***	0.02
Average yearly employment rate (%)	0.73	0.66	0.58	0.15 ***	0.08 ***	0.07 **
Average quarterly earnings ($)						
Year 1	2,870	2,671	2,064	806 ***	607 *	199
Year 2	4,686	3,466	3,833	853 *	-367	1,220 ***
Year 3	6,393	5,778	5,832	561	-54	615
Average yearly earnings ($)	4,650	3,972	3,910	740 *	62	678 *
<u>Welfare receipt</u>						
Average quarterly benefits ($)						
Year 1	8,195	8,319	7,550	645 ***	769 ***	-124
Year 2	6,899	7,641	6,394	505 *	1,246 ***	-742 **
Year 3	5,965	6,634	5,421	544	1,214 ***	-669 **
Average yearly benefits ($)	7,019	7,531	6,455	564 **	1,076 ***	-512 **
<u>Income</u>						
Average quarterly income ($)						
Year1	11,065	10,990	9,614	1,451 ***	1,376 ***	75
Year2	11,585	11,106	10,227	1,357 ***	879 **	479
Year3	12,357	12,412	11,252	1,105 **	1,160 **	-55
Average yearly income ($)	11,669	11,503	10,365	1,305 ***	1,138 ***	166
Sample size	*306*	*292*	*281*			

SOURCE: MDRC calculations using MFIP administrative, child survey and baseline survey data.

NOTES: The sample includes long term welfare recipients randomly assigned from April 1, 1994 to October 31, 1994, excluding the small percentege who were receiving only Food Stamps at random assignment.

Two-tailed significance levels are indicated as: * = 10 percent; ** = 5percent; *** = 1 percent.

Table 2

OLS Estimates of the Effects of Income

On Children's School and Behavioral Outcomes

	Year one Income (No covariates)	Year one Income (With Covariates)	Three year Income (No covariates)	Three year Income (With Covariates)
School Achievement	-0.02 **	-0.02 *	-0.02 **	-0.02 **
(mean = 4.06, sd = 1.10)	(0.01)	(0.01)	(0.01)	(0.01)
School Engagement	-0.02	-0.02	-0.01	-0.01
(mean = 10.10, sd = 1.82)	(0.02)	(0.02)	(0.02)	(0.02)
Behavior Problems	-0.13	-0.14	-0.18 **	-0.18 **
(mean = 11.69, sd = 9.20)	(.09)	(0.10)	(0.08)	(0.08)
Positive Behavior	0.11	-0.05	0.10	-0.03
(mean = 196.16, sd = 37.5)	(.39)	-0.39	(0.33)	(0.33)
Sample Size= 879				

SOURCE: MDRC calculations using MFIP administrative, child survey and baseline survey data.
NOTES: Standard errors in parentheses.
The sample includes long term welfare recipients randomly assigned from April 1, 1994 to October 31, 1994, excluding the small percentege who were receiving only Food Stamps at random assignment.
Income is measured in 1000's of dollars, in the first year post random assignment or as an average of three years post random assignment.
The regressions also include the following covariates measured at baseline: black, other racial/ethnic minority, number of children in the family, mother had no high school degree or equivalent, mother never maried, child gender, child age.
Sample sizes may vary across models because of missing values.
Two-tailed significance levels are indicated as: * = 10 percent; ** = 5percent; *** = 1 percent.

Table 3

The Effects of MFIP on Employment and Income

First Stage Regression Results for IV Model

	Year one Employment	Year one Income	Three year Employment	Three year Income
Full MFIP	0.20 ***	1.40 ***	0.16 ***	1.23 ***
	(0.04)	(0.29)	(0.03)	(0.36)
MFIP Incentives Only	0.09 **	1.32 ***	0.08 ***	1.10 ***
	(0.04)	(0.29)	(0.03)	(0.37)
F value	13.38 ***	14.65 ***	15.13 ***	6.89 ***
Sample Size= 879				

SOURCE: MDRC calculations using MFIP administrative and baseline survey data.
NOTES: Standard errors in parentheses.
The sample includes long term welfare recipients randomly assigned from April 1, 1994 to October 31, 1994, excluding the small percentege who were receiving only Food Stamps at random assignment.
Income is measured in 1000's of dollars, in the first year post random assignment and on average over the three year folloow-up. Employment is measured as ever employed, in the first year post random assignment, and as the average employment rate over the three-year follow-up.
The regressions also include the following covariates measured at baseline: black, other racial/ethnic minority, mother was a teen at child's birth, number of children in the family, presence of a child age 6 or less, mother had no high school degree or equivalent, mother never maried, mother on welfare 5 or more years, earnings in the prior year, and two indicators for the quarter of random assignment.
Sample sizes may vary across models because of missing values.
Two-tailed significance levels are indicated as: * = 10 percent; ** = 5percent; *** = 1 percent.

Table 4

OLS and IV Estimates of the Effects of Employment and Income On Children's School and Behavioral Outcomes

	OLS	IV Model 1	IV Model 2
Effects of Income			
School Achievement	-0.02	0.16	0.20
(mean = 4.06, sd = 1.10)	(0.01)	(0.14)	(0.20)
		H: p = .02	*H: p = .02*
School Engagement	-0.01	0.47 *	0.59
(mean = 10.10, sd = 1.82)	(0.02)	(0.27)	(0.42)
		H: p = .01	*H: p = .01*
Behavior Problems	-0.16	-1.57	-1.94
(mean = 11.69, sd = 9.20)	(0.12)	(1.23)	(1.72)
		H: p = .11	*H: p = .10*
Positive Behavior	0.17	11.04 *	14.01
(mean = 196.16, sd = 37.5)	(0.49)	(6.18)	(9.32)
		H: p = .06	*H: p = .06*
Effects of Employment			
School Achievement	-0.02	-0.17	-0.36
(mean = 4.06, sd = 1.10)	(0.09)	(1.08)	(1.66)
		H: p = .02	*H: p = .02*
School Engagement	0.13	-1.05	-1.87
(mean = 10.10, sd = 1.82)	(0.16)	(1.86)	(3.17)
		H: p = .01	*H: p = .01*
Behavior Problems	0.45	1.99	4.32
(mean = 11.69, sd = 9.20)	(0.80)	(8.30)	(12.87)
		H: p = .11	*H: p = .10*
Positive Behavior	-3.19	-65.05	-94.62
(mean = 196.16, sd = 37.5)	(3.24)	(41.63)	(69.74)
		H: p = .06	*H: p = .06*
Sample Size= 879			

SOURCE: MDRC calculations using MFIP administrative, child survey and baseline survey data.
NOTES: Standard errors in parentheses. "H" indicates Hausman test (p values of F test are indicated).
The sample includes long term welfare recipients randomly assigned from April 1, 1994 to October 31, 1994, excluding the small percentege who were receiving only Food Stamps at random assignment.
Income is measured in 1000's of dollars, in the first year post random assignment (model 1) and on average over the three year folloow-up (model 2). Employment is measured as ever employed, in the first year post random assignment (model 1), and as the average employment rate over the three-year follow-up (model 2).
The regressions also include the following covariates measured at baseline: black, other racial/ethnic minority, mother was a teen at child's birth, number of children in the family, presence of a child age 6 or less, mother had no high school degree or equivalent, mother never maried, mother on welfare 5 or more years, earnings in the prior year, and two indicators for the quarter of random assignment.
Sample sizes may vary across models because of missing values.
Two-tailed significance levels are indicated as: * = 10 percent; ** = 5percent; *** = 1 percent.

[i] Under the assumption of uncorrelated disturbance terms across the dependent variables in structural models, maximum likelihood estimates are identical to those provided by ordinary least squares using regression analyses. While correlated disturbance terms can be included in structural models (and would solve this problem), this is rarely done in research in child development.

[ii] One way to estimate correlational associations is with ordinary least squares regression, one assumption of which is that each independent variable (across observations) in the estimating equation is uncorrelated with the corresponding error. If this assumption is violated, then the coefficient on one or more of the independent variables is biased.

[iii] This is a different use of the term "fixed effects" than that used in ANOVA models. In ANOVA, a fixed effects model is that in which the researcher "fixes" the level of the independent variable (e.g., by assigning individuals to varying levels of the independent variable). This is contrasted with "random effects models", in which researchers randomly select individuals from the population at varying levels of the independent variable. The use of the term "fixed effects models" in ANOVA has no relevance to the term as it is used in this paper.

[iv] Expanding equation (1) consider: $Y_{ij} = \alpha + Xj_i\beta + \varepsilon_{ij} + \delta_j$

where i represents the child in a family and j represents the family, or the same mother; Y_{ij} is the dependent variable of interest (e.g. a cognitive test score); X_{ij} represents the independent variable of interest (e.g. poverty); ε_{ij} represents the error associated with the child in the family, and δ_j represents the error associated with the family. In this case, $\hat{\beta} = \beta + \frac{\sum X_i(\varepsilon_{ij} + \delta_j)}{\sum X_{ij}^2}$. Unless $E(\sum X_i\delta_j) = 0$ the least-squares estimator will be biased. The fixed effects technique estimates the previous model by taking differences across siblings, or "subtracting out" the common unobserved family effect: $Y_{ij} - Y_{i-1,j} = (X_{ij} - X_{i-1,j})\beta + (\varepsilon_{ij} - \varepsilon_{i-1,j})$.

[v] Standard errors in ordinary least squares estimation are unbiased if and only if each independent variable (across observations) is nonstochastic (Greene, 1997). The second-stage equation of instrumental variables models includes a predicted value as an independent variable. The standard error correction is necessary to adjust for the disturbance associated with the predicted value.

[vi] Some examples of "pre-programmed" procedures available in statistical packages that estimate two-stage least squares and automatically correct the standard errors include: Proc Syslin in SAS; Regress in STATA, and 2SLS in SPSS.

[vii] See Angrist, Imbens and Rubin (1996) for more information regarding these assumptions of Instrumental Variables estimation.

[viii] As discussed in more detail in Bound, Jaeger & Baker (1993), the estimates obtained from instrumental variables estimation may be biased if the correlation between the instrument and the endogenous independent variable of interest (e.g. income) is very weak, even if the sample size is very large. Some research has suggested that F values of at least 10 are required on the relation between the instrument and the independent variable of interest (Staiger & Stock, 1994). Angrist and Krueger (1994) suggest some alternative estimators under such conditions.

[ix] Notably, differential sample attrition from treatment and control groups would result in bias to the estimates of the effects of the program on outcomes examined. Here we are discussing experimental designs in which sample attrition does not bias the estimates of the effect of the program.

[x] Many aspects of family functioning may mediate the effects of income on child well-being. These do not confound the relation between income and child outcomes because they are included in the total effect of income.

[xi] For example, the artificial regression includes the actual value of income and the predicted value of income in the same regression, and then tests if the coefficient on the predicted value of income is significantly different from zero.

[xii] Notably, this technique was attempted using data from the Self Sufficiency Project to identify the effects of income on child outcomes, but with limited success: depending on the baseline characteristic that was interacted with the program dummy, the estimates of the effects of income varied widely, and the overidentified models failed to find that the instruments were uncorrelated with the error term in the child outcomes equation.