Microeconomics of the Family: Three Essays

by

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Abstract

In this dissertation, I examine the effects of several different government programs on families. The first two chapters focus on different effects of the United States child support enforcement system. The third chapter considers the effects of the Special Supplemental Food Program for Women, Infants and Children on both pregnancy outcomes for women and developmental outcomes for children.

In chapter one, I examine the effects of the child support enforcement system on absent fathers' allocations of time and money to their children. Children's outcomes in later life are related to a variety of inputs that come from within the family. These inputs increasingly come from absent fathers who can contribute both money and time to their children. Government actions to collect child support could lead absent fathers to spend more time with their children or it could cause them to substitute money for time. Aggressive enforcement may also reduce contact with fathers who are afraid of being targeted for sanctions. Using data from the National Longitudinal Survey of Youth, I find that more aggressive enforcement at the state level reduces father-child contact as measured by number of visits and physical distance. Instrumental variables estimates suggest that time and money are substitutes for fathers affected by these child support enforcement mechanisms.

In chapter two, I examine the effects of the child support enforcement system on non-custodial fathers' labor supply. Government efforts to identify non-custodial parents and retrieve child support have recently become more aggressive. A large body of literature based on analysis of earnings after establishment of a child support award claims that non-custodial parents can afford to pay much more in child support than they actually do. These studies ignore behavioral responses of non-custodial parents' labor supply to child support awards, resulting in overestimates of the income non-custodial parents would have in the absence of child support awards. In the late 1980s, child support awards functioned as lump sum taxes on non-custodial parents, implying that these non-custodial parents' labor supply should rise if leisure is a normal good. Using data from the the National Longitudinal Survey of Youth and instrumental variable techniques, I find evidence of both a positive effect of paying any child support on hours of work and of each additional dollar of child support paid on hours of work. These results are consistent with my findings in chapter one - namely that state efforts to collect missing child support reduce the time fathers spend with their children. Chapter two suggests that fathers instead may be working more to comply with child support orders.

Chapter three, co-authored with Janet Currie and Duncan Thomas of the University of California at Los Angeles, examines the effects of the Special Supplemental Food Program for Women, Infants and Children (WIC), on both pregnancy outcomes for women and developmental outcomes for children. Previous studies have found extensive evidence of positive effects of WIC on a variety of pregnancy outcomes, but few have found any long-lasting evidence of WIC's effects on young children. Using data from the National Longitudinal Survey of Youth, we find that WIC has positive but small effects on some pregnancy outcomes and on some cognitive test scores and on Medicaid and Food Stamp use in family fixed-effect specifications. However, instrumental variables estimates suggest that WIC has a negative effect on one motor skill test score and no effect on other test scores.

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Introduction

Government programs affect families with children in many ways. In the current climate of welfare reform, Federal and state governments have turned to alternatives to cash support for disadvantaged families. This change has increased the policy relevance of quantifying the effects of government programs on both the behavior of families and measures of family well-being.

In this dissertation, I examine the effects of several different government programs on families. The first two chapters focus on different effects of the United States child support enforcement system. The third chapter considers the effects of the Special Supplemental Food Program for Women, Infants and Children on both pregnancy outcomes for women and developmental outcomes for children.

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Chapter 1

Fathers' Time vs. Fathers' Money: Effects of the Child Support Enforcement System

Introduction

Over the last few decades, increasing numbers of children have been spending time in single parent households, either because of divorce or because their mothers were never married. As of 1995, some 25 percent of children lived in non-intact households, and some 33 percent of births in 1993 were to unmarried women (United States House of Representatives 1996). Current debates over welfare reform have focused renewed attention on the absent fathers of these children. Policy makers have pointed out that marital dissolution can lead to decreased income for the custodial parent and thus for the children. Both the federal government and the states have turned to ever more aggressive policies to identify absent fathers and collect child support from them.

Money, however, may not be the only channel through which children's outcomes are affected. Marital dissolution can alter the amount of time spent with the child by the non-custodial parent (generally the father). In a small sample of divorced families, Furstenberg et al. (1987) found some 36 percent of children aged 11–16 in 1981 had not seen their fathers in five years and of the rest, only 16 percent saw their fathers as much as once a week. There is an extensive sociological and economics literature that shows that time spent with parents can affect childrens' outcomes beyond the effect directly attributable to any income contributed. Even after controlling for socioeconomic status, studies have found that individuals who grew up in non-intact families were worse off. Grogger and Ronan (1995) find that fatherlessness leads to reduced human capital accumulation. Angrist and Johnson (1998) show that time spent away by fathers deployed in the Gulf War has significant negative effects on some child outcomes.

Provided time with fathers has a positive impact on child outcomes, child support enforcement mechanisms should be evaluated according to how they affect fathers' contributions of both time and money. In this paper I quantify the indirect effects that efforts to retrieve income from non-resident fathers have on their allocation of time with their children. This issue bears not only on the evaluation of child support enforcement policies but also on whether time and money are complements or substitutes for fathers.

I begin by laying out simple theoretical models of the response of fathers to child support enforcement policies. I show that theoretical predictions are ambiguous on the question of the complementarity or substitutability of fathers' contributions of time and money. In section 2, I summarize the previous literature on the time-money tradeoff before describing the data used to address this question in section 3. Because the data covers the period from 1986–1994, a period of great change in child support enforcement in many states, I can examine the impact of many policy changes. In section 4, I lay out the empirical strategy. I present my results in section 5, finding evidence that time and money are substitutes, and conclude in section 6.

1.1 Theory

Time with fathers could substitute either for interactions with other family members (grandparents, etc.) or for cash from the father or the state. Theory of the family does not offer clear predictions about whether time and money should be complements or substitutes. A simple utility maximizing model can yield either prediction. Consider the father's problem in the absence of child support enforcement legislation. Let s be the amount of money the father spends on his child, t the amount of time, and x a composite representing all other goods he consumes. The father's problem is to

$$Max_{\{x,s,t\}} U(x,s,t)$$

such that

$$F(x,s,t) \leq R \quad (BC),$$

with solution x^*, s^* and t^*. Suppose we add the constraint

$$s \geq S \quad (Order).$$

For any father whose $s^* < S$, there will be new solutions $x^{cons}, s^{cons} = S$, and t^{cons}. It is unclear without making further assumptions whether $t^{cons} > t^*$. If Order binds and s and t are gross substitutes, then $t^{cons} < t^*$. If Order binds and s and t are gross complements, $t^{cons} > t^*$. This is only the simplest possible such model.

One response of fathers to aggressive enforcement could be to end their contact with the mother or children. Thus, child support enforcement policies could induce fathers to devote neither time nor money to their children. In contrast, "good" fathers may choose to both spend time with their children and give money to their children. Fathers who spend more time with their children may instead spend more money as a result of being able to monitor the use of the money. Peters et al. (1993) show that continued involvement between fathers and children can result in self-enforcing parental visitation and payment arrangements akin to those in the intact marriage.

Increased enforcement may crowd out time spent with children. Women on the Aid for Families with Dependent Children (AFDC) program face an implicit tax rate of 100 percent on every child support dollar paid to the state on their behalf after the first $50 paid which is passed through to them.[1] Weiss and Willis (1985, 1993) have shown that if money spent on children is a public good to the parents, lack of ex-ante binding marriage contracts can result in inefficient underpayment by non-custodial fathers. Fathers' utility may incorporate time and money as a composite good; if forced to increase contributions of one, they may reduce contributions of the other. Alternatively,

[1] This provision was eliminated by the Personal Responsibility and Work Opportunity Reconciliation Act of 1996.

child support enforcement mechanisms could alienate fathers from their children, driving them to reduce the time spent with children.

There may also be general equilibrium effects if enforcement causes either mothers or fathers to alter their remarriage behavior. As either mothers or fathers remarry, fathers may choose to spend less time with their children or less money on their children. A recent working paper by Bloom et al. (1996) concludes that more aggressive child support enforcement results in a longer time to remarriage for low income fathers. This could result in these fathers spending more time with their children.

1.2 The Literature

Since theory offers no clear guidance, we turn to the empirical evidence. Surprisingly, other forms of kinship care are not crowded out by public provision of child care or AFDC benefits (Hao and Leibowitz 1994). Many researchers have found money and time to be complements.[2] However, many of these studies are either cross sectional, limiting the number of policies and the time period they can analyze, or they focus on divorced families, leaving the dynamic between the children of never married women and their fathers unexplored. By pooling information from the period 1986–1994, I can consider the effects of more recent tactics such as license suspension. Since my sample includes the children of never married women, I can analyze the effects of child support enforcement on their fathers.

Using the National Longitudinal Survey of Youth (NLSY), Veum (1993) finds that the presence or absence of child support is unrelated to visitation; however, his identification scheme is somewhat suspect. In a simultaneous equation framework with changes in visitation and changes in child support as the jointly determined endogenous variables, he uses distance away the father lives as an instrument for child support paid. The distance between the child and the non-custodial parent is an outcome variable, jointly determined along with payment of child support and visitation.

There are numerous studies on the effects of time spent with fathers. Children who grow up in single parent or step parent families are worse off than children in intact two parent families along different dimensions including future job performance, entry level wages and educational attainment (Hernandez 1995, McLanahan and Sandefur 1994). Using retrospective information about family structure, Grogger and Ronan (1995) find that one additional year spent in a fatherless home as a child leads to six months less human capital for an adult. Duncan et al. (1996) find important effects of fathers on children's eventual outcomes using the Survey of Income and Program Participation. Angrist and Johnson (1998) find that fathers' absence due to Gulf War deployment has a strong

[2]See Seltzer and Bianchi (1988), Seltzer et al. (1989), Seltzer (1991), Seltzer et al. (1997) and Furstenberg et al. (1987).

effect on the probability that a child will have a temporary handicap, further evidence that fathers matter. However, some other studies find little or no effect of fathers' time.

Many studies have examined the determinants of child support payment and the effects of these dollars on child outcomes. Knox (1996) finds in ordinary least squares (OLS) regressions that increased child support payments may improve the academic performance of children more than other sources of income, after conditioning on time spent with the father. She does not include visitation in her eventual instrumental variables (IV) analysis, leaving the effect of time spent with fathers unaddressed. Others have found this same result, including Graham et al. (1994), although Hernandez et al. (1995) find that the strength of this relationship has been weakening as states' child support enforcement efforts have become more aggressive. Others examine the effect of various enforcement policies on collections, orders and paternity establishments.[3] Few have addressed the interrelations of time, money and government policy.

One exception is a recent paper by Seltzer et al. (1997) using the National Survey of Families and Households (NSFH). They find that more strict enforcement is associated with more frequent visits but not with whether or not any visits occur, after conditioning on parental conflict variables. Interestingly, while they do not use any state policy variables in the longitudinal analysis because of small sample sizes, in the cross sectional IV analysis they find that it is whether any child support is paid, rather than how much was paid, that is statistically significantly related to visitation. Similarly, in the longitudinal analysis, they find it is whether any child support is paid, rather than how much is paid, that matters, even when pre-separation differences are controlled for. However, their panel has only two observations for each family, one from 1987–1988 and one from 1990–1991. Also, their sample only includes families that divorced between the first and second waves of the panel.

My analysis will use the NLSY and information about the changes in child support enforcement policies over time to see what effect these enforcement policies have on the time spent by fathers with their children and on their child support payments. Using data from 1986–1994, I can look at many policy changes, using the the extensive geographic and demographic information in the NLSY to help control for omitted variable bias.

1.3 The Data

This analysis uses the National Longitudinal Survey of Youth Child-Mother Files (Baker et al. 1993). The NLSY is a long-running panel started in 1979 with 12,686 youths aged 14–21. It has a random sample and an over-sample of the following groups; families in poverty, blacks, Hispanics and the military. During the 1980s, the children of women in the NLSY began being followed; currently some 10042 children of 4483 mothers are in the survey. This survey is rich in outcome and family

[3] See Garfinkel and Robins (1994), Beller and Graham (1991) or Nixon (1997).

background variables for the children and mothers and has information about the county and state of residence. I link this information to other information about the county of residence from the 1990 Census and other county level data sets. One drawback of the NLSY is that these mothers are younger than a random sample of women with children. Another is that the early data on child support orders and fathers' visits is somewhat limited. The mothers are surveyed every year about the amount of child support paid. Information about some child outcomes is only available in the bi-annual Child Supplements.

The final sample is an unbalanced panel consisting of all children living with their mothers in the years 1986–1994 where the biological father of the child was alive and did not live in the mothers' home and the mother answered questions about both how much the child saw her father and about the child support the mother received.[4] If the child's father was in the household during the year, the observation is dropped from the sample. The information about how much time was spent with the father could be any of the following :

- the number or frequency of visits with the father in the last year (visits)
- the distance away the father lived in miles (distance)
- how long an average visit with the father lasted in days.

I focus on the visits and distance variables because I believe that the length of a usual visit is affected primarily by the custody agreement and is not a choice variable for the fathers. Since not all mothers answered all of these questions, there are slightly different samples for each of these variables measuring time spent with the father. Information about payment of child support is limited to whether any was paid (child support dummy) and how much was paid (in dollars) for the years before 1993. The child support dummy is a binary variable that is one if any child support was paid in the last year. Since 1993, there is more detailed information about the kind and size of the child support order. Thus, there is another variable — the number of dollars paid divided by the number of dollars due (share of child support paid). All these variables are reported by the mother for her whole family. Families with more than one child with a child support order might receive more child support money than those with only one child with an order for purely mechanical reasons.[5] Additionally there could be measurement error in the child support dollars variable — AFDC participants' child support is taxed away at a rate of 100 percent after they receive the $50 per month pass through. One might question whether these mothers on AFDC report the actual

[4]Dropping the observations on children missing information on either visits with the father or distance away lived by the father did not significantly change the sample along observable dimensions.

[5]Without more detailed state level information on presumptive guidelines, it is difficult to relate the amount paid to the amount of the order. Since we only know how much child support was paid to the family, not per child, the amount paid could be for one or more children.

dollars paid (most of which would go to the state) rather than the $50 or less they receive.[6] Also, there is possible reporting bias — perhaps many women do not report "unofficial" under-the-table support. This sort of bias is unlikely to affect the child support dummy as much as it should affect the child support dollars paid dummy. For these reasons, I focus on the child support dummy and the share of child support paid variable.

Table 1 has the variable means for a 1994 cross section of the panel. The first column contains the means (standard errors) for the combined random sample and over-samples (except for the military sample) for all children who do not live with their fathers. The second column contains the means (standard errors) for the part of the sample with information about the number of times the child saw her father in the last year (visits sample). The third column contains the means (standard errors) for the part of the sample with information about the distance away from the mothers' home the father lives (distance sample). The means of the relevant variables in my samples are quite similar to those in the overall NLSY.

The visits variable is a categorical variable in the NLSY — for this analysis I recoded it to be the midpoint in the range of number of days a year each category represented. Thus for the response "Saw Father Once a Week in the Last 12 Months", visits would be recoded to be 52. The distance variable denotes how far away from the child the father lives in miles. The numbers here for visits are similar to those found in other large survey data sets. In the full sample, 41 percent of the children had seen their father only once or not all in 1992 compared with almost $\frac{1}{3}$ of children who had not done so in the last year in the NSFH, another large survey with information about childrens' living situations. The average amount of child support received by these women is below the 1991 national average (United States House of Representatives 1996) of $2,961. The sample averages were $989 for the visits sample and $1,019 for the distance sample. This is not surprising as this sample consists of the already born children of young mothers who are more likely to have had their children out of wedlock. Never married mothers are less likely to receive the full amount of child support owed them and less likely to even have court orders for child support than older or once married mothers. They are also more likely to be owed the money by younger, less well-off males, making their average payments lower even if they were paid all they were owed (child support awards in most states are tied to the earnings of the non-custodial parent). If time and money are complements, we expect that visits will be positively correlated with how much child support is paid while distance will be negatively correlated (fathers who are more attached to their children live closer to them).

[6]These women receive a form every month telling them how much child support was paid to the state on their behalf. Actual reported means of child support paid differ according to AFDC status, while the percent reporting some child support paid last year was similar (mean child support paid was $920 for non-AFDC mothers but $229 for AFDC mothers, while the mean share reporting some child support paid was .37 for the non-AFDC mothers and .25 for the AFDC mothers for the visits sample).

1.4 Empirical Strategy

The equation of interest is:

$$TIME = \alpha MONEY + \beta' X + \epsilon, \tag{1.1}$$

where TIME is either times the child saw her father last year or distance away her father lived. MONEY is a measure of child support paid by the father (either a dummy for some child support paid or the share of order paid) and X includes other variables about the child.[7]

Table 2 contains OLS estimates using several different measures for TIME and MONEY. As more detailed demographic and regional controls are added, the sign of the coefficient on the child support dummy remains positive and becomes statistically significant. In the specification with the most controls, if the father paid any money at all in the last year, he saw his child for another 12 days. Since the mean number of days that fathers saw their children was 53 days, this is a substantial increase. As found by others, the coefficient on whether any child support was paid is significant and positive in OLS regressions with time on the left-hand side. It is negative and significant in regressions with distance away the father lives on the left-hand side. Fathers who pay more child support see their children more and live closer to their children. The coefficients on the various demographic controls (not shown here) had the expected signs; for example, generally the older children, black and Hispanic children and children whose fathers did not live with their mothers at birth were less likely to see their fathers and had fathers who lived further away. Clearly, one worries about interpreting these OLS regressions causally, as discussed above.

In order to address the problem of endogeneity, I use a variety of state by year (or sometimes county by year) level indicators of the aggressiveness of the states' child support enforcement mechanisms as instruments.[8] Child support enforcement is administered by the states with considerable cross sectional and time series variation in assertiveness. Some states have always been very aggressive while others only adopt tools after their mandate at various times by Congress.[9] In a 1991 report rating the states for overall effectiveness, Alabama had the highest rating while Oklahoma had the lowest (United States Office of Child Support Enforcement 1992). The original 1975 legislation was passed to increase collections of unpaid child support obligations owed to children whose mothers were on AFDC.[10] Over time, this mandate has expanded to force states to assist non-AFDC participants, to use presumptive guidelines in establishing awards (and to include medical insurance

[7]Some specifications not shown here included AFDC status, poverty status, other family income and the mother's parents' highest grades completed. The results were substantively the same.

[8]This section draws on United States House of Representatives (1996), Garfinkel and Robins (1994) and Williams (1994).

[9]Relevant legislation includes the 1975 changes to the Social Security Act establishing the Federal Child Support Enforcement program, the 1984 Child Support Enforcement Amendments, the 1988 Family Support Act and the Omnibus Budget Reconciliation Act of 1993.

[10]Women on AFDC are required to assign their support rights to the state — some states collect as much as 13 percent of their AFDC expenditures through this avenue.

in the awards), to improve their paternity establishment rates, to increase the percentage of eligible children with legal awards for support and to adopt certain legislation for cases involving more than one state's jurisdiction. The Federal Office of Child Support Enforcement (OCSE) issues an annual report to Congress compiled from program information submitted by the states under the requirements of the Social Security Act.

All of the instruments offer variation at the state- or the county-by-year level (United States OCSE various years). These instruments were chosen by examining the voluminous literature on child support collections. There are several state-level indicators of child support enforcement effort that I could use — a measure of state administrative spending (child support dollars collected per administrative dollar spent, administrative dollars normalized by case load or total administrative dollars), paternities established (normalized by the number of out-of-wedlock births) and full-time staff (normalized by the state population).[11] I also use dummy variables for the introduction of state administrative license suspension as well as dummy variables for the introduction of presumptive child-support guidelines in the state and dummy variables for the use of any administrative sanctions in the state. In order to avoid confounding the effect of the many federal child-support waivers issued to counties (particularly counties in New York and Wisconsin), I include dummy variables for counties affected by federal waivers affecting child support enforcement.

Currently child support awards are usually settled in the state of residence of the child — the state where the child lived for a considerable amount of time before the order is brought to the court for consideration. I assume that the effort level of the state in collecting child support is unrelated to unobserved characteristics about the father that might be correlated with either contributions of time or money by the father. Conditioning on county- and family-level variables helps account for such unobserved characteristics. Additionally, to avoid bias resulting if the mother moves after the award is ordered, I instrument for the state of residence with the state in which the original child support order was established.

I included various county-level controls as well as year-fixed effects in all the regressions. County-level variables from the 1990 Census included the percentage of the population under five years old, the percentage of the population that is black, the percentage of households headed by single mothers, the percentage of school eligible population in public schools, the percentage of adults 25 and over who completed high school, median personal income in the county and average travel time to work. Average commute time accounts for how difficult it is for the father to get to the mother's household.[12] The regressions also include the percentage of voters who voted for Perot for president

[11]The case load and administrative dollars numbers are inconsistent across years, while dollars collected is clearly an outcome variable — thus I do not use these variables in any reduced form or two-stage least squares (2SLS) regressions.

[12]The county data from the 1990 Census came from the Regional Economic Information Service (United States Department of Commerce Bureau of Economic Analysis 1996).

in 1992.[13]

The first stage equation is:

$$MONEY_{ijklt} = \delta' INSTRUMENT_{klt} + \gamma' X_{ijklt} + \epsilon_{ijklt}; \quad (1.2)$$

where ijklt denotes the ith child in family j in state k in county l in year t. The MONEY variable takes on the same value for all children in the same family in the same year. I adjust the standard errors to allow for correlations among the error terms within the family, as the the NLSY has observations on all children from the same mother. In the unbalanced panel, the same child can appear more than once as can more than one child with the same mother. Without this heteroscedasticity correction, I would be treating these observations as if they were independent, understating the standard errors in these pooled regressions. Because the money variable is reported at the mother level, but the time variables are reported at the child level, one could, in principle, include mother-fixed effects, but it would soak up a great deal of the variation.[14] All specifications include in the X vector whether the father was present at the birth of the child, a quadratic in the age of the child (or year of child's birth dummy variables), a quadratic in the age of the mother at first birth, the highest grade completed by the mother (or her AFQT score), dummy variables for black and Hispanic origin, dummy variables for the mother having never married or being divorced (the omitted category is remarried), family size and a dummy variable for living in an urban setting.

There are two outcome variables of interest, the number of times the child saw her father (visits) and distance away the father lives (distance), and two possible endogenous regressors, a dummy for whether any child support was paid in the last year (child support dummy) and a variable denoting the share of the order that was paid (share of child support paid).[15] I chose to group the tables according to the choice of endogenous regressor. The first column of table 3 contains first stage regressions of the effect of these various policies on whether or not any collections were made, with the upper panel having results for the visits sample and the lower panel having results for the distance sample. Table 8 contains the analogous coefficients for the share of child support order paid variable.

The reduced form regressions of the outcome variables on the various policy instruments are also of interest. The reduced form equation estimated is:

$$TIME_{ijklt} = \delta' INSTRUMENT_{klt} + \gamma' X_{ijklt} + \epsilon_{ijklt}; \quad (1.3)$$

[13] I experimented with including Catholic church membership in the county in 1990 to control for conservatism and attitudes towards marriage, but it was not significant.

[14] I ran similar specifications (not reported here) with family fixed effects and found that the sign on the child support dummy was the same as in the regressions reported here, although this coefficient was not statistically significant at the usually accepted five percent level.

[15] Results with child support dollars paid were similar in sign in all specifications but rarely statistically significant at more than the 15 percent level.

where the subscripts and assumptions about the error term are the same as in the first stage regressions. Note that the time variable varies at the child-by-year level, while the child support variables vary at the family-by-year level. For the reduced form results for both samples of the effects of the policies on the outcomes (visits in the top panel and distance in the lower panel), see the second column of tables 3 and 8.

I report 2SLS results, which estimate equation (1), instrumenting for MONEY with all the policy variables. For 2SLS estimates of the effect of having received any child support in the last year on the two outcome variables see table 4. For 2SLS results for the same effect broken down by race see table 7.

1.5 Results

I discuss the first stage regressions first. Turning to table 3, each row contains results for two regression, a first stage regression and a reduced form regression. The label in the left column describes the instrument. The column labeled first stage contains the coefficient (standard error) on the instrument in that row if the child support dummy is regressed on the instrument. In the row labeled all instruments, the F statistic (p-value) for excluding all the instruments is displayed. Demographic and area controls and year fixed effects are included in all specifications. I correct the standard errors for groupwise heteroscedasticity within the family. The coefficients on the controls were similar in most specifications and are not displayed here.[16] Table 8 presents the same coefficients when the share of child support paid is the endogenous regressor. Finally, table 5 contains estimates for the first stages when the sample is split according to race for the child support dummy.[17]

While some of the instruments measuring overall aggressiveness of the state predict whether some child support was paid, few seem to predict well how much of the order was paid. As found in other surveys, almost no mothers reported being paid more than they were owed. This may be a function of how the question is asked. Clearly mothers on AFDC have no incentive to ever report anything above the $50 pass through, as they face a 100 percent tax rate on that income. In regressions not reported here, I find that while it is possible to predict whether any child support was paid, it is harder to predict the amount paid.

Interestingly, in table 5 we see that the number of paternities per unmarried birth seems to be more effective at predicting whether black children's fathers paid any child support, while the estimates suggest that staff per capita is a better predictor of whether non-black children's fathers paid any child support.

The coefficients in reduced form regressions of the number of visits (or distance away lived) on

[16] See table A-1 for regression output with a full set of coefficients on the controls.

[17] The share of child support paid variable is only available for 1993 and 1994 and therefore the sample sizes are too small to precisely estimate the effects of the child support enforcement variables separately by race.

the various policy indicators one at a time and all together are in the second column of table 3 (for the child support dummy), and table 8 (for the share of child support paid variable). For the black vs. non-black breakdown in the visits sample see table 6. While one cannot interpret these results structurally, there is some negative relation between aggressiveness of the state's child support enforcement system and how frequently the father sees his children. This negative relationship holds across all specifications in the larger sample containing information from 1986–1994 (see table 3). In the sample from 1993 and 1994, most of the estimates of any policy effect on visits are also negative, except for the coefficient on whether the state had any administrative sanctions (such as license suspension). Table 6 shows the reduced form estimates conditioning on race. In table 6, the effect of these policies on visits for blacks is negative and significant for visits and positive and significant for distance. Both of these results are consistent with either the interpretation that fathers' attachment to their children is being crowded out or with the interpretation that aggressive child support collection is driving the fathers away. The effects for non-blacks are much less precisely measured, although again staff per capita seems to crowd out non-black fathers' attachment to their children.

I use these instruments to construct 2SLS estimates of the effect of MONEY fathers are compelled to pay on TIME. For 2SLS estimates with the child support dummy, see table 4 and for 2SLS estimates for the child support dummy broken down by race, see table 7. The coefficient on the child support dummy for the visits regressions is negative and statistically significant, while for the distance regressions it is positive and significant. As seen in table 7, the 2SLS estimates of the coefficient on the child support dummy are negative and large for the visits sample for blacks, and statistically insignificant for non-blacks although in the black sample, the 2SLS estimates fail the over-identification test. All these results are consistent with the interpretation that time and money are substitutes. The coefficient on distance away lived by the father is positive but not significant in the 2SLS regressions for the full sample. Again, for blacks, the coefficient on the child support dummy in the distance sample is positive and significant, also the expected sign if time and money in fact are substitutes — fathers who are compelled to pay more child support in fact live further from their children. The effects for non-blacks are imprecisely measured.

One possible concern about the identification strategy is that this large coefficient on money paid in the 2SLS regressions may not reflect the underlying structural relationship between time and money. Instead there may be a correlation between unobserved characteristics of the father and the state of residence. This correlation may result in the coefficient being biased in either direction. For example, the coefficient would be biased towards zero if "bad" fathers moved to states with weak laws before their children were born. I address this concern by controlling for many things about the county of residence. If the identification strategy is valid, there should be stronger effects for groups more likely to be affected by the laws. One such group is fathers of older children; their children are

likely to have been born before the laws were passed. Thus they could not have moved to a state in order to avoid strong laws. In regressions not reported here, I split my sample according to the median age of the child; putting children older than the median age of 93 months in one sample and children 93 months old and younger in the other sample. In the sample with older children, the estimated 2SLS coefficients on the child support dummy were similar to those in table 4 and the estimated coefficient was statistically significant for the visits variable. In the sample with younger children, the coefficient in the visits regression was statistically insignificant from zero and smaller than that in table 4. These results support my choice of an identification strategy.

1.6 Conclusion

I have shown evidence that fathers' money and fathers' time are negatively correlated when it is the effect on visits that is being examined and positively correlated when it is distance away the father lives that is being analyzed. When forced to pay more child support, fathers see their children less frequently and live further from them. These results differ from those of previous researchers such as Seltzer et al. (1997). However, Seltzer et al. concentrated on families where the parents had been married and were divorced and also only considered the period between 1987 and 1991. Finally, there are important differences between the effects of the state policies on the behavior of fathers of black children and the effects on the fathers of non-black children.

These results suggest that time and money may be substitutes for some non-custodial fathers. They also imply that there may be unexpected outcomes of state child support enforcement policies on the amount of time non-custodial fathers spend with their children. Some measures intended to force "deadbeat dads" to pay child support are more effective at making fathers already paying some child support pay the full amount they owe than at raising the probability that fathers pay any child support. Increasingly aggressive state and federal tactics may never reach the many children whose mothers do not currently have child support orders. This makes it all the more important that policy makers consider the effects of these policies on fathers' allocation of time.

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Table 1.1: Means of Variables for 1994 Cross Section

Variable	Full Sample	Visits Sample	Distance Sample
Frequency of Visits	50.3	44.1	46.7
	(1.3)	(4.4)	(4.5)
Distance Away Father Lives	72.1	75	75.2
	(1.3)	(4.2)	(4.4)
Child Support Paid in 1990 $	609.1	988.7	1019.3
	(43.9)	(99.9)	(104)
Dummy for Child Support	0.3	0.4	0.4
	(1.4E-02)	(2.7E-02)	(2.8E-02)
Fraction of Child Support Order Received	0.6	0.5	0.5
	(3.6E-02)	(3.2E-02)	(3.3E-02)
Father Present at Birth	0.2	0.1	0.1
	(1.1E-02)	(1.5E-02)	(1.6E-02)
AFDC	0.2	0.3	0.3
	(1.2E-02)	(2.5E-02)	(2.5E-02)
First Born Child	0.6	0.6	0.6
	(1.5E-02)	(2.7E-02)	(2.8E-02)
Boy	0.5	0.5	0.5
	(1.5E-02)	(2.8E-02)	(2.8E-02)
First Born Boy	0.3	0.3	0.3
	(1.4E-02)	(2.5E-02)	(2.5E-02)
Age of Child (Months)	68.1	90.4	89.9
	(1.6)	(3)	(3.1)
Black	0.2	0.3	0.3
	(1.3E-02)	(2.6E-02)	(2.6E-02)
Hispanic	0.1	0.1	0.1
	(9.0E-03)	(1.6E-02)	(1.7E-02)
Never Married	0.1	0.2	0.2
	(1.0E-02)	(2.3E-02)	(2.3E-02)
Divorced	0.3	0.4	0.4
	(1.4E-02)	(2.7E-02)	(2.8E-02)
Mother's Age at First Birth	19.8	19.3	19.2
	(0.1)	(0.2)	(0.2)
Urban	0.7	0.7	0.7
	(1.3E-02)	(2.4E-02)	(2.5E-02)

Notes: Means and standard errors of the demographic and county-level variables for the 1994 cross section of the panel. First column contains mean and standard errors for the entire NLSY. Second column contain means and standard errors for visits sample and third column for the distance sample.

Table 1.1: Means of Variables for 1994 Cross Section — Continued

Variable	Full Sample	Visits Sample	Distance Sample
Below Poverty Level	0.2	0.3	0.3
	(1.3E-02)	(2.5E-02)	(2.6E-02)
Family Size	4.3	4.2	4.2
	(0.1)	(0.1)	(0.1)
Mother's HGC	12.2	12	12
	(0.1)	(0.1)	(0.1)
AFQT Mother	628.6	598.7	594.5
	(6.3)	(11.7)	(12.1)
% of Pop. Under 5 †	7.4	7.4	7.4
	(3.0E-02)	(0.1)	(0.1)
% of Pop. Black †	13.5	15.3	15.4
	(0.4)	(0.8)	(0.8)
% Female Headed HH †	11.5	12	12
	(0.1)	(0.2)	(0.2)
% of Pop. with at Least High School †	73.1	73.1	72.9
	(0.3)	(0.5)	(0.5)
Median Income †	29600.7	29461.8	29489.8
	(212)	(373.1)	(386.1)
% Perot Votes †	19.4	19.7	19.6
	(0.2)	(0.6)	(0.6)
Travel Time †	21.5	21.7	21.6
	(0.1)	(0.2)	(0.2)

Notes: Means and standard errors of the demographic and county-level variables for the 1994 cross section of the panel. First column contains mean and standard errors for the entire NLSY. Second column contain means and standard errors for visits sample and third column for distance sample.
† All these variables are 1990 Census county-level variables except percent voting for Perot in 1992.

Table 1.2: OLS Regressions of Visits on Dummy for Receiving Any Child Support Last Year

	Visits			Distance		
Child Support Dummy	7.5	11.6	11.7	-16	-19	-19
	(3.4)	(3.3)	(3.3)	(3)	(3)	(3)
Demographic Controls	N	Y	Y	N	Y	Y
County-Level Controls	N	N	Y	N	N	Y
State and Year FE	N	N	Y	N	N	Y
Mean of Dep. Var.	52.6			71		
Mean of Child Support Dummy	.28			.29		
Observations	7113	7113	7113	6712	6712	6712
R-Squared	.001	.08	.10	.001	.06	.10

Notes: Table contains OLS coefficients on a dummy for whether any child support was received last year in regressions of the number of times the child saw her father. Sample includes all observations for any year in the panel from both the random sample and the poverty over-sample.

Table 1.3: OLS Estimates of the Effect of Child Support Enforcement Variables (CS Dummy)

	Mean	First Stage	Reduced Form
Visits			
Paternities per Unmarried Birth	.39	.17	-24
		(.08)	(11)
Staff per Capita (millions)	.15	.48	-119
		(.32)	(46)
All Instruments:			
F-Statistic		1.8[6]	1.6[6]
		(.10)	(.14)
Families	786		
Observations	3056		
Distance			
Paternities per Unmarried Birth	.39	.16	15
		(.08)	(11)
Staff per Capita (millions)	.15	.55	57
		(.32)	(48)
All Instruments:			
F-Statistic		1.9[6]	.8[6]
		(.08)	(.55)
Families	777		
Observations	2908		

Notes: Top number is coefficient (F statistic) on row variable in the regression indicated by the column header and panel. Standard errors (p-value) in parentheses. Top panel is visits sample; bottom panel is distance sample. All regressions include year-fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level. Mean of child support dummy for visits sample: .33, mean of visits variable: 47.2 days-per-year. Mean of child support dummy for distance sample: .35, mean of distance variable: 72.7 miles.

Table 1.4: 2SLS Estimates of the Effect of Child Support Enforcement Variables (CS Dummy)

Visits	
	Coef. on CS Dummy
All Instruments:	-122
	(62)
χ^2 for Over-Id. Test	3.9[5]
	(.57)
Families	786
Observations	3056
Distance	
	Coef. on CS Dummy
All Instruments:	64
	(54)
χ^2 for Over-Id. Test	3.8[5]
	(.58)
Families	777
Observations	2908

Notes: Top number is coefficient on the child support dummy (χ^2 statistic on excluded instruments) in 2SLS regressions of dependent variable (visits or distance) on child support dummy using all policy variables as instruments. Standard errors (p-value) in parentheses. Top panel is visits sample; bottom panel is distance sample. All regressions include year-fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level. Mean of child support dummy for visits sample: .33, mean of visits variable: 47.2 days-per-year. Mean of child support dummy for distance sample: .35, mean of distance variable: 72.7 miles.

Table 1.5: Differences Across Race in the Effect of Child Support Enforcement Variables — First Stage (CS Dummy)

	Black	Non-Black
Visits		
Paternities per Unmarried Birth	.39	.07
	(.13)	(.09)
Staff per Capita (millions)	.41	.61
	(.47)	(.41)
All Instruments:		
F-Statistic	2.8[6]	1.5[6]
	(.01)	(.17)
Families	218	567
Observations	1066	1990
Distance		
Paternities per Unmarried Birth	.39	.06
	(.13)	(.09)
Staff per Capita (millions)	.33	.71
	(.38)	(.41)
All Instruments:		
F-Statistic	3.0[6]	1.8[6]
	(.01)	(.09)
Families	216	560
Observations	1012	1896

Notes: Top number is coefficient (F statistic) on row variable in the regression indicated by the column header and panel. Standard errors (p-value) in parentheses. First column has results for blacks and second column has results for non-blacks. All regressions include year-fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level.

Table 1.6: Differences Across Race in the Effect of Child Support Enforcement Variables — Reduced Form (CS Dummy)

	Black	Non-Black
Visits		
Paternities per Unmarried Birth	-56	-9
	(25)	(12)
Staff per Capita (millions)	-222	-80
	(80)	(50)
All Instruments:		
F-Statistic	1.8[6]	1.0[6]
	(.11)	(.39)
Families	218	567
Observations	1066	1990
Distance		
Paternities per Unmarried Birth	73	-7
	(19)	(15)
Staff per Capita (millions)	202	-26
	(69)	(83)
All Instruments:		
F-Statistic	3.7[6]	.38[6]
	(.001)	(.89)
Families	216	560
Observations	1012	1896

Notes: Top number is coefficient (F statistic) on row variable in the regression indicated by the column header and panel. Standard errors (p-value) in parentheses. First column has results for blacks and second column has results for non-blacks. All regressions include year-fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level.

Table 1.7: Differences Across Race in the Effect of Child Support Enforcement Variables — 2SLS (CS Dummy)

	Black	Non-Black
Visits		
All Instruments:	-76	-74
	(54)	(55)
χ^2 for Over-Id. Test	13[5]	2.6[5]
	(.02)	(.86)
Families	218	567
Observations	1066	1990
Distance		
All Instruments:	112	-45
	(48)	(45)
χ^2 for Over-Id. Test	17[5]	2.1[5]
	(.01)	(.81)
Families	216	560
Observations	1012	1896

Notes: Top number is coefficient on the child support dummy (χ^2 Statistic on excluded instruments) in 2SLS regressions of dependent variable (visits or distance) on child support dummy using all policy variables as instruments. Standard errors (p-value) in parentheses. Top panel is visits sample; bottom panel is distance sample. First column has results for blacks and second column has results for non-blacks. All regressions include year-fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level.

Table 1.8: OLS Estimates of the Effect of Child Support Enforcement Variables (Share Paid)

	Mean	First Stage	Reduced Form
Visits			
Paternities per Unmarried Birth	.49	.08	-37
		(.12)	(17)
Staff per Capita (millions)	.17	.05	-91
		(.48)	(84)
Any Administrative Sanction	.19	.08	28
		(.07)	(12)
All Instruments:			
F-Statistic		1.6[5]	.83[5]
		(.17)	(.54)
Families	262		
Observations	382		
Distance			
Paternities per Unmarried Birth	.48	.11	6
		(.11)	(21)
Staff per Capita (millions)	.17	.08	-12
		(.49)	(83)
Any Administrative Sanction	.19	.08	-11
		(.07)	(11)
All Instruments:			
F-Statistic		1.6[5]	.82[5]
		(.17)	(.55)
Families	258		
Observations	371		

Notes: Top number is coefficient (F statistic) on row variable in the regression indicated by the column header and panel. Standard errors (p-value) in parentheses. Sample only contains observations from 1993 and 1994. Top panel is visits sample; bottom panel is distance sample. All regressions include year-fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level. Mean of share paid for visits sample: .56, mean of visits variable: 49.7 days-per-year. Mean of share paid for distance sample: .55, mean of distance variable: 70.8 miles.

Appendix A

Table A-1: OLS and 2SLS Visits Regressions — Full Specification

Independent Variable	OLS CS Dummy	OLS Visits	2SLS Visits
Dummy for Child Support			-143
			(95)
Paternities per Unmarried Birth	0.17	-24	
	(.08)	(11)	
Father Present at Birth	-0.09	29	16
	(.05)	(12)	(15)
Black	-0.07	12	1
	(.05)	(9)	(13)
Hispanic	-0.12	-12	-29
	(.06)	(10)	(19)
Poor White Sample	-0.04	12	6
	(.06)	(10)	(12)
Never Married	0.05	13	21
	(.04)	(8)	(11
Divorced	0.17	7	31
	(.03)	(5)	(17)
Mother's Age at First Birth	0.004	-1.5	-0.8
	(.005)	(1)	(1.5)
Family Size	3.8E-05	3.9	4
	(.01)	(1.9)	(2)
Urban	-0.05	4	-3
	(.04)	(5)	(10)
Mother's Highest Grade	0.02	2.0	5
	(.01)	(1.4)	(3)
Mother's AFQT	1.8E-4	-0.01	0.01
	(1E-4)	(.01)	(.03)

Notes: Coefficients on all controls for one specification of a visits regression. First column is an OLS regression of a dummy for any child support paid in the last year on the policy variable and all the controls, the second column is an OLS regression of the number of visits last year on the policy variable and all the controls and the third is a 2SLS regression of visits with a child support dummy as the endogenous variable. Policy variable is paternities per unmarried birth. All regressions include year-fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level.

Table A-1: OLS and 2SLS Visits Regressions — Full Specification — Continued

Independent Variable	OLS CS Dummy	OLS Visits	2SLS Visits
First Born	-0.03	-9	-13
	(.03)	(6)	(9)
Boy	5.5E-04	-3	-3
	(.03)	(6.)	(8)
First Born Boy	0.003	7	7
	(.05)	(8)	(10)
AFDC Benefits in 1990 $	-1.3E-05	4.0E-04	-1.4E-03
	(9.6E-06)	(1.6E-03)	(2.3E-03)
% of Pop. Under 5 †	-0.04	-2	-7
	(.02)	(3)	(5)
% of Pop. Black †	-0.43	-1.9	-89
	(.19)	(2.6)	(60)
% Female Headed HH †	1.90	152	424
	(.74)	(130)	(248)
% of Eligible Pop. in School †	0.01	-0.2	0.7
	(2.9E-03)	(.5)	(.9)
% Pop. with at Least High School †	-6.3E-04	-0.68	-0.7
	(3.0E-03)	(.51)	(.6)
Median Income †	5.2E-06	8.2E-04	1.6E-03
	(3.0E-06)	(4.3E-04)	(9E-04)
% Perot Votes †	-3.0E-05	-0.16	-0.17
	(.002)	(.21)	(.37)
Travel Time †	-2.7E-03	-0.76	-1.15
	(3.8E-03)	(.63)	(.97)

Notes: Coefficients on all controls for one specification of a visits regression. First column is an OLS regression of a dummy for any child support paid in the last year on the policy variable and all the controls, the second column is an OLS regression of the number of visits last year on the policy variable and all the controls and the third is a 2SLS regression of visits with a child support dummy as the endogenous variable. Policy variable is paternities per unmarried birth. All regressions include year-fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level.

† All these variables are 1990 Census county-level variables except percent voting for Perot in 1992 (also at the county level) and AFDC benefits (at the state level).

Chapter 2

The Effects of Child Support Enforcement on Non-Custodial Parents' Labor Supply

Introduction

A growing share of parents do not live with their children in intact nuclear families. As the United States child support enforcement system grows more aggressive, an increasing number of non-custodial parents owe child support to the custodial parent. Fathers' groups, in particular, argue that court-mandated awards are unfairly large. In contrast, many studies conclude that non-custodial parents could pay more child support than they actually do. Estimates of the effects of child support enforcement on the labor supply of non-custodial parents would inform this debate. If the imposition of a child support award itself mechanically makes non-custodial parents work more, it may be important to separate that effect from efforts of these parents to escape their obligations.

If child support awards operate as lump sum taxes on the non-custodial parents, then non-custodial parents should increase their hours of work if leisure is a normal good. Moreover, estimates of the effect of child support awards on non-custodial parents' labor supply can quantify the income effect for non-custodial parents. In this case, assuming that there are no labor supply responses to these child support awards leads to incorrect estimates of these non-custodial parents "ability to pay" child support, because it leaves out the response of the non-custodial parents to the incentives incorporated in the awards themselves. Many policy prescriptions for setting child-support award guidelines still rely on conclusions reached with data from the 1980s.

Simple ordinary least squares estimates of the correlation between payment of child support and measure of labor supply do not identify this "ability to pay." Child support payments by non-custodial parents are endogenous — "better" non-custodial parents may earn more, work harder and care more about their children. Alternatively, the causality may run from earnings to payment of child support. In this paper, I use instrumental variable techniques to identify the effects of child support enforcement policies in the late 1980s on the labor supply of non-custodial parents. Section 1 sketches the theoretical implications of a static neo-classical labor supply model for these non-custodial parents, lays out the relevant information about child support enforcement in the United States and summarizes the literature Section 2 discusses the data set used for this analysis and gives information about the sample. Section 3 presents the empirical strategy used to identify this effect. In section 4, I present the results and conclude in section 5.

2.1 Labor Supply of Absent Parents

Labor supply of absent parents can differ from that of the general population. Non-custodial parents with a court order to pay child support face an extra tax (in the form of the child support they must pay) that can affect their labor supply. The non-custodial parents' response is theoretically ambiguous if the award functions as a marginal tax, but if instead it is a lump sum tax they should work more and we can estimate an income effect. I briefly discuss the implications of the static

utility maximizing model under different assumptions for the nature of the "tax" (the child support award). I then discuss the current child-support enforcement system and argue that at least until quite recently child support awards were akin to lump sum taxes with ever more stringent penalties for avoidance, implying that estimates of their effects on labor supply identify an income effect. A synopsis of previous literature on the topic follows the theoretical discussion.

2.1.1 Theory

A child support award can function as a marginal tax on each dollar of a non-custodial parents' labor income or as a lump sum tax. A simple, static, utility maximizing model can be used to compare the choice of hours of work for a non-custodial parent facing a large child support award with that of a parent facing a lower award.[1]

Consider a non-custodial parent who is sufficiently attached to the labor force that the child support award can only affect their hours or weeks worked, not their participation decision.[2] If child support awards are set as a certain percentage of earned income, then they act as a marginal wage tax.[3] In this case, the effect of such an award on the non-custodial parent's labor supply is theoretically ambiguous. A higher child support award lowers the net wage and lowers the price of leisure, leading to substitution and income effects on hours or weeks worked if leisure is a normal good. Whether the non-custodial parent works more in equilibrium depends on the relative sizes of the substitution and income effects. Standard assumptions about the size of the income effect imply that the overall net effect on labor supply will be negative.

A child support award can instead act as a lump sum tax. In the 1980s, courts almost invariably set child support awards as fixed amounts, regardless of state guidelines. In this case, a non-custodial parent with a child support award faces no change in the price of leisure, only a lower income. Higher child support awards thus lead to lower consumption of leisure and more hours of work. A measure of the income effect for these non-custodial parents can be calculated from estimates of how much more the non-custodial parents work.

More complicated models that incorporate feedback effects of other adults' labor supply on that of the non-custodial parent or intertemporal considerations yield different implications for the effects of child support awards on the labor supply of these individuals in the case of either a marginal tax on labor income or of a lump sum tax.

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[1]This simple model abstracts away from the possibility that the utility or consumption of other members of a non-custodial parent's household or the utility or consumption of the non-custodial parent's children outside the household may enter into the non-custodial parent's utility function.

[2]Traditionally, it is assumed that prime-age males behave in this fashion. Since custodial parents only pursue awards if they expect to receive substantial child support payments, men paying child support might be more attached to the labor force than the general population. However, women on AFDC (and now on the Transitional Assistance to Needy Families program) must assign their support rights to the state. Thus, the set of men who are being pursued for child support might be more or less attached to the labor force than all prime-age males.

[3]Recent law changes mean that in many states child support awards are withheld from wages much as are income taxes.

2.1.2 Child Suppport Enforcement

Clearly, this analysis is complicated by the endogeneity of child support payments. It is possible that causality could even run from earnings to payment of child support. Non-custodial parents who earn more may spend more of it on their children, implying that the observed positive correlation represents earnings causing payment of child support.[4] In any case, custodial parents are more likely to pursue child support orders from non-custodial parents who will be able to pay child support. There could be omitted variables that explain both labor supply and payment of child support. Without a clear structural model of child support payment by non-custodial parents that incorporates these omitted variables, the size of behavioral responses to this "tax" is indeterminate. Selection bias makes ordinary least squares estimates of an income effect suspect. One solution to the selection/omitted-variable bias problem is to find an exogenous source of variation in the endogenous right-hand-side variable that is not correlated with the omitted variables and use that to identify the effect. In the case of child support payment, differences across states over time in child support enforcement provide such variation. In this section, I briefly discuss the child support enforcement climate of the 1980s to introduce the candidate instrumental variables.

The federal government has been involved in child support enforcement since the 1950s.[5] Until 1975, federal government involvement with child support enforcement was restricted to trying to recover funds spent on AFDC mothers.[6] In 1975, Congress amended the Social Security Act, establishing a federal Office of Child Support Enforcement (OCSE). Congress also authorized using federal funds to match states' expenses for tracking down non-compliant parents. Over time, the OCSE has gained more responsibility for monitoring state child support enforcement efforts. Currently 60 percent of total collections pass through some government entity.

States have many means to sanction "deadbeat" parents. States can apply liens to property, use mandatory wage withholding for all cases in arrears and inform credit bureaus of non-custodial parent's non-compliance. States must inform AFDC recipients of child support payments collected on their behalf.[7] The Tax Reform Act of 1984 changed the definition of alimony to make it harder for non-custodial parents to disguise child support payments (which are not tax deductible for the payer and are not taxable income for the recipient) as alimony (which is tax deductible for the payer and is taxable income for the recipient). The 1988 Family Support Act required states to make their guidelines for child support awards somewhat binding (or presumptive) for judges. This removed

[4] I partially address this by including controls for the non-custodial father's education and AFQT score and by presenting results where the endogenous right-hand-side variable is a dummy for having paid any child support.

[5] This section draws on United States House of Representatives (1996), Garfinkel and Robins (1994) and Williams (1994).

[6] AFDC and now Transitional Aid to Needy Families (TANF) recipients are required to assign their legal support rights to the state and to assist the state in tracking down non-custodial parents.

[7] The Child Support Enforcement Amendments of 1984 made these instruments available to states. The Child Support Enforcement Amendments also increased federal incentives for states to establish paternity and collect child support payments.

much of the discretion judges had had in setting awards of different sizes for otherwise similar cases. It also mandated that states update awards every three years after 1993 and required that states meet federal paternity establishment goals or risk losing some federal funding.

Currently the states are responsible for providing services to both AFDC and non-AFDC applicants and can use a wide variety of instruments including license revocation, interception of income tax refunds, interception of unemployment insurance and interception of other transfers.[8] The federal government assists states mainly by tracking down absent parents and by withholding IRS refunds. It is becoming more difficult for non-compliant, non-custodial parents to avoid their obligations. In 1989, the Office of Child Support Enforcement located 80 percent of the 2.5 million missing parents whose addresses were sought by custodial parents. In 1989, the IRS withheld $475 million of income tax refunds.

Child Support Guidelines

Under the current system, states use one of three types of guidelines to set presumptive child support awards (Williams 1994). The guidelines are a) an income shares standard, now used by 32 states; b) a percentage of income standard, used by 17 states; and c) the Melson standard, used by three states. All of the standards consider the income of the non-custodial parent.[9] The income shares guideline combines the income of both parents, assigns some share of that total income as child support and then assigns each parent a share of that total amount according to his or her share of combined income. The custodial parent is assumed to have paid his or her share. In the percentage of income standard, the non-custodial parent's income is calculated and he is required to pay a percentage of that income depending on how many children for which he owes support. The Melson standard, first used in Delaware in 1977, assigns a baseline subsistence amount to the non-custodial parent. The child support award is then a percentage of total parental income of both parents above this subsistence amount and the non-custodial parent's share is his fraction of total income above the subsistence level.

The following formulas describe non-custodial parents' obligations under each standard, where Order is the size of the child support order, IncomeFather (IncomeMother) is the income of the father (mother) and SetAside is the subsistence amount allowed for the non-custodial father:
Share of Income:

$$Order = \alpha\gamma IncomeFather,$$

$$\alpha = \frac{IncomeFather}{IncomeMother + IncomeFather},$$

[8]Non-AFDC individuals must pay a nominal fee to receive the same state services.
[9]For simplicity, I now refer to the non-custodial parent as he since my analysis is focused on non-custodial fathers.

Percentage of Income:

$$Order = \gamma(IncomeFather),$$

Melson:

$$Order = \alpha\gamma(IncomeFather - SetAside),$$

$$\alpha = \frac{IncomeFather - SetAside}{IncomeMother + IncomeFather - 2SetAside},$$

where in all cases γ is set by law. Until the late 1980s, these guidelines were supposed to set the amount of child support owed in the original court order. In reality, awards were infrequently updated and were written usually as fixed nominal amounts in the settlement.

States implemented various guidelines at quite different times. Arizona had an income shares guideline in 1973, while California, which originally had an income shares standard, changed to a percentage of income standard in late 1991. States also made the guidelines presumptive at different times — Illinois implemented presumptive guidelines in 1985, while a number of other states delayed implementing presumptive deadlines until they were at risk of losing federal matching funds in 1991.[10]

Collections

Collections of child support vary widely by state. In spring 1992, some 9 million women and 1.6 million men were custodial parents (United States Bureau of the Census 1995). Four million of these custodial parents requested state or federal assistance in collecting support. Of these four million parents, 35 percent of the women and 13 percent of the men lived below the poverty line. Fifty four percent of the custodial parents had a court award for child support and of these 76 percent of the mothers and 63 percent of the fathers received some payment in 1991. Mean payments among those receiving payment were $3,011 for women and $2,292 for men. In aggregate, $11.7 billion in child support were collected, 67 percent of what was due. Approximately 500,000 paternities were established in 1993 (United States OCSE 1994). Collections per case ranged from $161 in Arizona to $975 in Pennsylvania in 1991.[11] Differences in states' practices provide an exogenous source of variation in whether or not the non-custodial parent paid any child support and in the amount of child support paid.

[10] Presumptive guidelines are those which the court must use in setting awards unless a finding of fact permits the judge to set the award differently. State systems differ widely on other margins also, with some states running their enforcement systems from the department of revenue or attorney general's office while others administer it from the department of human services. A number of states require all payments to go through a court agency.

[11] Here cases includes both AFDC and non-AFDC custodial parents who used the federal and state system.

2.1.3 The Literature

Much of the literature on non-custodial parents' labor supply focuses on the ability of non-custodial parents to pay child support rather than on the possible effects of child support enforcement on non-custodial parents' labor supply.[12] One recent example is Miller et al. (1997). They examine the ability of fathers to pay in the United States and conclude that the fathers could pay up to five times as much as they are now paying. Phillips and Garfinkel (1993) found that there is a large increase in the incomes of both fathers of children whose mothers never married and of fathers who divorced, with most of the increase coming several years after paternity establishment or divorce. They concluded from this that fathers could pay much more than they are now paying. Garfinkel and Oellerich (1989) find that fathers could pay more than they do, measuring ability to pay as the projected income of the fathers based on characteristics of mothers. They find that this holds even for very poor men and for men whose children are on AFDC.[13] Using a sample from the Panel Study of Income Dynamics (PSID) restricted to once married couples, Nichols-Casebolt (1992) finds that men's incomes increase after divorce. She does not consider the behavioral responses of the non-custodial parents to the awards. Duncan and Hoffman (1985) use a sample of men in the PSID who had been divorced for at least five years and find that the income of these men falls immediately after divorce and then rises to above pre-divorce levels.

There is mixed evidence on which guidelines and what other child support enforcement practices best increase collections or establishment of child support orders for women who do not have them already. Garfinkel and Oellerich (1989) find that routine income withholding in Wisconsin increased payments by up to 10 percent. Bartfield and Garfinkel (1996) find that percentage-expressed orders resulted in larger collections over time than fixed amount awards that were updated.[14] Argys et al. (1995) find little difference between payments in states with different child support guidelines.[15] If the guidelines actually made the orders function as marginal wage taxes, then payments should respond differently to the different types of guidelines. Garfinkel and Robins (1994) report that few awards are actually updated over time, supporting my claim that in this period awards functioned as a lump sum tax.

Most studies of fathers' ability to pay fail to consider the possible effects of child support enforcement on non-custodial parents' labor supply. Theoretically, non-custodial parents and their spouses and partners will react to the "tax" implicit in child support in much the way they react to other taxes. Depending on the nature of the child support order — whether it functions as a

[12] In some studies, ability to pay is a measure of the total income of the non-custodial parent relative to the income of the custodial parent's family.

[13] Their estimates suggest that in 1983 fathers could have paid $24–28 billion, a year during which the fathers actually paid $7 billion.

[14] Percentage expressed orders were introduced in an experiment in different counties in Wisconsin at different times during the 1980s.

[15] This suggests that in practice the awards are not functioning as intended because there are different incentives embodied in the different sorts of guidelines.

marginal wage tax or as a lump sum tax — economic theory predicts different behavioral responses. Analysis of this response permits the calculation of an income effect for these non-custodial parents and informs the debate about fathers' ability to pay.

2.2 The Data

This analysis uses the National Longitudinal Survey of Youth Files (NLSY) (Baker et al. 1993). The NLSY is a long-running panel started in 1979 with 12,686 youth aged 14-21. It has a random sample and an over-sample of families in poverty, blacks, Hispanics and the military.[16] The NLSY is rich in outcome and family background variables for non-custodial fathers. It also has county and state of residence, so 1990 Census information about the father's county of residence is linked to the individual-level data.

I use several measures of labor supply, including hours worked last year and weeks worked last year.[17] The information about child-support paid available in the NLSY is either a dummy variable for having paid any child support in the last year or the actual amount paid. To identify fathers who did not pay any child support, I rely on their admission that they have children who do not live with them. This creates sample-selection issues because some men in the NLSY who are at risk for paying child support may not know that they have children or may not acknowledge that they have children. These sample-selection problems can be dealt with either by choosing a criterion for separating those men who have children not in their household or by using all the men in the sample. I limit the sample to men who admit to having children that live in someone else's household, leaving the rest of the sample available for specification testing.[18]

For the analysis that follows, I use a sample of all men who are at risk for paying child support. From the set of men who responded to the survey questions in any of the years 1984–1988, I construct an unbalanced panel consisting of all men in the NLSY in those years who are not living in the same household as all of their children and who answered the questions about payment of child support.[19] Table 1 shows variable means for all men and for the sample I use in this analysis.

The men in my sample are similar to all men in the NLSY along many dimensions. Men in my

[16] I drop the military subsample because they face different child support rules than the general population.

[17] Hours worked in a usual week and employment status are also available, but results using these measures were not informative.

[18] Since I use instrumental variable techniques, I can test the validity of the instruments by seeing whether they are significant in regressions of labor supply of the men who do not admit to having children out of the household on the instruments and controls. The instruments, if valid, should not affect the labor supply of men who do not have absent children.

[19] I focus on observations from the 1980s for several reasons. First, answers to the relevant questions about fertility and payment of child support are available for these years. The fertility questions were part of the 1988 supplement to the core questionnaire. Additionally, it is in this period that child support awards were most likely to function as lump sum taxes. This predates the widespread introduction of wage withholding and indexing of awards to wages. It also predates extensive and frequent updating of awards. Thus, I am most confident that I am identifying purely an income effect in this period.

sample worked slightly more hours in the last year than men in the whole NLSY.[20] The same holds for usual hours and for weeks worked last year. Most of the men in my final sample are well attached to the labor force — in 1988 the individual at the 10th percentile worked 15 weeks and 560 hours last year and reported usually working 12 hours a week. Of those men reporting children living in another household, 17 percent reported having paid some child support in 1988. This accords well with national data. Making the assumption that each man paid only child support to one absent custodial parent, men who paid anything paid an average of $1,420 — a little below national averages for 1988.[21]

My sample differs from the full NLSY in the share of men who are never married. Not surprisingly, a larger percentage of my sample reports being currently married while a larger share of the full NLSY (which includes all men) reports never being married. The never married men are much less likely to acknowledge having children not in their household. The other notable difference is in AFQT score — the men in my sample have higher scores.[22] Family size is also larger in my sample, which is a consequence of the higher probability of being currently married in my sample. Means for the child support enforcement measures in the state and for measures of average demographics in the county are similar across the two samples.

2.3 Empirical Strategy

I want to identify the effect of some measure of child support paid on men's labor supply. The likelihood of sample-selection/omitted-variable bias requires the use of instrumental variable techniques to estimate this effect.[23]

In my sample as found in the previous literature, fathers who pay child support to custodial parents work more than all men. This is consistent with the hypothesis that I am identifying an income effect.[24] One can not interpret the coefficients in simple ordinary least squares (OLS) regressions of labor supply variables on controls and child support causally — omitted-variable bias may be driving the OLS findings in the previous literature. There are two potential sources of omitted-variable bias. Fathers who admit to having children who do not live with them may be better fathers and better earners. This biases the sample of men who are observed to owe child support. Additionally, mothers may only seek child support awards from fathers with higher wages

[20] The difference is small — 40 hours on a mean of around 1900.

[21] Women who reported having child support awards in the NLSY also report lower averages than national samples of all age cohorts. This could be because this sample is younger than the population of all people either paying or receiving child support.

[22] This is consistent with theories of the marriage market — those with more skill are more attractive spouses and thus those in my sample are more likely to have been married.

[23] While there are concerns about systematic measurement error in women's self reports of child support received because of the ambiguity about what exactly women on AFDC report as the amount received, there is no such systematic measurement error concern here. Men have no incentive to misreport child support payments.

[24] Paying child support means that non-wage income is lower, which leads to more work if leisure is a normal good.

or more attachment to the labor force. This biases the sample of men who are observed to pay child support. As in other settings, one potential solution is to seek exogenous sources of variation in how much child support these men paid and use them as instruments. State-level indicators of aggressiveness in collecting child support provide such variation.

The instruments vary at the state-by-year (or county-by-year) level. Child support enforcement varies at the state level both cross sectionally and over time. Some states have always been very aggressive while others only adopted tools after various Congressional mandates. In a 1991 Ways and Means Committee rating of the states for overall effectiveness in enforcing child support collection, Alabama had the highest rating while Oklahoma had the lowest (United States OCSE 1992).

I chose the instruments from the large literature on child support collections.[25] The state level indicators of child support enforcement effort that I use are paternities established (normalized by the number of out-of-wedlock births) and full-time staff working for the state child-support agency (normalized by the state population) (United States OCSE various years).[26] I also use dummy variables for use of presumptive child support guidelines in the state.[27] Finally, in order to avoid confounding the effect of direct wage withholding with the income effect I wish to measure, I include a dummy variable for the small fraction of the sample where wage withholding was in effect in this period. I assume that the effort level of the state in collecting child support is unrelated to unobserved characteristics about the father that might be correlated with his labor supply decision.

There are two other categories of controls included in the analysis. I include demographic variables about the man, both to account for the sampling frame and because they would naturally enter into any selection equation. Controlling for these variables helps deal with the potential sample-selection/omitted-variable bias.

These controls include dummy variables for being black, Hispanic or being in the poor white over-sampled stratum (all three are separate sub-strata of the sample with the omitted category being non-poor, non-black and non-Hispanic). I also control for being never married or divorced, for family size, for urban residence and for the AFQT score of the father. Years of educational attainment and dummy variables for age are included in the regressions. These variables are all plausibly related to labor supply decisions.

County-level and state-level variables are included in the regressions to control for omitted characteristics about the area of residence and address concerns about legislative endogeneity.[28] I include variables for the county from the 1990 Census measuring the percentage of the population that is

[25] See Nixon (1997), Garfinkel and Robins (1994) and Bartfeld and Garfinkel (1996).

[26] While information is available about case loads and administrative dollars spent, these numbers are inconsistent across years. Dollars of child support collected are clearly an outcome of the policies themselves and should not be regressors in the reduced form or two-stage least squares (2SLS) regressions.

[27] I experimented with separate dummy variables for the different kinds of guideline but chose to use a sparser set of instruments.

[28] The county data from the 1990 Census are from the Regional Economic Information Service (United States Department of Commerce Bureau of Economic Analysis 1996).

black, the percentage of the population with at least a high school degree, the percentage of households headed by single females, median income and average commute time to work. Finally all specifications include year dummy variables and all standard errors are corrected for groupwise heteroscedasticity at the individual level (as one individual may be in more than one year of the sample).

2.4 Results

This section contains the results. I first briefly discuss the OLS results, which are in table 2. Then I turn to a discussion of the instrumental variables results.

For comparison with the later results, I display OLS results from regressions of the labor supply measures on various controls and a dummy variable for whether any child support was paid out last year in table 2. I find that hours-last-year and weeks-last-year are both highly correlated with having paid child support last year (as do other studies), with having paid any child support being associated with working 112 more hours and 2.1 weeks in the last year. This relationship remains significant as more controls are added. When individual fixed effects are included instead, the coefficient is smaller than in the regressions with various controls but larger than the simple correlation. Usual hours worked last week are not correlated with having paid any child support last year, but as different controls are added, the coefficient on having paid any child support in the last year becomes positive.

As discussed above, one should be hesitant to infer causality from the OLS estimates of the relationship between having paid any child support and the various labor supply outcomes. I turn to IV techniques to correctly identify the response.

The first stage regressions for the dummy for having paid any child support in the last year and those for the amount paid last year are generally similar. Column 1 of Table 3 shows the full first stage regression when it is whether any child support was paid that is being predicted and column 1 of table 4 shows the same coefficients when it is the amount paid being predicted. Blacks were more likely to have paid child support last year and poor whites were less likely to have done so. Divorced men were much more likely to pay than currently married men as were never married men. Surprisingly, presumptive guidelines decrease the likelihood of having paid, controlling for the county of residence, while residents of states that are more aggressive in identifying absent fathers are much more likely to pay.[29] For the other first stages see either column 1 of table 4. For the rest of the first stages, which are quite similar, I report only the F statistic on excluding the instruments. These F statistics are reported in column 2 of tables 5 and 6 for any child support paid in the last year and the amount of child support paid in the last year, respectively.

[29]The F statistic on excluding the instruments F(3,2333), is 7, which translates into a p-value of .0001. Also the net effect of moving all the instruments from the 10th percentile to the 90th percentile is an increase of .03 in the probability that the average father paid any child support last year.

In reduced form regressions of labor supply variables on the controls and instruments, the instruments are significant. For full specifications for hours worked last year and weeks worked last year, respectively, see column 2 of tables 3 and 4, respectively. Blacks, Hispanics and poor whites worked less as did never married and divorced men. Men with higher AFQT scores worked more. In column 3 of tables 5 and 6, I report the F statistics for excluding the instruments from regressions of the various labor supply variables. While the relationship is not as strong as the first stage one, it is clear that there is some response to the instruments.

Finally, I present the 2SLS estimates of the effect of either having paid any child support last year or of the amount that was paid last year on labor supply. Column 3 of tables 3 and 4 contains the full specifications for hours and weeks, respectively, with a dummy for having paid any child support as the endogenous right-hand-side variable. Column 4 of table 5 contains the coefficient on having paid any child support last year in 2SLS regressions with different labor supply variables on the left-hand-side — standard errors are below in parentheses.[30] All regressions allow for correlation among observations from the same individual and contain year and age dummy variables as well as demographic and area controls. I find significant effects on hours worked last year and on weeks worked last year, although the latter results may be somewhat spurious.[31]

Hours Worked

Turning first to table 4, we see that the 2SLS regressions imply that having paid any child support last year increases hours by 942 with a standard error of 549. This specification passes the over-identification test. While not statistically significant at standard levels, the sign of this coefficient is consistent with the hypothesis that I am identifying an income effect. This effect is too large to be attributable fully to an income effect — it implies that these men move from working an average of 34 hours a week (if one assumes a 52 week year) to 52 hours a week. To better quantify the size of the effect, I consider the effect of a increase of 0.1 in the probability that the fathers paid any child support last year.[32] A 10 % increase in the probability of paying child support translates into working for $.1 * 942 = 94.2$ more hours in a year — a more plausible increase.

Turning to the effect of dollars paid, I again find a positive coefficient that is only marginally significant. The coefficient of .30 translates into $.30 * 359$ or 108 hours more worked last year at the means, similar in size to the effect obtained with the dummy for having paid any child support

[30] Column 5 contains the χ^2 statistic for an over-identification test of the instruments with the p-value below.

[31] While I experimented with usual hours worked last week and a dummy variable for being out of the labor force as well, there seemed to be little effect of the predicted child support payment on either of these variables. No effects for usual hours worked in a week could arise if being compelled to pay child support results in men working more at their current job if they do not consider these overtime hours to be "usual hours." It could also be a result of measurement error in the usual hours measure, which is heavily bunched at 40. No effect on the probability of being out of the labor force might result if these fathers are indeed so well attached to the labor force that the child support award does not alter their work force attachment.

[32] Since in 1992, 41 percent of eligible women received some payment, this is a substantial increase in the probability of paying some child support (United States Bureau of the Census 1995).

last year. Here the specification passes the over-identification test and the corresponding elasticity is $-.30(359/1775) = -.06$. This elasticity is not out of the bounds of previous estimates as reported in Pencavel (1983). Hausman (1981), for example, reports an income elasticity of $-.17$.

Weeks Worked

The results for weeks worked are consistent with those for hours worked last year. In table 5 we see that being forced to pay child support increases weeks worked and here the coefficient is significant at standard levels. The 2SLS coefficient of 26.6 is again large but similar in magnitude to the 2SLS coefficient on hours. Unfortunately this specification fails the over-identification test rather starkly. Finally, the last row of table 5 shows that weeks worked also responds to how many dollars were paid last year and the coefficient is again significant at standard levels. Here, unlike the weeks worked result with the dummy variable for paying any child support last year, the specification passes the over-identification test. This estimate translates into an elasticity of $-.0008(359/41) = -.007$.

Specification Check

As another specification check, I estimate reduced form regressions for the part of the sample of all men in the NLSY who do not report having a child out of the household. If the methodology used above is correct, then the instruments should not be jointly significant in these regressions and the labor supply of these men not reporting children out of the household should not respond to the child support enforcement variables. In regressions not reported here, I find that for hours worked last year, the p-value on excluding the instruments is .3, while for weeks worked it is only .02. This suggests that the results for weeks worked are untrustworthy, as in theory these individuals should not respond to the child support enforcement regime. However, the specification check does support the conclusion that the hours results are valid.

2.5 Conclusion

This paper presents evidence that the effects of child support awards on the labor supply of non-custodial fathers were substantial during the 1980s. 2SLS estimates for hours worked last year with a dummy variable for "paying child support last year" imply fathers compelled to pay child support worked as much as 942 hours more. This translates into an elasticity of as large as $-.06$ for being forced to pay the average sized child support award of $359. All of these estimates are within the bounds of other estimates. While the latter estimates are close to those of most previous studies, they are still rather large. While weeks worked respond a fair amount to being compelled to pay child support in 2SLS regressions, over-identification tests suggest that this relationship is spurious. This analysis implies that previous measures of non-custodial parent's ability to pay as measured

by carnings after the child support order was assigned by the court may be overstated. Confusing the mechanical response of non-custodial fathers' labor supply to a lump sum tax with their earning capacity before any child support was awarded is misleading. This may be of importance to policy makers updating child support guidelines using information about fathers' ability to pay.

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Table 2.1: Means of Variables for 1988 Cross Section

Variable	All Men Mean (SE)	Final Sample Mean (SE)
Hours Last Year	1902.95	1946.75
	(12.91)	(18.65)
Out of Labor Force	0.08	0.08
	(4E-03)	(5E-03)
Weeks Last Year	42.63	43.12
	(0.23)	(0.33)
Paid Child Support	0.08	0.17
	(3.9E-03)	(0.01)
Dollars of Child Support Paid (1990 $)	174.8	375.1
	(11.16)	(23.2)
Age	26.94	27.35
	(0.03)	(0.05)
Black	0.26	0.29
	(0.01)	(0.01)
Hispanic	0.17	0.18
	(0.01)	(0.01)
Poor White Oversample	0.17	0.2
	(0.01)	(0.01)
Never Married	0.47	0.22
	(0.01)	(0.01)
Divorced	0.11	0.16
	(4.5E-03)	(0.01)
AFQT Score	40.01	34.08
	(0.43)	(0.58)
Highest Grade Completed	13.79	13.66
	(0.01)	(0.01)
Family Income	27385.52	26100.3
	(326.02)	(17519.62)
Family Size	2.88	3.47
	(0.02)	(0.03)

Notes: Means and standard errors of sample characteristics for 1988 cross section of panel. The first column has means (standard errors) for full sample of men, the second means (standard errors) for the sample of men at risk for owing child support. The full sample had 4741 men in it 1988 and the at risk sample had 2204 men in 1988.

Table 2.1: Means of Variables for 1988 Cross Section — Continued

Variable	All Men Mean (SE)	Final Sample Mean (SE)
Presumptive CS Guideline	0.39	0.38
	(0.01)	(0.01)
Melson CS Guideline Presumed	8.4E-04	1.4E-03
	(4.2E-04)	(7.9E-04)
Percentage of Income CS Guideline Presumed	0.09	0.09
	(4.2E-03)	(0.01)
Share of Income CS Guideline Presumed	0.28	0.29
	(0.01)	(0.01)
FT CSE Staff per Capita	0.13	0.13
	(6.1E-04)	(8.9E-04)
Paternities Estab. per Unmarried Birth	0.34	0.35
	(2.9E-03)	(4.4E-03)
Wage Withholding	0.08	0.09
	(3.9E-03)	(0.01)
% of Pop. Under 5 †	7.45	7.5
	(0.01)	(0.02)
% of Pop. Black †	14.13	14.04
	(0.2)	(0.3)
% Female Headed HH †	11.97	11.98
	(0.06)	(0.08)
% of Pop. with at Least High School †	73.95	72.95
	(0.13)	(0.19)
Median Income (1990 $) †	38962.92	37810.8
	(135.08)	(186.01)
Average Commute Time †	22.29	21.96
	(0.07)	(0.1)

Notes: Means and standard errors of sample characteristics for 1988 cross section of panel. The first column has means (standard errors) for full sample of men, the second means (standard errors) for the sample of men at risk for owing child support. The full sample had 4741 men in it in 1988 and the at risk sample had 2204 men in 1988. † County level variables. Details are in text.

Table 2.2: OLS Regressions of Labor Supply Outcomes on Child Support Paid — Various Controls

Labor Supply Var.	Coef. on any Child Support Paid	SE	Individuals	R-Sq. Adj.	Family FE	Family Controls	County Controls
Hours Last Year	112	34	2334	.002	N	N	N
Hours Last Year	391	37	2334	.15	N	Y	N
Hours Last Year	376	37	2334	.16	N	Y	Y
Hours Last Year	175	48	2334	.61	Y	N	N
Weeks Last Year	2.1	.58	2341	.002	N	N	N
Weeks Last Year	7.5	.58	2341	.16	N	Y	N
Weeks Last Year	7.2	.67	2341	.17	N	Y	Y
Weeks Last Year	3.0	.89	2341	.59	Y	N	N
Usual Hours	-.001	.43	2301	.0001	N	N	N
Usual Hours	1.40	.44	2301	.03	N	Y	N
Usual Hours	1.33	.44	2301	.04	N	Y	Y
Usual Hours	.61	.66	2301	.37	Y	N	N

Notes: Regressions are OLS regressions of different labor supply measures on a dummy for having paid child support last year and varying controls. Each panel has results for one labor supply variable with different controls. The first column is the coefficient on the child support paid dummy, the second the SE, the third the number of observations, the fourth the adjusted R-Squared and columns five through seven tell if that control was in the regression. All regressions with controls include year dummies and all regressions control for groupwise heteroscedasticity at the individual level. Last regression in each panel has family fixed effects.

Table 2.3: OLS and 2SLS Regressions of Child Support Payment and Hours of Work — Full Specification

Variable	First Stage Coef. (SE)	Reduced Form Coef. (SE)	2SLS Coef. (SE)
Paid Child Support			942.35
			(548.88)
Black	0.05	-173.47	-217.16
	(0.02)	(48.13)	(57.71)
Hispanic	0.02	-87.67	-102.08
	(0.02)	(47.15)	(46.95)
Poor White Oversample	-0.04	-173.15	-135.19
	(0.01)	(42.87)	(45.28)
Never Married	0.19	-478.84	-661.7
	(0.02)	(42.12)	(114.01)
Divorced	0.37	-280.38	-630.51
	(0.02)	(40.94)	(209.22)
Urban	-1.5E-03	38.19	45.7
	(0.02)	(44.6)	(43.4)
AFQT Score	4.3E-04	4.11	3.69
	(2.5E-04)	(0.64)	(0.7)
% of Pop. Under 5 †	-4.4E-03	44.92	51.31
	(0.01)	(19.33)	(19.27)
% of Pop. Black †	1.6E-03	10.02	9.06
	(9.2E-04)	(2.16)	(2.32)
% Female Headed HH †	-6.8E-04	-34.07	-35.72
	(3.6E-03)	(9.78)	(9.37)
% of Elig. Pop. in Public School †	3.0E-03	2.7	0.22
	(1.4E-03)	(3.61)	(3.88)
% of Pop. with at Least High School †	-1.8E-03	-10.75	-9
	(1.1E-03)	(2.79)	(3.04)
Median Income (1990 $) †	3.5E-06	0.01	0.01
	(1.2E-06)	(2.8E-03)	(3.4E-03)
Average Commute Time †	-8.0E-04	-6.28	-5.75
	(1.6E-03)	(4.19)	(4.29)
Wage Withholding	-0.04	-46.27	19.93
	(0.03)	(63.76)	(63.1)
Presumptive CS Guideline	-0.04	-27.68	
	(0.01)	(33.2)	
FT CSE Staff per Capita	-0.25	-684.2	
	(0.17)	(422.51)	
Paternities Estab. per Unmarried Birth	0.12	122.77	
	(0.03)	(84.68)	

Notes: First (second) column contains coefficients on control variables and instruments in first stage (reduced form) regression of whether or not the non-custodial parent paid any child support last year (hours of work last year) on controls and instruments for part of sample answering question about total hours last year. Third column contains coefficients for 2SLS regressions with same instruments and controls, with dummy for whether any child support was paid last year as the endogenous RHS variable. Each column contains coefficient on row variable with SE below. All regressions include year fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the individual level. F statistic on exclusion of instruments in first stage is F(3,2333) = 7.14 with a p-value of .0001.
† County level variables from 1990 Census.

Table 2.4: OLS and 2SLS Regressions of Child Support Payment and Weeks of Work — Full Specification

Variable	First Stage Coef. (SE)	Reduced Form Coef. (SE)	2SLS Coef. (SE)
Paid Child Support			26.64
			(10.38)
Black	0.05	-2.14	-3.29
	(0.02)	(0.88)	(1.09)
Hispanic	0.02	-0.07	-0.43
	(0.02)	(0.83)	(0.88)
Poor White Oversample	-0.04	-3.38	-2.29
	(0.01)	(0.72)	(0.82)
Never Married	0.19	-9.44	-14.64
	(0.02)	(0.83)	(2.21)
Divorced	0.37	-6.27	-16.15
	(0.02)	(0.77)	(3.98)
Urban	-8.0E-04	0.82	1.1
	(0.02)	(0.78)	(0.8)
AFQT Score	4.3E-04	0.1	0.08
	(2.5E-04)	(0.01)	(0.01)
% of Pop. Under 5 †	-3.9E-03	0.73	0.92
	(0.01)	(0.34)	(0.35)
% of Pop. Black †	1.6E-03	0.18	0.15
	(9.2E-04)	(0.04)	(0.05)
% Female Headed HH †	-1.1E-03	-0.47	-0.53
	(3.6E-03)	(0.17)	(0.18)
% of Elig. Pop. in Public School †	2.7E-03	0.07	0.02
	(1.4E-03)	(0.07)	(0.07)
% of Pop. with at Least High School †	-2.0E-03	-0.17	-0.11
	(1.1E-03)	(0.05)	(0.06)
Median Income (1990 $) †	3.4E-06	2.6E-04	1.3E-04
	(1.2E-06)	(5.1E-05)	(6.4E-05)
Average Commute Time †	-9.4E-04	-0.12	-0.1
	(1.6E-03)	(0.08)	(0.08)
Wage Withholding	-0.05	-2.47	0.13
	(0.03)	(1.13)	(1.18)
Presumptive CS Guideline	-0.04	-1	
	(0.01)	(0.6)	
FT CSE Staff per Capita	-0.25	-24.9	
	(0.17)	(7.33)	
Paternities Estab. per Unmarried Birth	0.13	3.04	
	(0.03)	(1.52)	

Notes: First (second) column contains coefficients on control variables and instruments in first stage (reduced form) regression of whether or not the non-custodial parent paid any child support last year (weeks of work last year) on controls and instruments for part of sample answering question about total weeks worked last year. Third column contains coefficients for 2SLS regressions with same instruments and controls, with dummy for whether any child support was paid last year as the endogenous RHS variable. Each column contains coefficient on row variable with SE below. All regressions include year-fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the individual level.

F statistic on exclusion of instruments in first stage is F(3,2333) = 7.35 with a p-value of .0001.

† County level variables from 1990 Census.

Table 2.5: 2SLS Regressions of Labor Supply Measures on Dummy for Having Paid Child Support

LS Outcome	Mean of LS Outcome (Any Paid Last Year)	F 1st Stage (p-Val.)	F Red. Form (p-Val.)	Coef. on CS Paid 2SLS (SE)	χ^2 Over-Id. (p-Val.)
Hours Last Year	1775.73	7.2	1.4	942	2.42
	(0.16)	(0.00)	(0.24)	(548)	(0.5)
Weeks Last Year	40.93	7.4	4.7	26.6	10.1
	(0.16)	(0.00)	(0.00)	(10.4)	(0.02)

Notes: Column entries are either F statistics (p-values) for the exclusion of the instruments from OLS regressions of whether any child support was paid in the last year on the controls and instruments (first stage), of the labor supply measure on the controls and instruments (reduced form) or coefficients (SEs) from 2SLS regressions with labor supply outcomes on the LHS. Endogenous regressor is a dummy for non-custodial parent having paid any child support last year. Last column is χ^2 statistic (p-value) for over-identification test. For details see text. All regressions include year fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the individual level.

Table 2.6: 2SLS Regressions of Labor Supply Measures on Amount of Child Support Paid

LS Outcome	Mean of LS Outcome ($ Paid Last Year)	F 1st Stage (p-Val.)	F Red. Form (p-Val.)	Coef. on CS Paid 2SLS (SE)	χ^2 Over-Id. (p-Val.)
Hours Last Year	1775.73	5.0	1.4	0.30	1.1
	(359.5)	(0.00)	(0.24)	(0.17)	(0.77)
Weeks Last Year	40.93	5.0	4.7	0.009	3.8
	(359.5)	(0.00)	(0.00)	(0.004)	(0.3)

Notes: Column entries are either F statistics (p-values) for the exclusion of the instruments from OLS regressions of how much child support was paid in the last year on the controls and instruments (first stage) of the labor supply measure on the controls and instruments (reduced form) or coefficients (SEs) from 2SLS regressions with labor supply outcomes on the LHS. Endogenous regressor is how much child support the non-custodial parent paid last year. Last column is χ^2 statistic (p-value) for over-identification test. For details see text. All regressions include year-fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the individual level.

Chapter 3

The Effects of WIC on Children's Outcomes

Joint with Janet Currie and Duncan Thomas

Introduction

Society is engaged in an ongoing debate over what transfers the government should provide individuals. The Clinton administration has increasingly emphasized in-kind transfers while cutting cash support. The Temporary Assistance to Needy Families program (successor program to AFDC) leaves many states free to cut benefits in several years and decouples states' grants from case loads. At the same time, the president has proposed large increases in in-kind programs. While there is extensive evidence that programs such as Head Start and WIC have short-term effects on children's outcomes, evidence of long-term effects of these programs is scarce. In this paper, we analyze the effects of the WIC program on both short-term outcomes and on longer-term cognitive and developmental outcomes for children.

Numerous studies have concluded that WIC is beneficial for children. Unfortunately, many merely regress measures of children's well being on WIC use. It is problematic to interpret these results from ordinary least squares (OLS) regressions as evidence about the causal effects of WIC. Since WIC is not funded fully, and since WIC participants are not representative of the population at large, simple OLS estimates of the effects of WIC may be biased in either direction. To be eligible for WIC, women must be both nutritionally at-risk and under 185 percent of the poverty line. If these women are less well-off on other dimensions than the population at large, OLS estimates of the effects of WIC may be biased down. Alternatively, if only the more skilled mothers among women eligible for WIC manage to obtain benefits, OLS estimates of the effects of WIC may be biased upwards. To address this sample selection/omitted variable bias, we take several approaches, using both instumental variable methods and specifications that identify off of within family differences by including family fixed effects. In the instrumental variable analysis, we use state- and county-level indicators of WIC generosity to predict WIC use and then do two-stage least squares (2SLS) analysis of the effects of WIC on various outcomes. The other approach uses family level fixed effects to proxy for unobserved heterogeneity, both for the full sample and for a subsample of families where siblings had different histories of WIC use.[1] Using these approaches we find mixed evidence of the effects of WIC on pregnancy outcomes, test scores and other program use.

The paper is organized as follows. We begin with a brief background section about the WIC program. Section 2 describes the previous literature. Section 3 discusses the data set. The empirical strategy is laid out in more detail in section 4. Section 5 presents the results and section 6 concludes.

[1] By comparing outcomes for siblings with different histories of program use, we follow the example of Currie and Thomas (1995) who used this approach to assess the effects of Head Start, another in-kind transfer program.

3.1 The WIC Program

WIC is a federally-funded, state-run program to provide direct nutritional supplements and nutritional advice to pregnant women, postpartum women, infants and children who are nutritionally at-risk and low-income.[2] Established in 1972 in an amendment to the Child Nutrition Act of 1966, WIC was introduced to improve pregnancy outcomes. It has been expanding rapidly over time — federal program expenditures increased from $256 million in fiscal year 1977 ($650 million in 1995 dollars) to $3.45 billion in 1995 while participation went from 848,000 in 1977 to 6.9 million in fiscal year 1995. Almost half of 1995 participants were children.

Participants receive either nutritional supplements or vouchers for such supplements. By law, the foods must contain protein, calcium, iron and vitamins A and C. There is a list of approved foods including milk and cheese. Participants receive nutritional counseling and are encouraged to breast-feed their children. Many WIC programs are contracted out to non-profit agencies. In order to be eligible for the program, individuals must be low income (under 185 percent of the poverty level), nutritionally at-risk (definitions of this vary by state) and in one of the categories (pregnant or postpartum women, infants or children under five). Medicaid, Food Stamp and AFDC participants are presumed eligible and WIC providers refer mothers to other health care providers such as Medicaid and immunization programs. These other agencies also refer eligibles to WIC clinics. Two-thirds of program recipients also participated in some other public assistance program.[3] WIC is not an entitlement and states vary widely in WIC generosity.[4] Recent expansions in the WIC program mean its effects on children are of interest to policy makers.

3.2 The Literature

An extensive literature documents the benefits of WIC for fetal development, ranging from increased birth weight to longer gestation periods.[5] WIC has been repeatedly shown to lower infant mortality, reduce the probability of having a low-weight birth and decrease incidence of childhood anemia. Devaney et al. (1992) find that every dollar spent on WIC cut Medicaid expenditures by at least $1.33 and as much as $3.13. A range of studies find WIC use is associated with increased birth weight. Others find WIC use is associated with a decrease in the proportion of low birth-weight deliveries, but has no effect on overall average birth weight. The methodologies used in these studies range from quasi-random assignment to matched control groups, yet few convincingly deal with the

[2]Program information is drawn from United States Department of Agriculture (1995a, 1995b).

[3]Twenty five percent of WIC recipients also received AFDC in 1995 while 42 percent of AFDC participants used WIC (United States House of Representatives 1996). In 1994, 52 percent of WIC participants received Medicaid benefits (United States Department of Agriculture 1995a).

[4]In 1993, 98 percent of eligible infants and 60 percent of all eligibles received WIC. Vermont serves almost 100 percent of eligibles (United States Department of Agriculture 1995b)

[5]This summary draws heavily from Currie (1995).

issue of selection bias. One exception is a recent paper by Brien and Swann (1996) using instrumental variable techniques and sibling differences to identify the effects of WIC. They find significant effects of WIC on pregnancy outcomes using sibling differences but not using 2SLS, with the largest effects being for blacks. Their instrumental variables analysis can only make use of state-level variation in WIC program characteristics. WIC is found to have extensive effects on children's consumption of total calories and micronutrients by Fraker (1990). Fraker uses a sample selection model but his identification is not clearly exogenous.

Few studies document either the benefits of WIC for children's outcomes after birth or the possible effects of past WIC use on older children, perhaps because few data sets have information about both WIC use after birth and other outcomes. This leaves room for more work on the longer term effects of the program, the focus of this paper.

3.3 The Data

This analysis uses the National Longitudinal Survey of Youth Child-Mother Files (NLSY) (Baker et al. 1993). The NLSY is a long-running panel started in 1979 with 12,686 youth aged 14–21. It has a random sample and over-samples of the following groups; families in poverty, blacks, Hispanics and the military. During the 1980s, children of original NLSY participants were included and now the NLSY has data on 10042 children of 4483 mothers. The NLSY is rich in outcome and family background variables for the children and mothers. Since it also has county and state of residence, individual data can be linked to information about the county of residence from the the 1990 Census and other county-level data sets. Other data sets with information about WIC use are the Survey of Income and Program Participation and the National Center for Health Statistics' Natality data. Clearly the Natality data only provides limited information about children's outcomes. The Survey of Income and Program Participation has much of the same background information as the NLSY, but only provides information about state of residence.

WIC Participation

Because mothers are only asked about WIC use in survey waves after 1989, there are at most five years for which one knows whether a child was exposed to WIC. Furthermore, many of the outcome variables are from the child assessments (done only in even years), so for some outcomes there is only one year when there are measures of both the outcome and contemporaneous WIC use. Since it is impossible to tell anything about the family's use of WIC in years before 1990, we limit the sample to child observations in years after 1990 when the child was age eligible for WIC. We also know if some children were exposed to WIC prenatally.

There are many measures of WIC participation, the simplest being whether the child's family

used WIC in the previous year. Since WIC is reported by the mother for the whole family and not for each child, for children aged one-to-five in households with either prgnant women or infants it is never clear whether these children in fact used WIC or if it was just used by the mother or infant.[6] For children aged one-to-five, we construct a dummy variable for being in a family on WIC when the mother is not pregnant and there are no infants in the household. This variable is a better measure of WIC use for these older children as in every state, infants and pregnant women are a higher priority group for receiving WIC benefits. We create dummy variables for having been on WIC at various ages and a dummy variable for having been on WIC at any time in the sample. Finally, there is a variable ranging from zero to one that measures the share of time in the sample the child spent on on WIC.

Table 1 contains means for these WIC participation variables. Different rows represent different measures of WIC use. In 1994, 16 percent of age eligible children were on WIC and 12 percent were on WIC in households with no pregnant woman or infant. Twelve percent of the sample was on WIC during gestation, while 26 percent were on the program at birth. Program participation drops as the children age — only 16 percent of the children were still on the program at age four.[7] The average share of time in the sample these children spent on WIC was .20 (i.e. the average child in the sample spent 20 percent of her time in the sample on the program). These numbers are similar to those from other sources. A 1992 Department of Agriculture study found that 73 percent of WIC participants had incomes below the poverty level. In this sample, 73 percent of the children lived in homes with incomes below 185 percent of the poverty line (United States Department of Agriculture 1995b).

In 1994, 33 percent of WIC participants in the sample were on AFDC and 53 percent were receiving Food Stamps, while nationally 33 percent of WIC participants were on AFDC and 87 percent received Food Stamps (United States House of Representatives 1996). Nationally, 22 percent of AFDC participants also use WIC, while in our sample 32 percent reported being on WIC. Sample numbers understated national measures for WIC participants' receipt of Food Stamps, while they match closely those for AFDC use. Sample means for AFDC participation are larger than national estimates for 1994 while sample means for Food Stamp receipt are lower than national numbers.[8]

[6]WIC nutritional packages for different categories of eligibles are different so in principle the supplements received by children on the program differ from those used by the mothers. In practice, it seems likely that some of the WIC products would be used by anyone in the household, whether age eligible or not. By dropping children too old to be on WIC (aged five or older) we ignore possible spill-over effects on these children.

[7]This could be due either to either a decreased likelihood of being on the program as the child ages or to increased funding of WIC in later years.

[8]This discrepancy could be due either to measurement error or to the fact that the NLSY sample is disproportionately young.

Child Outcomes

The child outcomes fall into one of several categories — they are indicators of infant health, measures of the mothers' behavior during pregnancy or test scores. Not all tests are administered to all children in each child assessment — either because the tests are not age appropriate or because of survey limitations, so there are many fewer observations of each test score than of the years of exposure to WIC. Other outcomes we can examine are use of Food Stamps, Head Start or Medicaid. Program participation information is available in all years.

The following information about the pregnancy is available: if the mother had any prenatal visits, if the mother smoked less, if the mother smoked less because of a doctor's advice, if the mother drank, if the mother drank less, if the mother drank less because of a doctor's advice, if the mother took vitamins, if the mother took vitamins because of a doctor's advice, if the mother cut calories, if the mother cut calories because of a doctor's advice, if the mother cut salt use, if the mother cut salt use because of a doctor's advice. Other indicators include the weight change of the mother during the pregnancy, the length of gestation of the child in weeks, the birth weight of the child, if the child was a low birth-weight baby, if the child got all three DPT vaccinations, and if the child was breast-fed.

There is a smaller sample size for these measures than for WIC use. We only know about prenatal WIC use for the cohort of children born after 1989. The vaccination questions were only asked until the 1990 wave of the survey, so there are no observations with information about both WIC participation and measles vaccinations and very few with information about the DPT vaccinations and WIC.

For means of the pregnancy outcomes see table 2. Note that participants are worse off on some observable dimensions and better off on others. They were just as likely to have had prenatal visits or to have had mothers who smoked and their mothers were just as likely to take vitamins on a doctor's advice during pregnancy as were mothers not on WIC. Their average gestation time was the same as that of children whose mothers did not use WIC. The children on WIC during gestation were more likely to have mothers who did not drink, who reduced drinking due to a doctor's advice, who reduced salt intake on a doctor's advice, who cut calories, who cut calories on a doctor's advice, who had less salt, who reduced salt due to a doctor's advice or who took the children to get all their DPT shots; all positive outcomes for the children. However these children were also more likely to have been low birth-weight babies (under 2.5 kilos), were less likely to have had mothers who gained weight during pregnancy, were less likely to have been breast-fed and on average had lower birth weights; all possibly bad outcomes.[9]

All the test scores are standardized for age. There are three sorts of tests in the NLSY —

[9]Lower incidence of breast-feeding among participants is not surprising. Until recently, WIC promoted formula use.

tests of cognitive development, tests of motor and emotional development and tests of the child's home environment. The cognitive test scores are:[10] the Peabody Individual Achievement Test (PIAT) given to children five and over with three parts, the PIAT Math test (ages 5 and older), the PIAT Comprehension test (ages 4 and older), measuring the ability to read and understand words, and the PIAT Reading test (ages 5 and older), measuring oral reading ability; the Peabody Individual Vocabulary Test–Revised (PPVT) test (ages 3 or older), measuring the hearing vocabulary in "Standard American English" and the Digit Span Subscale of the Wechsler Intelligence Scale for Children (DIGS) (ages 7 and older), measuring short term memory. The motor or emotional development tests are: the Behavioral Problems Index (BPI) test (ages 4 and under), completed by the mother and measuring the child's behavior in six problem areas, and the Motor and Social Development Scale (MSDS) (ages 0–4), completed by the mother and measuring whether the child has attained "developmental milestones." The only home environment test is the Home Observation for Measurement of the Environment (HMS) test (all ages), measuring the safety of the home environment and carried out by the assessor.

For means of these test scores broken down by WIC use, see table 3. Children on WIC are worse off than those not on WIC according to these measures, albeit not statistically significantly so. They have lower scores on every cognitive test and motor or social development test listed. Interestingly, they have larger score differences on the tests measuring oral ability than on those measuring written ability. None of these differences are statistically significant at the five percent level. However, children on WIC have slightly higher (but not statistically significantly so) BPI scores.

There is also interaction between other programs and WIC. The NLSY asks whether the children are covered by Medicaid or MediCal, have ever been on Head Start or live in a household receiving Food Stamps. These programs are usually required to refer mothers to WIC if the mothers are eligible. For means of these variables, see table 4. As expected, children in households receiving WIC are more likely to have Medicaid and to be receiving Food Stamps than children in households not on WIC.

For other characteristics of the sample, see table 4. Children in households using WIC are younger, more likely be black or Hispanic, more likely to be living in never married or divorced households, are born to younger mothers and are born to mothers who scored lower on the AFQT (a test of readiness for work created by the US Army). These children live in larger households and are less likely to have been first born. Counties of residence of children on WIC are similar in the percentage of black residents, the percentage of the population under five and the percentage of single mother headed households. Children on WIC live in counties with lower median income and a lower percentage of high school graduates than those not on the program.

[10]The ages at which the test is administered appear in parentheses.

Final Sample

The final sample consists of an unbalanced panel with all children in the sample in any of the years between 1990 and 1994, who lived with their mothers and were age eligible for WIC in that year. All the older children are dropped as are those who do not live with their mothers.[11] The sample consists of observations at either the child or the child-year level. If the child was age eligible for WIC in more than one year, then more than one observation may be included for that child. Since all the children of an original participant are followed, there can be more than one observation per child (a child could be in the sample for up to four years) and more than one child per family. In all the regressions, standard errors are corrected for the groupwise heteroscedasticity introduced by having many observations from individuals in the same family.[12]

3.4 Empirical Strategy

If there were no issues of sample selection, the coefficient of interest would be that on the WIC variable in the following equation:

$$ChildOutcome = \alpha WIC + \beta' X + \epsilon,$$

where X includes represents demographic and locational information about the child and family. Clearly, simple OLS regressions of the effect of WIC on these child outcomes can not be interpreted causally, due to concerns about self-selection into WIC or omitted variable bias. We deal with this in three ways. First, we carefully control for observables about the family and county of residence. Then we identify causal effects in two ways; by using instrumental variables techniques to identify the effects of WIC stemming from exogenous shifts in the probability of WIC use and by including family fixed effects. The results using family fixed effects are informative, but still open to some criticism about sample selection. We also create a smaller sample with children from the same families who were and were not exposed to WIC at some age and compare their test scores in regressions with a family fixed effect.[13] Since the tests were not given to all children in all years, the samples created this way are very small.

There are two possible effects of WIC on outcomes we might care about; the effect of contemporaneous WIC use in that year and the effect of past WIC use. In regressions to assess the effects of contemporaneous WIC use, we simply pool the observations from different years for WIC, outcomes and demographic controls for estimation. The latter regressions contain measures of WIC use at various ages or of the share of the time spent in the sample that the child spent on WIC before the

[11] We drop the older children to focus on the direct effects of WIC use rather than spill-over effects.
[12] Clearly observations from the same family are not independent.
[13] This is similar to the analysis in Currie and Thomas (1995).

age at which the test was administered.

Instrumental Variables (IV)

One solution to sample selection or omitted variable bias is to look for exogenous sources of variation in the use of WIC. Potential instruments are variables which are strongly correlated with WIC use but not correlated with the omitted variables. If the instruments satisfy these conditions, the instruments can be used to obtain 2SLS estimates of the effect of WIC on the various outcomes.

We use two kinds of instruments — measures of WIC generosity which vary at the state-year level and measures of ease of access to a WIC clinic which vary at the county level.[14] Unlike some other welfare programs, WIC benefits are too small to make it likely that women moved to a state for WIC benefits. However, states with generous WIC may be generous on other dimensions which induce people to move. Alternatively, states may respond to a need for WIC by increasing WIC generosity. In either case, program characteristics may not be suitable instruments. We control for county of residence in all specifications either by explicitly including a variety of county-level controls from the 1990 Census or by including county-fixed effects, which subsume state-fixed effects.

The state-level indicators include: the percentage of the case load that is children, infants or mothers; actual case load numbers separated by category of recipient; average administrative dollars spent per case; average food dollars spent per case and the total number of clinics (United States Department of Agriculture various years). Actual total dollars spent (separated by category of participant) were also available but the average expenditures per case are a better measure of how generous are the states' WIC programs. For means of the different instrumental variables, see table 5. Children on WIC live in states with more WIC clinics per capita and with a higher percentage of the case load that is children. Average WIC expenditures per case on food or administration are similar for those on and not on WIC.[15]

The county level instruments are constructed from data used in Currie and Gruber (1996). In that paper, zip-code level measures from the 1990 Census of the probability that a woman of a given age-race-education cell lived in that zip code were created. These probabilities were used as weights to aggregate information for the zip code on the distance to the nearest WIC regional headquarters to the county level. Thus, probabilities were constructed that a woman in a given age-race-education cell in the county was 0 miles from a clinic, 0–10 miles from a clinic, 10–20 miles from a clinic, 20–30 miles from a clinic, 30–40 miles from a clinic, 40–50 miles from a clinic, 50–70 miles from a clinic, 70–100 miles from a clinic or more than 100 miles from a clinic.

The age cells are either 25–34 or 35–44, with the vast bulk of the women falling in the 25–34

[14]The distance measures were constructed as in Currie and Gruber (1996).
[15]All dollar figures are in 1990 dollars. They were converted from nominal figures using the CPI.

range. The race cells are black or non-black.[16] The education categories are less than nine years of schooling, 9–11 years of schooling, exactly 12 years of schooling, 13–16 years of schooling and 16 or more years of schooling.

Instead of using these 10 WIC variables representing probabilities as instruments, we create dummy variables for the probability that the child lives less than 10 miles from a clinic, lives between 10 and 20 miles from a clinic or lives more than 20 miles from a clinic. In table 5, we see children on WIC are less likely to be more than 20 Miles from a WIC clinic in their county of residence and more likely to live within 10 miles of one.

Within Family Differences

Another solution to potential omitted-variable bias is to specify the error term to account for unobserved heterogeneity. The simplest such specification is to include family fixed effects in regressions of the outcomes on WIC use. A cleaner identification scheme uses the same specification with the sample restricted to families where one child was on WIC at some age and another child was not. Unfortunately, there are only a few such families.[17] While results from this intra-family comparison may be interesting, the small sample sizes mean they are unlikely to be terribly informative.

There are two other categories of controls included in the analysis. First, we include demographic variables about the family and child, both to account for the sampling frame and because they would naturally enter into any selection equation for WIC use.[18] These controls include dummy variables for being black, Hispanic or being in the poor white over-sampled stratum. Also included are dummy variables for being in a never married or divorced family, a family size variable, a dummy variable for urban residence, the AFQT score of the mother, a quadratic in the age of the mother at first birth and the age of the child at the mother's interview. In the regressions with the PIAT and PPVT tests, the standardized (by school attendance) age of the child is included.

The other controls are county-level and state-level variables (means are in table 4). Their inclusion should ameliorate concerns about legislative endogeneity.[19] Where relevant, there are year-dummy variables. The regressions include both county-level aggregates and variables constructed from the zip-code level 1990 Census tabulations (aggregated from Census data to the county level for the race-age-education cell of the mother).[20] The percentage of the population that was black, the percentage of the population under five, the number of births, the percentage of households

[16]For Hispanics, the mothers' self reported primary racial identification allows one to impute whether the individual is black or not. This treats all non-blacks as whites, done because of the small probability in many zip codes of being neither black nor white.

[17]Only 2.1 percent of the sample had been exposed to WIC at some age that one of their siblings had not. Since not all of these children will have taken all tests, this means that the sample sizes here are small.

[18]By including these variables in the regressions, we hope to help account for potential selection.

[19]The county data from the 1990 Census came from the Regional Economic Information Service (United States Department of Commerce Bureau of Economic Analysis 1996).

[20]These also borrow from the Currie and Gruber (1996) methodology.

headed by single mothers, median income in 1990 dollars and the percentage of the population in the county that had at least a high school education are all county level aggregates. The percentage of the population under five, the percentage of female headed households, median family income, the percentage of blacks and the percentage of high-school dropouts are also available as probability weighted, zip-code level 1990 Census data aggregated to the county level. In the results, the zip-code-aggregated variables are included in the first stage, reduced form and 2SLS regressions. Clearly the number of female-headed households and the number of births are proxies for the demand for WIC services. Median family income is another measure of how well off the community itself is, as is the percentage of high-school dropouts. These variables were chosen because they form a limited set of variables that capture aspects of the county environment that either account for WIC generosity in a county (supply side characteristics) or control for demand for WIC in the county.[21]

3.5 Results

For comparison purposes with the other results, we include some simple OLS regressions in table 6. Next we present results for the infant and pregnancy outcomes finding more evidence of some positive effects of WIC. We present evidence of the effect of WIC on test scores using IV analysis and within-family variation. This is followed by a brief discussion of the effects of past WIC use on current test scores. Finally, results about the effect of WIC on the use of other social insurance programs are presented.

OLS Results

In table 6, each row represents a different regression. More controls are added to the regression in each row of the panel (the inclusion or lack of family fixed effects, demographic or area controls is indicated the last three columns). In the first panel, the outcome variable is the PIAT math score, in the second it is the PPVT score and in the third it is whether or not the child was a low birth-weight baby. Turning to the first row, the PIAT math score is negatively correlated with contemporaneous WIC use. This effect disappears as demographic and county of residence controls are added. In the regressions with family fixed effects, we see that WIC is actually associated with an increase in PIAT math test scores (but not a statistically significant one). Demographic variables seem to add more explanatory power than the area controls. Similarly, the PPVT score is negatively correlated with WIC participation. This coefficient is still negative and significant in the regressions with all

[21] We experimented with other county variables including federal government spending, Hispanic population share and age-range population shares. However F tests showed that they added little. Furthermore, many of the population numbers are highly correlated with one another. The federal grant numbers are perhaps not the most appropriate measures of WIC generosity.

controls and is negative but insignificant in the family fixed effect regressions. Finally, we see that the probability of being a low birth-weight baby is not significantly correlated with WIC use.

Results for Pregnancy Outcomes

The 2SLS results for the effects of being on WIC during pregnancy are in table 8. In table 8, each row entry is the coefficient (SE) in a regression of a pregnancy outcome on all the controls and on use of WIC during pregnancy, instrumenting with the policy variables. For a typical first stage regression for the 2SLS analysis, see table 7.[22] Since the samples are similar across outcomes, the other first stage regressions are quite similar. In table 8, each pair of rows represents the results for one regression. Only the coefficient on WIC from the 2SLS regressions is reported, along with the variable means and whether or not the specification fails the over-identification test.[23]

The first stage regressions are encouraging for all outcomes except smoking less, drinking less on a doctor's advice and cutting calories on a doctor's advice. The reduced form regressions show promise for predicting smoking less, drinking during pregnancy, drinking less and whether the child was breast-fed. Turning to the 2SLS results, we find that WIC use increases the likelihood that the woman will smoke less based on a doctor's advice, decreases the likelihood that the the mother drank less, increased the probability that the mother took vitamins on a doctor's advice and decreased the likelihood that the mother breast-fed. All of these specifications but the one for having breast-fed the child and smoking less pass the over-identification tests. While some of these results match findings in the previous literature, the results for the woman drinking less are contrary to what other researchers have found.

Next we turn to within-family identification to look at the effects of WIC on pregnancy outcomes. The first three columns of table 9 contain family fixed effect regressions for pregnancy outcomes where the RHS variable of interest is WIC use during pregnancy. Here we see that WIC reduces the likelihood that the mother cuts salt intake but increases the mother's weight change during pregnancy, increases the birth weight of the child and decreases the likelihood that the child is of low birth weight, all (save the result about salt) similar to what previous studies have found. We also experimented with similar regressions restricting the sample to those families where the mother was using WIC during one pregnancy and not during another pregnancy. These results are presented in columns 4–6 of table 9 but small sample sizes makes them of doubtful use.[24]

[22] In table 7, we see blacks and Hispanics are more likely to be on WIC, never married and divorced women are more likely to be on WIC, larger families are more likely to be on WIC (more chances to learn about WIC) and families where the women had a higher AFQT score are less likely to be on WIC. The F statistic for the instruments being jointly insignificant in the regression is F(5,2407) = 2.26. The average administrative dollars per case is left out of the set of instruments.

[23] For the F statistics for the first stage and reduced form regressions and the value of the χ^2 variable in the over-identification test, see appendix table A-1. The over-identification test consists of regressing the residuals from the 2SLS regressions on the instruments and all the exogenous variables and seeing if the R-squared in this regression is large. If it is, then there is a problem with the identification strategy.

[24] Here WIC use for these few families seems to decrease the likelihood that the mother took vitamins by 26 percent

Results for Test Scores

While we have found some evidence to support other findings of positive effects of WIC on pregnancy outcomes, this is not the focus of the analysis. Now, we turn to analysis of the effects of WIC use on later cognitive outcomes. We might expect WIC to have positive effects on test scores, both contemporaneously and in the long run. WIC is a nutrition program and it has been shown that lack of access to micronutrients has negative effects on cognitive development. Furthermore, access to the educational component of WIC may result in better nutrition for all the children. In table 10, we present results from 2SLS regressions of test scores on contemporaneous WIC use.[25] In column 2, we see that the coefficient on WIC in 2SLS regressions is only statistically significant at standard levels for the Motor and Social Development test, where it has a fairly large (-22 points on a score normed to 100) negative effect.

One might worry that these results confound the spill-over effects of WIC when the mother or an infant are receiving it with the true effects for these older children when they receive it. To identify the direct effects, we estimate the same 2SLS regressions with a dummy variable that is one only if the family received WIC in the last year, the mother was not pregnant and there was no other infant in the household. Table 11 is similar to table 10, but here the endogenous regressor is WIC use this year when there was no pregnant woman or infant in the household. In the 2SLS regressions, WIC again is associated with a significant negative effect on the MSD score. Again, the specification for the MSD test passes the over-identification test.

One reason why these estimates for the test scores may be so indeterminate is that these regressions use only a measure of contemporaneous WIC use. One expects that this is the important thing for the pregnancy outcomes. Perhaps it is the entire history of WIC use that matters for some test scores. We now turn to results with family fixed effects. Inclusion of a family fixed effect resolves some concerns about omitted variables or selection. The upper panel of table 12 contains the results of regressions with the endogenous RHS variable being the share of time spent on WIC for a variety of the test scores. The lower panel of table 12 contains similar regression results when the endogenous RHS variable is the share of time spent on WIC in a household when no other infants and no pregnant women were present.

In table 12, we see that WIC seems to be significant only in the regressions for the PPVT, the Digit Span Subscale and interestingly enough not for the MSD. Thus, these regressions seem to hint that WIC cause a mild increase in the PPVT score, while causing DIGS scores to go down a huge amount.[26]

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while it increased the likelihood that if she took vitamins it was because of a doctor's advice.

[25] Again more information about the first stage and reduced forms is available in appendix tables A-2 and A-3. Turning to the reduced form regressions in the appendix, the instruments are jointly significant at the 15 percent level or lower for the PIAT cognitive test, the PIAT reading test, the PPVT, the Digit Span Subscale, the Motor and Social Development Scale (MSD) test and the Home Inventory test.

[26] The DIGS score is standardized at 10 and the coefficient on share of time spent on WIC is -7.

Looking at the lower panel of table 12, we see the coefficients for the share of time on WIC with no pregnant women or infants are similar to those for contemporaneous WIC use except that the coefficients are slightly larger. Thus the within-family results seem to indicate that WIC use has some mild benefits, while the 2SLS results are fairly inconclusive except that WIC use seems to have negative effects for children's motor development as measured by the MSD test.

Results for Use of Other Programs

Finally, we turn to the effects of WIC on take-up of other programs. Again we discuss the 2SLS results first. Table 13 presents the 2SLS results with the endogenous regressor being contemporaneous use of WIC.[27] Here, the coefficient on WIC use is large and positive for all three other programs - Food Stamps, Medicaid use and ever having been on Head Start, although not overwhelmingly significant. Only the specification for use of Head Start passes the over-identification test. The effects here are large — being on WIC means a considerably higher probability of ever having been on Head Start. This also holds in the regressions where the endogenous RHS variable is WIC use when no infants or pregnant women are present.

We now turn to the regressions including family fixed effects. As before, the upper panel of table 14 contains results with the endogenous RHS variable being the share of time in the sample spent on WIC. The lower panel of table 14 contains similar results when the endogenous RHS variable is the share of time spent on WIC in a household when no other infants and no pregnant women were present. Here we see that having been on WIC for a large share of time in the sample is associated with a statistically significant increase in the probability of using Food Stamps and Medicaid.

3.6 Conclusion

Using data from the NLSY, we find mixed evidence about the effects of WIC on various children's outcomes. In regressions with family fixed effects, we find, as have others, that WIC use increases the birth weight of the child and decreases the likelihood of being born weighing less than 2.5 kilos. The results for the effects of WIC use on test scores is more mixed. Contemporaneous WIC use has a significant negative effect on the Motor and Social Development Scale test score of children in 2SLS analysis. In other 2SLS analysis, we find that the effects of WIC are generally insignificant for other test scores. These inconclusive results are not too surprising as the first stage regressions are weak. In OLS regressions that control for family fixed effects, we find WIC use is associated with a mild increase in Peabody Individual Vocabulary Test-Revised scores and no effect on Motor and Social Development Scale scores. There is a huge negative effect on the Digit Span Subscale of

[27] Again, more information about the 2SLS results is in appendix tables A-2 and A-3.

the Wechsler Intelligence Scale for Children score.[28] In family fixed effect regressions, we also find that WIC use is associated with a higher probability of using Medicaid, and of using Food Stamps, which suggests that the referrals among these programs are effective at increasing take-up. Thus, while we find evidence of positive effects of WIC on pregnancy outcomes, confirming many previous studies, the evidence for longer term outcomes is mixed and rather weak.

[28]However, for the pool of children who took this test, we do not know whether they received WIC before age three.

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Table 3.1: Means of WIC Use

Variable	Mean	S. Dev.	Observations
On WIC Now	.16	.36	3550
On WIC Before Born	.12	.33	1102
On WIC at Birth	.26	.44	1706
On WIC at Age 1	.27	.45	1808
On WIC at Age 2	.20	.40	1909
On WIC at Age 3	.18	.39	1971
On WIC at Age 4	.16	.37	2034
On WIC Now No Pregnant Woman or Infant	.12	.32	3550
Share of Total Time in Sample On WIC	.20	.34	3550

Notes: Means and standard deviations of WIC participation variables for the full panel. First column has means and second columns has standard deviations.

Table 3.2: Means of Pregnancy Outcomes for 1994 Cross Section

	On WIC		Not On WIC	
Variable	Mean	S. Dev.	Mean	S. Dev.
Prenatal Visits	.99	.10	.99	.07
Smoked Less (Smoked)	.61	.49	.61	.49
Smoked Less Dr.'s Advice (Smoked Less)	.58	.49	.43	.49
Drank Prenatally	.38	.48	.48	.50
Drank Less (Drank)	.72	.45	.81	.40
Drank Less Dr.'s Advice (Drank Less)	.38	.49	.29	.45
Vitamins	.92	.27	.96	.21
Vitamins Dr.'s Advice	.96	.20	.96	.20
Cut Calories	.29	.45	.24	.43
Cut Calories Dr.'s Advice (Diet)	.62	.49	.60	.49
Less Salt	.52	.50	.51	.50
Less Salt Dr.'s Advice (Less Salt)	.59	.49	.55	.50
Weight Change of Mother	31	16	32	14
Birth Weight	117	23	118	22
Low Birth-Weight	.10	.29	.08	.27
Child Breast-Fed	.38	.48	.53	.50
Gestation of Child	39	2	39	2
Child Got All DPT Shots	.92	.28	.91	.28

Notes: Means and standard deviations of pregnancy measures for children with information about prenatal WIC use who are also in 1994 cross section. The first column has means and second column has standard deviations for children on WIC in 1994. Third column has means and fourth column has standard deviations for children not on WIC in 1994. 649 children were in families on WIC in 1994 and 3823 children were in families not on WIC in 1994.

Table 3.3: Means of Test Scores for Full Panel

	On WIC		Not On WIC	
Variable	Mean	S.Dev.	Mean	S.Dev.
PIAT Math	96	14	100	13
PIAT Reading	101	13	104	14
PIAT Cognitive	104	12	105	12
PPVT Score	78	19	90	21
Digit Span Subscale of IQ	9	3	10	3
Behavioral Problems Index	107	17	104	15
Motor and Social Development	98	15	101	15
Home Inventory Test †	886	176	981	159

Notes: Means and standard deviations of test scores. Tests are not given every year to all children. First column has means and second column has standard deviations for children on WIC. Third column has means and fourth column has standard deviations for children not on WIC. Means for the entire sample.
† HMS scores are for 1992.

Table 3.4: Means of Control Variables for 1994 Cross Section

	On WIC		Not On WIC	
Variable	Mean	S.Dev.	Mean	S.Dev.
Age of Child	3.88	2.4	5	2.5
Black	.48	.50	.24	.42
Hispanic	.27	.44	.20	.40
Poor White Oversample	.03	.18	.04	.18
Never Married	.30	.46	.10	.30
Divorced	.27	.44	.17	.37
Mother's Age at First Birth	22	4.4	24	4.4
AFQT Score of Mother	504	192	666	208
Years of School of Mother	12	2	13	2
Urban	.80	.40	.82	.38
Birth Order of Child	2.7	1.5	2.1	1.1
Food Stamps	.62	.48	.15	.36
AFDC	.39	.49	.11	.31
Medicaid	.60	.49	.15	.36
Head Start Ever for Child	.24	.42	.15	.36
Family Size	4.8	1.7	4.3	1.3
Under 1.85 of Pov. Line	.59	.49	.24	.43
% of Pop. Under 5 †	8	1	8	1
% of Pop. Black †	18	15	14	14
% Female Headed HH †	13	4	12	4
% of Pop. with at Least High School	72	9	74	9
Median Income (1990 $) †	32757	75087	35731	8445

Notes: Means and standard deviations of sample characteristics for 1994 cross section. First column has means and second column has standard deviations for children on WIC. Third column has means and fourth column has standard deviations for children not on WIC. 649 children were in families on WIC in 1994 and 3823 were in families not on WIC in 1994.
† County level variables from the 1990 Census.

Table 3.5: Means of WIC Policy Variables for 1994 Cross Section

	On WIC		Not On WIC	
Variable	Mean	S. Dev.	Mean	S. Dev.
Clinics per Capita	.034	.021	.032	.020
Ave. Food $ (1990 $) per WIC Case	25.3	3.6	25.6	3.4
Ave. Admin. $ (1990 $) per WIC Case	9.4	.7	9.4	.8
% of WIC Case-Load Children	50	6	49	7
% of WIC Case-Load Infants	27	3	27	4
Prob. Live < 10 Miles from a clinic †	.52	.28	.45	.25
Prob. live 10-20 Miles from a clinic †	.15	.22	.16	.20
Prob. Live > 20 Miles from a clinic †	.32	.26	.39	.26

Notes: Means and standard deviations of WIC policy variables for 1994 cross section. The first column has means and second column has standard deviations for children on WIC. Third column has means and fourth column has standard deviations for children not on WIC. 649 children were in families on WIC in 1994 and 3823 children were in families not on WIC in 1994.
† County level variables. Details are in text.

Table 3.6: OLS Regressions of Outcomes on WIC — Various Controls

Outcome Variable	Coef.	SE	Families	R-Squared	Family FE	Family Controls	County Controls
PIAT Math	-5.0	.9	1409	.01	N	N	N
PIAT Math	-.5	.9	1409	.21	N	Y	N
PIAT Math	-.3	.9	1409	.21	N	Y	Y
PIAT Math	2.4	1.5	1409	.46	Y	Y	N
PPVT Score	-13.8	2.0	1819	.04	N	N	N
PPVT Score	-1.0	2.0	1819	.23	N	Y	N
PPVT Score	-3.2	1.1	1819	.67	N	Y	Y
PPVT Score	-.16	4.2	1819	.17	Y	N	N
Low Birth-Weight	-.05	.07	808	.004	N	N	N
Low Birth-Weight	.002	.03	808	.06	N	Y	N
Low Birth-Weight	.002	.03	808	.07	N	Y	Y
Low Birth-Weight	-.05	.07	808	.47	Y	N	N

Notes: Regressions are OLS regressions of different child outcomes on WIC use and varying controls. In each panel, successively more controls are added. The first column is the coefficient on WIC, the second the SE, the third the number of observations, the fourth the R-Squared and the next three columns describe the controls. All regressions with controls include year dummies. All regressions with PIAT or PPVT score also control for the age at which the test was administered. Last regression in each panel has family fixed effects.

Table 3.7: First Stage Regression for WIC Use — Full Specification

Variable	Coef.	SE
Black	0.04	0.02
Hispanic	0.07	0.02
Poor White Sample	0.01	0.03
Never Married	0.14	0.02
Divorced	0.07	0.02
Family Size	0.04	5.0E-03
Urban	-0.03	0.02
AFQT Score of Mother	-1.6E-04	3.7E-05
Mother's Age at First Birth	0.05	0.02
Mother's Age at F B Squared	-9.6E-04	4.0E-04
PPVT Test Standardized Age	-2.6E-03	2.0E-03
% of Pop Under 5 †	-5.0E-03	0.01
% Female Headed Households†	0.04	0.23
%†Median Income (1990 $)	-3.0E-06	1.2E-06
% of Pop. Black †	0.04	0.09
% of Pop HS Dropouts †	-0.27	0.15
Clinics Per Capita ‡	-0.06	0.37
Ave Food $ (1990 $) per WIC Case ‡	-2.4E-03	2.2E-03
% of WIC Case-Load Infants ‡	-0.08	0.4
% of WIC Case-Load Children ‡	0.22	0.23
Prob. Live < 10 Miles from a Clinic‡	-0.06	0.04
Prob. Live 10-20 Miles from a Clinic‡	-0.06	0.04

Notes: Coefficients in first stage regression of WIC use on controls and instruments for sub sample with PIAT math scores. First column contains coefficient on row variable and second column has the SE. All regressions include year fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level.

† County level variables from 1990 Census.

‡ WIC participation numbers are at the state by year level and distances lived from a WIC clinic are at county level.

Table 3.8: 2SLS Regressions of Pregnancy Outcomes on WIC Use

Outcome	Mean of Outcome (Mean WIC)	Coef. on WIC 2SLS (SE)	Passes Over-Id.
Smoked Less	0.60	-0.08	N
	(0.18)	(0.5)	
Smoked Less Dr.'s Advice	0.46	0.67	Y
	(0.18)	0.49	
Drank Prenatally	0.79	.49	Y
	(0.18)	(0.35)	
Drank Less	0.79	-0.60	Y
	(0.18)	(0.38)	
Drank Less Dr.'s Advice	0.32	-0.01	Y
	(0.18)	(0.43)	
Vitamins	0.95	-0.14	Y
	(0.18)	(0.16)	
Vitamins Dr.'s Advice	0.96	0.25	Y
	(0.18)	(0.17)	
Cut Calories	0.26	0.07	Y
	(0.18)	(0.26)	
Cut Calories Dr.'s Advice	0.62	-0.31	Y
	(0.18)	(0.68)	
Less Salt	0.52	-0.26	Y
	(0.18)	(0.36)	
Less Salt Dr.'s Advice	0.57	0.21	Y
	(0.18)	(0.33)	
Weight Change of Mother	31.63	-5.3	Y
	(0.18)	(10.74)	
Birth Weight	118.17	-2.8	Y
	(0.18)	(14.5)	
Low Birth-Weight	0.08	0.12	Y
	(0.18)	(0.14)	
Child Breast-Fed	0.50	-0.85	N
	(0.18)	(0.30)	
Gestation	38.59	1.0	Y
	(0.18)	(1.1)	

Notes: Regressions are 2SLS regressions of pregnancy outcomes for children on predicted WIC-use. For details see text. Regressions have contemporaneous WIC-use on RHS. Each pair of rows represents one regression. Column one contains the mean for the program (mean for the WIC variable). Column two contains the coefficient on WIC use in the 2SLS regressions with the standard error below in parentheses. Column three indicates whether specification passed the over-identification test. All regressions include year fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level.

Table 3.9: OLS Regressions of Pregnancy Outcomes on WIC Use during Pregnancy

	Full Sample			Restricted Sample		
Outcome Variable	Coef.	SE	Obs.	Coef.	SE	Obs.
Smoked Less (Smoked)	-.11	.11	389	0	.38	5
Smoked Less Dr.'s Advice (Smoked Less)	.16	.22	255	NA	NA	NA
Drank Prenatally	-.11	.09	1119	-.13	.25	14
Drank Less (Drank)	-.16	.12	697	NA	NA	NA
Drank Less Dr.'s Advice (Drank Less)	-.13	.25	569	NA	NA	NA
Vitamins	-.03	.06	1120	-.26	.13	17
Vitamins Dr.'s Advice	.04	.06	1073	.23	.13	17
Cut Calories	.05	.09	1119	-.26	.24	17
Cut Calories Dr.'s Advice (Diet)	.27	.25	258	NA	NA	NA
Less Salt	-.16	.09	1119	.13	.14	17
Less Salt Dr.'s Advice (Less Salt)	-.13	.18	563	NA	NA	NA
Weight Change of Mother	.53	2	1102	4	4	17
Birth Weight	7.1	4.2	1114	6	10	17
Low Birth-Weight	-.11	.06	1116	0	.17	17
Child Breast-Fed	-.03	.06	1027	-.17	.16	17
Gestation	.84	.39	1115	-.13	.9	17

Notes: Regressions are OLS regressions of pregnancy outcomes for children on whether or not the mother was on WIC during conception with family fixed effects. Columns 1-3 are regressions with the full sample, columns 4-6 are restricted to children in families where at least one child in the family was on WIC at some age and at least one was not. First column contains coefficient on WIC variable, second column is SE of WIC coefficient and third column is number of families for the sample. Columns four through six represent the same coefficient, SE and number of families respectively for the restricted sample. All regressions include family fixed effects, those in columns 1-3 also contain year and age dummies. Columns with NA are ones without enough observations to estimate an effect.

Table 3.10: 2SLS Regressions of Test Scores on WIC Use

Test Score	Mean of Test Score (Mean WIC)	Coef. on WIC 2SLS (SE)	Passes Over-Id.
PIAT Math	99.4	-9.8	Y
	(0.18)	(12.1)	
PIAT Cognitive	105.6	6.4	N
	(0.18)	(8.8)	
PIAT Reading	104.0	-2.5	N
	(0.18)	(10.6)	
PPVT Score	59.0	-10.9	N
	(0.18)	(12.2)	
Digit Span Subscale of IQ	9.5	-0.17	N
	(0.18)	(4.0)	
Behavioral Problems Index	104.3	-3.5	N
	(0.18)	(12.6)	
Motor and Social Development	100.5	-21.8	Y
	(0.18)	(8.1)	
Home Inventory Test	967	-98.5	N
	(0.18)	(73.9)	

Notes: Regressions are 2SLS regressions of test scores for children on predicted WIC-use. For details see text. Contemporaneous WIC-use on RHS. Each pair of rows represents one regression. Column one contains the mean for the test score (mean for the WIC variable). Column two contains the coefficient on WIC use in the 2SLS regressions with the standard error below in parentheses. Column three indicates whether specification passed the over-identification test. All regressions include year fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level.

Table 3.11: 2SLS Regressions of Test Scores on WIC Use if No Pregnant Women or Infants in HH

Test Score	Mean of Test Score (Mean WIC)	Coef. on WIC 2SLS (SE)	Passes Over-Id.
PIAT Math	99.4	-11.0	Y
	(0.13)	(12.9)	
PIAT Cognitive	105.6	11.2	N
	(0.13)	(12.1)	
PIAT Reading	104.0	0.45	N
	(0.13)	(11.6)	
PPVT Score	59.0	-5.8	N
	(0.13)	(12.4)	
Digit Span Subscale of IQ	9.5	0.34	N
	(0.13)	(6.7)	
Behavioral Problems Index	104.3	-6.0	N
	(0.13)	(12.9)	
Motor and Social Development	100.5	-25.4	Y
	(0.13)	(9.9)	
Home Inventory Test	967	-101.8	N
	(0.13)	(73.8)	

Notes: Regressions are 2SLS regressions of test scores for children on predicted WIC use. For details see text. Contemporaneous WIC use when no pregnant women or infants in HH on RHS. Each pair of rows represents one regression. Column one contains the mean for the test score (mean for the WIC variable). Column two contains the coefficient on WIC use in the 2SLS regressions with the standard error below in parentheses. Column three indicates whether specification passed the over-identification test. All regressions include year fixed effects and demographic and county controls. SE's corrected for groupwise heteroscedasticity at the family level.

Table 3.12: OLS Regressions of Test Scores on Share of Time Spent on WIC

Test	Coef.	SE	Families
Share of Time on WIC			
PIAT Math	4.9	4.0	1409
PIAT Cognitive	-2.1	5.7	982
PIAT Reading	-4.1	3.9	1427
PPVT Score	6.7	4.6	1807
Digit Span Subscale of IQ	-6.7	3.0	751
Behavioral Problems Index	-.30	2.7	1687
Motor and Social Development	1.1	2	1804
Home Inventory Test	-24.7	23.5	1997
Share of Time on WIC			
If No Pregnant Women or Infants in HH			
PIAT Math	6.3	4.0	2437
PIAT Cognitive	-1.0	5.5	1353
PIAT Reading	1.9	3.8	2430
PPVT Score	8.4	4.2	2736
Digit Span Subscale of IQ	-6.7	3.0	803
Behavioral Problems Index	2.8	2.6	3675
Motor and Social Development	0.8	1.8	3730
Home Inventory Test	-19.8	21.8	4486

Notes: Regressions are OLS regressions of test scores for children on the share of time in sample the child spent on WIC with family fixed effects. Regressions in top panel have share of time on WIC as RHS variable, regressions in bottom panel have share of time on WIC when no pregnant women or infants was in the HH as RHS variable. First column contains coefficient on WIC variable, second column is SE of WIC coefficient and third column is number of families. Regressions include family fixed effects, year dummies and age dummies.

Table 3.13: 2SLS Regressions of Other Program Use on WIC Use

Program	Mean of Program (Mean WIC)	Coef. on WIC 2SLS (SE)	Passes Over-Id.
WIC Use			
Family Gets Food Stamps	0.22	0.62	N
	(0.18)	(0.18)	
Child on Medicaid	0.22	0.28	N
	(0.18)	(0.18)	
Child Ever Used Head Start †	0.12	0.51	Y
	(0.18)	(0.33)	
WIC Use			
No Pregnant Women or Infants			
Family Gets Food Stamps	0.22	0.71	N
	(0.13)	(0.21)	
Child on Medicaid	0.22	0.33	N
	(0.13)	(0.21)	
Child Ever Used Head Start †	0.12	0.76	Y
	(0.13)	(0.46)	

Notes: Regressions are 2SLS regressions of program use by children on predicted WIC-use. For details see text. Regressions in top panel have contemporaneous WIC-use on RHS, regressions in bottom panel have contemporaneous WIC-use if no pregnant women or infants are in the HH on RHS. Each pair of rows represents one regression. Column one contains the mean for the program (mean for the WIC variable). Column two contains the coefficient on WIC-use in the 2SLS regressions with the standard error below in parentheses. Column three indicates whether specification passed the over-identification test. All regressions include year fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level.

† Regressions for ever having used Head Start restricted to 5 year olds.

Table 3.14: OLS Regressions of Other Program Use on Share of Time Spent on WIC

Program	Coef.	SE	Families
Share of Time on WIC			
Family Gets Food Stamps	.07	.02	2517
Child on Medicaid	.06	.03	2337
Child Ever Used Head Start †	.01	.22	904

Notes: Regressions are OLS regressions of program use by children on the share of time in sample the child spent on WIC with family fixed effects. Regressions have share of time on WIC as RHS variable. First column contains coefficient on WIC variable, second column is SE of WIC coefficient and third column is number of families. Regressions include family fixed effects, year dummies, and age dummies.

† Regressions for ever having used Head Start restricted to 5 year olds.

Table A-1: 2SLS Regressions of Pregnancy Outcomes on WIC Use

Outcome	Mean of Outcome (Mean WIC)	F 1st Stage (p-Val.)	F Red. Form (p-Val.)	Coef. on WIC 2SLS (SE)	χ^2 Over-Id. (p-Val.)
Smoked Less	0.60	1.17	1.71	-0.078	10.50
	(0.18)	(0.32)	(0.12)	(0.514)	(0.06)
Smoked Less Dr.'s Advice	0.46	2.21	1.09	0.668	3.65
	(0.18)	(0.04)	(0.37)	(0.385)	(0.60)
Drank Prenatally	0.47	2.96	2.46	0.492	6.35
	(0.18)	(0.01)	(0.02)	(0.348)	(0.39)
Drank Less	0.79	2.07	2.02	-0.645	7.11
	(0.18)	(0.05)	(0.06)	(0.375)	(0.21)
Drank Less Dr.'s Advice	0.32	1.68	0.69	-0.006	4.78
	(0.18)	(0.12)	(0.66)	(0.429)	(0.57)
Vitamins	0.95	2.94	0.28	-0.143	0.54
	(0.18)	(0.01)	(0.95)	(0.163)	(0.99)
Vitamins Dr.'s Advice	0.96	2.76	1.43	0.246	4.22
	(0.18)	(0.01)	(0.20)	(0.168)	(0.65)
Cut Calories	0.26	2.89	0.92	0.071	6.49
	(0.18)	(0.01)	(0.48)	(0.261)	(0.26)
Cut Calories Dr.'s Advice	0.62	0.54	0.51	-0.309	3.18
	(0.18)	(0.78)	(0.80)	(0.676)	(0.67)
Less Salt	0.52	2.93	1.07	-0.259	5.88
	(0.18)	(0.01)	(0.38)	(0.360)	(0.32)
Less Salt Dr.'s Advice	0.57	3.30	0.81	0.212	4.90
	(0.18)	(0.00)	(0.57)	(0.330)	(0.43)
Weight Change of Mother	31.63	2.89	0.90	-5.320	6.99
	(0.18)	(0.01)	(0.49)	(10.747)	(0.22)
Birth Weight	118.17	2.94	0.64	-2.821	4.28
	(0.18)	(0.01)	(0.70)	(14.493)	(0.64)
Low Birth-Weight	0.08	6.71	0.15	0.115	0.57
	(0.18)	(0.00)	(0.99)	(0.136)	(1.00)
Child Breast-Fed	0.50	6.71	5.79	-0.851	77.81
	(0.18)	(0.00)	(0.00)	(0.303)	(0.00)
Gestation	38.59	6.71	0.85	0.998	15.96
	(0.18)	(0.00)	(0.53)	(1.144)	(0.01)

Notes: Regressions are 2SLS regressions of outcomes for children on WIC-use while the mother is pregnant. For details see text. Each pair of rows represents one regression. Column 1 contains mean of outcome variable, with the WIC mean for the subsample below in parentheses. Column 2 contains the F statistic for excluding instruments in the first stage regression of WIC-use on all instruments and controls with the p-value below in parentheses. Column 3 contains the F statistic for excluding the instruments from a reduced form regression of the outcome on the instruments and all controls with the p-value below in parentheses. Column 4 contains the coefficient on WIC use in the 2SLS regressions with the standard error below in parentheses. Finally, column 5 contains the χ^2 statistic for an over-identification test with the p-value below. All regressions include year fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level.

Table A-2: 2SLS Regressions of Outcomes on WIC Use

Outcome	Mean of Outcome (Mean WIC)	F 1st Stage (p-Val.)	F Red. Form (p-Val.)	Coef. on WIC 2SLS (SE)	χ^2 Over-Id. (p-Val.)
PIAT Math	99.43	1.98	0.96	-9.796	8.06
	(0.18)	(0.07)	(0.45)	(12.126)	(0.15)
PIAT Cognitive	105.62	2.84	1.65	6.357	10.51
	(0.18)	(0.01)	(0.13)	(8.776)	(0.06)
PIAT Reading	103.97	2.25	2.69	-2.487	15.64
	(0.18)	(0.04)	(0.01)	(10.618)	(0.02)
PPVT Score	58.96	4.50	2.41	-10.932	18.44
	(0.18)	(0.00)	(0.03)	(12.225)	(0.00)
Digit Span Subscale of IQ	9.54	1.98	3.39	-0.173	17.17
	(0.18)	(0.07)	(0.00)	(4.030)	(0.00)
Behavioral Problems Index	104.27	3.14	0.95	-3.525	9.82
	(0.18)	(0.00)	(0.46)	(12.635)	(0.08)
Motor and Social Development	100.48	4.34	2.35	-21.826	4.51
	(0.18)	(0.00)	(0.03)	(8.089)	(0.48)
Home Inventory Test	967.06	3.56	2.90	-98.452	16.52
	(0.18)	(0.00)	(0.01)	(73.895)	(0.01)
Family Gets Food Stamps	0.22	6.47	2.93	0.625	22.68
	(0.18)	(0.00)	(0.01)	(0.176)	(0.00)
Child on Medicaid	0.22	4.99	2.07	0.280	32.69
	(0.18)	(0.00)	(0.05)	(0.178)	(0.00)
Child Ever Used Head Start	0.12	1.88	1.27	0.506	3.63
	(0.18)	(0.08)	(0.27)	(0.328)	(0.73)

Notes: Regressions are 2SLS regressions of outcomes for children on predicted WIC-use. For details see text. Each pair of rows represents one regression. Column 1 contains the outcome variable's mean, with WIC mean for the subsample below in parentheses. Column 2 contains the F statistic for excluding the instruments in the first stage regression of WIC use on all the instruments and controls with the p-value below in parentheses. Column 3 contains the F statistic for excluding the instruments from a reduced form regression of the outcome on the instruments and all controls with the p-value below in parentheses. Column 4 contains the coefficient on WIC use in the 2SLS regressions with the standard error below in parentheses. Finally, column 5 contains the χ^2 statistic for an over-identification test with the p-value below. All regressions include year fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level.

Table A-3: 2SLS Regressions of Outcomes on WIC Use in Absence of Pregnant Women and Infants in Household

Outcome	Mean of Outcome (Mean WIC)	F 1st Stage (p-Val.)	F Red. Form (p-Val.)	Coef. on WIC 2SLS (SE)	χ^2 Over-Id. (p-Val.)
PIAT Math	99.43	2.16	0.96	-11.001	7.99
	(0.13)	(0.04)	(0.45)	(12.924)	(0.16)
PIAT Cognitive	105.62	2.07	1.65	11.220	9.62
	(0.13)	(0.05)	(0.13)	(12.063)	(0.09)
PIAT Reading	103.97	2.29	2.69	0.451	15.77
	(0.13)	(0.03)	(0.01)	(11.631)	(0.02)
PPVT Score	58.96	5.23	2.41	-5.826	19.41
	(0.13)	(0.00)	(0.03)	(12.444)	(0.00)
Digit Span Subscale of IQ	9.54	0.92	3.39	0.335	17.09
	(0.13)	(0.48)	(0.00)	(6.675)	(0.00)
Behavioral Problems Index	104.27	3.75	0.95	-6.033	9.52
	(0.13)	(0.00)	(0.46)	(12.891)	(0.09)
Motor and Social Development	100.48	4.55	2.35	-25.426	5.09
	(0.13)	(0.00)	(0.03)	(9.856)	(0.40)
Home Inventory Test	967.06	4.88	2.90	-101.847	16.32
	(0.13)	(0.00)	(0.01)	(73.738)	(0.01)
Family Gets Food Stamps	0.22	6.92	2.93	0.707	28.91
	(0.13)	(0.00)	(0.01)	(0.209)	(0.00)
Child on Medicaid	0.22	5.65	2.07	0.333	30.86
	(0.13)	(0.00)	(0.05)	(0.207)	(0.00)
Child Ever Used Head Start	0.12	1.40	1.27	0.758	2.42
	(0.13)	(0.21)	(0.27)	(0.463)	(0.88)

Notes: Regressions are 2SLS regressions of outcomes for children on a dummy for WIC use when there is no infant or pregnant woman present. For details see text. Each pair of rows represents one regression. Column 1 contains mean of outcome variable, with WIC mean for the subsample below in parentheses. Column 2 contains the F statistic for excluding the instruments in the first stage regression of WIC use on all the instruments and controls with the p-value below in parentheses. Column 3 contains the F statistic for excluding the instruments from a reduced form regression of the outcome on the instruments and all controls with the p-value below in parentheses. Column 4 contains the coefficient on WIC use in the 2SLS regressions with the standard error below in parentheses. Finally, column 5 contains the χ^2 statistic for an over-identification test with the p-value below. All regressions include year fixed effects and demographic and county controls. SEs corrected for groupwise heteroscedasticity at the family level.